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IN-VESSEL COOLABILITY AND RETENTION OF A CORE MELT

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Center for Risk Studies and Safety
Departments of Chemical and Mechanical Engineering
University of California, Santa Barbara
Santa Barbara, CA 93106

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FURTHER CONSIDERATION OF SCENARIO ASPECTS

T.G. Theofanous and J.J. Sienicki

O.1 INTRODUCTION

The basic approach taken in the report is that thermal loads are bounded by those found in the final steady state, and that thin metal layers sufficient to produce a large amount of "focusing" cannot be expected in coincidence with a large molten pool under any physically reasonable relocation/meltdown scénario. This leads to a scenario-independence that is attractive in allowing a Quality Grade B-type assessment (see Appendix A), and it has received a favorable response from many of the reviewers. However, there have been concerns and questions expressed by several reviewers as well. The purpose of this appendix is to provide some additional perspectives that support further our approach. Consistent with this approach we limit ourselves to global, basic-principle-type arguments and certain specific calculations intended to demonstrate such physical behavior. A more focused discussion of all reviewers' comments on this topic is presented in Section O.4 at the end of this appendix.

From the information provided already in the report, it is abundantly clear that the only way to failure is by what we identified as the "focusing" problem due to a thin metal layer. Most of the reviewers' concerns on this topic of scenario-dependence (i.e., intermediate states) seem to focus on this "focusing" problem. We believe that this concern is undue, and we feel being mostly responsible for failing to provide an adequately complete perspective on this focusing problem and its potential consequences. In fact, the Extreme Parametric (Figure 7.16) gives the impression that margins rapidly deteriorate as the metal layer thickness decreases even further, notwithstanding the already mentioned, but unquantified, builtin conservatism of ignoring the radial temperature gradients within it. What is missing is consideration of the effect of oxidic pool height and of 2D conduction effects in the vessel wall. Unlike the radial gradient problem, both of these effects are quantifiable and create a much more realistic perspective and a more restrictive envelope on potential failure conditions even from a strictly parametric point of view (i.e., consideration of arbitrarily thin metal layers). This first-line of reasoning is developed in Section O.2 below. We proceed, then, in Section O.3 to shed further light on it by examining certain key features of the heatup/meltdown/relocation transient in the AP600 geometry. We find that the massive reflector and core support plate components of the lower internal package (see Table 7.2) dominate the behavior to such an extent that a reasonable timeline of the accident progression can be constructed. Finally, in Section O.4 the reviewers' suggested scenarios are revisited and evaluated vis-a-vis this new information. We conclude that no physically meaningful circumstances leading to failure through "focusing" could be identified. Furthermore, as explained in Section O.3, the major relocation events were found to involve moderately superheated oxidic melts confirming the applicability and completeness of the impingement assessments in Chapter 8 and Appendix H.

Finally, having gone so far, it is important to examine this extreme "focusing" problem completely; that is, including the consequences of failure. The random nature of boiling crisis (as found in ULPU), natural asymmetries in the thermal loading, and the very geometry of ablation (see next section) ensure that wall meltthrough will occur in one or more highly localized regions, through which the thin metal layer will dribble out into the reactor cavity, without jeopardizing the structural integrity of the lower head as a whole. This, of course, will leave the oxidic debris on the inside, most of it in the lower head as a pool (this has to be true if the thermal load on the metal layer is to be sufficient to cause failure—see Figure O.1 in the next section), and any remaining in the core region stabilized by the vessel reflooding following failure. Thus, in effect, the "focusing" phenomenon is unable to jeopardize the in-vessel retention function, even if according to calculation it appears to meet the thermal failure criteria. In a similar vein, it can be understood that any jet impingement resulting from sideways failure of the reflector (which is the dominant mode as explained in Section O.3) would be above the cylindrical section to lower head junction and again highly localized. As shown in Chapter 8 and Appendix H highly extreme conditions (physically unreasonable) are required to penetrate the wall; here, in addition, we propose that even if such failure were to occur it would be to a large extent inconsequential to the in-vessel retention function, and in fact it might even help stabilize some of the debris in the core region, due to the vessel reflood once the breech occurred.

O.2 SOME PERSPECTIVES ON THE "FOCUSING" PROBLEM

The drastic reduction of thermal loads from the Extreme Parametric values obtained by reducing the size (height) of the oxidic pool has been illustrated in Appendix P (Figure P.4). Here, we combine this effect with a parametric variation on the metallic layer height to obtain a fuller idea of the failure envelope, as illustrated in Figure O.1. This figure should be viewed with the understanding that small values of H_ℓ are not consistent (not combinable) with large values of H (this is schematically illustrated in Figure O.2), and that the fluxes are likely to be very conservative, not only because of the choice of $\dot{Q}$ and ϵ values, but also in ignoring radial temperature gradients within the layer itself, as explained already in Chapter 5.

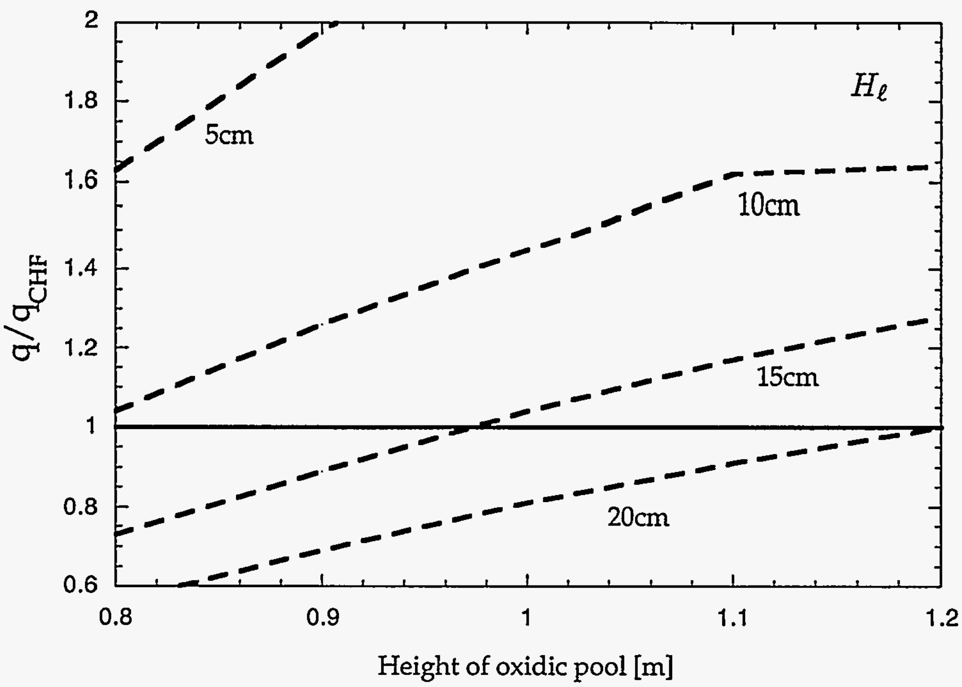


Figure O.1. The failure envelope for the "focusing" mechanism. Decay power density is 1.4 MW/m^3 and emissivity 0.45 both at the extreme conservative values.

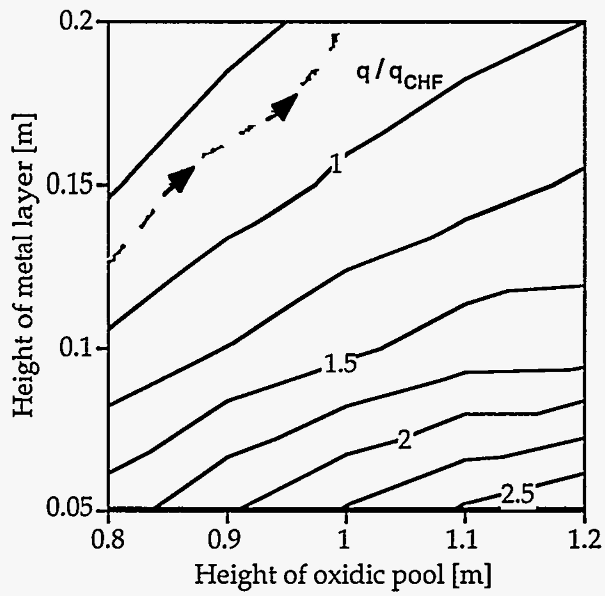


Figure O.2. The information in Figure O.1 replotted in the $H_\ell \times H$ space. The trajectory shown is to illustrate the simultaneous increase in both H_ℓ and H through the meltdown/relocation scenario.

Regarding 2D conduction effects in the wall, the results for three representative cases are shown in Figures O.3 through O.5. They were obtained with a full (2D) treatment of conduction in the wall, and the correct fluxes above (radiation) and below (oxidic pool convection) the metal layer region. These calculations were run in the manner described in the consideration of 2D effects in the addendum to Chapter 5. Note, however, that in the present case the effect of thinning down the metal layer ("focusing") overwhelms the dissipative effect of 2D conduction, such that the net result is a reduction in the outside peak heat flux (delivered to water) by at most $\sim$10%. For practical thicknesses the real mitigation comes from radial heat transport limitations within the layer itself, but as explained in this appendix, this is not (does not have to be) a crucial aspect of the IVR case.

O.3 KEY FEATURES OF THE MELTDOWN/RELOCATION BEHAVIOR

Besides the low power density, the AP600 design differs significantly from current PWRs by having a substantial ($\sim$13 cm-thick at the flats—see Figure O.6) stainless steel reflector as a core former, inside the core barrel. This reflector has a total mass of 40 tons (see Table 7.2), an 8% porosity due to the cooling holes that run through its length, and it sits on the core support plate, which, in turn, is hung from the upper vessel flange, as illustrated in Figure O.7. Its effect on the neutronics is to induce a much flatter radial power shape, as illustrated in Figure O.8, while its effect on the thermal hydraulics of severe accidents is to impose a very significant obstacle against a sidewards relocation of the core melt (as a path to the lower plenum). On the other hand, as in all PWRs core uncovery remains incomplete through the rapid oxidation phase, the lower portions of the core remain correspondingly cold, and there is a very significant heat sink associated with the core support plate (30 cm thick). In addition, in the AP600, there is a 30 cm length between the bottom of the fuel pellets and the top of the core plate, $\sim$15 cm of which is occupied (in the rods) with zirconium pellets (an additional substantive heat sink). Thus the downward relocation path for a melting core is formidable. We expect this path to be blocked by molten cladding (with up to $\sim$5% dissolved uranium) and the blockage to be robust, especially as long as the core support plate is supported by the secondary support system (the columns inside the lower plenum) from below. *As a consequence, the first relocation will occur after delayed failure of the reflector and core barrel at the upper side, and will be followed gradually by subsequent ones as the path opens more and more (downwards) by continuing melting of the reflector and core barrel.* This is fundamentally different than what occurred in TMI, where a relatively small oxidic pool could melt through the relatively thin baffle (of the core former) and discharge into the lower plenum through the so-called bypass region. Here, there is no such "open" bypass region, and the holes in the reflector would quickly

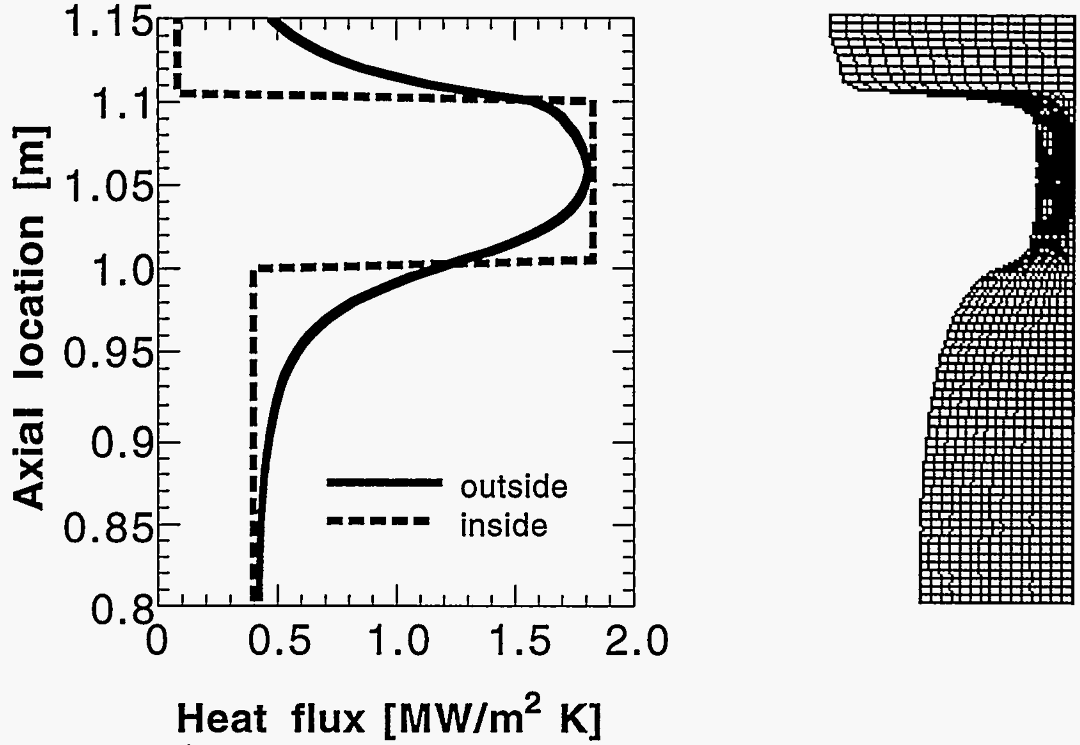


Figure O.3. The flux distributions and equilibrium wall erosion profile for a 10 cm metal layer on top of a 1 m deep oxidic pool.

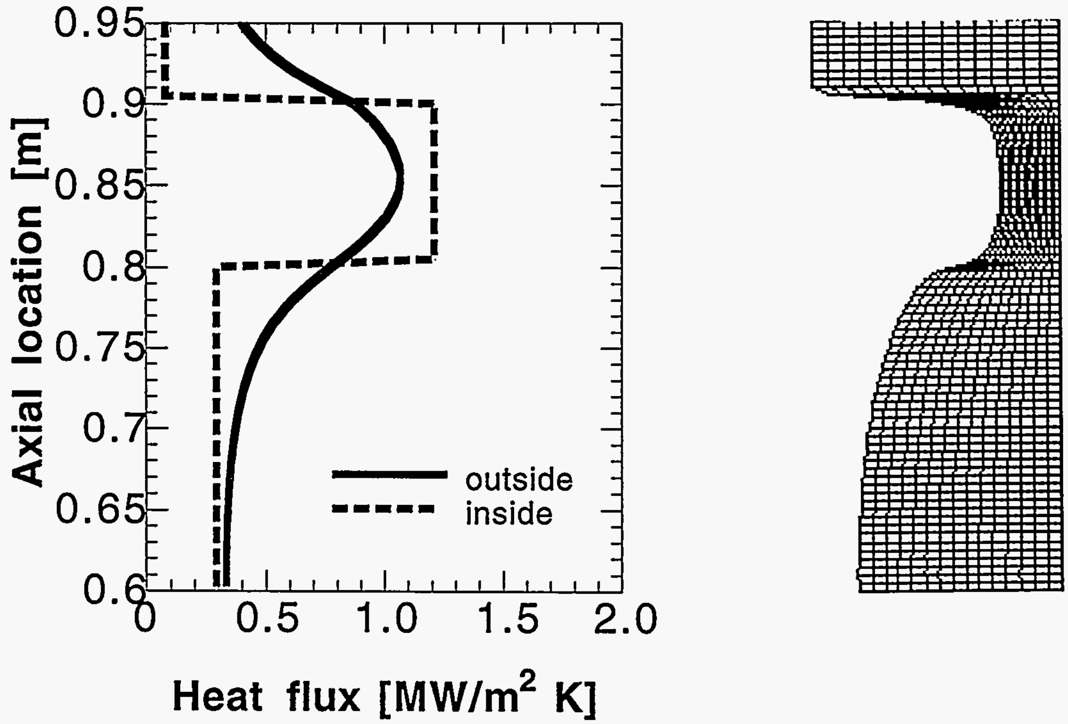


Figure O.4. The flux distributions and equilibrium wall erosion profile for a 10 cm metal layer on top of a 0.8 m deep oxidic pool.

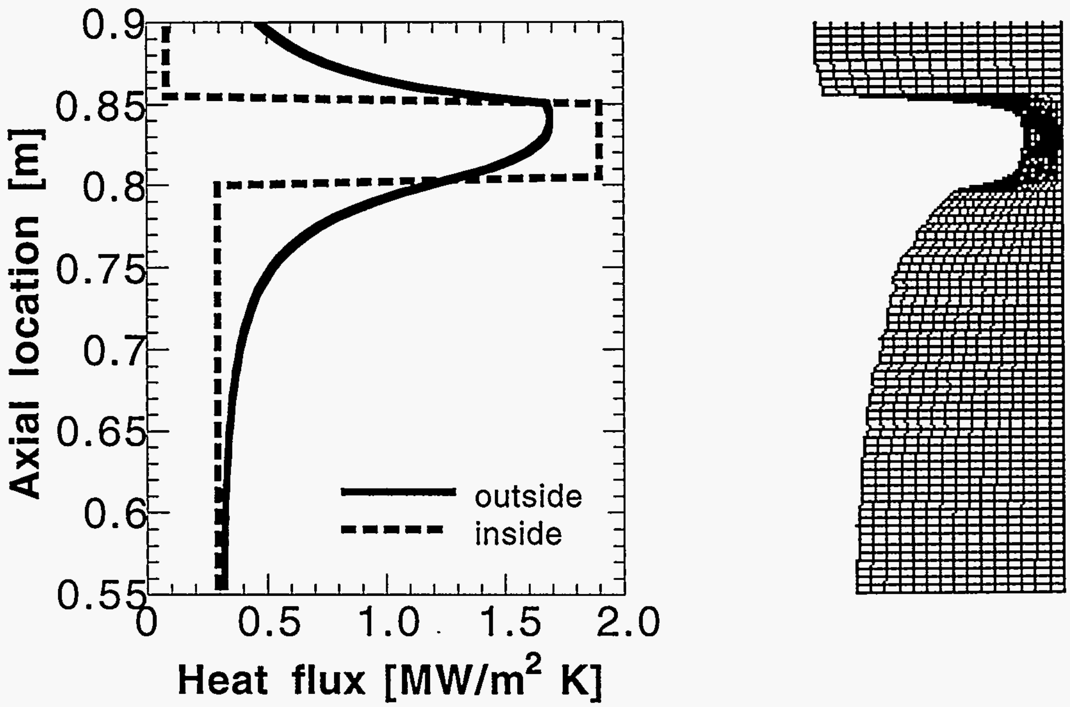


Figure O.5. The flux distributions and equilibrium for a 5 cm metal layer on top of a 0.8 m deep oxidic pool.

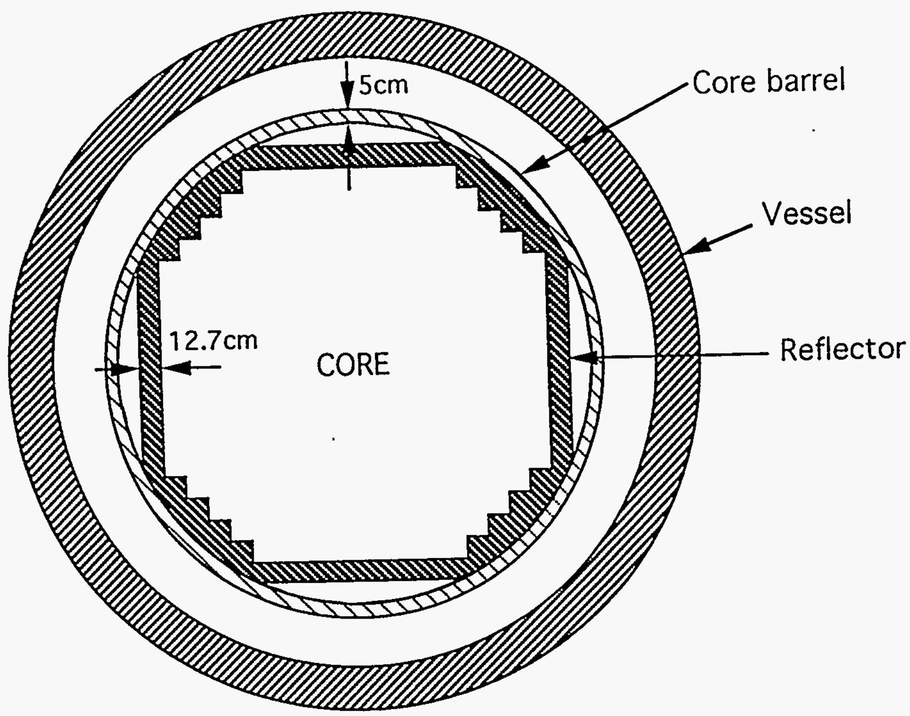


Figure O.6. The reflector and core barrel in the AP600 design.

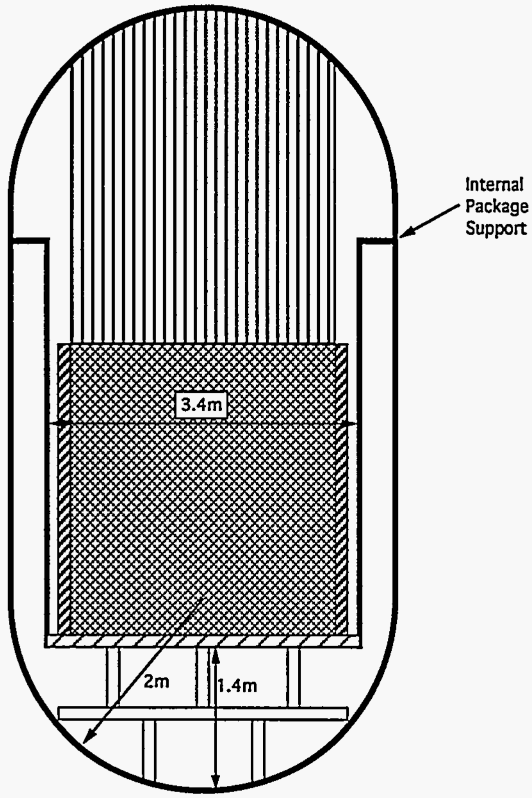


Figure O.7. The AP600-like design considered and some key dimensions.

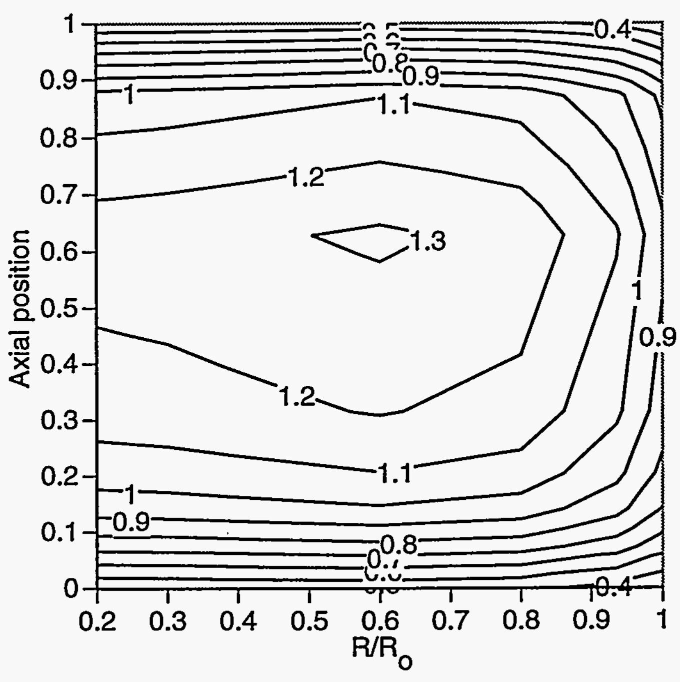


Figure O.8. The power distribution in an AP600-like core.

plug when they become accessible to the melt. The spaces between the flats and the core barrel in Figure O.6 are dead-ended at the bottom by the thick core support plate (30 cm). Around this pivotal idea we can build the broad terms of the relocation scenario and derive some major implications, as follows:

- A significant size molten pool will form in the core region prior to the first release. This results in significant smoothing of the high power region, as a consequence, to an average value of peaking of 1.02, for example, for a pool incorporating the upper 70% of the core.
- A largely separated configuration will form with any unoxidized cladding collected in the lower blockage, due to the great difference in melting points of metallic and oxidic components. Any late addition of the 2.5 tons of Zr in the upper portion of the cladding and the upper grid would be incorporated and dissolved easily in the oxide or otherwise drain from the sides into the lower core during the early development of the pool.
- An upwards peaked temperature distribution will be established in the reflector due to the history effect of pool formation dimensions, and radiation from the fuel rods (see power shapes in Figure O.8).
- Thermal and melt attack from the pool to the reflector occurs in a process very similar to the one considered for the lower head in this report. Some steel accumulates on the top of the oxidic pool. Failure of the flats and draining of this metallic melt into the four dead-end spaces—total capacity for ~10 tons. The presence of heat sinks at the boundaries of these regions, the low superheat of the metal, and the dimensions are favorable for refreezing.
- Core barrel heats up, loses strength, and finally melts through. The whole lower internals assembly would now be sitting on top of the secondary support system, while oxidic material is relocating into the lower plenum. Extensive breakup and thermal interactions would quickly deplete the lower plenum water, and the lower supports would be surrounded and eventually consumed by the melt. Maximum displacement allowed (by the core plate diameter of 3.4 m) to a distance of 1 m from the pole of the lower head. The volume of the thus created spherical segment is ~5 m^3, which corresponds to ~42% of the available oxidic debris in the base case analysis.
- Because of partial quenching in the lower plenum, thermal loads on the lower head do not build up until all water is vaporized and much of the initial oxidic debris is molten (any solid debris affords a significant heat sink). Meanwhile, further relocations occur with significant metallic content as the reflector and the quantities previously trapped between it and the core barrel continue to melt. Clearly, by the time we have 42% of the core in the lower plenum we have also a very significant quantity of relocated steel, well over the 15

cm needed (see Figure O.1) to obviate focusing concerns. And at this point the top of the pool is in contact with the massive core support plate. We call this the Lead-in Intermediate State (LIS) — see Figure O.9.

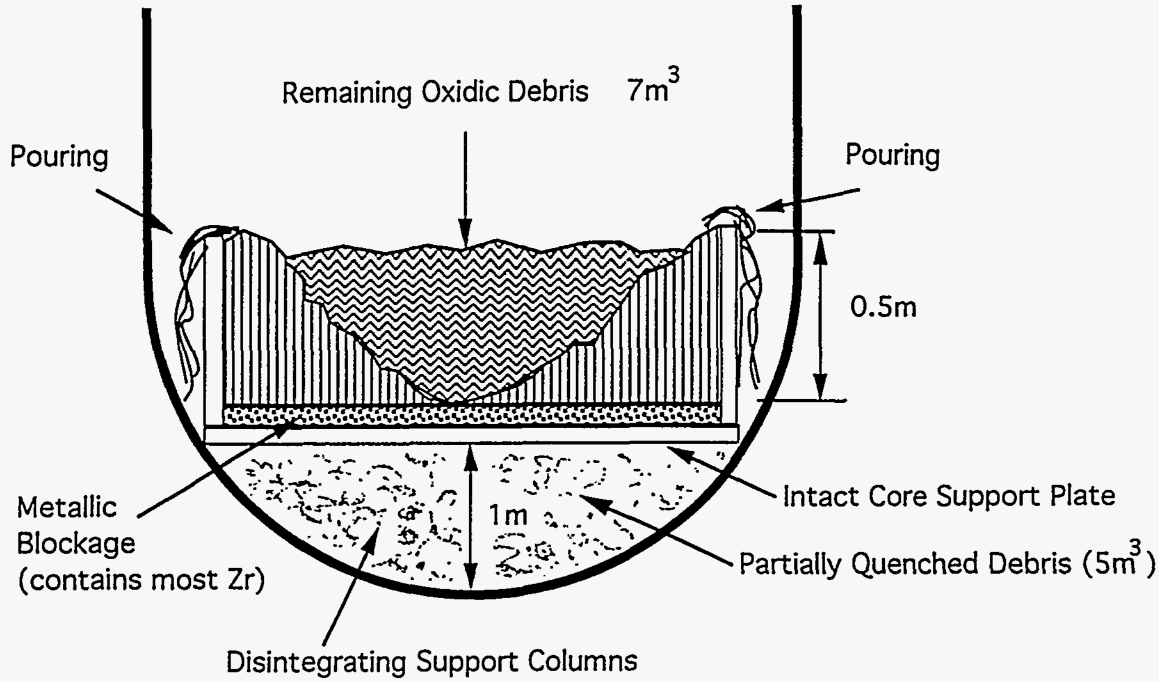


Figure O.9. Illustration of the Lead-in Intermediate State (LIS).

- Beyond the LIS, the oxidic debris continues to relocate through the sideways path, and the core plate continues to melt from below (primarily) into the metallic pool. Clearly, thermal loads through the metallic layer remain small, and so do those from the separated oxidic masses (one above and the other below the plate). Eventually the core plate is consumed, there is a layer inversion, and we have the Final Bounding State (FIBS) as envisioned by the Base Case in the report. Alternatively, the oxide melt from above may be gradually incorporated in the bottom pool by "lifting" the core plate due to buoyancy. Whichever is the case, the thermal loads to the reactor vessel remain benign and bounded by our base case results. In this fashion the metallic Zr is the last to be incorporated in the metallic pool. Thermal loads to the lower head are maximized when all internal phase changes have taken place, and the surface-to-volume ratio has been minimized, i.e., when the FIBS has been obtained.

There are three aspects in the above that require some further elaboration. One is the persisting integrity of the lower blockage. The second is concerned with the timing of reflector and core barrel failures. The third is about the lower plenum interactions in the pre-LIS time period. Each is examined in turn below.

1. The integrity of the lower blockage is due to the very high heat sink capability of the core support plate, supported also by heat rejection to water. As long as the water level allows direct contact, this occurs by boiling with the vapor escaping through the flow holes in the reflector; later on, cooling is by radiation, again to the water below. Downwards heat transfer from the molten pool is slight, and the blockage is only slightly heated (by any dissolved radionuclides). As mentioned already, the secondary supports prevent collapse of the core support plate even if it is heated sufficiently to allow a substantial radiative heat loss ($\sim$200 kW/m^2 at 1400 K).

2. An idea of the failure timing can be obtained as follows. Starting from the time of the rapid oxidation phase, which leaves most of the upper part of the core at $\sim$2800 K, we need about $\sim$30 minutes to reach the fully molten state ($\sim$ adiabatic heatup at $\sim$0.55 K/s, and latent heat of fusion), and another $\sim$5 minutes to obtain a superheat level consistent with a steady-state pool ($\sim$200 K). At this point the heat fluxes can be calculated (using the Steinberner-Reineke correlation for the vertical and lower boundary of a cylindrical pool, as well as the one discussed in this report for the upper boundary) as 1050 and 545 kW/m^2 for the top and vertical boundaries respectively. This heat flux split is not very sensitive to the height of the pool, expected to be in the range of 1 to 1.7 m, and the pool superheat is $\sim$200 K. Meltthrough at the top of the oxidic pool will occur at the flats (thinnest locations) and would require $\sim$15 minutes. This result was obtained by means of a simple model of the steel reflector subjected to 545 kW/m^2 on the front side, and with radiative loss over the back side. Earlier failure may occur in a highly localized fashion due to a steel layer accumulating on top; however, this will have a minor effect on the scenario since the oxide pool must have access to the core barrel to produce the first major relocation event. The systems effects of this timing is considered next, as it may have some implications on what follows.

3. According to MAAP calculations (in the AP600 PRA), in the case of major interest (3BE), the core reaches the rapid oxidation phase at $\sim$6400 s, at which time the operator actuates the cavity flooding system. Flooding to the top of the reactor vessel is calculated to occur at $\sim$10,000 s (accounting for some reactor water spilling directly into the cavity), or $\sim$12,000 s based only on the cavity flooding system. These available times of 60 minutes and 90 minutes must be compared to the time needed for the first relocation, as discussed above, to determine if vessel reflooding (through the broken DVI pipe found at this elevation)

can affect the sequence of events. We conclude that it cannot. On the other hand, the above timings establish the relevant time frame for IVR as 3 to 4 hours, as a minimum (choice of decay power density as used already), and 45 to 65 minutes (after operator action to initiate cavity flooding) as the time for the first relocation event. As shown in Figure M.6, this is quite adequate to provide effective flooding even with one line operating.

4. According to the above the pre-LIS relocation path is through the downcomer, and the melt comes down not as a coherent jet, but rather in a spread out stream (or small streams) splashed off the vessel wall. There is enough water in the lower plenum to quench about 2 m^3 of melt, thus we expect the pre-LIS period to involve easily about 5 m^3 of mixed solid/liquid debris in the lower plenum, and as such be characterized by low thermal loads to the lower head. Some small quantities of steel may evolve gradually over such a mixture, in these early stages of pool formation, but it could not produce significant focusing because it would be inadequately heated from below. Thus we expect LIS to be approached with an evolving pool situation, whereby the colder mixed-phase materials at the bottom are covered by subsequent relocation of hotter materials released from the core region by continuing relocation events.

In conclusion, we can now draw the timeline of the accident as shown in Figure O.10. The timing for FIBS is based on approximate overall energy requirement considerations. For example, just to heat up and melt the core support plate and the lower internal structures, a time of 31 minutes decay power is required. To vaporize the lower plenum water ($\sim$10 tons), we need another 23 minutes. For the reflector and lower two-thirds of the core barrel we may need anywhere from 20 to 40 minutes, depending on the heat up that occurred up to LIS. Accounting for some heat losses all this adds up to about one hour after the first relocation event.

O.4 CONSIDERATION OF REVIEWERS' SCENARIOS

The various scenarios suggested by the reviewers are summarized, in an easy-to-visualize form in Table O.1. We will use this table and the material presented above as a guide in evaluating them.

1. The Che2/Ola7 concern about high power density melt is not possible for the AP600 because of the major obstacle to relocation due to the reflector/core barrel, as explained above. The peaking factor of the first-relocating melt is very close to unity (i.e., 1.02).

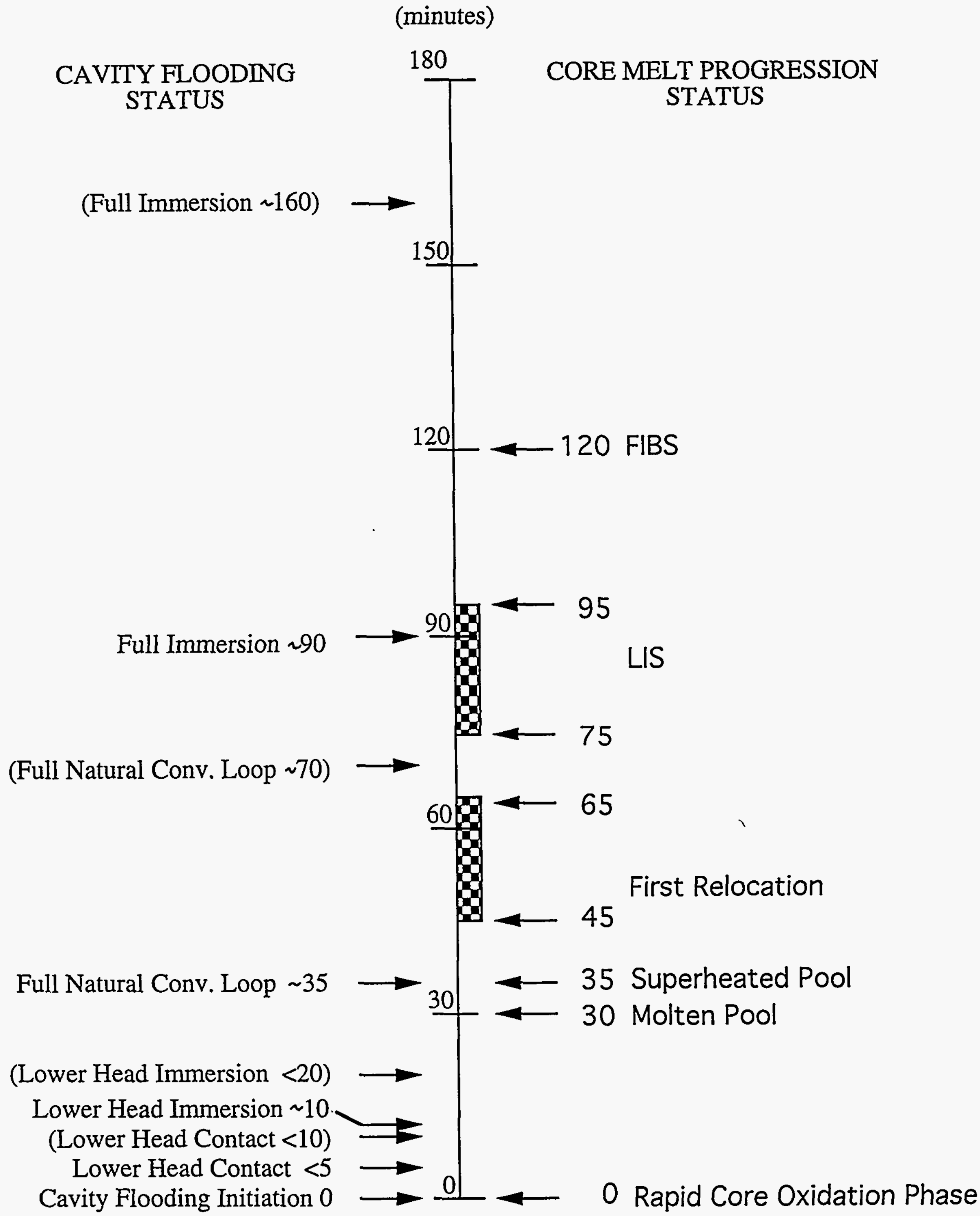


Figure O.10. The timeline of the accident. The parentheses indicate an arbitrarily assumed failure to function of one of the two cavity flooding lines.

Table O.1. Summary of Reviewer's Scenarios	
Reviewer	**Description**
Cheung, Che2* Olander, Ola7	Early relocation of high power density melt
Spencer, Spe3	Thermal loading due to large solid crusts
Tuomisto, Tuo5	Effect of steel droplet boiling in an oxidic pool
Turland, Tur3	Metal-oxide inversion due to U in metal
Olander, Ola10 Sehgal, Seh5	Homogeneous metal/oxide dispersion or slurry pool
Sehgal, Seh24	Bottom blockage failure and melt release
Olander, Ola7	Smaller oxidic pools with thinner metal layers
Levy, Lev9	Lower plenum steel rising through oxidic pool
Seiler, Sei2	Metal circulating through an oxidic porous matrix
Epstein, Eps3 Sehgal, Seh22	Jet diameter change with time (ablation)
Seiler, Sei16	Why did not consider metal jets?
*The notation follows that on the cover page of Appendix U.	

2. The Spe3 scenario is insignificant when one realizes that the maximum thermal load from a solid crust corresponds to the conduction thickness of $\sim$10 cm and is only 140 kW/m^2. Any thicker crust would melt away allowing convection of the decay heat upward.

3. For the Tuo5 scenario, although possible in principle, we can see no mechanism for entrapping significant quantities of steel below superheated oxidic melt [[until significant superheat is obtained the resulting thermal loads are negligible]], and releasing it in such a fashion as to allow thermal equilibration and development of steel vapor pressures. Small quantities could have no impact.

4. The Tur3 scenario is based on a non-confirmed hypothesis (by Powers). In the scenario timeline constructed above there is no opportunity for it to occur in the pre-LIS period, nor during the meltdown phase of the lower core plate. As the FIBS is approached there are large quantities of molten steel around, but layer inversion at this point would be rather of no consequence because of the low power density in the metal phase (see also Appendix R).

5. The Ola10/Seh5 scenario may pertain to the highly dispersed metallic fission products (such as the noble metals as discussed in Appendix R) but not to steel that is macroscopically melted in, such as during reflector or core plate melting, as discussed above. This would result in less direct thermal loading in the metal layer, but would hardly impact the properties of the oxidic pool. On the other hand, a "slurry" consistency is impossible to maintain at these power densities and at macroscopic dimensions (low surface-to-volume ratio) because the heat rejection behavior in slurries is too low and the slurry consistency cannot be maintained. The TMI accident cannot be considered as a guide to what might happen in an AP600-like design because of the fundamental differences explained in Section O.3. In addition, we should remember that the meltdown was interrupted and the relatively small quantity of debris ($\sim$20 tons) on the lower head was cooled rather efficiently.

6. As explained in Section O.3, the Seh24 scenario is not possible in the AP600-like design. [[See also DOE/ID-10541.]]

7. Some perspectives on the potential impact of the Ola7 hypothesis can be found in Section O.2. As explained in Section O.3, because of the water in the lower plenum the reflector melt that has to accompany the oxide as it relocates and the approach of LIS in the manner described, we find no significant concerns in this area.

8. The lower plenum steel is only 2.5 tons, and the melt of it is simultaneous with "falling in" of the core support plate and attainment of LIS. We see no significant concerns from Lev9 because the scenario affords no mechanism for trapping and superheating steel, particularly given the large heat sinks available at this time (see also 3 above).

9. The Sei2 scenario may be possible in a BWR lower plenum with the large quantities of steel and water in it, and the implied large porosity—if there is no penetration failure during a major relocation event. In a PWR lower plenum even with some quenching, we cannot expect a large enough porosity to allow significant thermal loading due to metallics circulating within a porous matrix. Moreover, as described above, the lower plenum contains a small quantity of steel (~2.5 tons), and the first relocation is mainly oxidic, with significant metallic quantities added subsequently on top.

10. The Eps3/Seh22 scenario is certainly correct. The calculations given in Chapter 8 and Appendix H were only to provide a perspective on how difficult it is to cause penetration due to impingement by an oxidic melt. Section O.3 provides further perspectives in terms of the relocation path (sideways), the potential quantities involved (well below that considered in the analyses), and the naturally dispersive mode of contact with the lower head. The impingement was very conservatively treated in both Chapter 8 and Appendix H by considering a coherent jet. The role of water in dispersing the jet was likewise conservatively treated in Appendix H, when the analysis is viewed in the context of the FARO experiments and the TMI experience (no lower head erosion whatsoever).

11. The Sei16 scenario was not considered because, as explained in Section O.3, the geometry provides for the entrapment of the metal layer following reflector failure, and the relocation of mainly oxidic debris upon core barrel failure. Subsequent relocations will contain metal, but not highly superheated, and they will be very gradual and off to the side.

O.5 CONCLUSIONS

In this appendix we have provided a timeline of *the meltdown/relocation aspects of the accident development.* This became possible because of some unique characteristics of the AP600 design. We considered parametrically the failure envelope due to the "focusing" effect, and the various scenario ideas proposed by the reviewers, and in light of this timeline we conclude that the base case in the original report is indeed bounding.

APPENDIX P

ADDITIONAL PARAMETRIC AND SENSITIVITY STUDIES

APPENDIX P

ADDITIONAL PARAMETRIC AND SENSITIVITY STUDIES

This appendix was prepared in response to specific requests for additional parametric calculations made by reviewers, as indicated in Table P.1. These inspired certain additional cases, as summarized under "authors," also in Table P.1. Many of these cases involve an emissivity value of 0.35, because once it was introduced (Kress) in conjunction with the Extreme Parametric, it became clearly interesting to see what happens as we back off, one parameter at a time, from their extreme conservative specifications. These include the decay power density (the 1.4 MW/m^3 value used in Figure 7.16 in the upper bound of the distribution in Figure 7.8), the depth of the oxidic pool (the 1.18 m is the maximum possible within the available geometry), and the wall melting point (the 1600 K, the lowerst possible value, at a eutectic composition, for any iron-zirconium composition where iron is the dominant component). Moreover, it is emphasized that an emissivity value of 0.35 is *outside the physically reasonable range* of this parameter. The minimum possible value is 0.45, obtained for a perfectly clean surface (of a *melt*), and it is expected to increase with impurities (i.e., oxides), or incomplete melting, towards a value of 1.

The results are summarized in Table P.2 and Figures P.1 through P.5. For all cases involving a thin metal layer the $q_{\ell,w}/q_{CHF}$ values reported are based on the critical heat flux simulations in ULPU carried out specifically for such highly peaked flux distributions (see Appendix E.3). On the results, the following comments may be made.

(a) As seen in Figure P.1 (in comparison to Figure 7.10) and in Figures P.2(a),(b), the effects of shifting the oxidized zirconium distribution, and of introducing imperfect crust-vessel contact are negligible.

(b) In Table P.2 we find that use of Kulacki-Emara more than compensates for the decrease of emissivity from 0.45 to 0.35, and the effect is of similar magnitude as increasing the wall melting point ($T_{\ell,m}$) by 150 K. Also note that our Extreme Parametric case is seen to have a 10% margin to failure as compared to a zero margin in Figure 7.16. This is due to an improved simulation (in ULPU) that accounts for the particular, highly peaked, flux shape pertinent to this case.

(c) In Figure P.3(a),(b), we see that the 0.35 value must be combined with the edge-of-distribution value of decay power density to produce failure. Use of the most probably value (1.3 MW/m^3) yields $\sim$10% margin.

(d) In Figure P.4(a),(b), we can see how the margin increases as the depth of the oxidic pool decreases (which causes the thermal load on the metal pool to decrease). The effect is quite striking, and provides important perspectives on failure, when we recognize the very special "construction" of the Extreme Parametric, i.e., the maximum quantity of oxide that would "fit" the geometry.

(e) In Figure P.5(a),(b), we see that shifting the volumetric heat source from the oxidic pool into the metal layer produces, as expected, a corresponding shift in thermal loads. However, even a 50% shift is not adequate to compromise integrity in the Base Case.

To conclude, we believe that these additional parametric results further illustrate the robustness of our conclusion that lower head failure is physically unreasonable.

Table P.1. The Additional Parametric Calculations			
Source	Case No.	Description	Results
Kress	1	A reduced emissivity value, $\epsilon = 0.35$	Table P.2
	2	Zr-oxidized distribution, shift by -10%	Figure P.1
Henry	3	Lower-head-to-crust gap conductance	Figure P.2
Olander	4	Transposing power from oxidic to metal pool	Figure P.5
Schmidt	5	Use Kulacki-Emara, Eq. (5.11), for q_{up}	Table P.2
Authors	6	Combine $\epsilon = 0.35$ with Kulacki-Emara	Table P.2
	7	Combine $\epsilon = 0.35$ with $T_{\ell,m} = 1700$	Table P.2
	8	Combine $\epsilon = 0.35$ with decay heat variations	Figure P.3
	9	Extreme Parametric with reduced pool depths	Figure P.4

Table P.2. Results of Parametric Cases					
Source Case No. in Table P.1	ϵ	Equation for q_{up}	$T_{\ell,m}$ (K)	$q_{\ell,w}$ (MW/m^2)	$q_{\ell,w}/q_{CHF}$
Extreme Parametric	0.45	(5.12)	1600	1.22	0.91
1	0.35	(5.12)	1600	1.35	1.01
5	0.45	(5.11)	1600	1.02	0.76
6	0.35	(5.11)	1600	1.15	0.86
	0.45	(5.12)	1750	0.97	0.72
7	0.35	(5.12)	1750	1.14	0.85

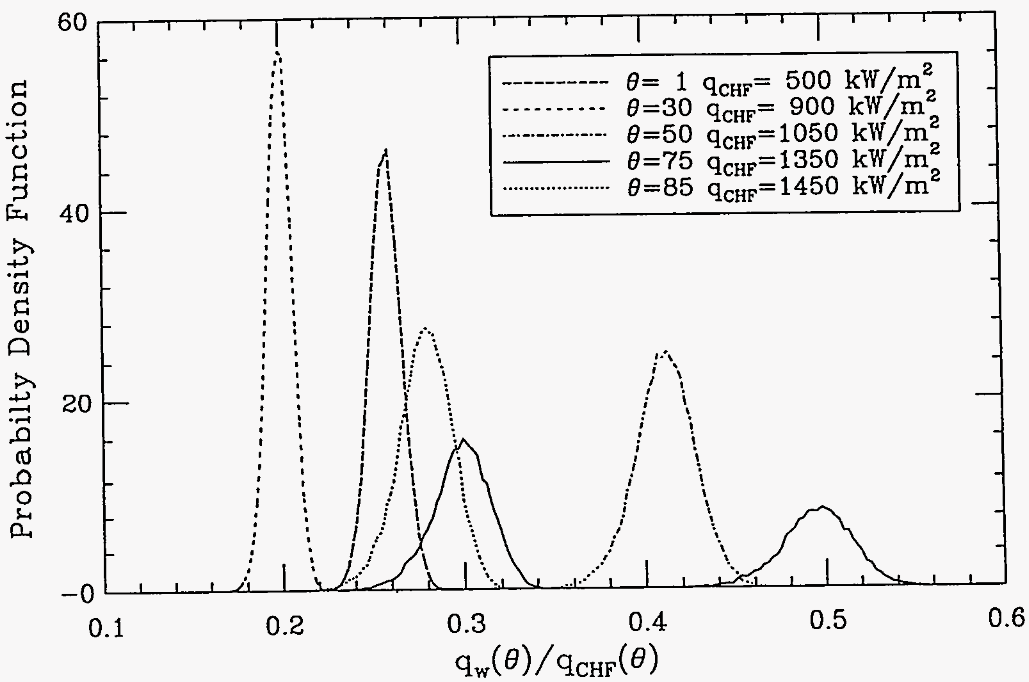


Figure P.1. A sensitivity on the results of Figure 7.10, carried out by shifting the zirconium-oxidized distribution (Figure 7.3) to the left by 10%.

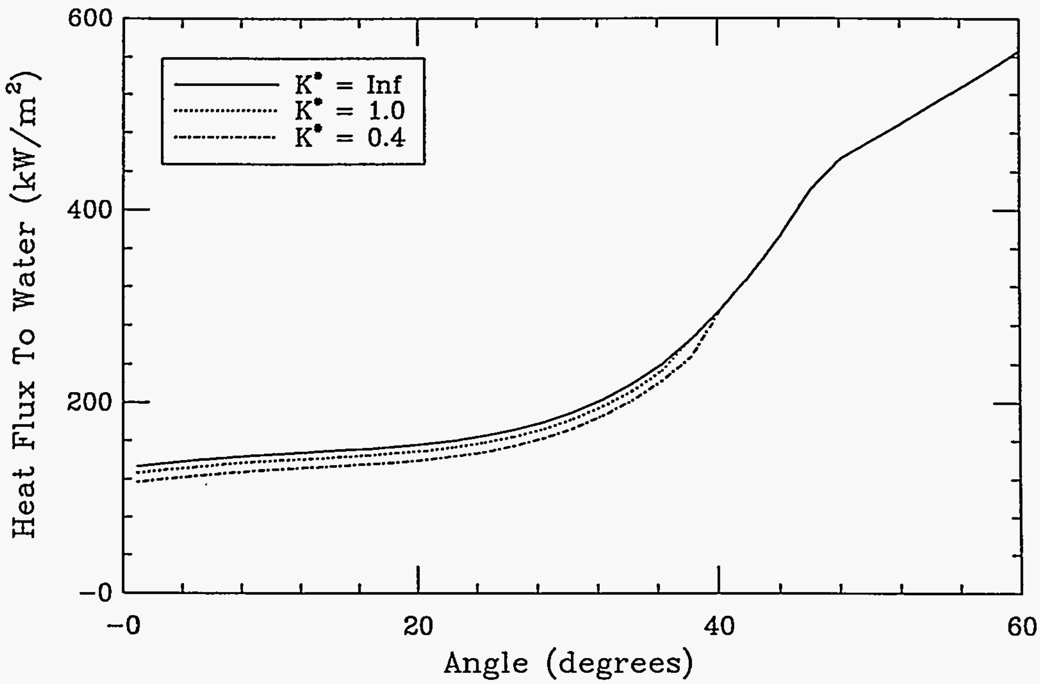


Figure P.2(a). The effect of gap conductance (expressed as multiple of the conductance of 1-cm-thick oxidic crust) on the corium crust thickness on the lower head.

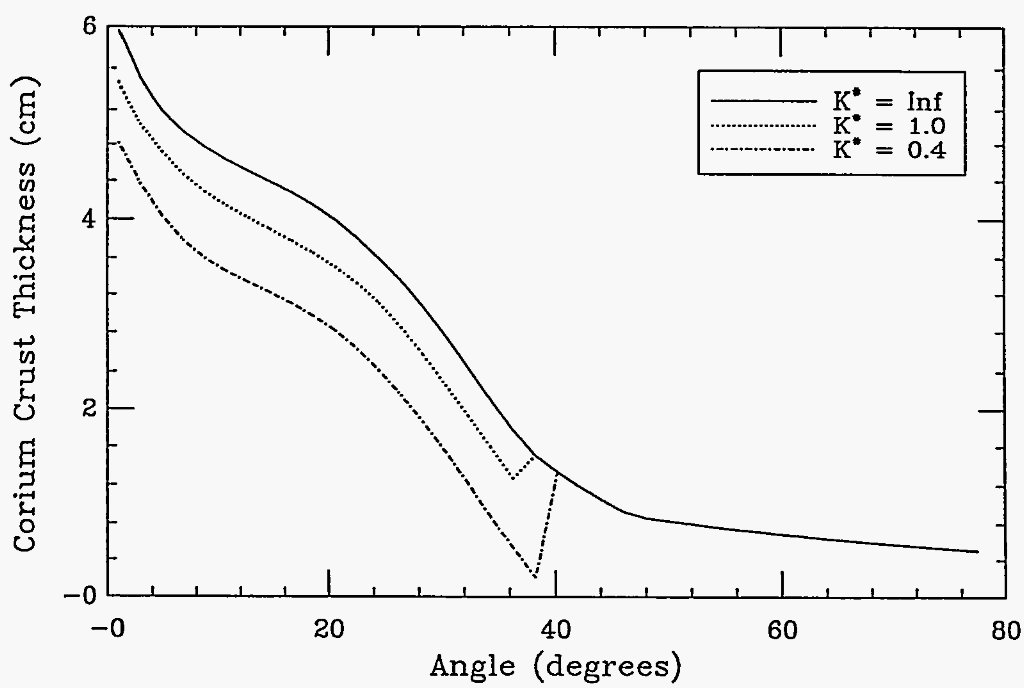


Figure P.2(b). The effect of gap conductance on the heat flux distribution at the lower-head-water interface.

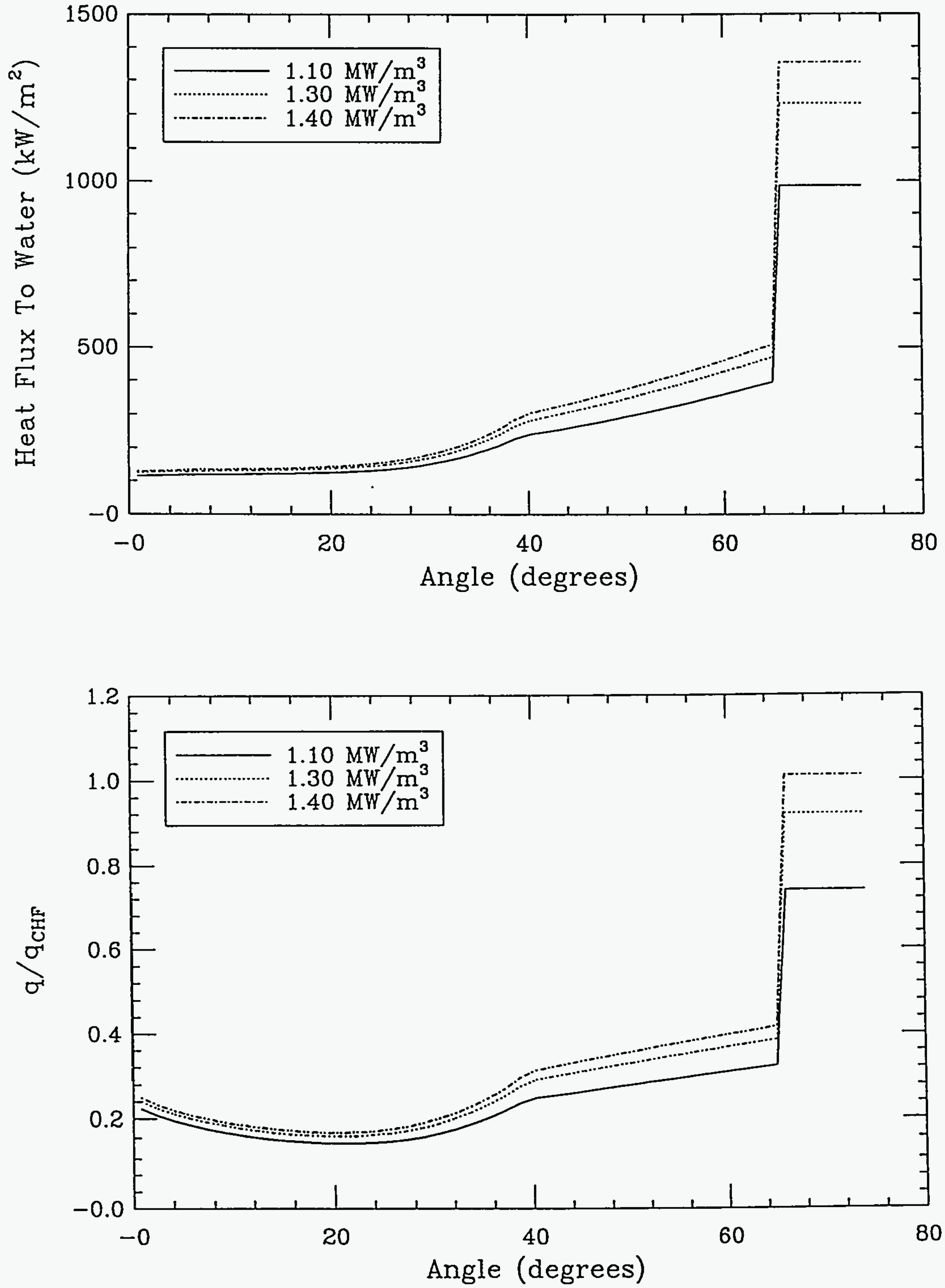


Figure P.3. Thermal loads and margins to failure for the Extreme Parametric case with $\epsilon = 0.35$, including the effect of decay power density.

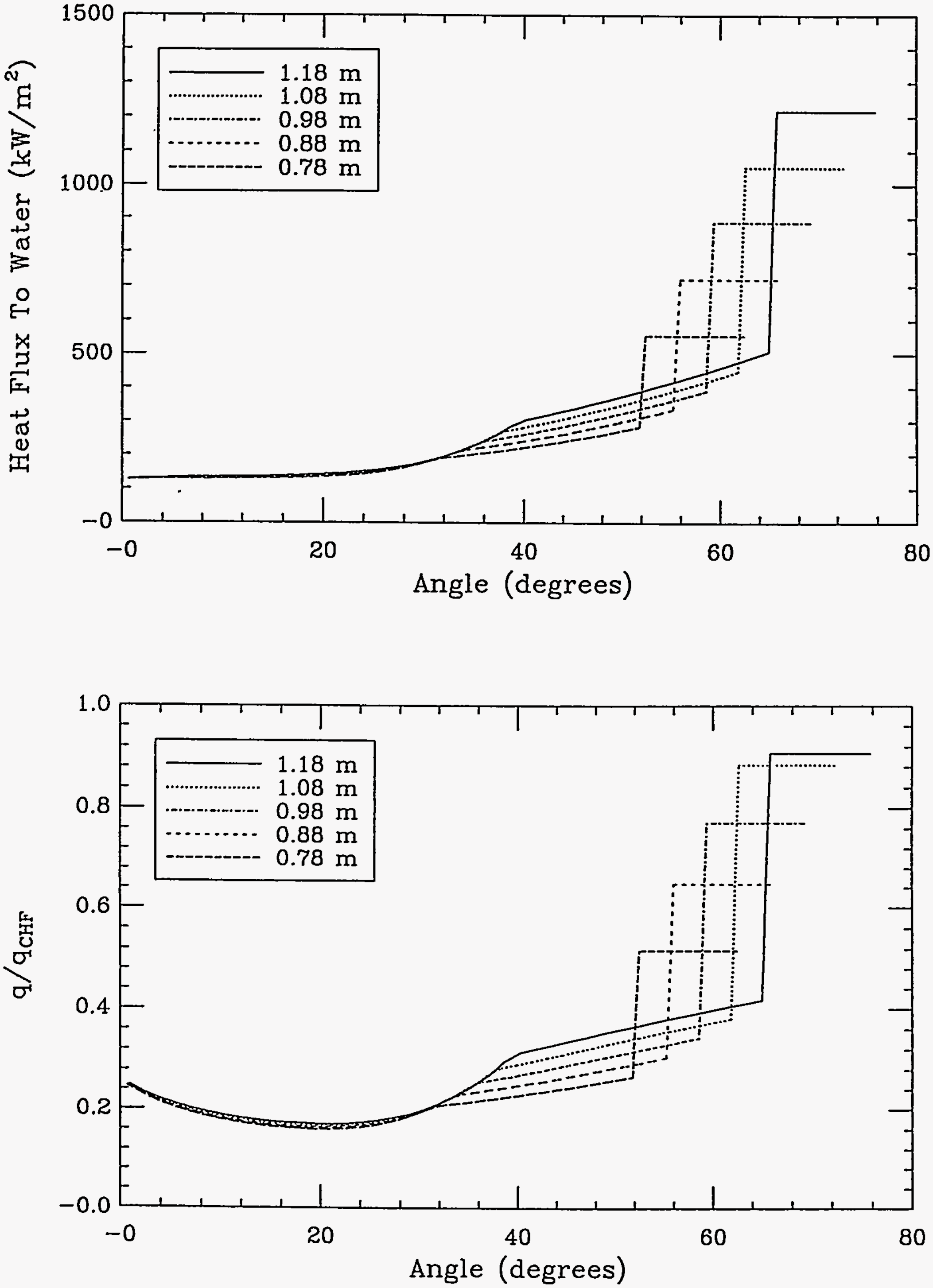


Figure P.4. Thermal loads and margins to failure variation with oxidic pool depth for the Extreme Parametric case.

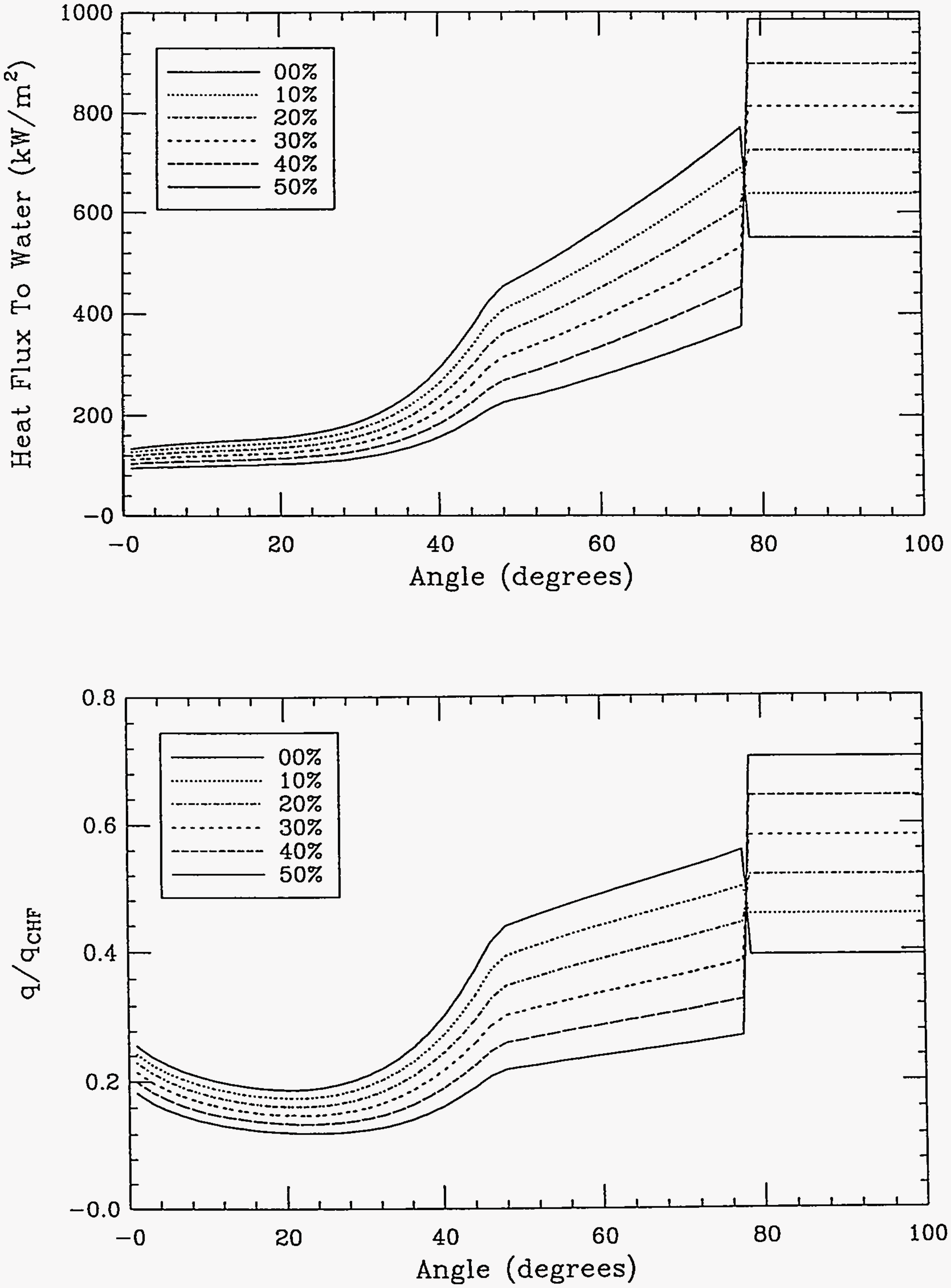


Figure P.5. The effect of shifting he volumetric heat source (expressed as fraction of total) from the oxidic pool to the metal layer directly.

APPENDIX Q

SAMPLE COMPLETE SETS OF ANALYSIS RESULTS

APPENDIX Q

SAMPLE COMPLETE SETS OF ANALYSIS RESULTS

This appendix is in response to one reviewer's request (Spencer) for "other key representative results," from the cases studied, besides those already included in Chapter 7. We supply such results here for two cases: the Base Case and the case characterized as Extreme Parametric (shown previously in Figure 7.16). In addition, since the purpose is to generate the basis for improved understanding, we take this opportunity to include results that allow one to identify quantitatively the trends in relation to variations in certain key parameters, such as decay power density, oxidic pool depth, and metal pool height.

The results are presented in Figures Q.1 through Q.12, where top figures refer to the Base Case, and bottom figures, to the Extreme Parametric. On the basis of material already in the report the interpretation and significance of the various trends are straightforward. We wish, therefore, to make only very few comments here, and only for emphasis.

(a) As seen in Figures Q.5(a),(b), the up-to-down split in heat flux is about equal, and increases slightly as the oxidic pool depth decreases.

(b) As seen in Figures Q.6(a),(b), the UO_2 crust in between the oxidic and metallic pools is rather substantial.

(c) As seen in Figures Q.11(a),(b), it does not take much of a decrease in decay power density and/or much of an increase in the metal layer thickness to gain significant margins to failure, even for the Extreme Parametric case.

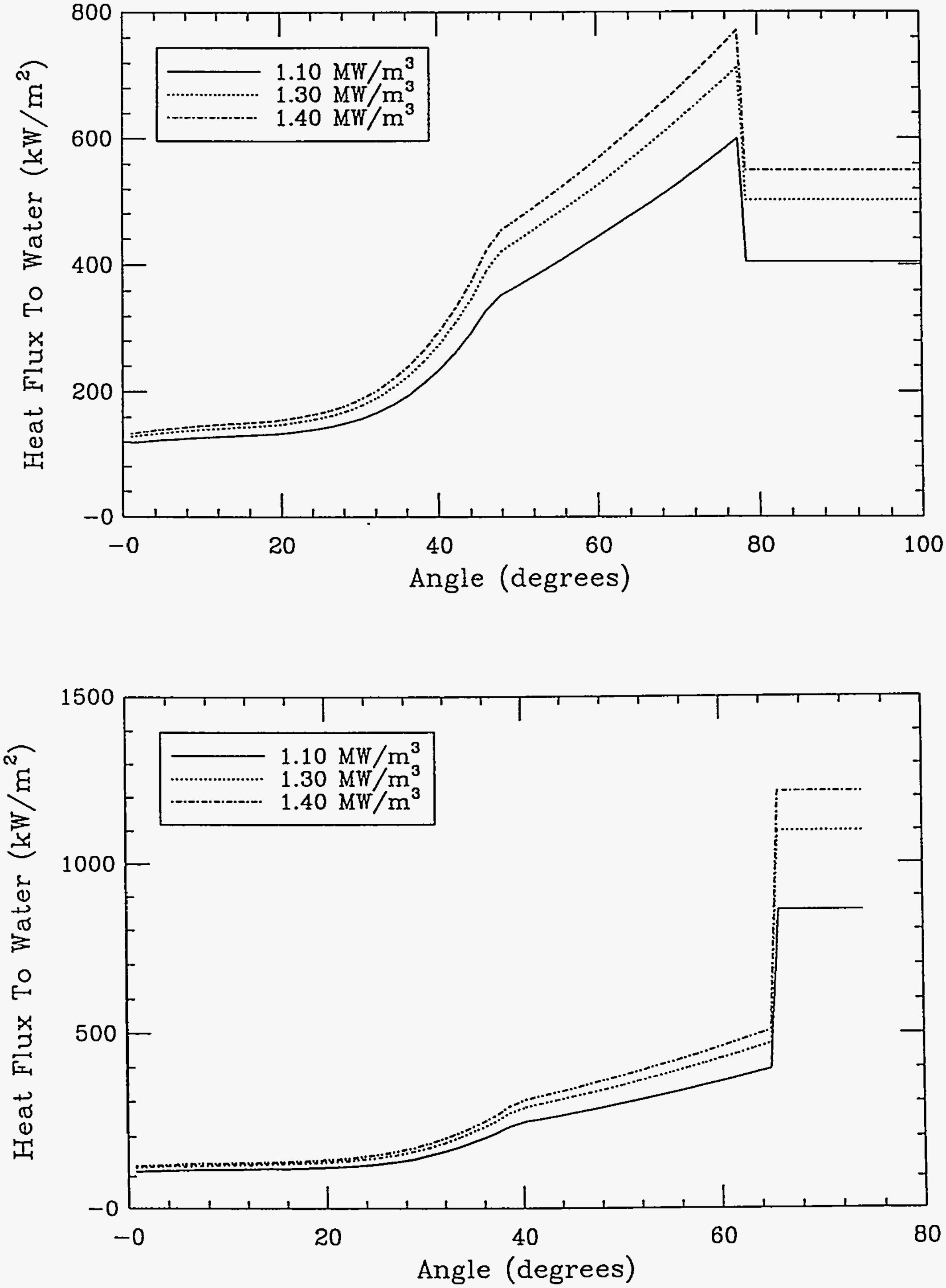


Figure Q.1. Heat flux distribution as a function of angular position on the lower head for the most probable and upper/lower limits of the decay power density (see Figure 7.8).

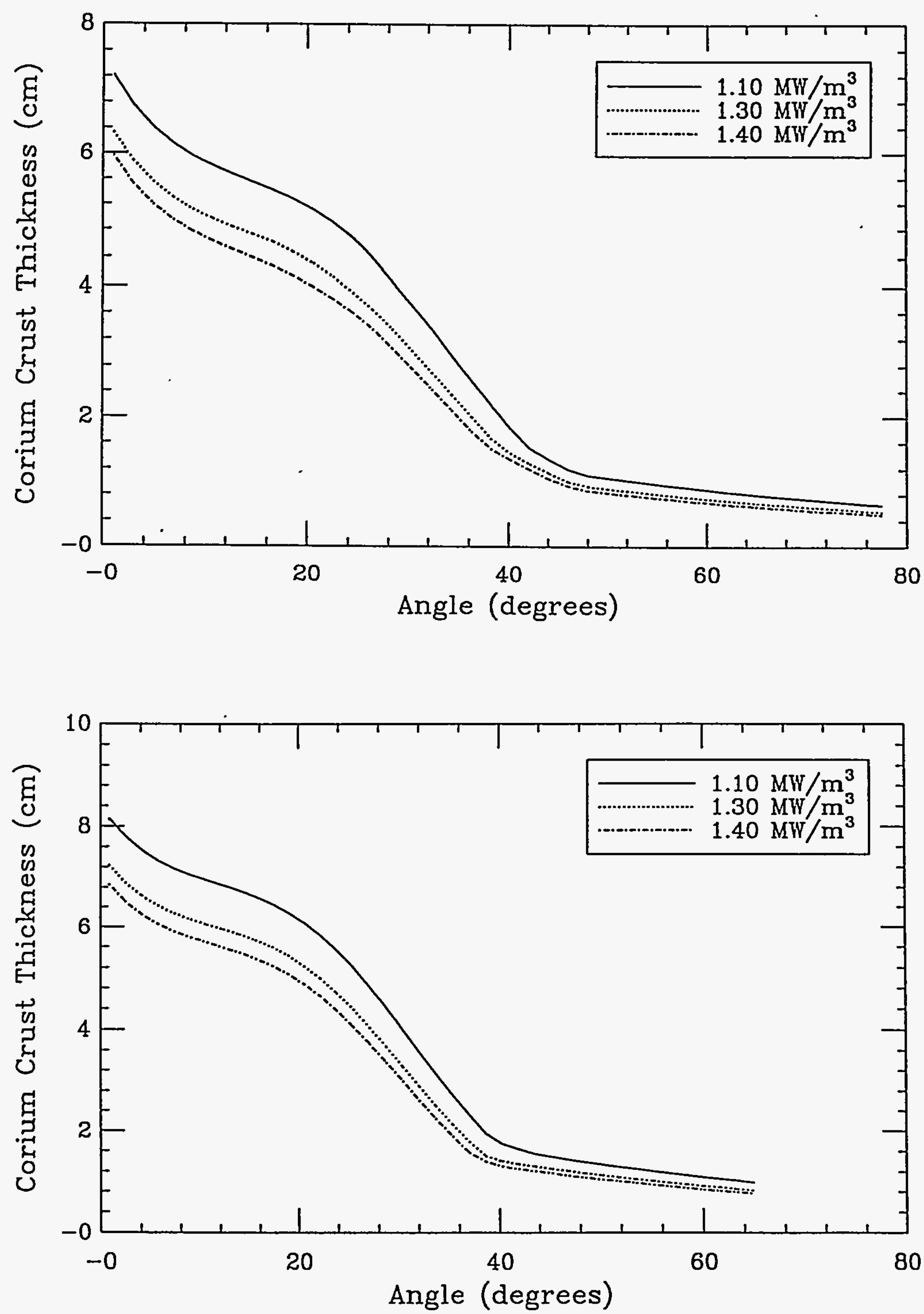
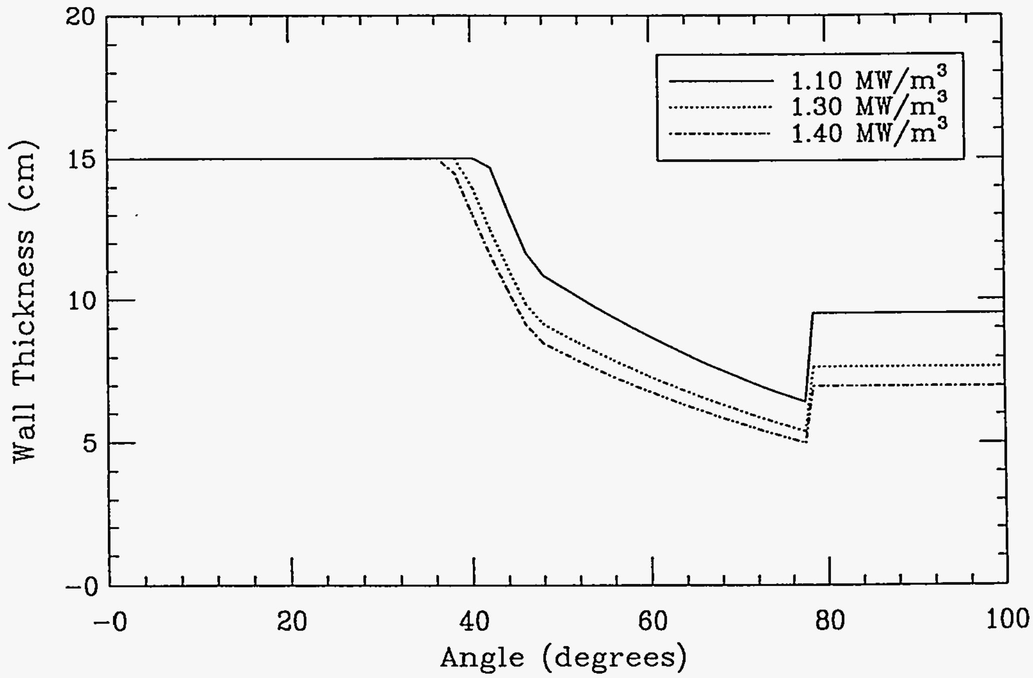


Figure Q.2. Oxidic crust thickness distribution on the lower head.



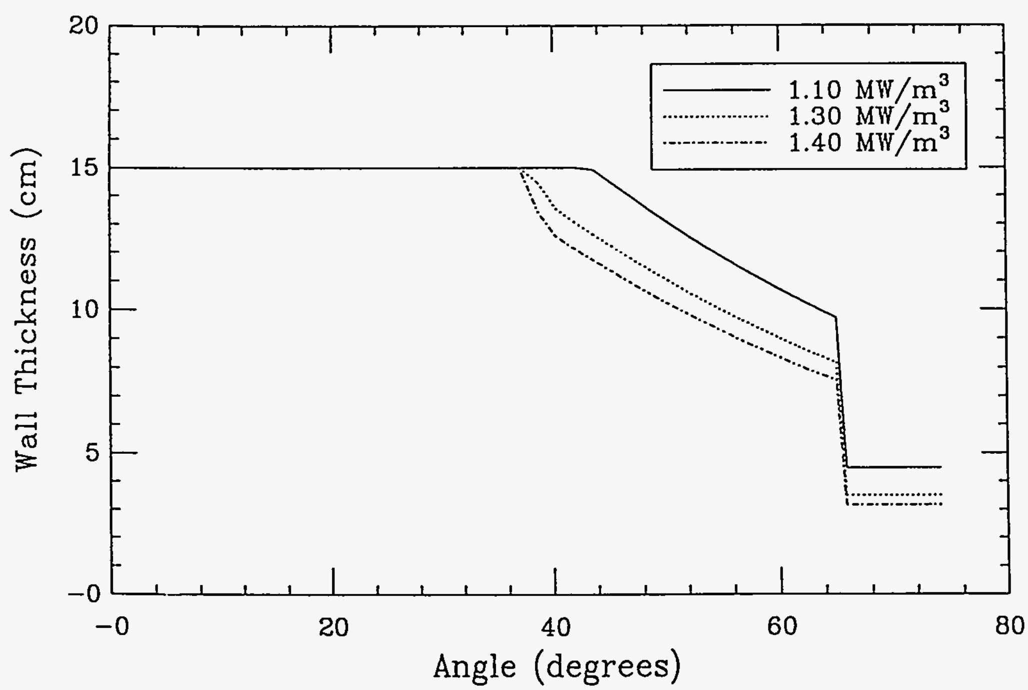


Figure Q.3. Lower head thickness after melt attack.

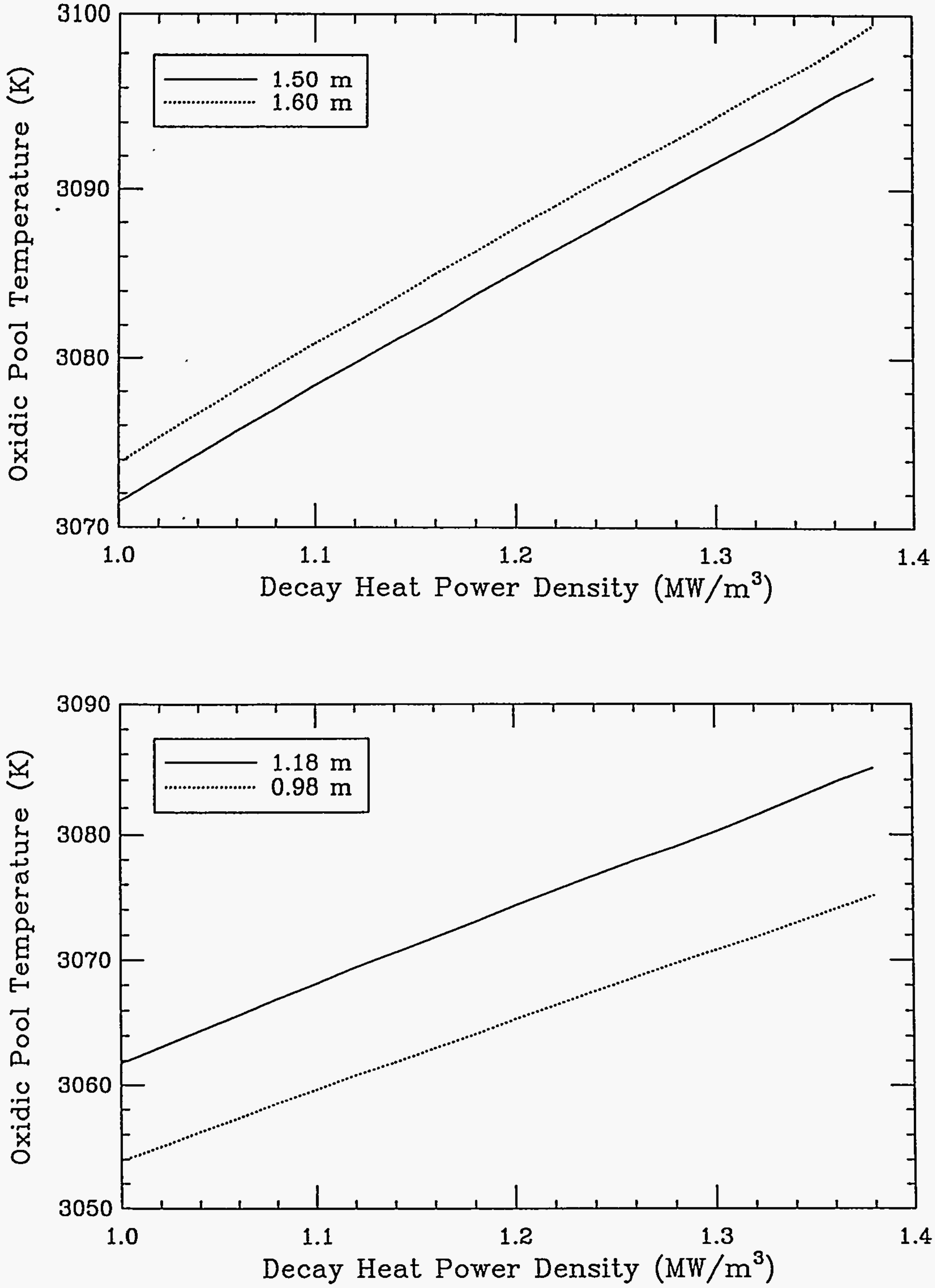


Figure Q.4. Variation of oxidic pool (maximum) temperature with decay power density and depth.

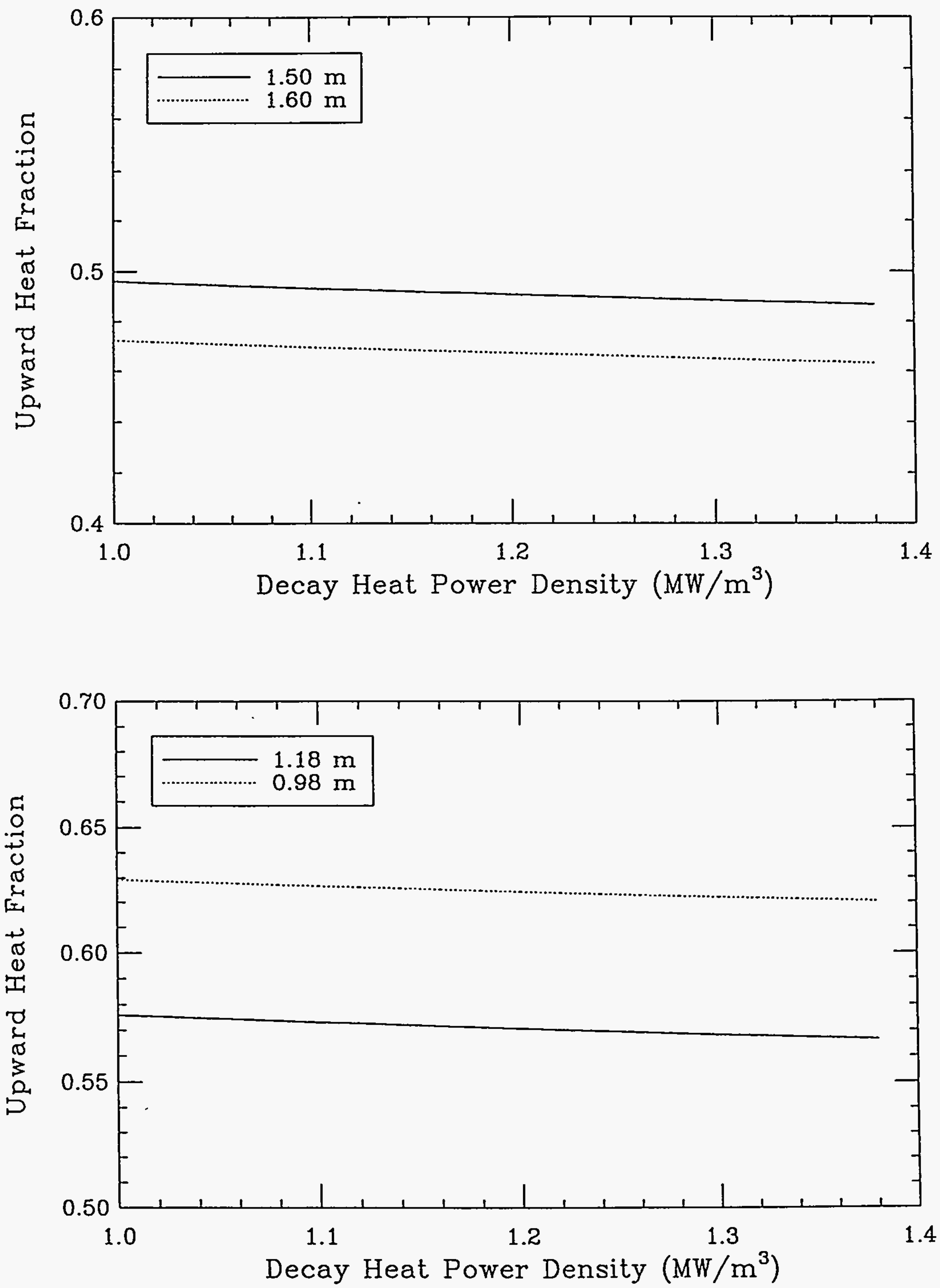


Figure Q.5. Variation of energy flow split with decay power density and depth of oxidic pool.

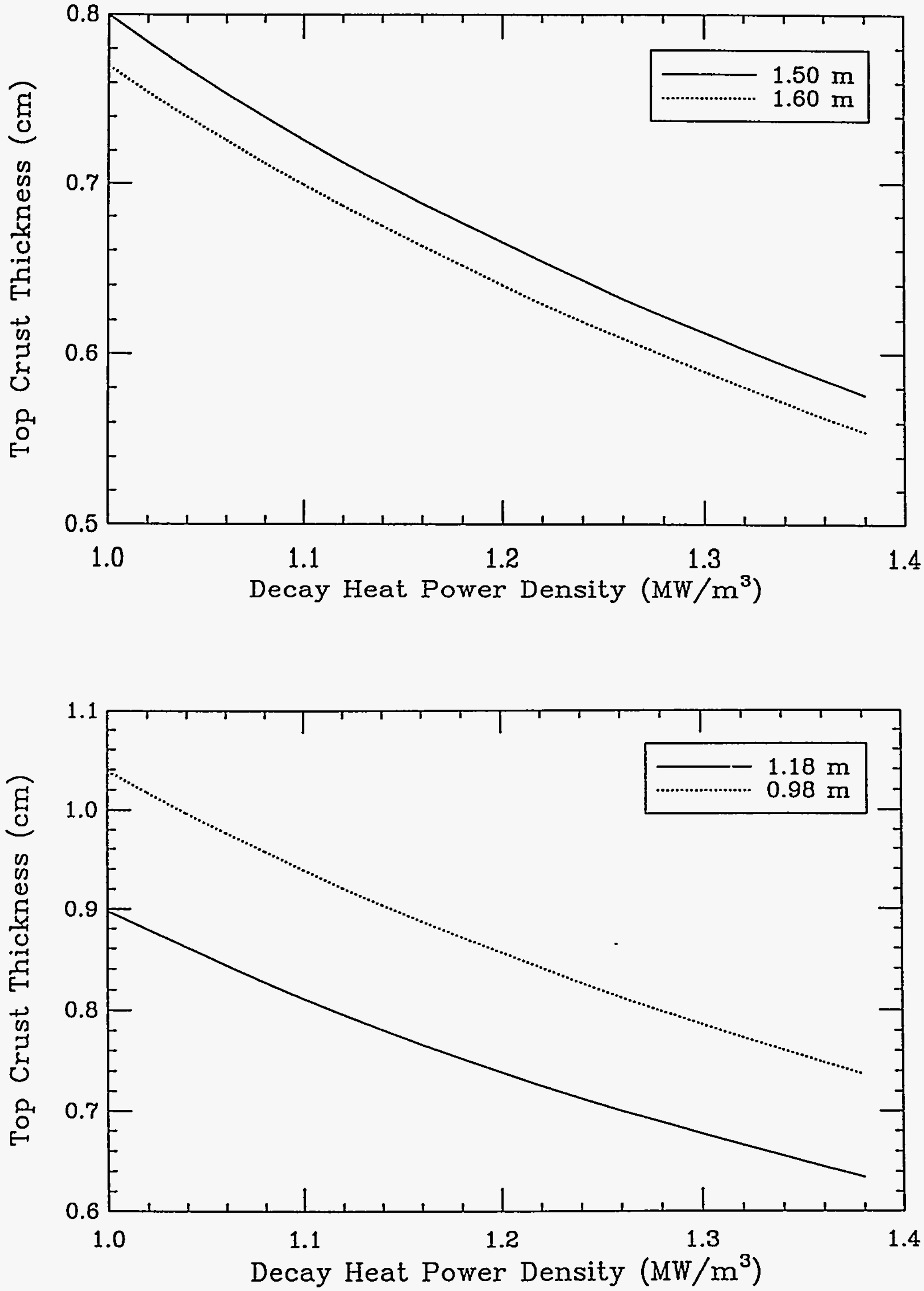


Figure Q.6. Variation of crust thickness, on top of the oxidic pool, with decay power density and pool depth.

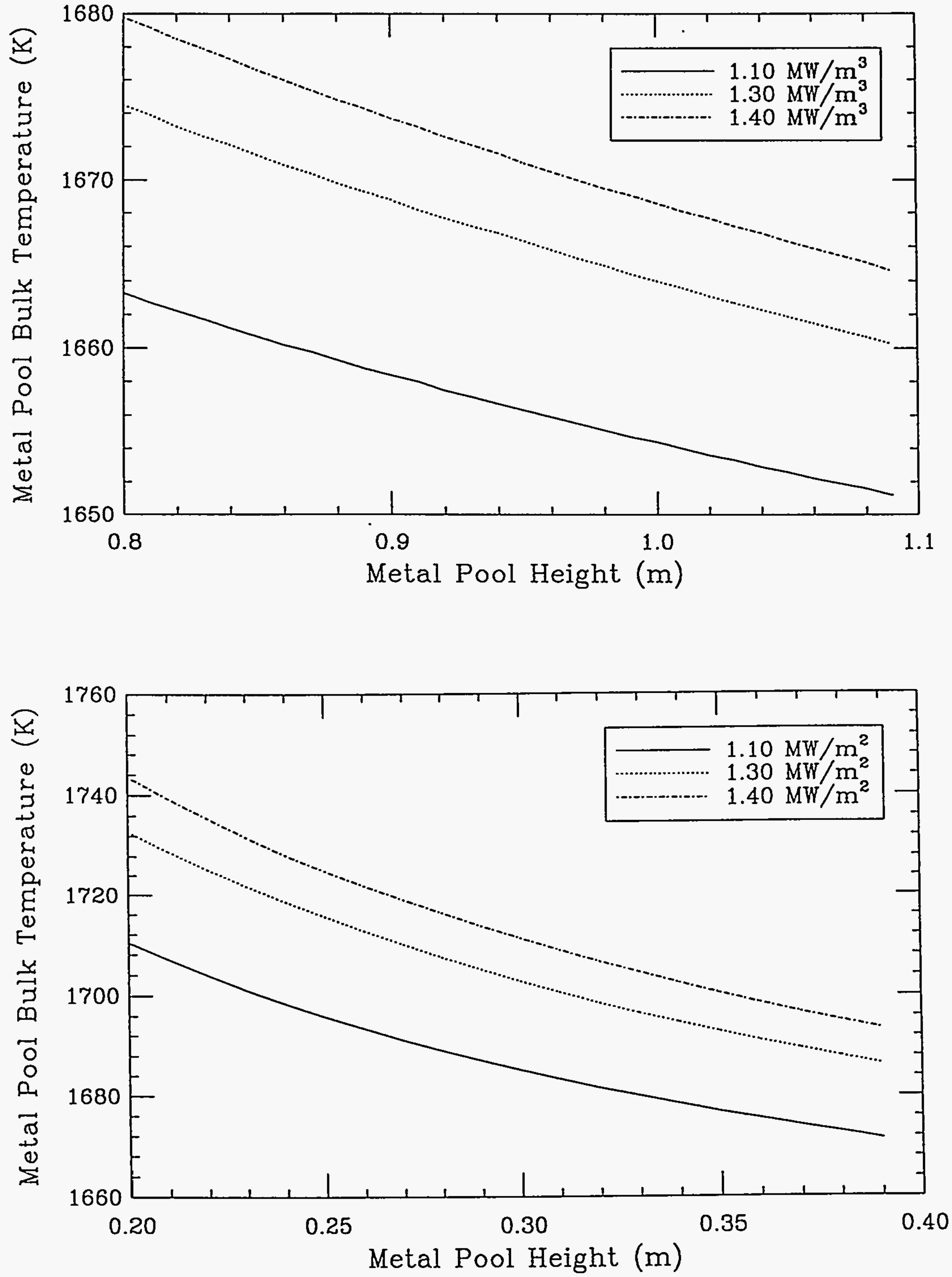
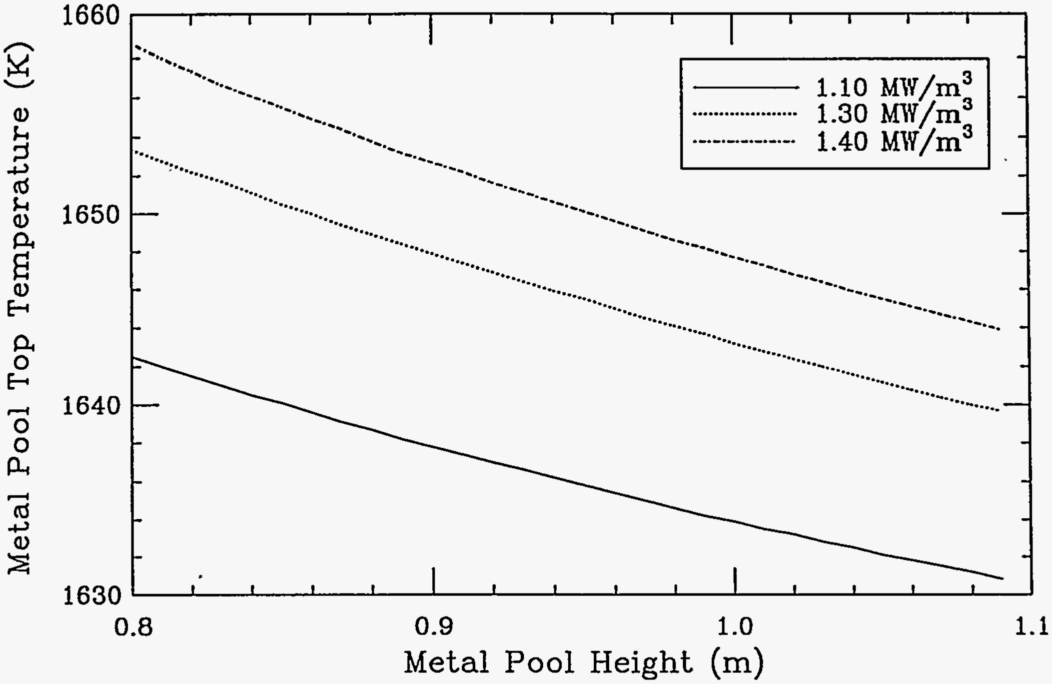


Figure Q.7. Variation of metal layer bulk temperature with depth height and decay power density.



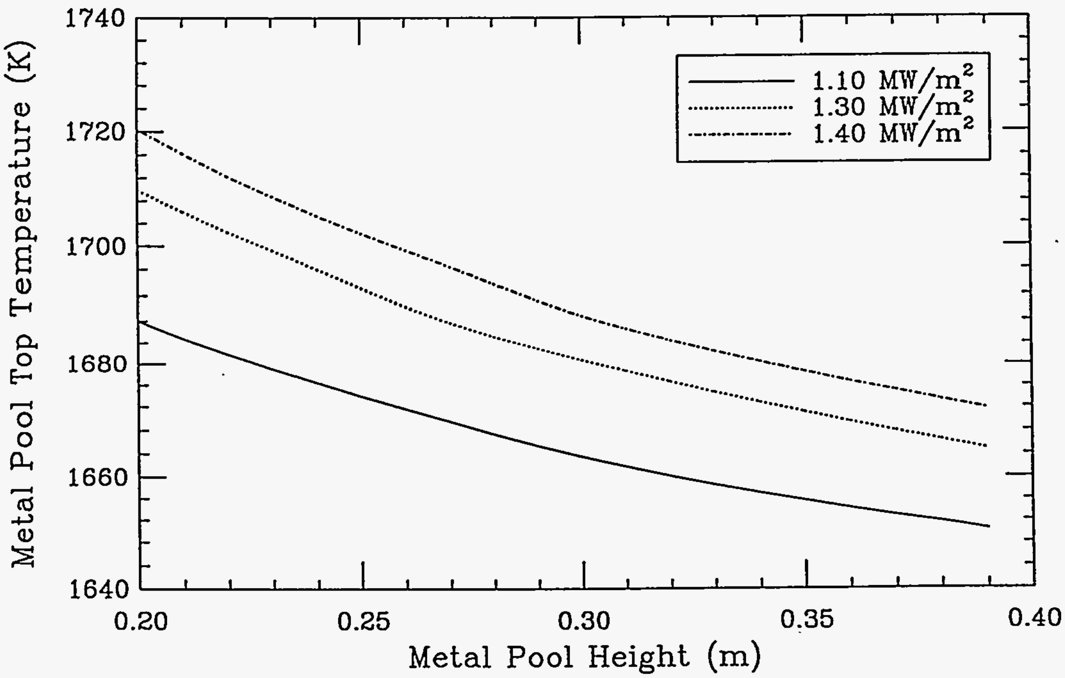


Figure Q.8. Variation of metal layer top temperature with height and decay power density.

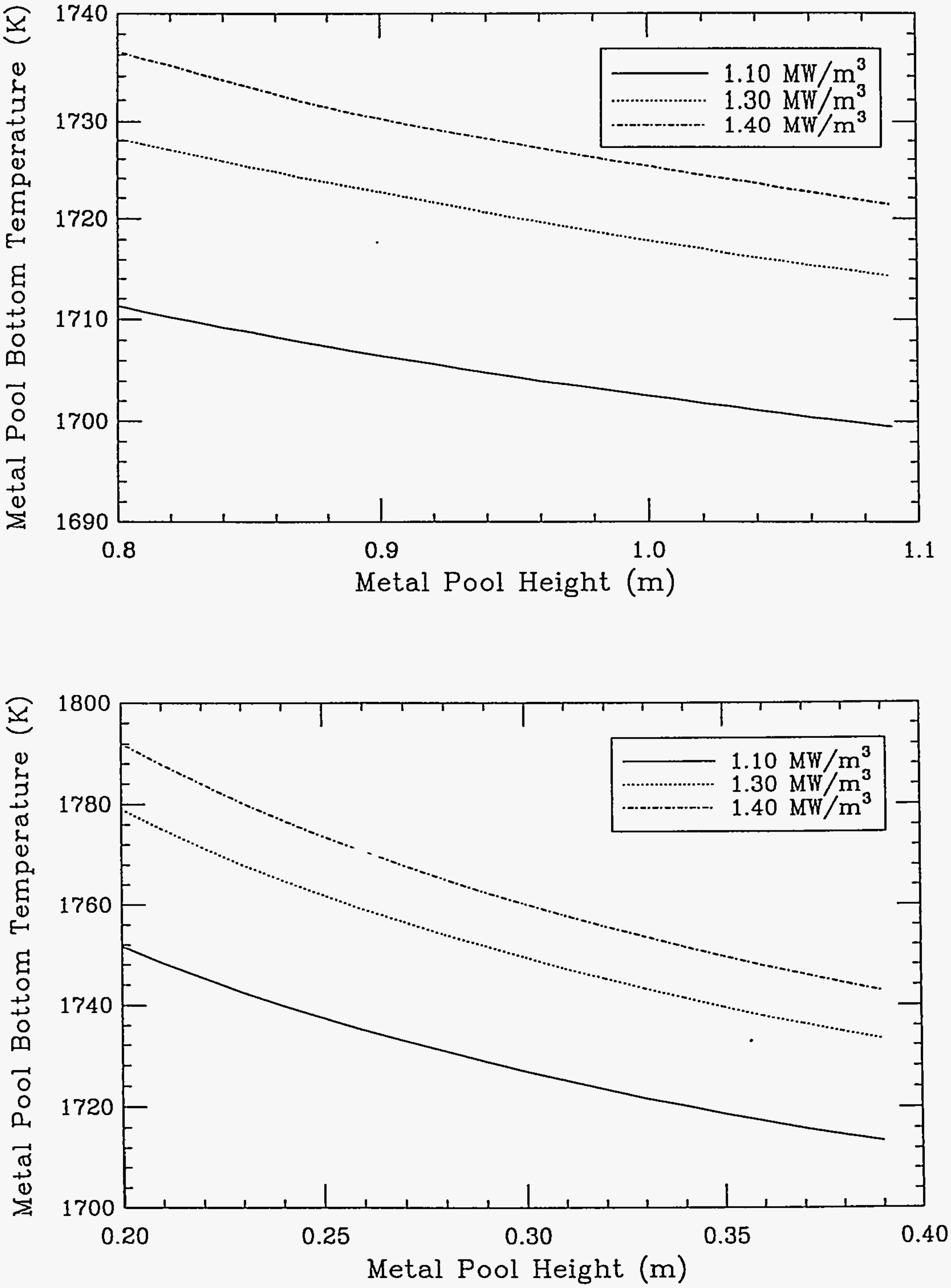


Figure Q.9. Variation of metal layer bottom temperature with height and decay power density.

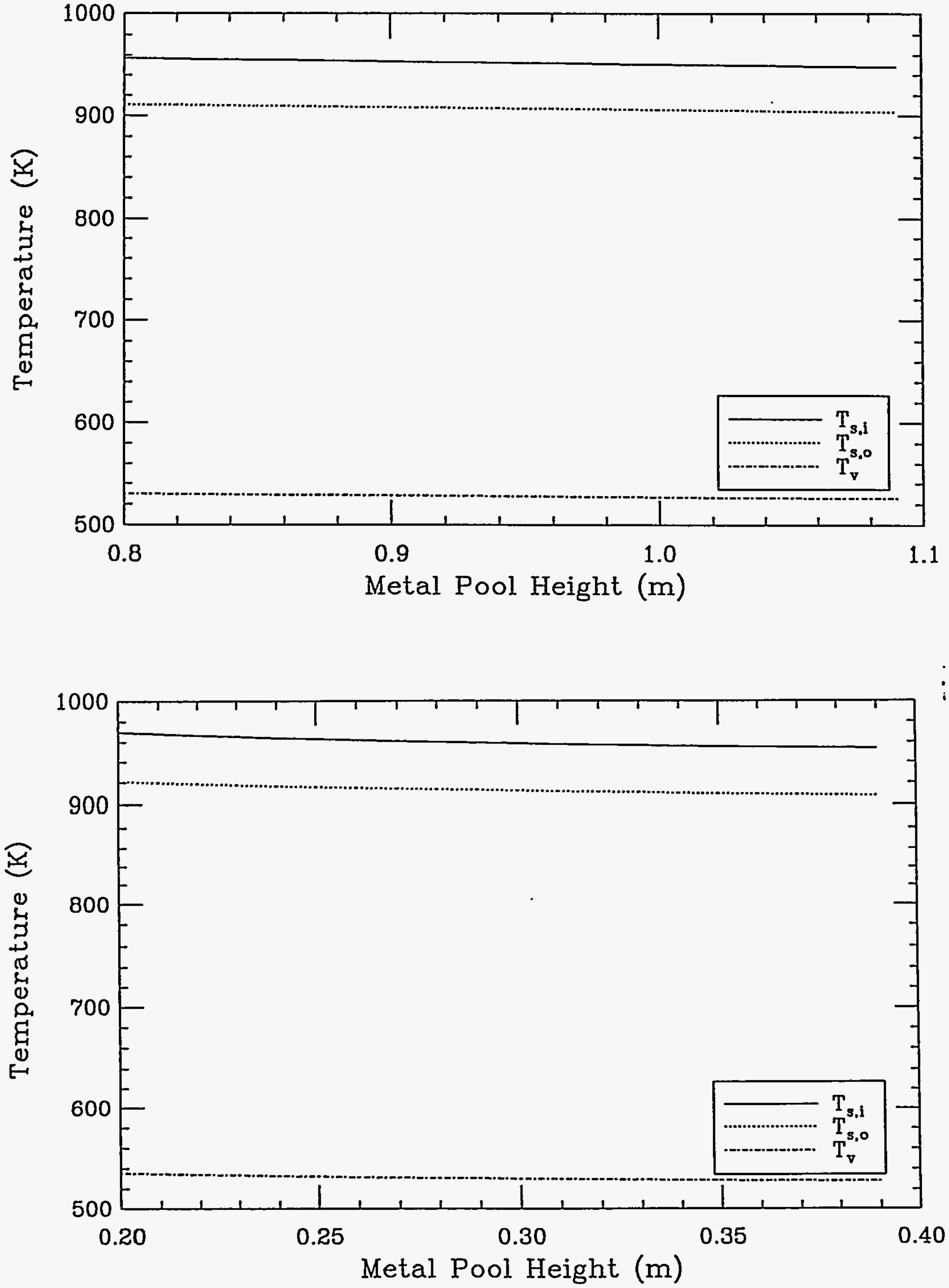


Figure Q.10. Variation of the upper core barrel (inner and outer) temperatures, and inner vessel wall temperature, with metal layer height (with decay power density 1.3 MW/m^3).

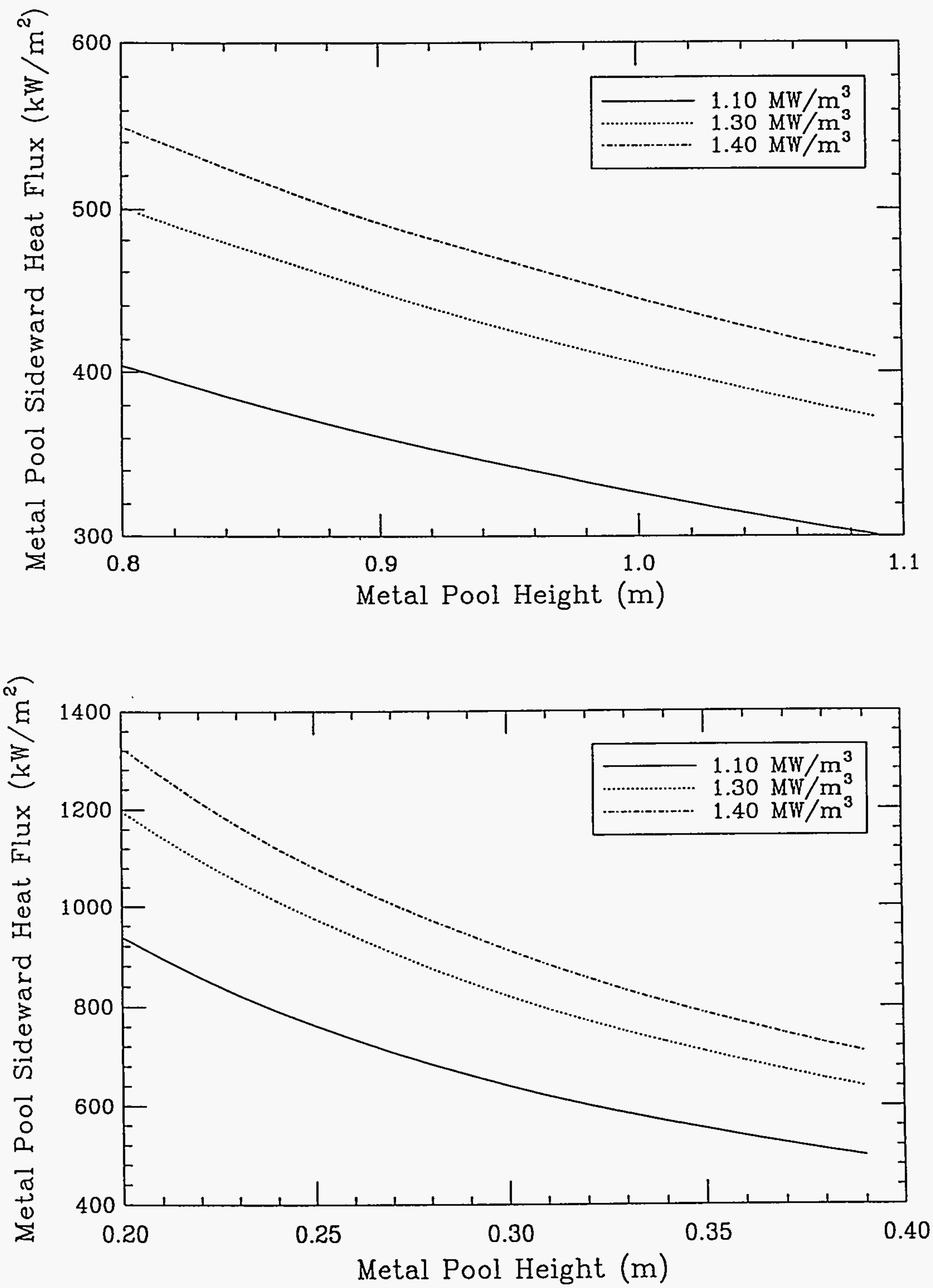
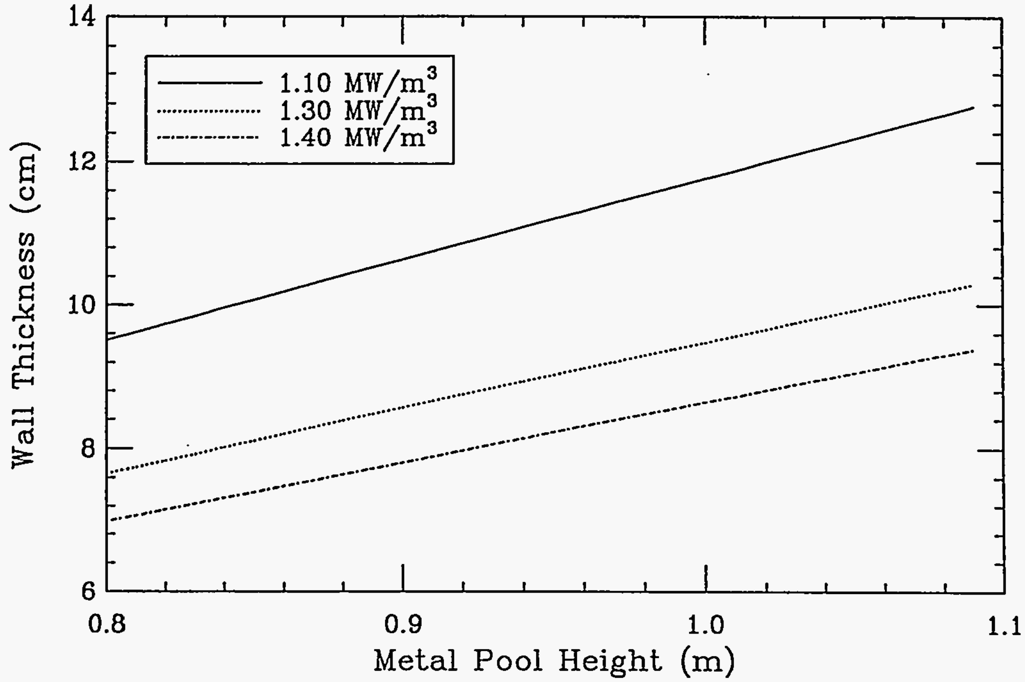


Figure Q.11. Variation of vessel wall thermal loading at the metal pool interface, with metal layer height and decay power density.



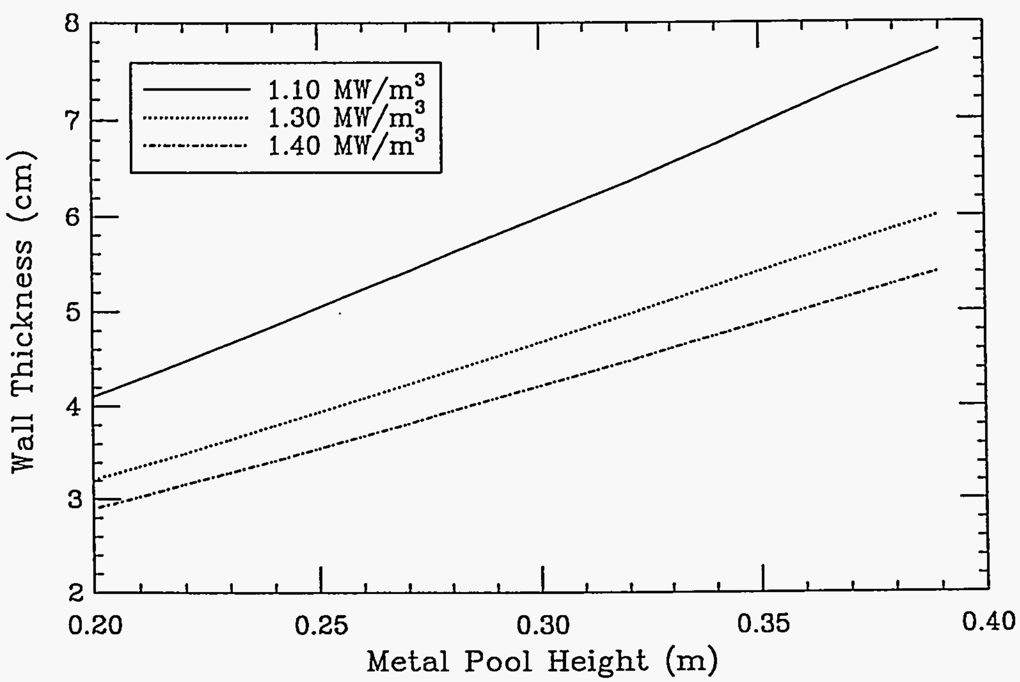


Figure Q.12. Variation of remaining wall thickness, after melt attack from the metal layer, with layer height and decay power density.

APPENDIX R

DECAY HEAT GENERATION IN THE METALLIC LAYER

APPENDIX R

DECAY HEAT GENERATION IN THE METALLIC LAYER

W.A. Bezella and J.J. Sienicki

Engineering Development Laboratories
Reactor Engineering Division
Argonne National Laboratory, Argonne, IL 60439

An estimate has been made of the decay heat source which might be postulated to reside in the metallic phase during a severe accident in the AP600. The decay heating estimates generated for the TMI-2 program by England and Wilson (1980) were used in this appraisal. While the fuel enrichments, fuel burnup, and neutron spectrum in TMI-2 differ from conditions expected in the AP600, the direct application of these TMI-2 results to the AP600 was felt to provide the needed approximate estimate.

In Table 1, the grouping of the fission product elements producing the decay heat generation in the England and Wilson (1980) study are listed. This grouping of fission product elements follows the WASH-1400 grouping except for a few modifications (i.e., Np and Pu isotopes were not included in the LASL study). Tables 2 and 3 summarize the fraction of decay heating (gamma + beta) that each of these seven groups of fission products contribute to the total decay heat generation for the TMI-2 reactor. The Table 2 results are for the actual 96.2 effective full power days of operation with the Table 3 results being based on a ~3 yrs (26,000 hrs) of full power operation.

The fraction of decay heat that is produced by fission products that remain in their metallic form is required to assess the heat generation in the metal layer. It is assumed that only pure elemental metals would be associated with the non-fission products comprising the majority of the metal in the metallic phase. The remainder of the fission product compounds would be retained in the oxide pool. In Table 4, a list of possible compounds taken from Akers, Jensen and Schuetz (1992) is presented for each of the Table 1 fission products. As this table indicates, all of the fission product elements of interest have the potential for forming oxides (and other compounds) and therefore remaining within an oxide pool. However, Bradley and Gardner (1992) have identified the noble metal grouping of elements (principally elements in Group 6 in Table 1) as potentially remaining in their metallic form. Examination of metallic inclusions in samples of ceramic removed from the TMI-2 core revealed that the particular fission products ruthenium, rhodium, and palladium were concentrated in a metallic alloy of nickel and tin (Olsen, Jensen, Carlson and Cook 1989). The six metal elements, Mo, Tc, Ru, Rh, Sb, and Te, shown in

Table 5, were assumed to be present in the metallic upper layer of the AP600 oxide pool/upper metal layer configuration.

The decay heat generation associated with the six noble metals identified as being present in metallic form in Bradley and Gardner (1992) were obtained from Tables [2] and [3] taking the fractional decay power estimates for the Group 6 (noble metals). For the AP600 time frame when a molten pool is expected to be created (between 7,200 and 18,000 seconds) the fraction of decay heat generation in the upper metal layer would be expected to range between 5% and 10% of the total decay heat generation. The lower 5% value would reflect a low irradiation period prior to the decay period with the high ~10% value being representative of that expected after a long irradiation build-up.

REFERENCES

1. England, T.R. and W.B. Wilson (Revised March 1980), "TMI-2 Decay Power: LASL Fission Product and Actinide Decay Power Calculations for the President's Commission on the Accident at Three Mile Island," LA-8041-MS.
2. Akers, D.W., S.M. Jensen, and B.K. Schuetz (July 1992), "OECD-NEA-TMI-2 Vessel Investigation Project Companion Sample Examinations," TMI V(92)EG10.
3. Bradley, D.R. and D.R. Gardner (1992), "CORCON Mod 3: An Integrated Computer Model for Analysis of Molten Core-Concrete Interaction User Manual," NUREG/CR-5843.
4. Olsen, C.S., S.M. Jensen, E.R. Carlson, and B.A. Cook (1989), "Materials Interactions and Temperatures in the Three Mile Island Unit 2 Core," *Nuclear Technology* **87**, 57.

Table R.1. Elements Considered In Each Group

Group	Elements
1	Xe, Kr (Noble Gases)
2	I, Br (Halogens)
3	Cs, Rb (Alkali Metals)
4	Te, Se, Sb (Tellurium Group)
5	Sr, Ba (Alkaline Earths)
6	Ru, Mo, Pd, Rh, Tc (Noble Metals)
7	La, Nd, Eu, Y, Ce, Pr, Pm, Sm, Zr, Nb (Rare Earths, etc.)[a]

[a]Np & Pu isotopes are not included in the LASL study.

Table R.2. Total Fission Product Decay Power For TMI-2 Actual Operation

Decay Time (sec)	Fraction of Total Fission Product Decay Power							Total Decay Heat (Mw)
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6	Group 7	
3,600	5.69E-02	2.21E-01	8.48E-02	6.63E-02	1.02E-01	4.83E-02	4.18E-01	31.7
7,200	5.56E-02	2.31E-01	5.23E-02	4.40E-02	1.04E-01	4.66E-02	4.63E-01	25.1
18,000	4.69E-02	2.21E-01	2.41E-02	2.73E-02	9.85E-02	5.76E-02	5.23E-01	18.4
36,000	3.85E-02	2.26E-01	9.25E-03	2.63E-02	8.80E-02	6.60E-02	5.46E-01	14.5

[a] Based on 96.2 effective full power days at 2772 Mwt power level.

Table R.3. Total Fission Product Decay Power For TMI-2 Long-Term Operation[a]

Decay Time (sec)	Fraction of Total Fission Product Decay Power							Total Decay Heat (Mw)
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6	Group 7	
3,600	3.76E-02	2.17E-01	6.86E-02	6.94E-02	7.80E-02	9.63E-02	4.26E-01	32.1
7,200	3.66E-02	2.24E-01	4.18E-02	4.77E-02	7.66E-02	9.65E-02	4.70E-01	25.8
18,000	3.29E-02	2.08E-01	2.30E-02	3.16E-02	6.88E-02	1.11E-01	5.18E-01	19.8
36,000	2.93E-02	2.02E-01	1.72E-02	2.89E-02	6.12E-02	1.18E-01	5.36E-01	16.4

[a] Based on 26,000 hrs operation at 2772 Mwt power level.

Table R.4. Metal Fission Product Radionuclide Elements

Group Number	Element Symbol	Element Name	Atomic Number	Temp (°C)	Density (g/cc)	Possible Compound
3	Cs	Cesium	55	28.6	1.854	CsI, CsOH, Cs_2O, Cs_2O_2
3	Rb	Rubidium	37	38.9	1.437	Rb_2O, Rb_2O_2
4	Te	Tellurium	52	451.	5.71	TeO_2, Te_2O2
4	Se	Selenium	34	217.	3.989	SeO_3, SeO_2
4	Sb	Antimony	51	630.5	6.483	$Sb2O_3$
5	Sr	Strontium	38	770.	2.48	SrO
5	Ba	Barium	56	727.	3.321	BaH_2, BaO, BaO_2, $Ba(OH)_2$
6	Ru	Ruthenium	44	2427.	10.9	RuO_2, RuO_4
6	Mo	Molybdenium	42	2607.	9.34	MoO2, MoO_3, Mo_2O_3
6	Pd	Palladium	46	1552.	10.49	PdO
6	Rh	Rhodium	45	1966.	10.8	RhO_2
6	Tc	Technetium	43	---	---	---
7	La	Lanthanium	57	930.	5.955	LaO, La_2O_3
7	Nd	Neodymium	60	1024.	6.638	Nd_2O_3
7	Eu	Europium	63	---	---	Eu_2O_3
7	Y	Yttrium	39	---	---	Y_2O_3
7	Ce	Cerium	58	804.	6.685	Ce_2O_3, CeO_2
7	Pr	Praseodymium	59	935.	6.611	PrO_2, Pr_2O_3
7	Pm	Promethium	61	---	---	Pm_2O_3
7	Sm	Samarium	62	---	---	Sm_2O_3
7	Zr	Zirconium	40	1850.	5.8	ZrO_2
7	Nb	Niobium	41	2468.	7.83	NbO_2, Nb_2O_5

Table R.5. Decay-Heat Elements and Groupings Olsen, Jensen, Carlson, and Cook (1989)

Pseudo-Element	Element	Mass Concentration (g-atom/MW [thermal])	Retention Fraction
FpM			
Metals	Mo	.6053	.97
	Tc	.1545	.97
	Ru	.3885	.97
	Rh	.0690	.97
	Sb	.00244	.85
	Te	.0627	.85
FpOx			
Monoxides	Sr	.2155	.90
	Ba	.1915	.90
Dioxides	Zr	.7352	.99
	Ce	.3870	.99
	Np	.0422	.99
	Cm	.00204	.99
	Nb	.01139	.99
	Pu	.7921	.99
	Am	.00593	.99
Sesquoioxides	Y	.1099	.99
	La	.1662	.99
	Pr	.1446	.99
	Nd	.4638	.99
	Sm	.0539	.99
	Eu	.01705	.99
FpAlkMet			
Alkali Metals	Rb	.0819	.19
	Cs	.3776	.19
FpHalogn	Br	.00530	.10
Halogens	I	.0320	.10
UO_2	U	user input	
Zr, ZrO_2	Zr (Structural)	user input	

APPENDIX S

REVIEWER LETTERS IN THE ORIGINAL

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PENNSTATE

(814) 865-2519
FAX: (814) 863-4848

Department of Mechanical Engineering
College of Engineering

The Pennsylvania State University
208 Reber Building
University Park, PA 16802-1413

December 27, 1994

Dr. L. W. Deitrich, Director
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439

Re: "In-Vessel Coolability and Retention of a Core Melt," by T. G. Theofanous, C. Liu, S. Addition, S. Angelini, O. Kymalainen, and T. Salmassi

Dear Dr. Deitrich:

It is a pleasure to participate in the review of the above-referenced report. As you requested, I have concentrated my review in the areas of *natural convection* and *critical heat flux* covered in the document.

The various chapters and appendices that address natural convection and critical heat flux in relation to lower head integrity are generally well written. They provide a detailed description of the major findings of the work performed by the authors and a concise summary of others' past and on-going research efforts. Overall, the information presented in the report appears to be quite convincing and complete. There are, however, several important technical points that are not well substantiated by experimental evidence and/or sound theoretical arguments. These technical points, which need to be further evaluated, are discussed below.

1. *Configuration Dominated by Natural Convection Phenomena*

The partition of thermal energy flow by natural convection presented in Chapter 5 and the formulation of thermal loads under natural convection presented in Chapter 6 were based on the steady-state configuration shown in Figure 2.2. This specific configuration represents the final state that would actually be realized in any in-vessel retention scenario. However, as explained below, this steady-state configuration may not bound all intermediate states and thus, it can not be solely based upon in assessing the natural convection problem at hand.

Following the initial, major relocation event but before the attainment of a final steady state, a transient situation could arise within the lower head in which a region of the molten pool developed a large local internal heat generation rate due to a concentration of the larger burnup portion of the uranium oxide fuel and fission products. This non-uniform, highly concentrated, volumetric energy source could cause a period of very intense heat transfer from the core melt to the local vessel wall. During this period, the downward heat fluxes in the local region could be considerably higher than those observed under steady-state conditions. Because of this intense, localized heating of the wall, a hot spot could develop in the lower head. This hot spot could lead to wall thinning and jeopardize the lower head integrity. However, the presence of a large localized heat source would induce strong convective currents in the local region, resulting in rapid dispersion and dilution of the fuel rich material. Once the fuel concentration becomes more uniform (i.e., diluted), it no longer would cause a high heat flux in the local vessel wall and the hot spot would diminish. This transient situation, which involves the development of a hot spot , is apparently *not bounded* by the enveloping configuration depicted in Figure 2.2.

It should be noted that a localized hot spot covering an elliptical region of approximately 1m by 0.8m was found to exist for about 30 minutes in the reactor lower head during the TMI accident. Results of the TMI-2 Vessel Investigation Project indicated that the hot spot was not caused by impinging molten corium jets. Rather, it was caused by a large localized heat source arising from sustained heat loading from the debris on the lower head. Conceivably, the transient situation described above could arise under certain circumstances and thus, it can not be excluded in risk analysis.

2. *Dependence of the Surface Heat Fluxes on the Length Scale of the Melt Pool*

For a volumetrically heated pool, the heat removed from the boundaries of the pool must exactly balance the energy generated within the pool under steady-state conditions. This is the case for the oxidic pool illustrated in Figure 5.2. Assuming a uniform volumetric heat generation rate, the energy generated in the pool is a monotonically increasing function of the pool depth. It follows that the surface heat fluxes at the pool boundaries must also increase with the pool depth (although the "up" to "down" energy flow split may either increase or decrease). Otherwise, a steady-state natural convection process can not be maintained in the pool. This is true no matter the natural convection flow regime is laminar or turbulent (see discussion on the turbulent flow regime in the next paragraph). Physically, the steady-state surface heat fluxes from a volumetrically heated pool can not be independent of the pool depth. In view of this, the arguments of length scale independency or small length scale dependency of the surface heat fluxes discussed in Chapter 5 and Appendix B are not physically meaningful. In conducting experimental studies of natural convection in a volumetrically heated pool, the geometry and the size of the pool are always among the key features that need to be correctly simulated.

For highly turbulent natural convection flow (i.e., at sufficiently high internal Rayleigh numbers), the convective heat transfer is expected to be independent of the physical dimensions of the pool. This is because the fine scales of turbulent mixing in the well-mixed region are considerably less than the pool depth. It follows that the Nusselt number - Rayleigh number relationship should be given by a correlation of the form

$$Nu \sim Ra'^{0.25} \quad \text{for} \quad Ra' \rightarrow \infty$$

which is consistent with the limiting behavior of the Nusselt number given by equation (5.15). Note that the product, *QH*, in equation (5.1) is proportional to the total heat generated in the pool per unit area of the upper surface. This product term always appears together and should not be separated. For highly turbulent flow, the upper surface heat flux is expected to vary linearly with the product term, with the remaining terms being independent of the length scale. To be physically meaningful, the index of 0.2 in equation (5.10) should be replaced by 0.25.

3. *Simulation of the Divergent Effect and the Three-Dimensional Aspects of the Two-Phase Boundary Layer*

The local flow structure on the external surface of the pie-segment geometry described in Appendix E.1 can not be fully simulated by using the constant-width test section of the ULPU facility. Although the local heat flux may be matched by using the power-shaping approach, the detailed hydrodynamic behavior of the two-phase boundary layer flow can not be fully simulated. For the Pie-segment geometry, the cross-sectional flow area is not constant but increases downstream in the flow direction. The local power levels in the lower part (i.e., upstream portion) of the pie-segment geometry are considerably higher than the corresponding values for the constant-width test section. Thus, the bubble activities in the upstream locations are more intensive for the pie-segment geometry than for the constant-width test section. As a result, more vapor per unit surface area will be produced upstream in the pie segment. The population of the vapor phase, however, tends to diverse downstream as they flow upward along the pie segment owing to the increase in the cross-sectional area. This divergent effect, which may strongly influence the boiling process and thus the critical heat flux, is absent altogether in the constant-width test section.

Besides the divergent effect, the constant-width test section of the ULPU facility can not simulate the three dimensional aspects of the boundary layer boiling process that takes place on the external bottom surface of a AP600-like reactor. The superficial vapor velocity represents only one of the several requirements that need to be satisfied in simulating the boundary layer boiling process. Other flow parameters including the local void fraction, characteristic bubble size, bubble growth-and-departure frequency, and the divergence of the vapor bubble population in the flow direction need to be matched in the simulation. These flow parameters may have important effects on the boundary layer boiling process and the local critical heat flux distribution. Note that as a result of the boundary layer flow effects, the dynamics of the two-phase flow may vary significantly along the curved and diverging heating surface. Conceivably, matching the superficial vapor velocity alone is not enough in simulating the actual 3-D process, as the superficial velocity represents only a necessary condition but not a sufficient condition for the simulation.

With the divergent and the three-dimensional effects, higher vapor velocities can be accommodated without exceeding the CHF limit. Thus more heat can be removed from the heating surface by nucleate boiling. This means that the local CHF values measured in the ULPU tests represent a conservative estimate (rather than the best estimate) of the actual situation. In the actual 3-D case, higher local critical heat fluxes can be anticipated.

4. Simulation of the Subcooling Effect due to the Gravity Head

In a fully flooded cavity, the water in the vicinity of the lower head would have ~ 14°C subcooling as a result of the gravity head. Thus it is necessary to properly simulate the phenomenon of subcooled boiling on the external bottom surface of the reactor vessel. However, exactly how this was done using the power-shaping method in the ULPU facility is not immediately clear.

For saturated boiling, the superficial vapor velocity at a given downstream location can be uniquely related to the accumulated power generated in the upstream portion of the test section. Thus matching of the local superficial vapor velocity can be conveniently accomplished by using the power-shaping approach. For subcooled boiling, however, the superficial vapor velocity at a given downstream location can not be uniquely related to the accumulated power generated upstream. This is due to the fact that condensation of the vapor phase would take place within the boundary layer in the presence of subcooling. The accumulated amount of vapor that is condensed before reaching a given downstream location depends on the size of the vapor bubbles, the local vapor velocity, the vapor population density, the cross-sectional flow area, and the degree of subcooling. None of these parameters except the degree of subcooling can be simulated in the constant-width test section. It does not appear to be feasible to match the superficial vapor velocity in the ULPU tests using the power-shaping approach for the case with subcooling. A more detailed description of the power shape used in the experiments for Configuration II should be given in the report.

Please let me know if you need any clarification on the above comments. Thank you very much for the opportunity to participate in the review.

Sincerely,

Fan-Bill Cheung

Fan-Bill Cheung
Professor

FBC/lc
cc: Dr. L. Baker, Jr.

Sandia National Laboratories

P.O. Box 5800
Albuquerque, New Mexico 87185-1137

February 24, 1995

L.W. Deitrich, Director
Reactor Engineering Division Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439

Dear Dr. Deitrich:

Enclosed is the my review of the report: In-Vessel Coolability and Retention of a Core Melt. Per your request, I have reviewed the material concerning **Critical Heat Flux**. I also commented on the mini-ACOPO experiment as well as metal/oxide phase separation.

Sincerely,

T.Y. Chu

Review of "In-Vessel Coolability and Retention of a Core Melt."

I. Comments on Critical Heat Flux

The review covers the material in Chapter 3 and Appendix E entitled The ULPU Experiments. The experiment appears to be well designed and executed within the constraints of the assumptions made.

The review will be presented from two points of view:
A. Does the ULPU experiment simulate the three-dimensional boiling process on the exterior of the reactor vessel?
B. The application of ULPU data to in-vessel core retention.

A. The two criteria: (1) matching superficial velocity at and beyond the point of interest, and (2) a gradual build-up of superficial velocity up to the point of interest, are reasonable; however, by no means guarantee that the flow fully simulates the actual 3-D flow outside of a reactor vessel. For example, there is no flow divergent effect in the strip and the velocity development is certainly different in the ULPU case due to the difference in the superficial velocity upstream of the point of interest. Furthermore, as pointed out in the report, the dynamic aspect of the flow and condensation effects are not properly taken care of by the criteria. Physically, the shape of a wedge cut from a hemisphere takes a $\sin\theta$ profile, since $\sin\theta$ varies rather slowly near 90°, the CHF data is likely to be accurate near the equator. However, in the bottom center region, the strip geometry does not adequately simulate the 3-D axi-symmetric two-phase boundary layer flow. A comparison of the data in Figure E.12. (Appendix E.2) and the recent data of Cheung and Haddad (Proceedings WRSM 22, October 1994), Figure 1, shows that the CHF values obtained in ULPU might be too low near θ=0°. It is interesting to note that away from the bottom center area, the two sets of data have similar trends.

Specific comments:

- What criterion is used to determine "For θ_m as small as 10°, the simulation is deemed to be acceptable, (p. E.1-6)?" The use of passive voice without giving a justification is not informative.
- Unless there are good reasons to discard the UF-6-0 and UF-5-0 data, they should be included in Figure E.18. These values are not far from the Cheung and Haddad data.
- The data presented in Figure E.16. suggest that there is considerable lateral gradient in the heating block. If this is not the case, a new plot should be used.
- The large axial conduction correction for Configuration I is disconcerting. What would happen, if the experiment is run with the heating zone around the point of interest twice as wide? Or more generally, does the width of the heating zone influence the measured CHF values?
- CYBL can be operated to 400 kW/m^2 as currently designed.

B. Because the margin to failure is fairly significant (as shown in Chapter 7), the reviewer feels that despite the inaccuracies involved, the critical heat flux data is sufficient for the present purpose, provided the following clarifications are made:

1. There is a substantial increase of CHF in Configuration II, due to the natural circulation loop. The data from Configuration II is used to demonstrate the large thermal margin. Therefore, the authors must provide a more detailed justification that the natural circulation observed is prototypic, in terms of flooding level, dimension of riser and downcomer, and the correspondence between the strip geometry and the axi-symmetric geometry in the *integral* sense. The arguments made in the power shaping principle are largely based on reproducing the *local* condition at the measurement location of interest .

2. The experimental methodology stresses "the determination of the critical heat flux... under the constraint of a specific power shape. (p.E.1-5)" Under this methodology and specifically the power shaping principle, the results presented in Figure E.12. (section E.2) are only valid for the power shape in Figure E.11. (section E.1) Therefore, there is a contradiction in principle, to apply the CHF curve to the assessment of different power shapes in Chapter 7, Figures 7.13 to 7.16. To borrow an expression from thermodynamics, one needs to answer the question of whether CHF is a point function or a path function. It is entirely likely that CHF is only a weak path function. But justifications (which may require sensitivity experiments) must be made to smooth out this apparent contradiction.

3. The authors repeatedly stress the importance of aging the surface; however, there apparently is no attempt to characterize the surface. At least a simple sessile drop observation or a SEM should be provided. This is especially important in the upcoming tests with the painted steel test section. How does the paint age under the test conditions? Should only data with new paint (never boiled) be used for in-vessel core retention assessment? How does the paint age in service? How can the test data be applied to the "real" accident conditions?

4. It is interesting that the Vishnev correlation (Vishnev et al., "Study of Heat Transfer in Boiling Helium on Surfaces with Various Orientations," Heat Transfer-Soviet Research, vol. 8, no. 4, p. 104-108) derived from laboratory scale experiments and using helium as a working fluid, actually predicts the ULPU data trend to within 10% (Figure 1). The Vishnev correlation specialized to the nomenclature of the present report is:

$$\frac{CHF_\theta}{CHF_{180}} = \left(\frac{10+\theta}{190}\right)^{0.5}$$

Where $\theta = 0°$ corresponds to horizontal downward-facing, and $\theta = 180°$ corresponds to horizontal upward-facing.

CHF phenomenology is still a mystery.

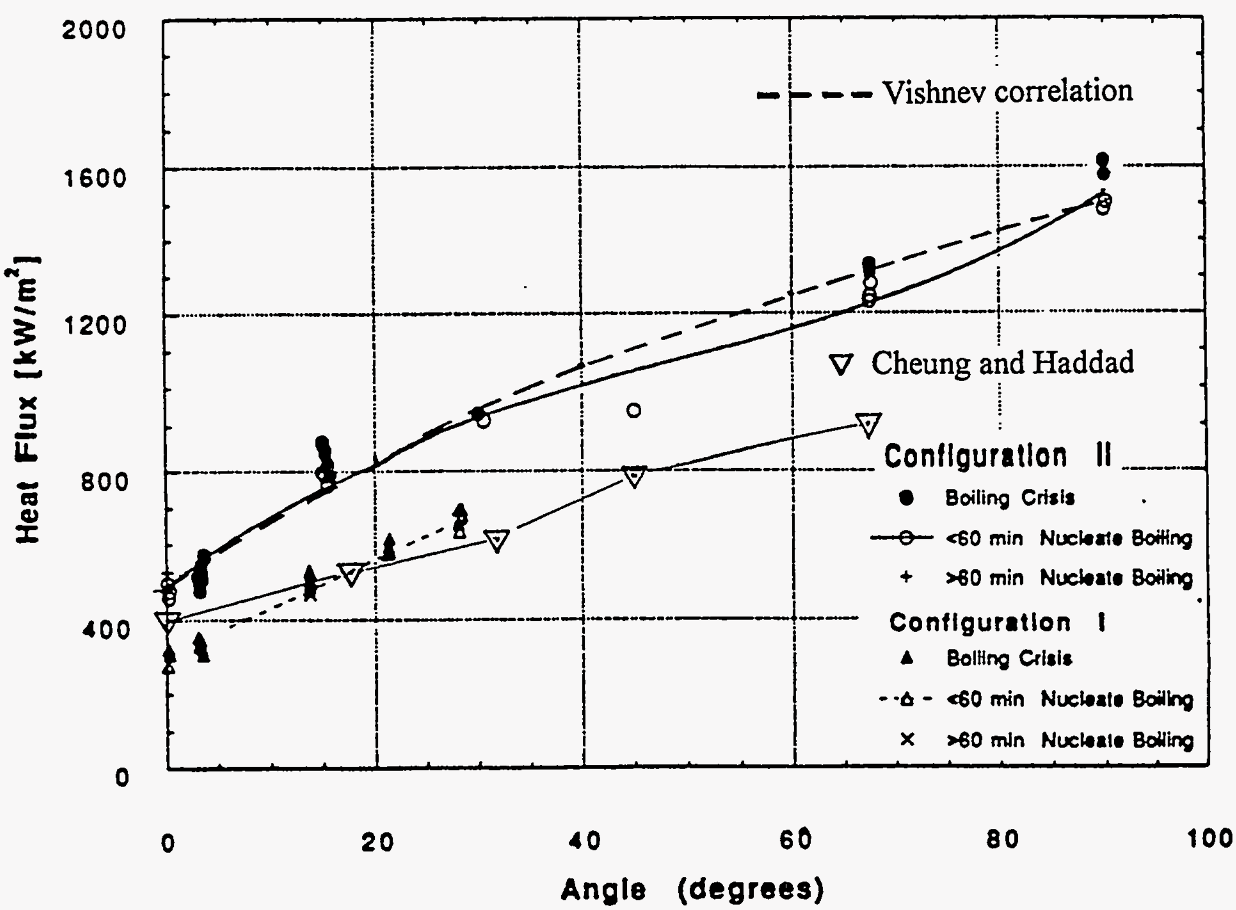

Figure 1. Comparison of Cheung and Haddad Data, Vishnev correlation and ULPU Data

II. Comments on mini-ACOPO Experiments

The authors should be congratulated for making a conceptual breakthrough in simulating natural convection in pools with internal heat generation. This problem has puzzled experimentalists for the last twenty years. However, for the experimental results to be applicable in a local sense, more detailed justifications will be needed than presented in the report. The energy equation for the problem of interest is (taken from Kelkar et al., 1993):

$$\frac{\partial(\rho C_p \phi)}{\partial t} + \overline{\nabla} \cdot (\rho C_p \overline{U} \phi) = \overline{\nabla} \cdot \left(\left(k + \frac{\mu_t C_p}{\sigma_\phi} \right) \overline{\nabla} \phi \right) + S$$

The authors' contention is that by assuming quasi-steady states during a cooldown experiment, the variation of the bulk stored energy (temperature) with time:

$$\frac{\partial(\rho C_p \phi_b)}{\partial t}$$

can be considered to be the internal heat generation rate S. This argument is reasonable in an integral sense. However, if one were interested in local behaviors such as local heat transfer coefficients, it might be necessary to show explicitly that the local (X_i) variation of the stored energy in the fluid

$$\frac{\partial\left[\rho C_p \phi(X_i)\right]}{\partial t}$$

is everywhere uniform because the problem of interest is for spatially uniform heat generation. This type of data should be available from the interior thermocouples. While these data are not accessible to the reviewer, the discussions of self-similar profiles, Figures D.4. and D.5. in the report suggest that perhaps the bottom 10% of the volume may follow a different decay history. If this observation were true, local heat transfer coefficients from $\theta = 0°$ to $40°$ could be in error. Another location of interest would be the $\theta = 80°$ to $90°$ region where there is large difference between the mini-ACOPO result and the UCLA result. The effect of boundary conditions should also be examined. Isothermal boundary conditions will promote mixing (uniform thermal response) but an adiabatic upper boundary may be more problematic. Again, these are observations based on incomplete information, but the reviewer feels that the authors need to examine the data carefully before extracting local information and apply the information to the assessment of in-vessel core retention.

There are other related issues the reviewer will not cover here. However, all these suspected uncertainties can perhaps be tested in a temperature decay experiment designed to reproduce the Kulacki-Emara data. Although, it must be recognized that a horizontal layer configuration is more likely to promote a uniform interior behavior.

III. Comments on Metal/Oxide Phase Separation

According to the analyses in the report, the location with the least thermal margin is near the equator of the hemisphere. The main reason for this behavior is due to the steel layer floating on top of the oxide melt. However, according to an analysis by Dana Powers (Dana Powers, "Chemical Phenomena and Fission Product Behavior During Core Debris/Concrete Interactions, Proceedings of the Committee on the Safety of Nuclear Installations (CSNI) Specialists' Meeting on Core Debris-Concrete Interactions, NP-5054-SR, Compiled by R.L. Ritzman, EPRI, September 3-5, 1986), the presence of metallic zirconium can lead to the formation of uranium metal and resulting in a denser metal phase. An experiment by Park et al. is quoted in the paper to illustrate this possibility. Since phase separation is associated with the location of least margin, the authors may want to look into the possible existence of a heavier metal phase.

Review of
Chapter 3, "Thermal Failure Criteria" of
In Vessel Coolability and Retention of a Core Melt,
DOE/ID-1046

In Chapter 3, the authors discuss the coolability of the reactor vessel with emphasis on the heat fluxes that can be accommodated under nucleate boiling conditions on the outer surface of the vessel. Local and global aspects of boiling on the vessel outer surface are discussed. Two sets of critical heat flux data have been obtained (Appendices E.1 and E.2) on a one dimensional full length representation of the reactor vessel. In the first set, the data are obtained under pool boiling conditions with heat supplied to only the lower portion; covering angular position from +30 to -30°. In the experiments liquid was saturated with angular position of the lower stagnation point being 0° and that of the equator being 90°. A correlation for the critical heat flux obtained from these data is reported. In the second set of experiments, a natural circulation loop was established. Heat flux distribution on the test surface was established to simulate a reference heat flux. The reference heat flux was obtained from an earlier study of Theofanous et al. The heated region spanned from 0 to 90°. Because of the hydrostatic head difference in the natural circulation experiments, a liquid subcooling of about 10°C existed near the lower edge. The critical heat fluxes obtained in natural circulation experiments are found to be higher than those obtained under pool boiling conditions. Again, the data have been correlated with angular position. The authors have done careful experiments and have obtained nearly full scale simulation of the prototype. They should be complimented for it. My other comments on the work are as follows:

1. The authors claim that their full length representation affords an essentially perfect full scale simulation. I cannot agree with this statement. At the stagnation point of a sphere, the behavior of the vapor bubbles at departure will be different than that for a plane surface.

2. In the reactor cavity, counter current type of flow simulation will occur rather than that of a natural circulation loop (co-current). Hence, I believe that the configuration shown in Figure E.4 is more appropriate. Data for this configuration have been obtained when the heated region spanned $-30° \leq \theta \leq 30°$. It is important that data be obtained for this configuration when the heated region spans $0° \leq \theta \leq 90°$. The critical heat flux in this configuration will be lower than that for the natural circulation case.

3. Some flashing of the superheated liquid is expected to occur in the upper region ($\theta \approx 90°$). The authors do not report any such observation. A discussion of the effect of flashing in the local critical heat flux in the upper region is also needed.

4. The heat flux imposed on the inner wall is obtained from the earlier work of Theofanous et al. I do not know if the imposed heat flux distribution represents an upper limit for all types of molten pool scenarios that can be envisioned. This includes partially filled lower vessel heads as well.

5. It would have been interesting and informative if the authors had compared their steady state critical heat flux data under pool boiling conditions with the data reported in the literature from small scale (a few centimeters in length) test sections. It should also be noted that most of the data reported in the literature on small scale test sections were obtained under transient conditions.

6. To isolate the effect of global versus local conditions, it would have been valuable if the authors had reported the critical heat flux obtained at a given location when all of the regions upstream of the given location are heated and when heating is provided only locally.

7. The actual heat flux profiles on the heated block surface were obtained by numerically solving the two dimensional conduction equation with appropriate boundary conditions. No information is given as to what those boundary conditions were. Also, we are given little information on the progression of the dryout front from zone to zone after occurrence of critical heat flux conditions at a given location.

8. It is stated that the annular gap in the prototype is 20 cm. From the information given in the report, I cannot ascertain if the hydraulic diameter in configuration I of ULPU is scaled properly with respect to the prototype.

Finally, I believe that the authors have obtained very valuable data. However, at this point, the information is incomplete and it is not possible to conclude that boiling heat flux on the outer surface of the vessel will be below the local critical heat flux under all types of heat fluxes imposed on the inner wall of the vessel.

Fauske & Associates, Inc.

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JAN - 3 1995

ACTION: LB

INFORMATION: [illegible]

December 22, 1994

Dr. L. W. Deitrich, Director
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439

RE: Review of "In-Vessel Coolability and Retention of a Core Melt," by Theofanous et al.

Dear Dr. Deitrich:

I have read with care the chapters of the above-referenced report that were assigned to me, namely Chapters 2, 6, 7, 8, and 9. I felt compelled to also read Chapter 5 in order to gain the required background for Chapter 6.

Overall I find the authors' version of in-vessel retention to be a scrutable and believable one. In particular, I liked the authors accident scenario-independent treatment of the subject. Moreover, I feel that the report will serve as a handy reference source for the pertinent, recent literature on natural convection in volumetrically heated pools, downward boiling, and thermophysical properties of high-temperature materials.

I only have two major comments with regard to the technical content of the report, both of which are aimed at strengthening the authors' already good case for in-vessel retention. These comments are listed below and are followed by several additional, but relatively minor comments that the authors may wish to consider.

(1) It is not clear to me that the authors have provided a conservative treatment of the melt layer, as stated in Section 5.2. My understanding is that Churchill and Chus' free convection heat-transfer correlation, Eq. 5.39, gives the average heat flux along the vertical segment of the reactor vessel wall in contact with the molten metal layer. I would anticipate a considerable variation of the local heat flux along this segment with a peak heat flux achieved just beneath the surface of the metal layer that may be of the order of a factor of two greater than that predicted with Eq. 5.39. Perhaps the authors feel that they have incorporated or compensated for "heat flux peaking" when they speak of the "focusing effect" and lateral eddy diffusion limitations in the bulk (on page 5-17). Unfortunately I have difficulty in following these arguments or pinpointing where in

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Telefax (708) 986-5481

Appendix N that these arguments are confirmed. Perhaps I am wrong, but my feeling is that the only major limitation to the lateral flow of heat is the laminar sublayer adjacent to the vessel wall and that, in order to properly assess the maximum heat flux from the metal layer to the vessel wall analytically, the appropriate coupling (thermal and mechanical) must be made between the upward flowing free stream just outside the side-wall free-convective boundary layer and the downward flow within the boundary layer itself. Alternatively, the heat flux variation along the side wall can be obtained by experiment, perhaps with a modified version of the apparatus described in Appendix N.

(2) I think the authors can provide a more convincing jet impingement analysis (argument) than the one presented in Section 8. In particular, I believe more information is needed to justify the lower bound jet diameter of 10 cm. It seems to me that a breach on the core-side boundary may first appear as a small opening (pin-hole or crack). Thus the early stages of the core draining process may occur via a narrow, high-impingement heat-transfer jet. Of course, the jet heat flux will decrease with time owing to the enlargement of the breach. An analysis of this process should appear in Section 8, and apparently such an analysis is available (Sienicki, 1995). More detail regarding Turland's (1994) work should also be included. In other words, all the available arguments that put the jet impingement issue to rest should be spelled out in Section 8. Also, something should be said about the unlikelihood of molten metal jet impingement during core relocation.

Minor Comments:

(3) The authors may wish to reference Epstein and Fauske (Nuclear Technology 87, 1989, 1021-1035), as they were the first to suggest the core relocation picture illustrated in Fig. 2.3 (for TMI) and to my knowledge they were the first to examine heat loads from in-vessel molten-core material pools by using a methodology that is very similar to the one used in the subject report.

(4) Is there any experimental data that supports the last sentence of the paragraph that follows Eq. (H.8) in Appendix H (page H.6)? I believe that this sentence should read "when the stream diameter becomes sufficiently small compared to the boundary layer thickness ahead of the ablation front ......". It would seem to me that the head thickness is not an important parameter with respect to the erosion rate, as long as melt is removed from the cavity formed by the jet as the jet erosion process proceeds.

(5) I was particularly interested and impressed by the experimental work reported in Appendix D. I might mention that we (FAI) proposed the idea of a quasi-steady cool down experiment to simulate steady-state turbulent natural convection with volumetric heating some time ago (verbal and written solicitations to ARSAP and EPRI, respectively, June 1992 through February 1993). I was pleased to learn by reading the report that the method works and I hope it will be utilized to once and for all settle the issue of the heat transfer split in hemispherical segment pools at "infinite" Rayleigh number.

(6) The inequality $Ra < 10^{12}$ on the top of page 5.17 bothers me. Given the form of the correlation (Eq. 5.39) I am sure that there is a lower Rayleigh number below which this correlation is invalid.

(7) Typos: (i) Page C-17, change ragid to rigid in figure caption for Fig. C.6 and (ii) Page N-5, 4 lines from bottom: "furtitious"?

I hope my review comments are useful to the authors.

Sincerely,

Michael Epstein

Michael Epstein, Vice President
Consulting Services

ME:lak

Fauske & Associates, Inc.

January 9, 1995

RECEIVED
REACTOR ENGINEERING DIVISION
—DIRECTOR'S OFFICE—
JAN 11 1995
ACTION: LB
INFORMATION: LWD

Dr. L. Walter Deitrich
Argonne National Laboratory
9700 South Cass Avenue
RE 208 C224
Argonne, Illinois 60439

SUBJECT: *Review of DOE/ID-1046*

Dear Walt:

As requested, I have reviewed the report entitled, *"In-Vessel Coolability and Retention of a Core Melt"*. I agree with the general approach taken in the report, the formulation of the analyses for the molten pool, the relative distribution of heat fluxes from the pool and the conclusions of the report. While I believe some additions need to be made to the report, which are discussed below, this report can be used as a document which assembles the major works performed in this area and provides sufficient justification for the conclusion that external cooling of the reactor pressure vessel lower head and cylinder can prevent failure of the structures even when molten core debris exists in the lower head.

There are some elements of the discussion and the analyses presented, which I believe need to be expanded.

1. The discussion with respect to the molten pool is focused on a fully molten pool with a rigid boundary at the melting temperature. Certainly this is the case for experiments such as the COPO and UCLA tests. However, as discussed in the report, the core debris in the lower head would be expected to have different temperatures for the solidus and liquidus states. The report clearly specifies the temperature that should be used to characterize the heat transfer from the molten pool, i.e. the liquidus temperature. However, there is no discussion on the influence of a "slush layer" between the fully molten pool and the rigid frozen crust on the vessel inner surface when there is a significant difference between the solidus and liquidus. How would this be expected to influence the correlations that have been developed from pools in which the solidus and liquidus temperatures are equal, i.e. a single melting temperature?

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My intuition is that this would tend to decrease the downward heat transfer and increase the upward heat transfer. If this is the case, the use of the correlations by the authors for fully molten pools tend to be a conservative representation of the reactor system. Some discussion should be included with respect to the importance of this slushy layer between the pool and the crust and the general influence this would have on the calculated results. The details of this behavior are relatively complex, but likely not of first order importance. However, the qualitative influences of this difference should be considered in the report.

2. As discussed in the report, the sequences which are considered are generally those in which the RPV lower plenum is full, or almost full of water, at the time that molten core debris enters the lower head. Experience with such situations indicates that there could be a non-trivial contact resistance developed between the crust and the wall when this occurs. Such a contact resistance is not considered in the analysis presented in the report. Neglecting such a resistance is a conservatism in the analysis for the downward energy transfer to the RPV lower head. Conversely, this increases the upward heat transfer to the remainder of the RPV and therefore the heat flux transferred to these other parts of the reactor vessel. Estimates from the available information suggests that the contact resistance could be the equivalent of conduction through a few centimeters of UO_2. Here again, the details of the analyses do not have to be included; rather the influence of such behavior should be discussed and perhaps included as part of the sensitivity analyses at the end of the report.

3. There is discussion with respect to the influence of a boil-up level in the gap between the insulation and the reactor vessel cylinder. The inleakage of water through the gaps in the insulation must be considered as a two-way street. Water certainly can readily ingress into the insulation, but the boil-up level can also tend to leak out through the gaps in the insulation thereby decreasing the influence of such a boiled-up situation. This should be discussed in terms of both behaviors.

4. The bottom line to the integral evaluation is discussed in Section 6. Since this documents the integral analysis, I recommend that this discussion be expanded to make several of the central elements of the analysis more clear. For example,

   a. Equation 6.6 describes the heat flux into the wall. Does $\delta_{cr}(\theta)$ include the power generated in a "slushy layer" dictated by the temperature difference between the liquidus and solidus conditions?

   b. The upward radiation calculation described in 6.10 assumes one characteristic temperature for the steel internal structures and therefore does not need to consider the respective view factors to individual parts of the reactor vessel, i.e. the downcomer and the upper internals. If the discussion is only focused on the integrity of the lower head, this is sufficient. Conversely, if the intent is to describe the potential for in-vessel core debris retention, then it is important to justify that the upward energy flux does not cause the vessel to fail at some other location between the metal layer and the vessel support location, i.e. the hot legs and cold legs. To accomplish this, the analysis should be somewhat more detailed than that which was represented by Equations 6.10 and 6.12.

d. The solution scheme for Section 6.12 discusses using T_s as an iteration parameter. By deduction it appears that this is the average temperature between T_{si} and T_{so}. However, I could not find this stated in the discussion. Since the upward heat flux from the pool and the dissipation to the respective parts of the reactor vessel and its internals are equally as important for in-vessel retention as the behavior of the lower head, the specific details of how this solution is determined and the respective split between upward and downward energy transfer should be displayed in this section. This needs to be done to justify the conclusion that "thermally-induced failure of an externally flooded AP600-like reactor vessel is physically unreasonable."

As mentioned above, I believe that this report provides the necessary foundation for documenting the case for in-vessel retention using the numerous attractive features of the AP600 design. However, to provide this foundation, several of the discussions in the report should be enhanced such that the approach and conclusions are clear.

I hope that you find these comments constructive and should you have any questions regarding any of these, please feel free to call me at any time.

Sincerely yours,

Bob

Robert E. Henry
Senior Vice President

REH:jal

April 18, 1995

Dr. Theo G. Theofanous
Center for Risk Studies and Safety
University of California
Santa Barbara, CA 93106

CC:

Business Services
University of California
Santa Barbara, CA 93106-2090

Dr. L. W. Deitrich
Argonne National Laboratory
Building 208
Room C213
Argonne, Illinois 60439

Subject: Review of DOE/ID-10460, "In-Vessel Coolability and Retention of a Core Melt" by T. G. Theofanous, et. al.

Dear Theo:

After some delay due to travel and work schedules, I have completed my review of the subject document. My review comments are attached.

Yours truly,

T. S. Kress

T. S. Kress

Tom S. Kress
102-B Newridge Rd.
Oak Ridge, TN 37830

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INFORMATION:

April 18, 1995

Review of DOE/ID-10460, "In-Vessel Coolability
and
Retention of a Core Melt"

T. S. Kress

Introduction:

In this review, I considered the key items that would influence the ability of external cooling to prevent vessel failure to be:

1. Quantity of melt
2. Composition of melt
3. Decay heat level
4. Internal pool heat transfer
5. Radiation heat transfer off top surface
6. Boiling heat transfer on outside of vessel
7. Integration of Items 1-6 (resultant wall temperatures, wall thinning, ability of wall to carry loads, and treatment of uncertainties).

My review comments that follow are ordered as above and are intended to address the adequacy with which each of these were dealt.

1. Quantity of Melt and 2. Composition:

The analysis included all of the oxidic core, all of the Zr available, the lower support plate, the reflector, the lower supports, and some portion of the core barrel. The fraction of Zr oxidized was treated probabilistically in three ranges:

- most likely range..... .4 to .6 (probability of P)
- unlikely range........ .6 to .7 (P/10)
- highly unlikely range. .7 to .9 (P/100).

Comments:

The greater the quantity of ZrO_2 added to the melt, the more dilution effect you will have (that is, you will reduce the effective volumetric heat generation rate). In addition, putting more of the Zr into the melt as the oxide reduces the thickness of the metallic layer overlying the fuel melt. Thus, I would expect higher values of ZrO_2 fraction to be non-conservative with respect to this problem.

I think the probability density function for the fraction of Zr oxidized should have included some relatively high probability that it would be less than .4.

Similarly, when one adds the amount of steel in the lower support plate, the reflector, and the lower supports one gets a total of 77 tons without adding in any of the core barrel. I would have expected to see the "likely probability" range for the steel mass in Figure 7.5 to extend upwards to beyond 80 tons instead of the 72 tons shown.

With the ROAAM procedure, I worry about cliff effects. An abrupt and severe change in the probability between ranges could mask a strong sensitivity in the region near the abrupt change. Because of the focussing effect of the metallic layer, the content of steel might be such an area to expect such a strong sensitivity.

3. Decay Heat Level (volumetric heat generation rate):

The report chose to look at a bounding sequence (3BE) as being "of main interest to IVR". According to the MAAP code, this sequence gives the fuel melt in the vessel bottom head at about 4 hours after shutdown. To get the decay heat level at that time, the procedure was to multiply the total decay heat by the fractional contribution due to the non-volatile fission products.

Comments:

I have some concerns about the above procedure. The choice of bounding sequence appears to be well founded. I would not be comfortable, however, in relying on only one codes calculation to determine the timing. I recognize that Figure 7.12 results from shifting this timing to one hour sooner and that this is an appropriate manner to address the sensitivity to this. Nevertheless, I see some strong sensitivity in the calculated $q_w(\theta)/q_{chf}(\theta)$ to this shift although the decay heat increase was small. My concern stems from concern about the validity of the decay heat value.

The overall decay heat curve (that includes all nuclides) looks reasonable for a ~2000 MW th reactor compared to what I am familiar with for higher power reactors. (The 2000 MW value is my guess for the AP600. The report is remiss in not giving the real value or the source of its decay power curve). The modification to account for the loss of volatiles could be in error. The correct procedure would be to remove the appropriate volatiles at the initial time and redo the ORIGIN-type calculation that includes the decay schemes to determine the evolution of decay heat versus time. I am concerned that the process used may underestimate the decay heat because the decay schemes may build in additional volatiles not correctly accounted for by the procedure and which would remain in the pool to contribute their decay heat.

In addition, core melt accidents do not necessarily release all the volatiles before the melt enters the lower head. Estimates I have seen range as low as 50% released for the Iodine and Cesium and as low as 10% for the Te and Sb. Generally, even some small amounts of the Xe and Kr are assumed to remain with the melt. The conservative approach would have been to retain some portion of the volatiles within the melt.

The report is remiss in not defining exactly what nuclides it considers to be volatiles and in not defining what fraction of these are assumed to be removed from the melt. This is all wrapped up in Figure 7.2 which, incidentally, looks suspect to me. I do not believe the fractional contribution of the non-volatiles approaches 1 immediately after shutdown.

4. <u>Internal Heat Transfer Coefficients:</u>

Equation 5.28 was basically used for the pool-to-wall heat transfer coefficients as corrected for local distribution by Eqs. 5.30a and 5.30b. For the upward heat transfer to the overlying metallic layer, the Steinberner-Reineke correlation (Eq. 5.12) was used. Each of these was validated (or derived) via the Mini-ACOPO experiments as discussed in Appendix D. For heat transfer within the metallic layer, an existing literature correlation (Globe-Dropkin) was modified to allow separate application to heat transfer from the pool crust through the bottom boundary layer in the metal and from the metallic layer through the upper boundary layer to the top surface. For the "sideways" heat transfer from the metallic layer to the vessel wall, another existing correlation (Churchill-Chu, Eq. 5.35) was used which, coincidentally, gave a heat transfer coefficient approximately 1/2 that of the modified Globe-Dropkin correlation. The MELAD experiments reported in Appendix N were conducted to demonstrate the validity of the correlations for the metallic layer.

<u>Comments:</u>

The internal heat transfer aspects of this problem are, in general, well done and acceptable. The Mini-ACOPO experiments appear to be well founded and well conducted. The results from the 1/8 scale facility should be applicable to the full scale. I have one major comment and then a number of minor comments on this part of the evaluation.

The major concern I have here is with the use of the Churchill-Chu correlation for the sidewards heat transfer from the metallic layer. I see no good reason why this heat transfer coefficient should be so much less than that for the bottom and top surfaces. The MELAD experiments reported in Appendix N appear to validate the proposed use but these were conducted in a significantly different geometry from that of the disc shape in the reactor case. I would like to see some additional theoretical analyses to justify these results.

There is a need to better describe in the report the thermocouple locations in the Mini-ACOPO experiments.

More justification is needed for the use of transient experiments to model steady-state conditions. This was addressed by Runs A4 and A5 in Appendix D. However, some comparisons of characteristic times would be helpful to completely close this issue.

Figures D4 and D5 should identify the various data points shown at a given value of V_i/V (I assume they are for different times during the transient-but we are not told).

There is no figure showing that lateral temperature gradients are negligible as claimed on page D-11. An oversight?

The report should do a better job of defining "Φ". It does not appear on the Figures or in the Nomenclature.

5. Radiation Off Top Surface:

Radiation off the top surface of the metallic layer was treated in a standard manner that includes back radiation from the sink which was given a single constant temperature (to be solved for from the equations that include the total heat upward through the top surface, radiation, conduction through the heat sink, radiation off the back side of the heat sink to the vessel internal wall, and conduction through the vessel wall essentially to the water temperature). An emissivity of .45 was used and a sensitivity analysis was done for higher emissivity values.

Comments:

The procedure used is appropriate and acceptable. Nevertheless, I would have liked for the sensitivity study to have included lower emissivity values if only as an artificial means to try to enhance the "focussing" effect. I don't know whether or not the metallic layer has a crust on the top surface. A newly formed frozen layer of metal may have a low emissivity value.

6. Boiling Heat Transfer on Outside of Vessel:

The objective here was to determine the distribution of critical heat flux on the bottom head submersed in a water bath. This was accomplished experimentally by the use of the innovative 1-D ULPU test facility that had the following characteristics:

- full length/correct curvature

- a "slice" geometry
- power input varied with position to match the distribution of heat transfer from the pool side as measured in Mini-ACOPO
- an "aged" copper surface.

Comments:

I found the description of the experiment procedure in Appendix E to be somewhat obtuse. With persistence, however, you can figure out what was done.

I believe the experiment procedure to be valid (i.e. determining the local CHF as a function of angular position by matching the steam flow into the local region that would be obtained as produced in upstream areas for the total heat required to produce the local CHF. It is recognized that a 2-D prototype is modelled by 1-D tests. I believe this is conservative because the 2-D streamlines are divergent whereas the 1-D streamlines in the test are parallel. This should result in a slightly lower measured CHF than one would expect in the real case.

I believe when these tests are validated for the surface material, this will be sufficient to determine the distribution of CHF on the external surface of the bottom head.

7. Integration to Determine Resultant Wall Temperatures, Wall Thicknesses, Loads, and the Ability to Carry the Loads:

Mostly, deterministic calculations were used. However. the ROAAM procedure was used with assigned probability distributions for

- decay power
- quantity of Zr oxidized
- quantity of steel in metallic layer, and

some sensitivity studies were also made.

Comments:

I commented earlier on the probability ranges for the above parameters. I also believe the sensitivity studies should have included variations in the opposite directions to those made. For example,

- a lower value of emissivity
- an overprediction of the downward heat flux (rather than Mayinger's correlation which underpredicts the downward heat flux)
- a shift of the fraction of Zr oxidized to the left rather than to the right.

For the "thermal jets" issue, the use of only 1/3 of the fuel volume and a jet diameter of 10 cm need better justification. Figure 8.1 shows that even with V_r = 1/3 of the fuel volume and D_j = 10 cm, you get a total ablation depth of 12.5 cm -- perilously close to the wall thickness of 15.24 cm. It doesn't take much more fuel or a much smaller jet diameter to ablate through.

Final Comments:

This was indeed a comprehensive and competent piece of work to address this issue. I checked all of the equations presented and could find no errors.

The report itself suffers, I believe, from including too much peripheral material put there for "perspective". I think the report would have been better if it focused more on what was actually done and on the correlations actually used in the analyses.

The defense of the case, in my mind, strongly rests on justifying the choice of decay heat value. The comments I made earlier in this review on the content of volatiles, the timing, and the appropriate modification of the curve for loss of volatiles are very important.

December 30, 1994

Dr. L. Baker
Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439

Dear Lou:

Subject: Review of DOE/ID-1046, In-Vessel Coolability and Retention of a Core Melt, by T. G. Theofanous, C. Liu, S. Additon, S. Angelini, O. Kymalainen, and T. Salmassi.

As per Argonne National Laboratory (ANL) request of November 10, 1994, I have reviewed the subject report and I wish to first command the authors for their extensive analytical and experimental work in support of the concept of "in-vessel retention" in the AP600 passive nuclear Pressurized Water Reactor (PWR). However, I have several concerns about the studies and I have attempted to group them by specific topic areas to help the authors prepare responses to my comments:

A. Boiling Crisis or Critical Heat Flux (CHF)

DOE/ID-1046 relies upon data from Figure 3.3 for CHF as a function of position on the lower head for quantifying the thermal failure criteria. These data were taken under full submergence and natural convection in the ULPU facility. My concerns are as follows:

1. Natural convection enhances the CHF condition. This is clearly visible by comparing the results of Figure 3.3 with those of Figure 3.2 obtained for pool boiling. The increase in CHF is 67% at the zero degree angle position and 36% at the 90 degree angle. This means that the natural circulation in the tests must simulate accurately the flow behavior in the AP600. It should be noted first that in Figure E.1 the cold water is returned at the bottom of the cavity rather than "draining into the reactor cavity through a tunnel at the compartment floor elevation which spills into the cavity at the elevation of the top of the lower head" when the IRWST drain valves are actuated (see page M-4 and Figure M-2). Subsequently during "passive reflux to the cavity" (which is being simulated by the ULPU tests), water "would enter in the outlet nozzle region and drain down through the octagonal portion of the cavity" (see page K-4). During this mode of

operation steam water flow will rise in a counter flow mode to the returning water in the cavity annulus. This countercurrent flow will produce less natural circulation flow than in the ULPU tests and also it most likely will impact the subcooling of the water reaching the bottom head.

2. The authors have recognized that their tests do not include reactor pressure vessel insulation. The insulation is bound to interfere with the natural circulation flow not only by reducing the size of the annular gap but also by providing increased resistance for the water to reach the vessel outer surface. An allowance should be provided for this reduction until tests with prototypic insulation can be carried out.

3. The tests were performed with thick highly conducting walls. Past CHF tests have shown that such circumstances will increase the local critical heat flux. While the reactor pressure wall thickness is large to start with, it could thin down significantly during the course of the severe accident and tests with less conduction might be appropriate.

4. The AP600 reactor vessel standoff insulation concept depicted in Figure K.1 shows narrow (about 2.5cm) flow passages between the vessel and the insulation panels. Even in the alternative insulation concept of Figure K.2, the flow passage is about 5cm. (The concept in Figure K.2 will create strong cavity air recirculation along the reactor vessel wall, which will reduce the effectiveness of the insulation and increase the temperature of the reactor cavity concrete). Such insulation configurations will not only reduce the natural circulation flow rate but they would encourage the steam to flow along the narrow spacing between the reactor vessel and the insulation. Therefore, they would tend to approach conditions found in thin rectangular channels submerged in saturated liquid. A significant amount of CHF data has been obtained in thin vertical channels and they show a drop in pool boiling CHF as the ratio of length to width of the channel increases. At atmospheric pressure and a length to width ratio of about 30 the CHF drops to 32 percent of the accepted pool boiling value (see M. Mode et al, Critical Heat Flux During Natural Convective Boiling in Vertical Rectangular Channels Submerged in Saturated Liquid, ASME Transactions, Journal of Heat Transfer, Vol. 104, pp 300-303, May 1982). Some similar and strong negative impact due to the presence of insulation is expected in the AP600 configuration and its magnitude will depend upon the final design of the insulation. Still, an allowance needs to be provided at this time.

5. The potential impact of the accident upon the insulation is noted in the report. However, if the large LOCA break takes place within the cavity, one can expect significant damage to the insulation and potential flow blockages in the cavity outlet nozzle region.

6. Because the water refilling the cavity is borated, its boiling will deposit boron on the reactor vessel surfaces and its impact upon CHF was not considered. Also, the water reaching the reactor cavity will contain dirt and dust and it will accumulate in a reactor cavity which cannot be expected to be clean and may contain paint flaking off the vessel. This lack of water purity conditions needs to be recognized.

7. In view of the preceding comments, significant degradation in the CHF values of Figure 3.3 are anticipated (possibly by as high a factor as 2 to 3). It is remarkable, therefore, that no sensitivity study of this important parameter was included in Section 7.3

and it is recommended that it be added.

B. Subdivision into Regimes and Lack of Analysis of Intermediate Transient States

DOE/ID-1046 is limited to the long term, natural convection-dominated thermal regime conditions depicted in Figure 1.2. There are several statements in the report that "this approach is conservative" (see page 2-1) and that "the thermal loads to the pool boundaries throughout the time period of a heat-up transient are bounded by the thermal loads in the final steady state" (see page 2-2) but very little basis and proof are offered for such positions. A few examples are given below to show that it might not be the case:

1. The proposed long term pool configuration depicted in Figure 1.2 shows an oxidic pool surrounded by an oxidic crust with a metallic layer above it. According to the report, most of the metallic layer comes from the melt out of stainless steel structures in the lower plenum and, during the heat up transient, the steel must rise through the oxidic pool before reaching the top layer. The temperature of the steel because of its high conductivity will approach that of the oxidic pool and during the melt out phase of the lower plenum it would be superheated and could reach temperatures above 2900 K. Such rising superheated molten material will have several negative impacts, including:

(a) As it reaches the vessel, it could lead to CHF conditions on the outside surface of the vessel.

(b) It could lead to failure of the vessel wall because superheated metallic material will attack and erode the vessel at an accelerated rate.

(c) It would not allow the formation of a top oxidic pool crust as depicted in Figure 2.1.

(d) It would radiate to top structural components and cause their melt and failure. Such top components would fall within the pool and disturb the natural circulation patterns as well as possibly produce cracking of the crust layer separating the oxidic pool from the reactor vessel.

In this reviewer's opinion, failure of the reactor vessel during this transient heat-up by the metallic layer or other causes (e.g. falling components) may be a dominant mode of failure and it is not considered in DOE/ID-1046 at the present time.

2. On the top of page 2-2 it is noted that the report is restricted to "scenarios in which failure to supply coolant into the reactor vessel persists indefinitely". On page 1-3 it is stated that "energetic interactions concerning late water injection are relatively benign due to the prevailing stratified configuration" and the "integrity in the early potentially energetic, steam explosion regime, can be assessed against the full lower head capability". Addition of water on top of a rising superheated metallic layer will not be benign and may approach the steam explosion regime particularly if it contains between 10 to 65 percent by weight of molten zirconium (see Table 1.2). It will not be benign even with a stratified layer. Furthermore, before such energetic interactions occur the reactor vessel wall would be thinned down by impingement of a molten jet and by erosion by the hot oxides and metals and the full structural capability would not be available.

3. On page 3-3, it is stated that "partially flooded conditions are of limited interest, as discussed in Appendix M". In fact, in Appendix M, it is reported that "the PRA concludes

that flooding was unsuccessful in 20% of the core damage cases" and this is high enough to justify dealing with a partially flooded reactor vessel. Under such conditions, the radiation would decrease to the vessel walls but it would increase to the top components and enhance their chance to fail and participate in the scenario. Also, there would be a sharp discontinuity in the vessel wall temperature much closer to the top of the metallic layer. Finally, the degree of water subcooling outside the reactor vessel would be lowered and so will the CHF condition.

4. There is no reason not to expect the partial melting material configuration depicted in Figure 2.1 to progress to that of the complete meltdown of Figure 2.2. If this is the case, the melt impingement produced by partial meltdown could erode the reactor vessel steel by as much as 12 to 14 centimeters (see page H-7). The corresponding weakening of the reactor vessel is not considered in the Structural Section 4.

It is therefore recommended to reassess the conclusion on page 2-5 that there are only two specific configurations to be considered because they "bound the thermal loads on the lower head with respect to any other intermediate state that can be reasonably be expected". Other configurations, scenarios, and transient intermediate states need to be included and shown to not impact the results.

C. Overstylized Pool Configuration

The pool configurations shown in Figures 1.2 and 3.1 are very stylized and some of the presumeded simplifications are expected to impact the predictions in DOE/ID-1046:

1. The pool configurations and the heat transfer results are predicated on the existence of a crust (or solid interface) separating the oxide pool from the metallic layer. As noted under comment B.1, there can be no crust as the molten material from the bottom stainless steel structure rises through the oxidic pool. Even under long term conditions, it is difficult to visualize how a strong crust could form "naturally" above the oxidic pool and support 67 to 72 tons of metallic material over the large reactor vessel span of the AP600. Without a crust/solid interface the heat transfer at that surface could be higher because there would be a wavy interface produced by two counter flowing fluids. Also, the temperature at that location would be higher and above the specified oxidic components liquidus temperature of 2973 K and so will the bulk metallic layer temperature.

2. A single molten bulk temperature is used in the oxidic pool and the metallic layer. Physically, one can expect stratification vertically and radially in the oxidic and metallic pools. The temperatures should rise away from the cooled walls in the radial direction. Also, vertical stratification due to gravity will lead to increased temperatures vertically. Such maldistribution of temperatures can be expected to have an impact upon crust formation, natural cirulation currents and upwards and downwards split in heat transfer. For example, with a reduced temperature towards the bottom of the vessel, the viscosity will increase (particularly if some solids become present) and the downwards heat transfer will drop. In contrast, the upwards heat transfer will rise which tends to strengthen the reviewer's concern about reactor vessel failure at the oxidic-metallic interface or above it.

3. The report considers only two phase diagrams: an uranium dioxide (UO_2) - Zr oxide (ZrO_2) phase diagram and an iron (Fe) - zirconium (Zr) phase diagram. According

to NUREG/CR-5869, several Zr, stainless steel (SS), UO2, and ZrO_2 eutectics were formed in melting experiments at Oak Ridge in 1987 (Nucl. Eng. Des., 121, 324-337, 1990) and they are listed in Table 18.3 in Attachment 1 taken from NUREG/CR-5869. Furthermore, there can be a large number of other material species involved as illustrated from Table 18.4 in Attachment 1 for a BWR bottom pool. They come from the species present in stainless and control rod materials which are also present in the AP600. It is also worth noting from Table 18.3 in Attachment 1 that the Zr-SS eutectic has a melting temperature of 1723 K (150 K above the metallic melting point of iron-zirconium used in DOE/ID-1046). There is also a strong possibility for the formation of a Zr-SS-UO_2 eutectic with a melting point of 1873 K (300 K above the metallic melting point used in DOE/ID-1046). This eutectic has the added complication of being able to produce some internal heat generation. There is no question that the phase diagrams in the reactor case will be much more complicated than those in the presumed overstylized pool and the presence of additional eutectic mixtures with higher liquidus temperatures and their potential formation of solid particles must be recognized.

D. Natural Convection in Oxidic Pool

DOE/ID-1046 relies upon pool natural convection correlations and the mini-ACOPO data to predict the heat transfer in the oxidic pool. There remain several concerns about this approach:

1. Some concerns yet to be resolved are listed in the report:
   (a) Timewise variation of the stratification pattern within the pool (see page 5-10) and the relationship of the final, truly steady state to the sequence of transient states leading up to it (see page 5-3).
   (b) Dependence upon Prandtl number. All the data in the report have been taken at Prandtl number of 7 (Kulacki-Emara, Jahn and Reinecke, and Steinberger-Reinecke), at a Prandtl number of 8 (UCLA) at Prandtl numbers of 2.6 to 10.8 (mini-ACOPO). The Prandtl numbers are higher than those anticipated in the reactor case. In 1955, the reviewer used integral methods to predict natural convection flows (see Attachment 2) and it was clearly shown that for laminar flow the Nusselt number was dependent upon the Grashof number times the square of the Prandtl number for low Prandtl numbers instead of the Grashof number times the Prandtl number. Also, there was an extra dependence found upon the Prandtl number in turbulent flow (this is also true in Eq. (5.39).
   (c) There is considerable scatter among the available data. This is illustrated in Figures 5.7 and 5.8. The scatter certainly exceeds the "30% discrepancy which could be potentially rather significant to our conclusions due to the importance of the upwards heat flux on the behavior of the steel layer" noted on page 5-6. Similarly, the exponent on the Rayleigh number exhibits considerable variation. This becomes all the more important at the very high Rayleigh numbers anticipated in the oxidic pool. Here again, it is worth noting that Attachment 2 shows that the Grashof and Rayleigh number exponent varies for a laminar boundary layer from 0.2 for a horizontal place facing upwards to 0.25 for a

vertical plate which explains the range of exponents shown in Eqs. (5.10) to (5.17), (5.19), (5.20), and (5.22) and (5.23). In the case of turbulent flow along the entire boundary layer, the exponent on the Grashof number according to Attachment 2 is found to vary from 0.36 for a horizontal plate facing upwards to 0.4 for a vertical plate. These turbulent predictions give partial support to the exponents in Eqs. (5.27 and (5.28), particularly if one takes into account the initial buildup of a laminar boundary layer. Also, the change in behavior observed in the mini-ACOPO data at a Rayleigh number of $3(10^{13})$ may be due to a local transition from laminar to turbulent flow.

2. All the tests have been performed with a pool completely liquid and with small temperature differences from the bulk to the heat transfer surface. The use of a film temperature to calculate the heat transfer is questionable, particularly in view of the large temperature differences expected in the reactor core, the great number of eutectics, and the presence of solids discussed under comment C.3.

It is hoped that the ACOPO experiments being performed presently will help resolve some of the concerns noted above. However, it is important to note that the ACOPO tests are non prototypic of the reactor case because they cannot account for the presence of several eutectics and their solidification at different temperatures or for a metallic layer in direct contact with the oxidic pool.

E. Globe and Dropkin Correlation for Metallic Layer

DOE/ID-1046 relies upon the Globe and Dropkin correlation to predict the heat transfer within the metallic layer. This correlation was supplemented by the use of a Churchill and Chu correlation to predict the heat transfer on the vertical wall of the metallic layer. The combination was justified by a simple simulant experiment (MELAD) described in Appendix N. Several concerns with this approach have already been noted and they are reproduced here for completeness purposes:

1. There will be no crust between the metallic layer and the oxidic pool. There will be direct contact between these two fluids at a wavy interface and the rates of heat transfer will be different and higher from those obtained from the Globe and Droplin correlation.

2. In order to take into account conduction within the fluid the Globe and Dropkin should be modified by adding 1.0 to the right handside of Eq. (5.34).

3. The Churchill and Chu correlation does not agree with the equations proposed in Attachment 2 and this may deserve further examination.

4. The use of film temperature is questionable again particularly close to the metallic layer-oxidic pool interface where the wavy interface could produce a much higher and oscillating temperature.

5. The energy balance equation (5.43) lacks a radiation term to account for reflected energy from the receiving surfaces. The right hand side of the equation should have a negative term which contains the emissivity of the receiving surface and its absolute temperature raised to the fourth power. This term could have a significant impact on the

results presented in DOE/ID-1046.

F. Other Comments

1. The reviewer spent little time on the structural aspects of the report except to note that:

(a) An impulse methodology is utilized in Figure 1.1 to determine the potential for the structural failures. As mentioned on the top of page 1-4, "this is illustrative of global considerations; the actual assessment is likely to require additional details, such as the space-time distribution of the loads" as well as the space distribution of vessel wall thickness and temperature.

(b) There will be discontinuities in vessel wall thickness and temperatures due to the initial melt impingement on the bottom reactor vessel head (see B.4) or due to different erosion rates at the oxidic pool-metallic layer interface, or due to partially flooding the reactor vessel. Stress concentration factors need to be applied to take such discontinuities into account.

2. The thermophysical properties derived in Appendix L utilize iron (Fe) rather than stainless steel. Stainless steel has about half the thermal conductivity of iron and similar variations are expected for other properties. This needs to be corrected. Also, as noted under comment C.3, stainless steel, zirconium and UO_2 can form several eutectics with higher melting temperatures. With the anticipated weight percent of zirconium (10 to 65 percent), it is not clear why the Zr-SS-UO_2 (0.3/0.6/0.1) eutectic would not play a dominant role and possibly produce a multilayered configuration.

3. An important assumption made in DOE/ID-1046 is that the heat generation is uniform and confined to the oxidic pool. With the suggested stratification and temperature maldistribution discussed in comment C.2, it is anticipated that UO_2 will tend to favor the upwards portion of the pool and that the heat generation per unit volume could be much higher in that region. Also, note that the SS-ZrUO_2, eutectic could be present in the metallic layer and provide some limited heat generation.

4. In Table 7.3 which tabulates the accidents contributing to the AP600 core damage frequency (CDF) from a Level 1 Probabilistic Risk Assessment (PRA), vessel rupture is shown to account for 23 percent of CDF and it is considered not relevant to in-vessel retention (IVR). This is not fully correct because it means that IVR cannot be effective on 23% of the accidents contributing to the CDF.

5. I continue to remain confused by the use of the Risk Oriented Accident Analysis Methodology and the judgments used to formulate probability density functions in Section 7 but I have decided to defer on this topic to other reviewers more familiar with probabilistic and risk assessment techniques.

6. It is recommended that the report title be limited to the specific case of the AP600 concept. It requires depressurization of the vessel, its lowerhead to be fully submerged and a low power density. The combination of such characteristics is found today in only the AP600.

In summary, at the present time I cannot support the conclusion on top of Section 9 that "thermally-induced failure of an externally flooded AP600-like reactor vessel is physically unreasonable". There is no question that the chances of in-vessel retention have been improved but the conclusion that failure is physically unreasonable will require dealing with the comments provided herein and particularly with the need of prototypic CHF tests and natural circulation tests with prototypic corium and metallic pools.

I hope that these comments are useful to you and I appreciate the opportunity to participate in the review. Before closing, let me reiterate that my negative comments are not meant in any way to detract from the progress made about in-vessel retention by the investigators participating in DOE/ID-1046.

Sincerely yours,

Salomon Levy

Salomon Levy

Table 18.3 Eutectic mixture compositions considered for the lower plenum debris bed

Eutectic mixture	Mole fractions	Melting temperature	
		°F	K
Zr – SS[a]	0.193 – 0.807	2642.	1723.
Fe – Cr – Ni[b]	0.731 – 0.190 – 0.079	2660.	1733.
Zr – SS – UO_2	0.300 – 0.600 – 0.100	2912.	1873.
ZrO_2 – UO_2	0.750 – 0.250	4172.	2573.

[a]SS represents stainless steel.
[b]This is the stainless steel eutectic mixture.

Table 18.4 Independent material species considered for the lower plenum debris bed

Material species	Molecular weight	Melting temperature (°F)	Heat of fusion (Btu/lb_m)
Fe	55.85	2960.[a]	117.
Ni	58.70	2960.[b]	129.
Cr	52.00	3400.	136.
Zr	91.22	3365.	108.
B_4C	55.26	4450.	814.
FeO	71.85	2510.	190.
Fe_3O_4	231.54	2850.	256.
NiO	74.71	3580.	292.
Cr_2O_3	152.02	4170.	296.
B_2O_3	69.62	4450.	148.
ZrO_2	123.22	4900.	304.
UO_2	270.07	4960.[c]	118.

[a]Actual melting temperature is 2800°F.
[b]Actual melting temperature is 2650°F.
[c]Actual melting temperature is 5066°F.

The adoption of a 1-min time constant for the movement of material liquids within the debris bed is the result of testing and experience. Use of too large a time constant will result in unrealistic predictions of free-standing liquid columns within the central control volumes. On the other hand, a time constant that is too small will result in the prediction of unrealistic sloshing of liquids between adjacent control volumes. Experience has shown that the use of a 1-min constant for lower plenum debris bed applications will result in a prediction of smooth and realistic spreading of liquids from their source control volumes.

18.1.3 Material Properties

The lower plenum debris bed model calculates composition-dependent properties of density, porosity, specific heat, and thermal conductivity for the debris mixture within each bed control volume each timestep. Specifically, the local porosity is based upon the relative mass fractions of solid metals and oxides within the control volume, while the representative local density, specific heat, and thermal conductivity are mass-averaged values based upon the relative amounts of each debris constituent present. (The relative masses of the solid and liquid phase of each constituent are also considered in the calculation of density and thermal conductivity.) The variation of material properties with temperature is considered where appropriate. A detailed discussion of the method by which these properties are calculated is provided in the "BWR Lower Plenum Debris Bed Package Reference Manual."

It is important to note here that almost all of the previous lower plenum debris bed response calculations have been performed for applications in which drywell flooding was not considered and bottom head penetration failures were predicted to occur soon after lower plenum dryout. Accordingly, the liquid fraction within any calculational control volume remained small in these cases, because the liquid would drain from the reactor vessel as it was formed. For the calculations discussed in this report, however, the lower plenum debris bed model is exercised without the provision of penetration failures so that the upper central bed control volumes eventually become primarily or even totally liquid. Within the upper liquid regions of the debris bed, heat transport would be greatly enhanced by the buoyancy-driven circulation of molten liquids. While the model has no representation of this liquid circulation, the associated increase in heat transport is crudely (but adequately) represented by increasing the effective mass-averaged and phase-averaged local thermal conductivity by a factor of 10 whenever the liquid mass within a control volume exceeds two-thirds of the total control volume mass.

As in the case of the relocation time constant, the use of a factor of 10 for enhancement of conduction to represent the effect of liquid circulation is the result of testing and experience. Use of too large an enhancement factor will result in a series of rapid phase changes within a control volume as excessive heat removal causes the liquid to freeze, the concomitant reduction in conduction heat transfer causes the solid to melt, and a new cycle begins. On the other hand, an enhancement factor that is too small will result in

TECHNISCHE UNIVERSITÄT MÜNCHEN
LEHRSTUHL A FÜR THERMODYNAMIK
Prof. Dr.-Ing. Dr.-Ing. E.h. F. Mayinger

Thermodynamik A - Technische Universität München - 80290 München

80333 München
Arcisstraße 21
TEL: (089) 21 05 34 35
21 05 34 36
TELEX: 522854 tumue d
FAX: (089) 21 05 34 51
may@thermo-a.mw.tu-muenchen.de

To the
Director
Dr. L.W. Deitrich
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439
U.S.A.

Ihr Zeichen	Ihr Datum	Unser Zeichen	Unser Datum
		M/ba	10.01.1994

Review report DOE/ID-1046
In-Vessel Coolability and Retention of a Core Melt

Dear Dr. Deitrich,

Please find enclosed my review about the above mentioned DOE-report.

Best regards,

F. M

Prof. Dr.-Ing. Dr.-Ing.E.h. F. Mayinger

Enclosure: Review report (8 pages)

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o. PROF. DR.-ING. F. MAYINGER

Review

on the Report DOE/ID-1046

In-Vessel Coolability and Retention of a Core Melt

by T.G. Theofanous et. al.

1. Introduction

In spite of the experience from the TMI-accident, where several tons of core melt were retained coolable in the lower plenum of the pressure vessel, most of the severe accident studies assume, that the melt penetrates the pressure vessel and the only way of retention would be a core catcher, integrated into the concrete of the containment. In the face of this opinion of many specialists, it is a great service to a realistic assessment of nuclear reactor safety, that the U.S. Department of Energy initiated and sponsored a study on In-Vessel Coolability and Retention of a Core Melt, which was performed by T.G. Theofanous and co-workers and which is subject of review here.

The study was concentrated on the future concept of the AP600-nuclear power plant, however many general conclusions can be drawn for other types, also for nuclear reactors being in operation. Therefore the report deserves general consideration in the nuclear community.

The capability of the pressure vessel to retain the molten core is a function of the heat transfer coefficient at the inner side of the pressure vessel wall (between corium and wall), of the heat conductivity in the material of the wall and the heat transfer at the outer side of the wall (between wall and boiling water). In case, that the heat conduction in the wall would be the limiting parameter one has to check, whether in a melting attack, the wall thickness is so much reduced, that it cannot carry the weight of the molten core any more, even being supported by the buoyancy force of the surrounding water-vapour mixture.

2. Heat transfer between corium and wall of the pressure vessel

The heat transfer between the corium and the wall, as well as the fluiddynamic conditions in the corium, which exists of an oxidic pool and an overlaying metallic layer, were very carefully studied in the report and the results are clearly presented in chapter 5. The authors compared own measurements with experimental and theoretical data from the literature and found agreement to such an extend, that they were able to predict the Nusselt-number for the heat transfer between the oxidic pool and the wall as average value, as well as in the form of local data versus the circumference of the lower hemisphere of the pressure vessel. Especially at high Rayleigh-numbers (in the order of 10^{15}), which are representative for the situation in a real molten pool, the agreement of the data is good, which means, that the heat transfer coefficient can be reliably predicted.

The temperature in the oxidic pool, however, is not only a function of the heat sources and the heat transfer from the melt to the wall, but it is also influenced by the metallic layer, which is superimposed to it. In the metallic layer the density of the heat production by decay heat is much smaller, than that in the oxidic pool. Therefore in a first approximation it was assumed in the report, that pure Benard convection exists, which has a different flow pattern from that of the convection with inner heat sources.

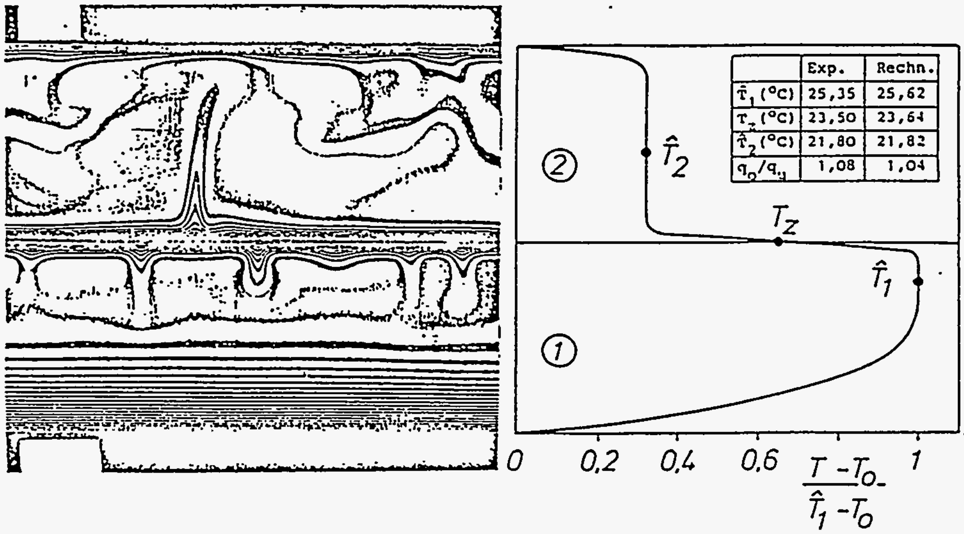
The fluiddynamic behaviour and the heat transfer in a cavity with Benard conditions and the heat transfer to the wall of rectangular cavities are well studied and also understood in the literature. The authors compared data from the literature and by assuming, that the convection in the metallic layer with its cylindrical surroundings can be treated like that in an rectangular cavity, they could derive reliable data for solving their problem. The simplification in the assumption for the geometry can be certainly justified.

If the layer is of pure metallic nature, than one can certainly assume, that there are no or at least neglectable heat sources in it. It is a metallurgical question, whether there could be dissolved some UO_2 in this metallic liquid. Then the situation would be a little more complicated to handle it.

There is a report in the literature, dealing with the thermal interaction between a lower oxidic pool and an upper metallic layer/1/, which however is a little hidden, because it can be only purchased from the "Gesellschaft für Reaktorsicherheit" (GRS) at Köln. It is not classified and therefore freely available. In this report Steinberner and Mayinger studied the heat transfer in two layers systems by using the holographic interferometry. In Fig. 1 an example of the interferograms measured in the two layers are presented. This figure is taken from the above mentioned report.

The aim of these experiments was to study the heat transfer at the phase-interface between the two layers and also the heat loss at the upper free surface of the metallic layer. From this one gets the temperature in the metallic layer.

The temperature distribution in both layers is a strong function of the heat transport from the oxidic to the metallic melt and of the heat transfer at the metallic surface. Of course in addition the heat sources in both layers play an important role. Fig. 2 shows three characteristic cases for the temperature distribution in these layers. The dotted lines in this figure represent the temperature distribution, if no heat transfer between the layers would exist.



	Exp.	Rechn.
$\hat{T}_1$ (°C)	25,35	25,62
T_z (°C)	23,50	23,64
$\hat{T}_2$ (°C)	21,80	21,82
q_o/q_u	1,08	1,04

Fig. 1: Flow pattern in cavities with 2 liquid layers, upper Bernard convection, lower internal heat sources. (Ra'_{lower} = 1 x 10^7, Ra_{upper} = 7,8 x 10^5, $T_{upper\ surface}$ = T_{Bottom})

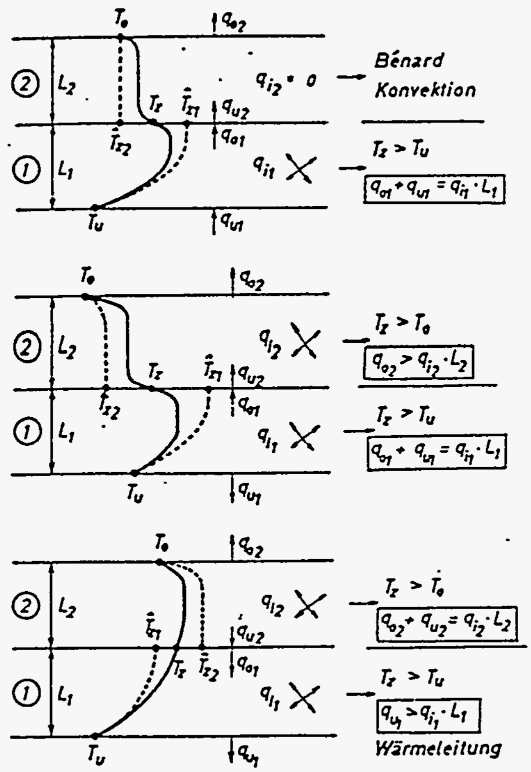


Fig. 2: Temperature profiles and directions of heat fluxes in 2 layer systems with various boundary conditions.

For the case, that there are no heat sources in the upper metallic layer, Steinberner and Mayinger/1/ developed simple correlations for predicting the heat flux from the lower boundary of the oxidic pool to the wall of the cavity, when the density of the heat source is given and the Rayleigh-numbers are known. These correlations have the following form.

$$(1 - \eta)^{0,87} = 0,172 \cdot Ra'^{0,226} (\eta^2 - 2\theta) \tag{1}$$

$$\eta = - \frac{0,104 \cdot \left(- \frac{1}{0,172\ Ra'^{0,226}} - 2 \cdot \theta \right)^{1,305}}{2,47 \cdot Ra'^{(-0,305)} + 0,104 \cdot \left(- \frac{1}{0,172 \cdot Ra'^{0,226}} - 2 \cdot \theta \right)^{1,305}} \tag{2}$$

The symbols in these correlations are defined as follows.

$$\begin{aligned} \eta &= \frac{q_u}{q_i \cdot L} \\ Ra' &= \frac{g \cdot \beta \cdot \dot{q}_i \cdot L^5}{\nu \cdot a \cdot \lambda} \\ \theta &= \frac{(T_o - T_u) \cdot \lambda}{q_i\ L^2} = \frac{Ra_E}{Ra'} \\ Ra_E &= \frac{g \cdot \beta \cdot (T_o - T_u) \cdot L^3}{\nu \cdot a} \end{aligned} \tag{3}$$

Equations (1) and (2) go back to a proposal by Baker et. al./2/ and contain also ideas, which Kulacki et. al. proposed in /3/. The solution of these equation is presented in Fig. 3 in a graphical form.

Please note, that the equations (1) and (2) and the results in Fig. 3 were elaborated for horizontal fluid layers with a flat bottom. They cannot give information about the heat flux at the side wall (90°) of a spherical bottom of a pressure vessel containing two layers of fluid, the lower one with and the upper one without internal heat sources.

In Fig. 3 and in the equations, being the basis of this Fig. q_u stands for the heat flux density at the flat bottom of a cavity and q_{i1} represents the heat source density in the heated fluid layer. The detailed derivation of the equation (1) and (2) can be found in /1/.

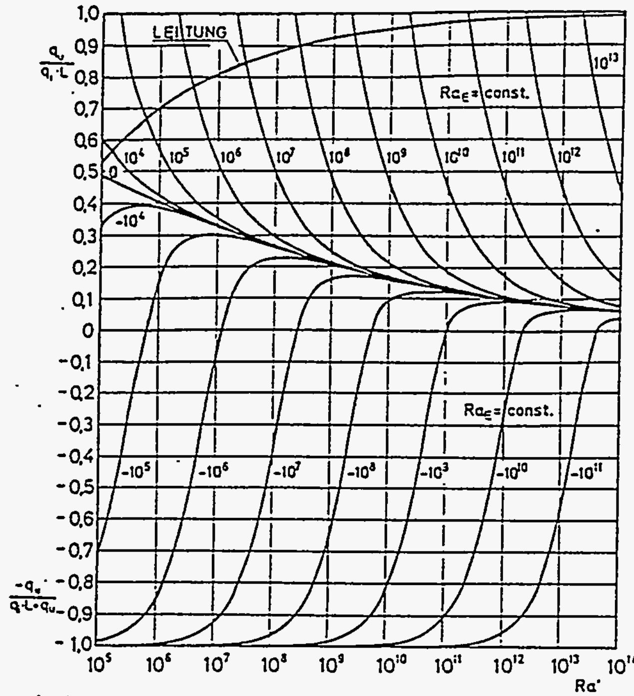


Fig. 3: Dimensionless heat fluxes at the flat bottom of a 2 layer system as a function of Ra'(with internal heat fluxes) and Ra_E (without internal heat fluxes).

The heat from the upper surface of the metallic layer is transported by radiation mainly. Radiative heat transfer is a strong function of the temperature (T^4) and one has to also take into account the heat, which is reflected or radiated from the top of the pressure vessel to the metallic layer. This heat exchange strongly influences the temperature in both layers, the metallic and the oxidic one. With very high temperatures of the fluids the wall of the pressure vessel may start to melt (especially at the side-parts) instead of forming an insulating crust as partially assumed in the DOE-report.

The authors of the DOE-report deliberately do not take into account the very first period of the pool-convection, when the jet of the flowing down melt penetrates the fluid layer and is impinging onto the bottom of the pressure vessel. They argue, that the period of filling up the lower plenum of the vessel is short compared to the time, when the molten pool is exposed to purely free convection. This statement is certainly correct.

There is another argument for this assumption of the authors. As Steinberner/4/ proved in his Ph.D. Thesis, the Nusselt-number at the impinging point of the jet is usually similar or smaller, than that one, which exists at the side wall (90°) with free convection, driven by internal heat sources. Only with very low pool heights these Nusselt-numbers are higher than those at the side wall. Fig. 4, taken from Steinberners work, shows the boundary conditions at a different pool hight, and also the relative Nusselt-numbers. Low pool heights exist only for a short time, when the melt-down process starts. In most accident-cases water would be still present in the lower plenum of the vessel during this very first period, which changes the situation completely and which produces a preliminary quenching of the melt.

In Fig. 4 also interferograms of the temperature distribution in the pool during jet impingement are presented. The black and white fringes can be read as isotherms.

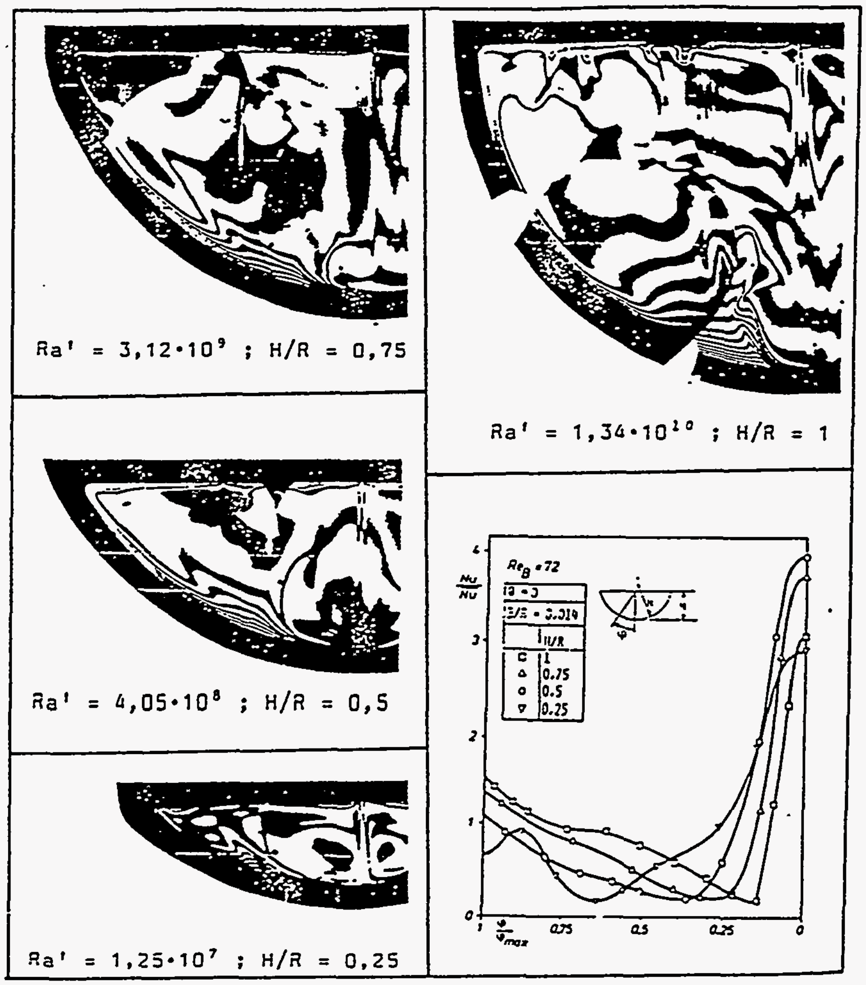


Fig. 4: Temperature fields and relative, local Nusselt-numbers with internal heat sources and liquid jet from the top, for various height-diameter (H/R) ratios. Re_{jet} = 72.

This difference in the pattern of the isotherms between free convection and under jet conditions can be clearly seen in Fig. 5, where the upper interferogram gives the situation without and the lower one with an impinging jet. Comparing the boundary layer at the impinging point and at the 90° position, one realizes, that the temperature gradient and by this the heat flux are similar, which can be deduced from the densely packed pattern of the isotherms.

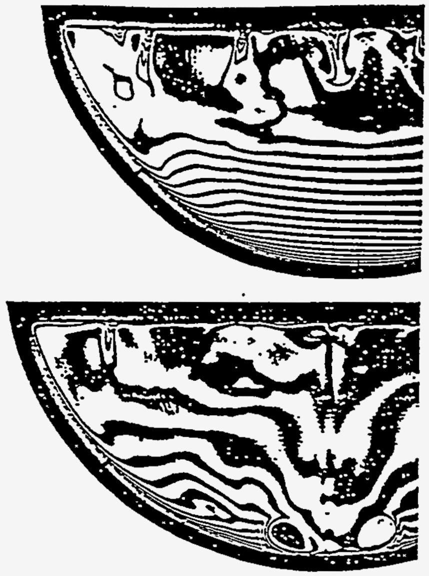


Fig. 5: Typical temperature fields in a cylindrical cavity with (upper interferogram) and without jet (lower interferogram).

So generally speaking one can draw the conclusion, that chapter 5 of the DOE-report precisely and reliably describes the heat transfer from a molten pool - with and without internal heat sources - to a spherical and cylindrical wall. The results presented there are a very good basis for analysing possibilities of retention of a core melt.

3. Heat conduction in the wall of the vessel

To calculate heat conduction in a solid wall is a very simple task, if the transport properties - especially thermal conductivity - are given at the relevant temperatures and if the boundary conditions - heat transfer coefficience and temperatures - are known. There is enough information in the literature and also in the DOE-report about the transport properties. However the boundary conditions at the outer and the inner side of the wall are more complicated to handle.

The heat transfer coefficient at the inner side of the wall is very well described in chapter 5 of the DOE-report, as already mentioned. Also the heat transfer at the outer side or the guarantee, that DNB will not be exceeded, is well documented in the report, as discussed a little later. An open question seems to be, whether at the inner side of the vessel also at the positions of highly convective flow (90°), a crust is formed or whether the material of the wall is eroded by the hot melt. The report presents data on the thermal conductivity of the steel up to 1500 K (appendix L) and also deals with creep considerations for the lower head (appendix G).

Including all the other informations in the DOE-report, it is possible to describe the stress in the wall of the lower head during meltdown and during the free convection of the melt. To do this one needs a small computer code, correlating the feedback control between boundary conditions at the inner and at the outer side of the wall, the heat conduction of the wall and the wall thickness. There are some deliberations in the report about this subject, however I missed detailed calculations of this problem.

A very simple estimation may demonstrate this subject. Let us assume, that the temperature at the outside of the wall is 373 K (nucleate boiling) and that the temperature on the inner side must not exceed 1400 K, then the thermal conductivity varies between 40 and 30 W/Km, with a minimum of 25 W/Km at 1100 K, as can be seen in Fig. L-3 (page L-21 of the report). Furthermore we take a heat flux of 500 kW/m^2 from the melt to the side wall. With a very simple application of Fourier's law, we then end up at a maximum wall thickness of 6 cm for these assumptions.

A parametric study of the temperature situation in the wall at various boundary conditions would still give more confidence to the final and certainly correct conclusion of the DOE-report, namely, that the pressure vessel of the AP 600 can retain a pool of molten core, just by flooding the cavity between the pressure vessel and the shielding concrete.

4. Heat transfer from the wall to the flooding water

Very sophisticated and detailed experiments are reported in the DOE-report, dealing with the subject of critical heat flux at facing-down surfaces and at vertical walls with free convective bubbly flow. This experimental data, together with the nice experiments on free convection heat transfer at the inner side of the vessel wall, proof very reliably, that a safety margin with a factor of 2 exists against critical heat flux, even at positions with very high thermal loads. So one can be sure, that the heat transfer from the wall to the water is negotiated by nucleate boiling, which has very high heat transfer coefficients, as is well known. This means, that the temperature difference between the outer side of the wall and the bulk of the water is very small - in the order of a few Kelvin.

5. Conclusion

The report DOE/ID-1046 is a very fine and reliable document on the coolability of a core melt in the pressure vessel of a medium-sized nuclear reactor and proofes, that a hypothetical core melt situation can be managed and that the debris can be safely retained in the pressure vessel.

Questions rising in connection with that problem are carefully discussed and satisfactory answers are given to all issues, being linked with the thermo- and fluiddynamic phenomena under core melt conditions. The report marks a great and very valuable step forward in the risk assessment of nuclear power plants, especially of nuclear reactors of future design. I would like to congratulate the authors to their work.

A few minor additions to the report - as mentioned in this review - would probably be of interest to the reader, who is not an expert in heat transfer and could improve the value of the report still more.

6. References

1. Steinberner U. und F. Mayinger
   Untersuchungen zur Schmelzpoolstabilisierung, BMFT Abschluß-bericht RS 166-79-05 Band II B. (Available from GRS Köln).

2. Baker Jr. L., Faw R.E. and Kulacki F.A.
   Postaccident heat removal - part I: Heat transfer within an internally heated, non boiling layer
   Nuclear Sience and Eng. 61, 22 - 230 (1976)

3. Kulacki F.A., Min J.H., Nguyen A.T. and Keyhani M.
   Steady and transient natural convection in single and multifluid layers with heat generation
   Dept. Mech. Eng. Ohio State University, Columbus Ohio
   Post-accident heat removal information exchange
   Oct. 10 - 12, Ispra, Italien, 1978

3. Steinberner U.
   Freistrahlinduzierte Mischkonvektion mit inneren Wärmequellen. Dissertation (Ph.D. Thesis) Universität Hannover, 1980.

AST Applied Science & Technology
Robert E. Nickell, Ph.D.

January 5, 1995

Mr. L. W. Deitrich
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439

Dear Mr. Deitrich:

Enclosed is a copy of my review and comment on "In-Vessel Coolability and Retention of a Core Melt" and "Rationale for a Standard on the Requalification of Nuclear Class 1 Pressure-Boundary Components," which I sent to Professor T. G. Theofanous at the University of California at Santa Barbara.

Please let me know if I can be of further service.

Sincerely yours,

Robert E. Nickell

Robert E. Nickell
Technical Director

REN:clc

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AST Applied Science & Technology
Robert E. Nickell, Ph.D.

December 31, 1994

Professor T. G. Theofanous
Department of Chemical and Nuclear Engineering
Director, Center for Risk Studies and Safety
University of California
Santa Barbara, California 93106-1070

Reference 1. "In-Vessel Coolability and Retention of a Core Melt," by T. G. Theofanous, C. Liu, S. Additon, S. Angelini, O. Kymalainen, and T. Salmassi, Report No. DOE/ID-1046, November 1994.
Reference 2. "Rationale for a Standard on the Requalification of Nuclear Class 1 Pressure-Boundary Components," by W. E. Cooper, Report No. EPRI NP-1921, October 1981.

Dear Professor Theofanous:

Thank you for the opportunity to review and comment on the referenced report (Reference 1). Specifically with respect to the experimental and analytical investigations of ex-vessel heat transfer phenomena for a submerged reactor vessel lower head following a severe (core melt) event, this report is very comprehensive. An excellent case is made for the bounding values of heat flux through the vessel wall. (These heat flux limits are referred to erroneously as "thermal loads" in the report, a term that should be reserved for the product of thermal expansion and structural stiffness.) However, my assignment was to review the structural implications of the report, concentrating on Chapter 4 (Structural Failure) and Appendix G (Creep Considerations for the Lower Head). Other portions of the report were examined for context. My comments on the structural sections of the report are provided below.

Chapter 4 contains an argument that the vessel lower head, in the submerged condition, will not fail absent a boiling crisis on or near its external surface. The structural failure criterion is not given explicitly but, from a close examination of the argument, appears to be based on a tensile membrane stress limit equal to the yield strength of the vessel material at an appropriate metal temperature. At the bottom of page 4-1, the required membrane wall thickness of 0.15 mm, when multiplied by a tensile yield strength of 355 Mpa and a vessel circumference of about 12 meters, gives a membrane resultant force of 71 tons. This required wall thickness is then compared to a minimum wall thickness of 1.1 cm that is kept sufficiently cool by the convective heat transfer in the external pool to maintain its strength. This argument is intended to address the stresses due to dead weight less buoyancy forces from displaced water in the pool, with the dead weight inclusive of the weight of the core melt that accumulates at the bottom of the head. The thermal expansion stresses due to temperature gradients across the vessel wall are treated in a

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similar, simplified manner by recognizing the longitudinal bending stress caused by the gradient (and the differential thermal expansion), but then limiting the discussion of the compressive (inside) and tensile (outside) bending stresses to regions away from any geometric or loading discontinuities.

These stresses were not identified in the report as longitudinal bending stresses, and this omission is unfortunate. The report also does not discuss longitudinal bending that might be caused by either a non-uniform distribution of the core melt weight, nor is the effect of non-uniform buoyancy force considered. A stress analyst would expect the deformation of the bottom head and cylindrical side wall to be non-uniform in the radial direction, reflecting the non-uniform distribution of weight, temperature, and buoyancy force, let alone the geometric discontinuity represented by the changes in curvature at the junction between the spherical lower head and the cylindrical side wall. The vessel would be expected to "pinch in" at some points around the longitude, relative to the outward radial motion elsewhere. This does not mean that the net radial displacement would be inward; it means that some portions of the vessel would have greater radial displacement than other portions, giving rise to reverse longitudinal curvature and possible tensile stresses on the inner surface of the vessel. One might suspect that one location of reversed curvature would be at the very bottom of the head, as the result of slightly greater buoyancy forces that cause the head to "dent." Another possibility is at the junction between the head and the cylindrical shell where the meridional curvature changes.

The finite element model shown in Figure 4.5 could be used to study these longitudinal bending effects, provided that the mesh layout in the radial direction (across the shell thickness) is sufficient bending stiffness, in addition to membrane stiffness.

In an effort to determine whether the longitudinal bending effects would be significant, this reviewer searched the other chapters and appendices of the report for: (1) any discussion on the distribution of dead weight (or distribution of equivalent internal pressure), as a function of the meridional coordinate, θ ; (2) distribution of the buoyancy forces, as a function of θ ; and (3) distribution of temperature, even for approximately the same gradient, as a function of θ . Some estimates of the variation in temperature are available (see Figure C.6), showing that the temperature at $\theta = 0$ will be lower than that at $\theta = 90$ degrees, with perhaps a 20 to 25 % variation, irrespective of heat flux.

In order to complete this study with respect to the potential for structural failure of the vessel lower head or cylindrical side wall, the following steps should be taken. *First*, real structural failure modes and structural failure criteria must be considered. Real structural failure modes include such phenomena as ductile rupture, ductile tearing, brittle fracture, low-cycle fatigue, corrosion fatigue, buckling, creep rupture, and creep fatigue. The report currently addresses ductile rupture, on a partial basis, and uses the value of membrane tensile stress (and its comparison to tensile yield strength) as the failure criterion. Ductile tearing at the inside surface of the vessel, caused by reversed longitudinal bending, with either a strain limit or a peak stress limit, would also seem plausible. Creep ruptue has been addressed in Appendix G, again for a simplified state of membrane tensile stress. The other failure modes do not apply to this loading and environmental situation.

Second, in order to determine the probable state of stress and deformation in the vessel as the result of the core melt event, the ABAQUS analysis reported in Chapter 4 should be revisited. The effects of longitudinal bending and potential reversed curvature caused by changes or discontinuities in the geometry or loading should be considered. Of particular importance is the effect of distributing the melt content weight, the temperatures, and the buoyancy resistance in the longitudinal direction. The buoyancy resistance will have an effect similar to a change in vessel stiffness; changes in wall thickness and in radii of curvature will also affect vessel stiffness. The existing ABAQUS model may be too crude, or the applied loadings may have been inappropriate, to detect these longitudinal bending effects.

Third, the calculated stresses and strains from any revised ABAQUS model should be subjected to a sensitivity study over a range of temperature distributions, wall thickness changes, etc. in order to scope out the worst case situations. Then, *fourth*, the stresses and strains for these worst cases can be compared to real failure criteria. A basis for the latter was prepared by Teledyne Engineering Services for the Electric Power Research Institute (EPRI) some years ago, following the TMI-2 event. The relevant pages from Reference 2 are provided as an attachment.

I hope that these comments are constructive, and will enable the excellent work done to date to be placed in a proper context. Once again, thank you for the opportunity to review and comment on this report.

Sincerely,

Robert E. Nickell

Robert E. Nickell
Applied Science & Technology

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DEPARTMENT OF NUCLEAR ENGINEERING
TELEPHONE: (510) 642-5010
FAX: (510) 643-9685

BERKELEY, CALIFORNIA 94720

Dec. 20, 1994

Dr. L. Baker, Jr.
Reactor Engineering Division
Argonne National Laboratory
Argonne, IL 60439

Re: Theofanous document on in-vessel coolability and retention of a core melt

Dear Dr. Baker:

Enclosed is my review of the above report.

Sincerely,

D. R. Olander
Professor

L. BAKER, JR.
JAN 3 1995
Reactor Engineering

Review of:

In-Vessel Coolability and Retention of a Core Melt by Theofanous et al

by D. R. Olander

The following are observations on the report that bear on its basic conclusion. The comments are divided into four categories:

I Chemical phenomena
II Amounts and composition of the liquids in the lower head
III Mechanical aspects
IV Miscellaneous

The report's technical strength is in the area of the thermohydraulics of the phenomena involved. Slightly less thorough is the treatment of the mechanical response of the vessel to the imposed loads. The chemical and materials components of the problem are scarcely dealt with.

I Chemical Phenomena

(a) Reaction of the metallic melt with steam

The report clearly indicates(pp 1-1 and 1-3) that the cavity above the metal pool is filled with steam. The metallic melt contains ~50% of the core's Zr in elemental form. It is impossible for steam and zirconium to remain unreacted during hours of contact at temperatures of ~1600 K. Reaction of steam and zirconium was responsible for the development of the accident in the first place. In the metallurgical industry, addition of small quantities of Zr to molten steel during the steelmaking process is used as a deoxidizing procedure. Contrary to oxidation of solid Zry, buildup of a coherent ZrO_2 layer on the upper surface of the metal pool is unlikely because the substrate is a liquid in turbulent flow.

The kinetics of steam reaction with Zr in the Fe-Zr liquid alloy is not known. It is probably very rapid because of the absence of a protective oxide scale. A conservatively high estimate of the reaction rate(and the corresponding heat release rate) can be made by assuming complete conversion of steam to hydrogen at the surface with the overall rate controlled by mass transfer in the gas phase adjacent to the pool surface. Mass transfer is by natural convection, driven by both the unstable temperature gradient and by the reduction of the gas density at the surface that accompanies conversion of H_2O to H_2. Using the Sherwood number in place of the Nusselt number for the turbulent natural convection correlation for heated plates facing upward(1), the mass transfer coefficient is given by:

$$k_g = 0.14D\left[\frac{g\left(\Delta\rho / \rho_f\right)}{\nu^2}Sc\right]^{1/3} \tag{1}$$

where, for an ideal gas,

$$\frac{\Delta\rho}{\rho_f} = \frac{\Delta T}{T_f} + \frac{\Delta M}{M_f} \tag{2}$$

$\Delta T = T_{l,0} - T_{b,g}$ and $T_{b,g}$ is the bulk steam temperature, taken as 1000K. $\Delta M = M_w - M_H$ is the difference in the molecular weights of water and H_2. T_f and M_f are the mean values of these two properties. With these values, $\Delta\rho/\rho_f \sim 2$.

D is the diffusion coefficient of the H_2O/H_2 system. It is calculated from the correlation given in the appendix of Ref. 2 to be ~ 11 cm^2/s at T_f = 1300 K and a total pressure of 1 atm. The viscosity of a 50 mole % steam-hydrogen mixture at T_f is ~4×10^{-4} g/cm-s and the mass density of this mixture is ~8×10^{-5} g/cm^3. Substituting these values into Eq(1) gives k_g ~ 5 cm/s.

The flux of water vapor to the upper surface of the metal layer is:

$$J = k_g S_{up} \left(\rho_f / M_f \right) \left(y_{b,g} - y_{surf} \right) \tag{3}$$

For an oxide pool volume of 10 m^3, S_{up} = 12 m^2. $y_{b,g}$ is the mole fraction of steam in the bulk gas and y_{surf} is the value in the steam at the surface. These are taken as 1 and 0, respectively. Equation(3) gives a water vapor flux to the surface of ~5 moles/s. At this rate, all of the Zr in the Fe-Zr alloy pool is consumed in ~12 hours(assuming 50% of available Zr in the metal pool).

The heat released by the steam-metal reaction is calculated from the enthalpies of formation of ZrO_2 and H_2O(g) (Ref. 3, Appendix) to be 293 kJ/mole H_2O. The chemical heat release at the surface of the metal pool is 5 x 293 x 10^{-3} = 1.5 MW. This is a significant addition to the ~13 MW from decay heat in the oxide pool. The metal layer surface heat source due to chemical reaction is ~120 kW/m^2.

(b) Metal pool emissivity

The report takes the emissivity of the upper surface of the metal pool to be 0.45, which is reasonable for a clean metal surface. This value was measured by the experiment described in Appendix I of the report. However, if steam had been mixed with the pure argon used in this experiment, the surface of the Fe-Zr liquid would have been oxidized and the emissivity would probably have been ~ 0.8. In the model, this would have increased the radiant heat loss from the pool upper surface and reduced the heat flux to the vessel wall. Credit should be taken for this reduction.

(c) Extraction of uranium from the oxidic pool by the metal alloy

It is well-established that molten cladding dissolves UO_2 pellets to produce a melt that contains up to 40 wt % uranium on an oxygen-free basis(4). Therefore, the elemental Zr in the metal pool should also extract uranium from the oxidic pool. The melts from the TMI 2 core contained small quantities of uranium(5). This process will reduce the eutectic temperature of the metal pool from that of the Fe-Zr binary to that of the U-Fe-Zr ternary alloy. A pseudo-binary phase diagram of this alloy can be approximated by averaging the Fe-Zr and Fe-U phase diagrams.

(d) Vessel wall melting temperature

The report used the eutectic temperature of the Fe-Zr binary for the melting temperature of the wall($T_{l,m}$ = 1335°C). This is correct only if the melt composition is x_{Zr} = 0.088 mole fraction zirconium. For $x_{Zr} \neq 0.088$, the appropriate value for $T_{l,m}$ is the liquidus temperature in the phase diagram shown in Fig. 6.1 of the report. For $x_{Zr} \leq 0.088$, this can be approximated by:

$$T_{l,m}(^{\circ}C) = 1536 - 2284x_{Zr} \tag{4}$$

The steady-state heat flux balance at the metal melt-vessel wall interface is:

$$h(T_b - T_{l,m}) = k(T_{l,m} - T^{**}) / \delta_S \tag{5}$$

where $h = A(T_b - T_{l,m})^{1/3}$ [Eq(5.41)] with A given by Eq(5.47) and δ_S is the thickness of the vessel wall adjacent to the metal layer. It is in general not equal to the as-fabricated value($\delta_{s0} = 5$ cm) because iron may precipitate on the wall or the wall may dissolve in the liquid to give a thickness that satisfies Eq(5) for the specified value of T_b. The wall thickness relative to the as-fabricated value calculated from Eq(5) is:

$$\frac{\delta_S}{\delta_{S0}} = \frac{T_{l,m} - T^{**}}{T_b - T_{l,m}} \frac{1}{Bi} \tag{6}$$

where

$$Bi = h\delta_{S0} / k \tag{7}$$

is the Biot number. Using the value A = 2764 given in the example on p. 5-19 of the report, $\delta_{S0} =$ 0.05 m, and k = 25 W/K-m(Table 7.1):

$$Bi = 5.5(T_b - T_{l,m})^{1/3}$$

and Eq(6) is:

$$\frac{\delta_S}{\delta_{S0}} = 0.18 \frac{T_{l,m} - T^{**}}{(T_b - T_{l,m})^{4/3}} \tag{8}$$

An example of this effect is given in Table 1 using the bulk metal temperature given in the example on p. 5-19 of the report($T_b = 1405$°C) and $T^{**} = 100$°C.

Table 1 Thickness of vessel wall opposite metal layer

x_{Zr}	$T_{l,m}$ (°C)	δ_S (cm)
0.05	1421	*
0.065	1387	25
0.088	1335	4

* bulk temperature is less than the liquidus temperature; Fe-Zr cannot exist as a single-phase liquid

The table shows that the wall thickness is very sensitive to the mole fraction of Zr in the metal melt. In the model developed in the report, Eq(4) above should be used for $T_{l,m}$ in the last term of Eqs(5.42) and (6.9). Equation(5) above needs to be added to the set of equations to determine the vessel wall thickness.

If $x_{Zr} > 0.088$, the phase that precipitates on the wall is Fe_2Zr and Eq(4) is replaced by the liquidus joining the eutectic point and the melting point of Fe_2Zr in Fig. 6.1 of the report.

(e) Melting temperature of the oxidic pool

The melting temperature of the oxidic pool given in Table 7.1 of the report is too high. Because of the addition of transition-metal oxides to the ceramic melt, a melting temperature of ~2700 K is suggested(p. 84 of Ref. 5). Other investigators suggest that the high-melting ceramic may flow as a solid carried like a slurry in the molten spinel(ref. 5, p. 187 and ref. 6). The spinel is $Fe(Al,Cr,Ni,Zr)_2O_4$, and may be present at levels as high as 10% in the oxidic material. The oxidic pool may not be a single phase liquid as assumed in the report(see also bottom of p. 5).

(f) Location of the decay heat source

The report assumes that the decay heat source is where the uranium is. However, the decay heat is due to the fission products, not the uranium. This fact was partially recognized by the authors of the report when they allowed for loss of volatile fission products(they need to state which fission products are volatile). However, a significant fraction of the fission products may be present in the metal layer. The presence of the noble metals (Ru,Rh, Pd) in the metallic phases of the TMI-2 core debris has been verified(Ref. 5, p. 91). Te is likely to follow elemental Zr in the metal layer. Zr fission product will distribute in the same manner as the structural Zr. Some Cs is found in the debris. The oxides of Mo have higher standard free energies of formation than UO_2 or ZrO_2 and Mo probably is more stable in the metal phase(6). Table 2 shows a possible partitioning of all fission products in one of three locations: volatilized and escaped; retained in the oxide; dissolved in the Fe-Zr metal layer.

Table 2 Distribution of fission products in core debris

Fission product	Released	in oxide pool	in metal layer
Zr,Nb*	0	0.15	0.15
Mo	0	0	0.24
noble metals	0	0	0.25
Cs	0.15	0.04	0.04
rare earths	0	0.53	0
Ba,Sr	0	0.15	0
Xe,Kr	0.25	0	0
others+	0.03	0	0.01
Total	0.43	0.87	0.70

* assuming 50% of Zry from core in each phase

\+ Te in metal phase

The sum of the numbers in the row for each fission product group is the elemental yield of this group from fission. The sum of the Total row is 2.

If the total fission product decay heat source is 13 MW, the above table suggests that it is divided into 7.2 MW in the oxidic pool and 5.8 MW in the metal layer. This heat source in the metal layer should be considered in the report's model.

II Amounts and composition of the liquids in the lower head

The report justifies the large amount of steel in the metal pool(~72 tons) with the claim that an oxidic pool height of 1.5 m would touch the core lower support plate. This, in consequence, would melt, and along with it, substantial portions of the core barrel and the reflector. The 1.5 m height is based on the assumption that all of the fuel in the core is relocated to the lower plenum. However, in TMI-2, a larger reactor, only 20 tons of core debris reached the lower head, and consideration must be given to the possibility that the initial oxidic pool height is less than 1.5 m and does not contact the lower support plate. The smaller quantity of oxidic material than the entire fuel loading would reduce the heat fluxes q_{up} and q_{dn} because the surface-to-volume ratio of the pool would increase. However, counteracting this is the probability that the fuel that did melt and reach the lower head would have a higher volumetric heating rate because it came from high-burnup regions of the core, near the center.

The most profound consequence of melting appreciably less than the entire fuel contents of the core is the reduced quantity of molten steel in the metal layer. If large portions of the core barrel, the lower support plate, and the reflector remained in place, the Fe concentration of the metal layer and the height of this phase would be greatly reduced, perhaps by as much as a factor of ten. The consequences of this are:

1. The heat flux to the vessel wall from the metal layer would be more focused, thus increasing $q_{l,w}$(Fig. 6.3).

2. The composition of the Fe-Zr alloy would be in the Zr-rich region of the phase diagram, which has a lower eutectic temperature than the eutectic in the Fe-rich region which is assumed in the report.

3. Because of the small height-to-diameter of the metal layer, it could no longer be characterized by a single bulk temperature, T_b. There would be some radial bulk temperature gradient in this layer.

4. The remaining core support plate above the metal layer would act as an additional radiation shield and reduce the radiative heat loss from the upper surface of this layer.

5. With a greatly-reduced quantity of steel melted, the metal/oxide system would more closely resemble that of the TMI 2 core debris than the neatly separated liquid phases on which the report is based. Examination of the TMI 2 rock samples was extensively reported in Ref. 5. These studies suggest that the metal and oxide phases were never fully separated. Instead, the metallic phases were interspersed with oxide phases to form a slush that one report characterized as wet sand(Ref 5, p 187); another study(6) suggested that the $(U,Zr)O_2$ is transported to the lower plenum as a solid with the spinel phase acting as a lubricant. Relocation of core material to

the lower head probably resembles a pour of wet concrete more than a clean flow of fully-liquid phases. It is even possible that distinct oxidic and metallic phases never separate in the core debris but remain in a dispersed state like oil and vinegar salad dressing. The analysis would then have to deal with a single composite medium with heat transfer through the connected liquid metallic phase and the heat source(at least part of it) in the dispersed solid oxide phase.

III Mechanical Aspects

(a) Wall loading by internal pressure

The report considers two sources of stress generation in the vessel wall: deadweight loading and thermal gradients. To these two should be added pressure loading, which may be important in high-pressure accident scenarios. The yield strength of the vessel steel drops sharply above 900 K. On p. 4-1, the wall thickness that retains full strength(σ_Y = 355 MPa) is given as δ = 1.1 cm. The internal pressure needed to achieve an equivalent stress in a thin-wall spherical shell that is equal to the yield stress is

$$p_{tot}(at\,yielding) = 2\delta\sigma_Y / R$$

Using the above figures and R = 2 m gives a pressure for yielding of ~ 4 MPa. This total pressure(or greater) is encountered in some accident scenarios.

(b) Wall failure by fracture

In the report, high thermal stresses are accomodated by yielding and by creep. However, the outer surface of the vessel wall is held at 100^{o}C by boiling water, so the possibility of brittle behavior should be considered. The problem is not unlike pressurized thermal shock, in which cold water contacts a hot wall resulting in temperature gradients and thermal stresses. In the present case, a hot liquid contacts a cold wall.

i) In Fig. 4.4 of the report, a significant fraction of the wall thickness on the outside surface is at stresses larger than the yield stress. This is also the region that is coldest. It is possible that the stress intensity factor(K_I)exceeds the fracture toughness(K_{Ic}) and cracks develop on the outer surface, propagating inward until the crack arrest fracture toughness(K_{Ia})is reached. Although through-wall cracking is not possible, the outer surface of the vessel could develop a population of cracks that render this region unable to sustain thermal stresses.

ii) The vessel wall above the metal layer is relatively cold throughout its thickness. Because the bottom of the vessel is hot, its thermal expansion places the upper vessel walls in tension. Again, K_I could exceed K_{Ic} and fast crack growth may occur.

iii) The temperature gradients developed during the initial thermal transient when the oxide liquid first pours into the lower head are steeper than those that prevail at steady state. As in the case of pressurized thermal shock, the transient behavior of the temperature distribution leads to crack propagation early in the event. Thermal stress distributions early in the core relocation to the lower head should be computed as well as the steady-state distributions treated in the report.

(c) Stability of the crust on the pool upper surface

The report makes a point that the crust separating the oxidic pool and the metal layer is very thin. Yet this crust, which is ceramic, sustains a sizable temperature gradient(leading to thermal stresses in it) and is bounded on both sides by moving liquids(which probably produce waves much as shown in Fig. 1.2 of the report). It is very difficult to imagine that such a crust would be mechanically stable in this environment. Instead, it would probably be broken into pieces which sink into the oxidic pool because the solid density is greater than the liquid density. The crust would continually reform, but its mechanical disruption would render its thermal resistance much less than if it were a coherent slab as assumed in the report. If this were so, the boundary condition $T = T_m$ at the upper pool surface would no longer be valid, and q_{up} would greatly exceed q_{dn}.

IV Miscellaneous

(a) Information

The report should contain summary tabular or graphical information on the reactor vessel which is the subject of its analysis. Even as basic a piece of information as the vessel wall thickness is only casually mentioned in the text and on the abscissa of some figures. Useful vessel information should include:

- geometry, including instrument penetrations(if any) of lower head
- composition of wall steel
- plot of yield strength Vs temperature
- thermal expansion coefficient
- elastic and creep properties
- fracture toughness properties as functions of temperature

This information as an appendix would be much more useful than the series of appendices describing the various heat transfer experiments. These contribute little to the tenor of the report and could simply be referred to in their original documentation. Appendix D describes an experiment that is not even built.

(b) Verification of numerical examples in the text

i) Starting with Eq(5.33) of the report with $S_{up} = \pi H(2R-H)$ and $S_{dn}=2\pi RH$(instead of hemisphere values) and V given by Eq(6.1), Eq(5.34) is:

$$q_{dn} = \dot{Q}FR \qquad where \qquad F = \frac{\gamma(1-\gamma/3)}{(2-\gamma)R'+2} \qquad and \qquad \gamma = \frac{H}{R}$$

For the example given on p 5-15, $\dot{Q}$ = 1.3 MW/m^3, V = 10 m^3 and R = 2 m. These values give H=1.45 m, and from the above equation for R' = 1.31, q_{dn} = 391 kW/m^2 instead of the value of 313 given in the text.

ii) The value of $\tilde{q}$ given in the example on p 5-19 should be $9.1\text{x}10^5$.

Using $q_{up} = 600$ kW/m^2 and A = 2764 in Eq(5.44) results in a difference between $T_{l,i}$ and T_b of ~0.4°C. This does not seem to be physically reasonable. However, using $T_{l,i} = T_b + 0.4 = 1678.4$ K and $\dot{Q} = 1.5$ MW/m^3, $q_{up} = 600$ kW/m^2 in Eq(6.14) gives $\delta_{cr} = 7$ cm, and the group $\delta_{cr}\dot{Q} / q_{up} = 0.17$, which violates the condition given by Eq(6.15)

References

1. W. H. McAdams, *Heat Transmission*, p. 180, McGraw-Hill (1954)

2. D. R. Olander, Nucl. Engin. & Design, **148**, 253 (1994)

3. D. R. Olander, Nucl. Engin. & Design, **148**, 273 (1994)

4. P. J. Hayward and I. M. George, J. Nucl. Mater., **208**, 35 (1994)

5. Nucl. Technol., **87** (1989)

6. H. Kleykamp, *Chemical and x-ray diffraction analysis of selected samples from the TMI-2 core*, KfK 4872(1991)

Sandia National Laboratories

P.O. Box 5800
Albuquerque, New Mexico 87185-1139

February 28, 1995

Dr. L. W. Deitrich
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439

Dear Dr. Deitrich:

Enclosed are my written review comments of the report:

"In-Vessel Coolability and Retention of a Core Melt,"

prepared by

T. G. Theofanous, S. Additon, C. Liu, O. Kymalainen,
S. Angelini, and T. Salmassi.

As requested, my review has focused primarily on Chapter 5 and the therein referenced Appendices (particularly Appendix D).

I wish to thank you for the opportunity to participate in this review. I found the work described in this report most interesting and valuable. If you have any questions regarding my comments, or if additional information is needed, please contact me.

Sincerely,

Rodney C. Schmidt

Rodney C. Schmidt
Reactor Safety Experiments Dept. 6423
Mail Stop 1139

RCS:6423

Copy to:

MS0742	J. Kelly (6414)
MS0739	K. Bergeron (6421)
MS1137	T. Y. Chu (6422)
MS1137	T. Heames (6422)
MS1139	K. O. Reil (6423)

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Review Comments of R. C. Schmidt on the Report "In-Vessel Coolability and Retention of a Core Melt," by Theofanous et al.

February 28, 1995

The review comments presented here are organized into four parts:

(1) General Comments on Chapter 5,
(2) General Comments on the ACOPO experiments (described in both Chapter 5, and Appendix D),
(3) Miscellaneous comments that cover all the sections that I read, and
(4) A brief technical note the Validity of the ACOPO Experiments for Natural Convection in Hemispherical Enclosures at High Raleigh Numbers

Comments on Chapter 5.

My comments here will be restricted to the discussion of heat transfer in the oxidic pool region.

Section 5.1

The fundamental goal of this section is to obtain the best estimates possible for heat transfer in the oxidic pool to the top and bottom surfaces, and the local heat flux variation on the curved surface. I have carefully reviewed this section and have the following comments.

Upper (Flat) Surface Heat Transfer:

In the paper cited for Eq. (5.11)[1] the correlation is given as $0.403\ Ra^{0.226}$. Also, the correlation for Eq. (5.13) is given as $0.233\ Pr^{0.239}\ Ra^{0.233}$. For clarity it would be useful to point out that the constants have been adjusted in this report to account for the different Rayleigh number definition.

I believe it more accurate to say that there are three (instead of two) correlations that are typically cited when considering the upper surface heat transfer. In addition to the two mentioned, the well known correlation of Jahn and Reineke for semicircular geometries[2] ($Nu_{up} = 0.36\ Ra^{0.23}$) is often used, and in fact has been (in the past) the most commonly used correlation in severe accident codes.

In addition to mentioning the Jahn and Reineke correlation, the discussion in 5.1 does not adequately point out the differences in the experiments from which the correlations cited are developed, and could be strengthened by doing so. The Kulacki and Emara study considered a plane fluid layer (rectangular cavity) where only the top surface was cooled. Steinberner and Reineke[3] considered three different thermal boundary conditions. However, the only case for which upper surface data was taken is the one with adiabatic sidewalls, with cooled (isothermal) top and bottom surfaces. The Jahn and Reineke correlation is for semicircular geometries. As can be seen from Fig. 5.2, none of these three situations is exactly the same as the problem of interest, i.e., a hemispherical pool with isothermal surfaces at all boundaries (top, bottom, and side). The remarkable thing to note is that despite major differences in geometry and thermal boundary

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[1] F.A. Kulacki, A. A. Emara, "High Rayleigh Number Convection in Enclosed Fluid Layers with Internal Heat Sources," NUREG-75/065, Technical Report 3952-1, Ohio State Univ., Columbus Ohio, Dept. of Mechanical Engineering, July 1975

[2] M. Jahn and H. H. Reinke, "Free Convection Heat Transfer with Internal Heat Sources, Calculations and Measurements," *Proc. 5th Int. Heat Transfer Conf.*, Tokyo, Japan, Paper NC2.8, Sept. 3-7, 1974

[3] U. Steinberner, and H.H. Reineke, "Turbulent Buoyancy Convection Heat Transfer with Internal Heat Sources," *Proc. 6th Int. Heat Transfer Conf.*, Toronto, Canada. Vol 2, pp 305-310, Paper NC-21, 1978

conditions, the correlations are all relatively close. This provides some confidence that the upper surface heat transfer in hemispherical pools with isothermal surfaces should be similar.

Overall, I basically concur with authors conclusions about the results for the heat transfer to the curved surface. However, to be "conservative, but not overly so", on the upper surface, I think the authors should consider the use of Equation (11) instead of Equation (12) as the reference correlation for the upward heat transfer. I have prepared a figure, (Fig. 1 below) to illustrate why I think this is so. Shown on this figure are the data from the mini-ACOPO experiments (taken from Fig. 5.3 in the report), the Asfia Dhir data (Appendix C), and the three experimentally derived correlations mentioned above. Considering the current uncertainty in the mini-ACOPO data, together with the results of Asfia and Dhir, it seems to me that an appropriately conservative approach (at least for the present) is to use the Kulacki and Emara correlation, not the Steinberner and Reineke correlation.

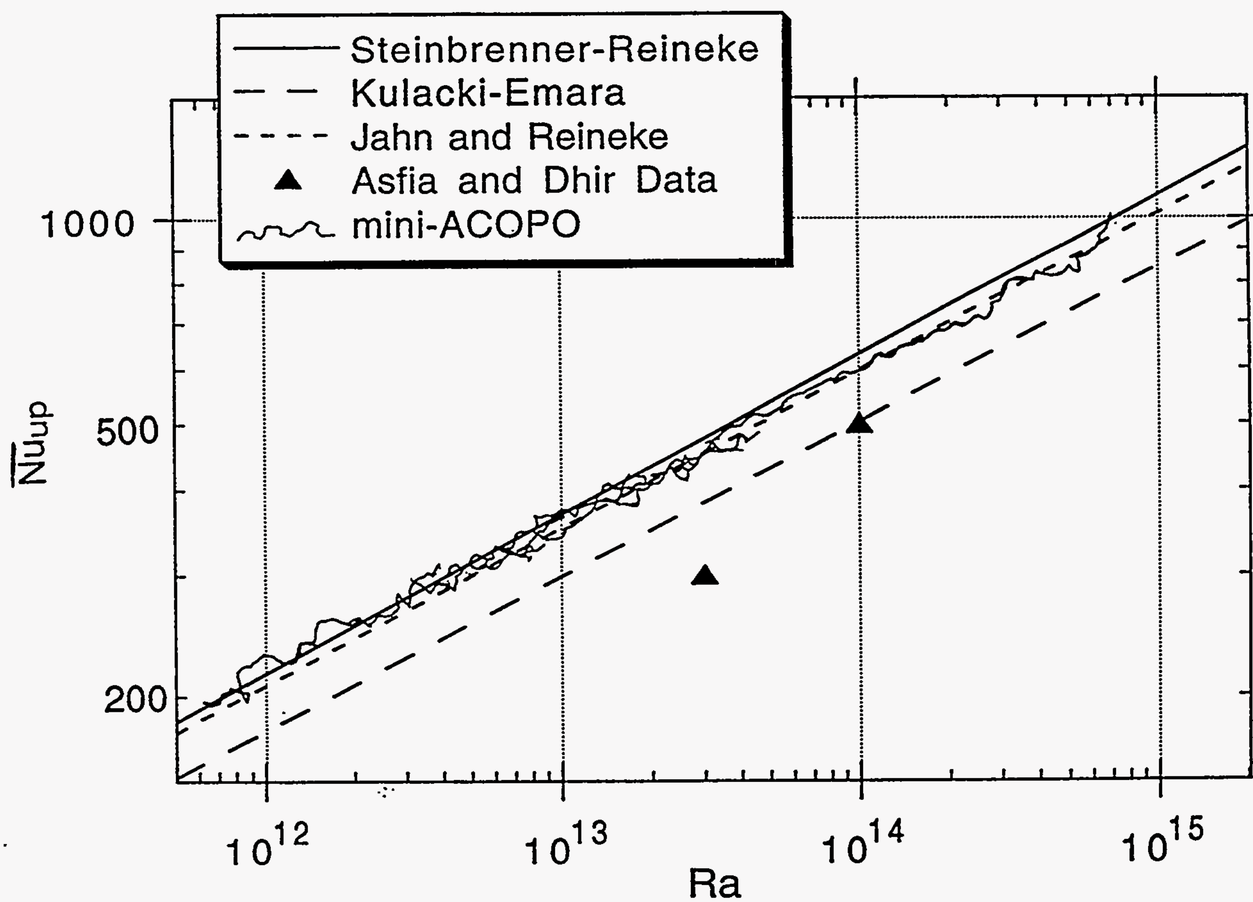


Figure 1 Comparison of data and correlations for upper surface heat transfer in different systems with internally heated fluids

Lower (Curved) Surface Heat Transfer

I basically concur with the conclusions drawn by the authors concerning the heat transfer to the lower surface. Figure 5.7 was particularly useful in illustrating the data which leads them to choose Equations (5.28) and (5.22) as representative of the spread in the current data base. However, it should be noted that the correlation of Jahn and Reineke (Eq. 5.21) was not included in Fig. 5.7. This correlation predicts much lower Nusselt numbers at these high Rayleigh numbers

(For example, at $Ra=10^{15}$, 270 vrs about 600). It would probably be more complete if the authors directly discuss why they choose not to use this data. My experience leads me however to concur with the apparent judgement of this report and discount these predictions as two low.

The constant shown in Fig. 5.7 for the Mayinger et al. correlation should be changed from 0.54 to 0.55.

Heat Flux Distribution on the Curved Surface:

I feel that the review of the data was sufficiently complete and that the base correlation used (Eq. 5.30) is adequate for this study. However, the use of the UCLA data (which shows a more peaked distribution) was definitely needed to bound the uncertainty in the current data.

General Comments on mini-ACOPO experiment (Section 5, and Appendix D)

My primary comments relative to the ACOPO experiment are contained in the last section, which provides a more technical review of how to validate the ACOPO approach. However, some general comments are appropriate here. First, I feel that the authors should be congratulated for developing and exploring a novel approach to solving a very difficult experimental problem. The approach taken is a variation of the approach used by Chow and Akins[1] for studying convection in Spheres (as well as a number of subsequent numerical studies by others[2,3]). I am very favorably impressed with the approach, and as a result of this review I am now a strong supporter of this method as being a good one. I none-the-less have some concerns about the strength of the validation arguments the authors have chosen to present. Furthermore, I might comment that within the context of the report (Chapter 5 in particular), I get a strong sense that the authors have a great deal of confidence in the results of the mini-ACOPO experiments. This is not wrong, and my assessment tends to confirm the validity of the approach, but further work needs to be done before the uncertainty level of the mini-ACOPO data can be clearly determined. Thus, I might recommend a somewhat higher sense of caution (for the present) then is reflected in the tone of the current report.

At the beginning of section D.5, the authors state that "the key point" validating the experimental concept is the establishment of a self similar stratification pattern during the cooldown. They define a local dimensionless temperature in Eq. (D.1), and plot the data for these temperatures in Figures D.4 and D.5. The claim is that because a "well defined, self similar temperature gradient exists in the intermediate 10% to 50% of the pool volume" that the approach is validated. I do not think that this is a correct path to validation. Even if quasi-static behavior is assumed, thermal profiles would be expected to change as the system moves from a high Rayleigh number to a lower Rayleigh number. To my knowledge, there is no basis for expecting the thermal profiles plotted using their dimensionless temperature to be exactly the same at say $Ra=10^{16}$ as they are at say 10^{14}. The argument is better made that because the range of Rayleigh numbers is not very great, and the pool is in the fully turbulent regime, that the normalized thermal profiles would not be expected to change very much. But this is quite different from claiming that "self similar" temperature profiles can be shown to exist at different Rayleigh numbers. Furthermore, I do not

[1] M. Chow and R. Akins, "Pseudosteady-state natural convection inside Spheres," ASME J. Heat Transfer, Vol. 97, pp 54-59, 1975.

[2] Y. S. Lin and R. G. Akins, "Thermal description of pseudosteady-state natural convection inside a vertical cylinder," Int. J. Heat Mass Transfer, Vol 29, No. 2, pp. 301-307, 1986

[3] J. Hutchins and E. Marschall, "Pseudosteady-state natural convection heat transfer inside spheres," Int. J. Heat Mass Transfer, Vol 32, No. 11, pp. 2047-2053, 1989

see how one can know that the profiles would not show approximately similar stratification patterns if the system was not at quasi-steady states. As mentioned earlier, I think there are better ways to argue the experimental validity, and I have outlined them in the last section.

I think the data in Figures D.4 and D.5 should be plotted as a function of depth, not as a function of normalized fluid volume. It would be easier for the reader to relate to and just as relevant.

On pg. D-11, top of the page, first complete sentence after Eq. (D.1): This sentence somewhat confused me. It states that lateral temperature gradients are always negligible, which is is an important piece of information, but no data is actually shown to support this statement. The authors should show the data in some form.

Msc. Editorial type comments, minor corrections and questions

Pg. 2-1, Next to last sentence: What is a "Grade B approach" ? A reader such as I has no idea what is meant here.

Pg. 2-2, Second Paragraph, last sentence: I suggest replacing the terms "production and dissipation" with "heat input and heat loss." The terms production and dissipation are more commonly used in terms of turbulence production and dissipation as compared to energy or heat transfer.

Pg. 2-2, Last Paragraph, 2nd and third sentences ("What occasionally . . .", and "In particular, . . ."): These statements seem out of place and confuse the point of the paragraph. Furthermore, they are technically confusing and I don't think they're needed. Steady state is approached slowly with the time constant of the system being a function of both the pool thermal capacitance and the flow strength. Who has suggested otherwise? Appendix D does discuss the presence of strong boundary layers but only makes a conjecture that this impacts the quasi-steady state assumption.

Fig. 7.3, pg 7-4: Why are there irregular wiggles on the flat portions of the probability density function plotted?

Page 7-11 3rd to last sentence: I might suggest changing "contrary to popular opinion" to "contrary to what might have been expected."

Fig. 7.9, pg 7-12: I cannot distinguish which curves correspond to which values of θ in this plot. Could something be done to correct this problem?

Pg. 10-2, Ref. 19: The Kelkar et. al. reference title should be changed to "Computational Modeling ....", instead of "Computer Modeling ...."

On the Validity of the ACOPO Experiments for Natural Convection in Hemispherical Enclosures at High Rayleigh Numbers

Rodney C. Schmidt
Sandia National Labs

Introduction

The ACOPO experimental program of Theofanous et al.[1] proposes to simulate the heat transfer behavior of natural convection in hemispherical enclosures with internal heating by using data from transient cooldown experiments with no internal heating. However, the cited reference does not provide a strong rationale for the validity of the experimental approach. The purpose of this technical note is to suggest a more rigorous mathematical basis for showing how the two physical problems can be compared, and under what conditions the results will be equivalent. This assessment will consist of three parts. The first part will show how the governing conservation equations are almost identical when a simple transformation of variables is applied. The second part will discuss the physical conditions that must be met in order for the ACOPO approach to be a good approximation. This section will also include a quantitative description of how increasing the length scale (from 1/8 scale to 1/2 scale) would be expected to improve the accuracy of the results. Finally, the last section will provide some specific suggestions as to how the basic approach might be refined or altered to improve the technique and reduce the uncertainty.

1. Mathematical Basis for the ACOPO Experimental Approach

The conservation equations for mass, momentum, and energy that are appropriate for turbulent flow in a system with an internally heated fluid can be expressed as follows.

$$\overline{\nabla}\bullet\left(\rho\overline{u}\right) = 0.0 \tag{1}$$

$$\frac{\partial}{\partial t}(\rho\overline{u}) + \overline{\nabla}\bullet\left(\rho\overline{u}\overline{u}\right) = -\overline{\nabla}P + \overline{\nabla}\bullet\left((\mu+\mu_t)(\overline{\nabla}\overline{u} + \overline{\nabla}\overline{u}^T)\right) + \rho\overline{g}\beta(T-T_w) \tag{2}$$

$$\frac{\partial}{\partial t}\left(\rho C_p T\right) + \overline{\nabla}\bullet\left(\rho C_p \overline{u} T\right) = \overline{\nabla}\bullet\left((k+\frac{\mu_t C_p}{\sigma_\phi})\overline{\nabla}T\right) + S \tag{3}$$

The definition of variables here is fairly standard and follows Kelkar et al.[8]. Note that the isothermal wall temperature T_w is taken as the reference temperature.

For the case with no internal heating (i.e., S=0.0), the energy equation is simply

$$\frac{\partial}{\partial t}\left(\rho C_p T\right) + \overline{\nabla}\bullet\left(\rho C_p \overline{u} T\right) = \overline{\nabla}\bullet\left((k+\frac{\mu_t C_p}{\sigma_\phi})\overline{\nabla}T\right) \tag{4}$$

We now define a transformation variable ϕ, such that:

$$\phi = T - T_b(t) \tag{5}$$

In these definitions the bulk temperature $T_b(t)$ is a function of time but not of space. Substitution of this variable ϕ into the energy equation yields

$$\frac{\partial}{\partial t}\left(\rho C_p \phi\right) + \overline{\nabla}\bullet\left(\rho C_p \overline{u}\phi\right) = \overline{\nabla}\bullet\left(\left(k+\frac{\mu_t C_p}{\sigma_\phi}\right)\overline{\nabla}\phi\right) - \frac{\partial}{\partial t}\left(\rho C_p T_b\right) \tag{6}$$

which is seen to be mathematically equivalent to Equation (3) if $S = -\partial/\partial t(\rho C_p T_b)$. Substitution into Equation (2) yields

$$\frac{\partial}{\partial t}(\rho\overline{u}) + \overline{\nabla}\bullet\left(\rho\overline{u}\overline{u}\right) = -\overline{\nabla}P + \overline{\nabla}\bullet\left((\mu+\mu_t)(\overline{\nabla}\overline{u} + \overline{\nabla}\overline{u}^T)\right) + \rho\overline{g}\beta\left(\phi-[T_w-T_b(t)]\right) \tag{7}$$

Equation (7) is identical to Equation (2) except that the buoyancy term now has a time dependence on the bulk temperature.

The bulk cooling rate can be related to an internal energy decay rate Q as

$$Q = \rho C_p \frac{\partial T_b}{\partial t} \tag{8}$$

The system can also now be characterized by a "cooling rate" Rayleigh number Ra_Q defined using the bulk temperature decay rate instead of the internal heat generation.

$$Ra_Q = \frac{g\beta Q R^5}{k\alpha\nu} \tag{9}$$

2. Physical Conditions under which the Systems are Equivalent.

In the physical situation of interest (i.e., constant volumetrically heated fluid in a hemisphere) the internal volumetric heating rate S (due to decay heat), the associated Rayleigh number (Ra_S), and the temperature difference between the isothermal walls and the bulk temperature are constant. Thus, the degree to which the cooling system can be considered equivalent is directly linked to the following approximations.

$$S = \text{constant} \approx Q = \rho C_p\left(\frac{\partial T_b}{\partial t}\right) \tag{10}$$

$$Ra_S = \frac{g\beta S R^5}{k\alpha\nu} = \text{constant} \approx \frac{g\beta Q R^5}{k\alpha\nu} \tag{11}$$

$$T_w - T_b(t) \approx \text{constant} \tag{12}$$

Equation (10) means that the for the systems to be equivalent, the heat loss from the system must be approximately constant over the time frame when data is taken. Equation (11) follows directly from Equation (10) since Ra is a function of Q. Equation (12) introduces a different constraint, The rate at which the temperature difference T_w-$T_b(t)$ changes must be small relative to the time scale over which the thermal hydraulics of the system can respond. This corresponds to the assumption of Theofanous et al. [1] that the cooldown "should be slow enough to allow the process to pass through a series of quasi-steady states that approximate corresponding steady-states with heating rates equal to the instantaneous cooling rates in the experiments."

In short, for the systems to be equivalent, the cooldown rate must be constant in time, and it must be slow. The next two subsections describe ways to quantify each of these two constraints.

2.1 Analysis of the System Cooldown Rate

The degree to which the cooldown is constant can be determined by deriving an ordinary differential equation which describes the overall system response. In doing so, we assume that the system actually does pass through a series of quasi-steady states (an assumption to be looked at in Section 2.2), and that the overall heat transfer behavior can be represented by a correlation of the form

$$\overline{Nu} = \frac{\bar{h}R}{k} = C_N\, Ra^n \tag{13}$$

where C_N and n are constants valid over the range of Rayleigh numbers seen in the experiment. The derivation begins by equating the overall cooldown rate to the heat lost at the boundaries.

$$Q\,V = \bar{h}\;A\;\Delta T = (\frac{k}{R} C_N\, Ra^n)\;A\;\Delta T \tag{14}$$

Dividing through by the total volume, V, and noting that the surface-area to volume ratio for a hemisphere is 4.5/R, we can write

$$Q = (\frac{4.5k}{R^2} C_N\, Ra^n)\;\; \Delta T = \frac{4.5k}{R^2} C_N\,(\frac{g\beta}{k\alpha\nu})^n\, Q^n\, R^{5n}\;\; \Delta T \tag{15}$$

where the definition of Ra has been used to show the Q and R terms. The next step is to consolidate terms and solve for Q. This leaves

$$Q = \left[4.5k\, C_N\,(\frac{g\beta}{k\alpha\nu})^n\right]^{1/(1-n)} (R)^{\frac{5n-2}{(1-n)}}\; (\Delta T)^{1/(1-n)} \tag{16}$$

Finally, the definitions of Q and ΔT can be invoked to write

$$Q = \rho C_p(\frac{\partial T_b}{\partial t}) = \left[4.5k\, C_N\,(\frac{g\beta}{k\alpha\nu})^n\right]^{1/(1-n)} (R)^{\frac{5n-2}{(1-n)}}\; (T_b - T_w)^{1/(1-n)} \tag{17}$$

With this equation several things can be determined. First, the bulk cooling rate for any driving ΔT can be explicitly calculated. Second, starting with any initially specified value of ΔT, the time dependent decay in the cooling rate can be found by solving the equation. A plot of T_b vrs time illustrates very clearly how linear its decay is in time. Third, the dependence of Q on R is shown, thus showing how Q decreases as the scale of the experiment increases. Fourth, the Ra range over which a given experimental configuration can operate can be bounded by calculating values of Q for the maximum and minimum temperature differences that can be achieved and measured, and then using these values in the definition of Ra to calculate upper and lower bound values.

The utility of this equation can be illustrated by considering test A3 in reference [1], which has a radius of R=.22 m, and uses Freon 113 as the fluid. For this example we take the values of C_N and n (see Eq (13)) to be 0.13 and .25 respectively (estimated by looking at the correlations for upward and downward heat transfer respectively), and use the following properties for Freon 113:

$\rho = 1550$ kg/m^3	$\beta = 17 \times 10^{-4}$ K^{-1}
Cp = 962 J/kg K	$\alpha = 0.5 \times 10^{-7}$ m^2/s
k = .0741 W/(m K)	$\nu = 4.12 \times 10^{-7}$ m^2/s

Substitution of these values into Eq. (17) above gives

$$Q_{A3} = \frac{-337.8}{R} \left(T_b - T_w\right)^{4/3} \quad \text{W/m}^3 \tag{17}$$

or, written explicitly in terms of bulk temperature,

$$\left(\frac{\partial T_b}{\partial t}\right) = \frac{-2.265 \times 10^{-4}}{R} \left(T_b - T_w\right)^{4/3} \quad \text{K/s} \tag{18}$$

Taking the upper and lower values of $(T_b - T_w)$ to be 30 and 2 degrees (see table D.1 in Ref. [1]), the bounding Rayleigh numbers that would be estimated for this configuration are

high-end Ra = 8.0×10^{14}

low-end Ra = 2.2×10^{13}

These calculated values correspond very well with the data shown in Fig. D.9 of [1].

Figure 1 plots results from the solution of Eq. (18) as a function of time, showing how the bulk temperature difference and associated Rayleigh number vary with time. This illustrates how the conditions most favorable to pseudo-steady state behavior correspond to near the end of the experiment, when the cooling rate is lowest. For this example calculation, case A3 was calculated based on starting with a guess of $T_b = 30$ C at t = 3 minutes.

2.2 Analysis of the Quasi-Steady-State Assumption

The quasi-steady-state assumption can be evaluated by comparing the time scales of the ACOPO experiment with the transient response time data of other internally heated fluid systems subject to step changes in power. Experimental data is available from Kulacki and Emara [2] for plane fluid layer cooled from above. Also, the results of numerical analysis are available from Kelkar et al.[3] and Aksenova et al.[4] that consider the transient response time of a system such as this. Consider the following two time scales, t_{exp} and t_σ, defined as follows:

- t_{exp}: The time over which the ACOPO experimental conditions vary over a given Ra range, ΔRa.
- t_σ: The time scale associated with a similar internally heated fluid system subject to a decrease in power to reach a steady state given a step decrease in Ra, ΔRa.

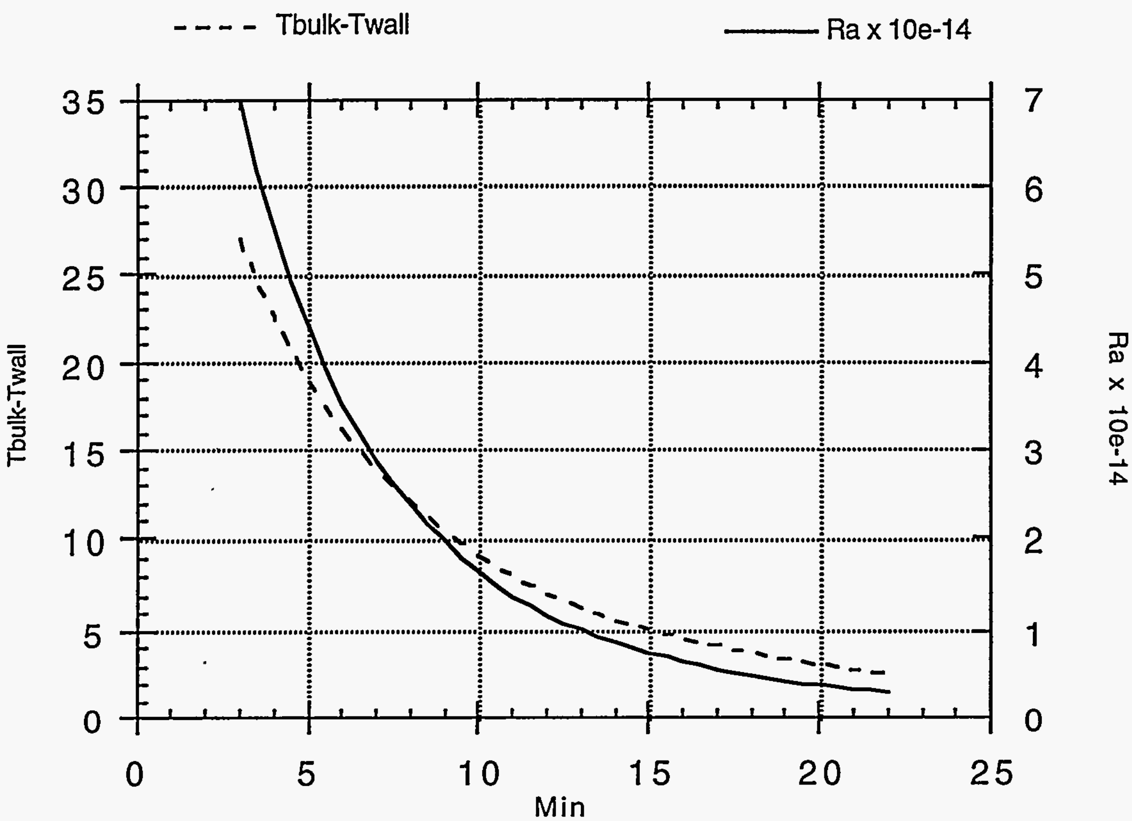


Figure 1 Variation in estimated change in bulk temperature difference and system Rayleigh number with time for case A3 based on Equation (18).

An important time scale ratio R_t to consider is

$$R_t = \frac{t_\sigma}{t_{exp}} \tag{19}$$

For quasi-steady conditions to be present in the experiment, one would expect that the ratio R_t must be small. As an example, from Figures D.6b and D.9, I would estimate that for case A3:

$t_{exp} \approx 18$ min (1080) sec, and $\Delta Ra \approx 2.6 \times 10^{14}$.

Equation (9) from Kulacki and Emara (1976) can be used to estimate the pseudo-first-order time scale for a step decrease in Ra. Using the radius of the test section (.22 m) and the diffusivity of Freon 113 ($.5 \times 10^{-7}$ m²/s), t_σ can be found as

$$t_\sigma = \frac{R^2}{\alpha} * 4.423 * (.5)^{-.275} * (\Delta Ra)^{-.275}$$

$$= \frac{(.22)^2}{.5 \times 10^{-7}} * 4.423 * (.5)^{-.275} * (2.6 \times 10^{14})^{-.275}$$

$$= 563 \text{ sec.} \tag{20}$$

Note that the term $(.5)^{-.275}$ has been inserted to account for a different definition of Ra used by Kulacki and Emara. Thus for this case (A3) the ratio t_σ/t_{exp} is about 0.5. Although a judgement cannot be made at this point as to how small is small enough, this would tend to indicate that if the relaxation time for the plane layer system is similar to the hemispherical system, that the mini-ACOPO experiments may only be marginally representative of steady state behavior.

The time scales analysis presented here can be coupled with the analysis given above in Section 2.1 to quantify the relative advantage of going to a larger system (i.e., the ACOPO experiments verses the mini-ACOPO experiments). Assuming that the length scale is to be increased by a factor of four, and assuming the range of practical temperature differences remain the same (i.e, $T_b - T_w$), then Equation (18) shows that the value of t_{exp} will be four times longer. The Rayleigh number will also be changed, thus changing the estimate of t_σ. Equation (17) shows that Q will be reduced by four, but the R^5 term in the Rayleigh number yields a net increase in Ra of 256. This multiplier in Equation (20) yields a new t_σ that is only .217 times as large. The net change is to decrease the ratio by a factor of about 18, a very significant improvement.

Mathematically, the time scale ratio changes with the radius R as follows:

$$\text{The ratio } \frac{t_\sigma}{t_{exp}} \text{ is proportional to } \frac{(R^{-1} R^5)^{-.275}}{R} = R^{-.2.1} \tag{21}$$

In summary, increasing the experiment scale by a factor X will increase the magnitude of the Rayleigh number range that can be treated by X^4, and reduce the time scale ratio defined in Equation (19) by a factor $X^{-2.1}$.

3. Suggestions for Improvements to the Approach

A few suggestions are made here which might lead to some enhancements to the basic approach.

3.1 Controlling the Isothermal Wall temperature

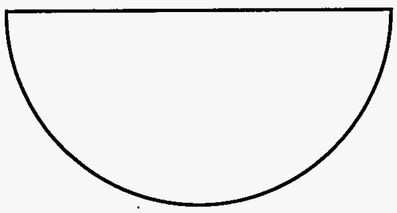
Equation (7) and (12) show that if the temperature difference T_w-$T_b(t)$ can be maintained constant, the system will correspond to a constant Rayleigh number system. This could be achieved by controlling the isothermal wall temperature, making it a function of time so as to keep the difference constant. This approach is in fact the approach taken in the experimental and numerical work of references [5] through [7].

Once the system is through the initial transient, the time dependent decay of the bulk temperature can be estimated fairly accurately (see section 2.1). This is particularly true if data from some preliminary experiments was first taken. If the water jacket cooling water could be controlled, then the water jacket temperature could be reduced in parallel to the bulk temperature, thus maintaining a constant temperature difference, a constant Q, and thus a constant Rayleigh number. One way to control the water temperature would be to mix two streams of water (cold and hot) in appropriate proportions so as to get the desired temperature. Even if the temperature difference wasn't kept exactly constant, the rate at which it changes could be slowed down, thus improving the pseudo-steady state approximation.

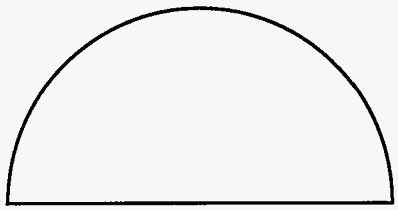
Controlling the water jacket temperature would also allow one to study the question of how close the system is to steady state behavior for different cooldown rates. By controlling the wall temperature with time, the rate at which the system passes through a given Ra range could be varied. Comparing the time scale ratio (Eq. 19) for the different rates, and comparing the results to a case where ΔT remains constant would be very useful.

3.2 Heating Instead of Cooling

One might consider the possibility of flipping the experimental system over, and then heating the walls instead of cooling them. For a Boussinesq fluid, this should produce identical results, and may enable the use of fluids which would have certain material property advantages.



Case 1: Cooled Walls



Case 2, Heated Walls

3.3 Comparing with previous data

In closing I would simply suggest that it would be of value to perform a set of experiments in a geometry for which reliable data is known to exist.

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ROYAL INSTITUTE OF TECHNOLOGY

1995-02-03

Div. of Nuclear Power Safety
Professor Bal Raj Sehgal
Tel: 46-8-790 65 41
Fax: 46-8-790 76 78

Dr. L.W. Dietrich
Director, Reactor Engineering Division,
Argonne National Laboratory
Argonne Illinois 60439

U.S.A

Subject: Review of the document, " In-Vessel Coolability and Retention of Core Melt" prepared by T.G. Theofanous, C. Liu, S. Additon, S. Angelini, O. Kymäläinen, T. Salmassi

Dear Walt,

I am sending you the subject review. As per your instructions, I have concentrated on the overall Approach and Assessment, covered in chapters 2, 6, 7, 8, 9 and there-in referenced appendices. Additionally, I have also read and reviewed chapters 3, 4 and 5 and have made comments on the work reported in Chapter 5. My colleague Dr. T.N. Dinh also read parts of the document and made some comments, which I have considered and have reported here as appropriate. The responsibility for the comments, as stipulated by you, is entirely mine.

I have enjoyed reading the report and thinking about the physics of the in-vessel retention of the core melt. It certainly would solve the ex-vessel severe accident progression issues for AP600. Professor Theofanous and colleagues have written a beautiful and a compelling document with a high purpose. They should be congratulated on the comprehensive character and excellence of the work they have performed.

My review of the document is enclosed. If you have any questions, please telephone or write.

Sincerely yours,

Raj

Bal Raj Sehgal
Professor

RECEIVED
REACTOR ENGINEERING DIVISION
—DIRECTOR'S OFFICE—
FEB 7 1995
ACTION: LB
INFORMATION:

Postal address
Kungl Tekniska Högskolan
S-100 44 STOCKHOLM, Sweden
Visitors address
Telephone
+46 8 790 60 00
Telegram
Technology

1995-02-03

Review of the Document
"In-Vessel Coolability and Retention of a Core Melt"
prepared by T.G. Theofanous, C. Liu, S. Additon, S. Angelini, O. Kymäläinen and T. Salmassi

by
B.R. Sehgal
Nuclear Power Safety
Royal Institute of Technology
100 44 Stockholm, Sweden

Professor Theofanous and co-workers should be congratulated for writing a beautiful report on the subject, which almost reads like a text book. I honestly enjoyed reading it and appreciate it very much, since the treatment is comprehensive and logical and I learnt a lot. They have performed much original work during the process of resolving this important issue. I do have a number of comments, which will be described in the following text. I will begin by providing general comments on the core melt in-vessel retention (IVR) concept and continue with comments on the document. This will be followed by comments on some specific items in the document and I will state my conclusions in a summary.

General Comments on the In-Vessel Core Melt Retention Concept

Ever since light water reactor severe accidents stepped into the consciousness of the reactor safety physicists and engineers, they are considered synonymous with molten core on the floor (core melt-down). Certainly, there are other detours, e.g. a steam explosion, or a high pressure melt ejection (HPME), which could distribute the core in particulate form all over the containment. Since the severe accident was considered as "fiction" during the years when the currently installed PWRs and BWRs were designed; the designers just threw up their hands, said "So be it! we will just make the containment strong and let us forget about this melt-down accident".

Since 1979, when the severe accident assumed a little larger reality, the nations of the World, possessing light water nuclear power reactors, have been spending millions of dollars, each, every year, on severe accident research to prove (or show with high assurance) that, (1) severe accidents are very rare indeed and (2) even if we get one, and the molten core lands on the floor of the containment, our strong containments (designed with foresight) will hold up long enough to, (a) reduce the radioactive emissions significantly and (b) enable evacuation of the near surroundings. The money spent has produced good results and, except for one or two remaining issues, the case has been made to the satisfaction of most technically- knowledgeable observers, if not to the satisfaction of the public at large.

In the last few years, accident management has come to the fore as a concept to upgrade the safety of the existing and the future plants. Accident management measures for the existing plants have varied from one country to another; e.g. Sweden has installed filtered vents on all of its plants and inerted the BWR containments, while U.S. has only inerted its BWR Mark I and II containments. Accident management has been brought into the design process for the future plants. In particular, the designs of the U.S. passive plants, and of the European pressurized power reactor (EPR), are incorporating accident management features which, hopefully, will provide substantial additional safety margins, so that even the need for public evacuation is virtually eliminated.

The concept of retaining the molten core within the vessel, in the event of a severe accident, should be very appealing to both the operators of the plant and the public. Certainly, keeping the radioactivity confined to a smaller volume and not having to cope with an extensive clean-up and decontamination operation, is a very worthwhile goal. The U.S. passive advanced PWR design, the AP600 has adopted this accident management concept, and establishing its feasibility and reliability is the aim of this document. If it is successful in achieving this aim for the AP600, it will open the door for considering this concept for other plants: present and future. Thus, the effort here is a milestone and should be so treated.

I believe, nature is quite partial to the in-vessel core melt retention concept. It has been found that the maximum heat removal, with boiling water on the external surface of the reactor pressure vessel, varies the same way, as a function of the polar angle, as the thermal loading imposed on the inside surface of the vessel by a naturally circulating core melt. Additionally, the maximum heat removal rate at the very bottom of the spherical vessel is substantial, due to the particular boiling mechanism that nature prefers there. If these two natural occurrences were not so disposed, the concept of in-vessel core melt retention could not materialize into reality.

Comments on the Document

I must start my comments on the document with Table 7.3. I was surprised to find that only 30% of the accidents contributing to core damage frequency (CDF) are relevant to IVR. Again, I was surprised to note that 23% of the CDF is caused by vessel rupture for which no accident management can be provided, and 18% of the CDF is related to high pressure melt ejection (HPME). Perhaps, the ARSAP program should also target these two events, i.e. vessel rupture and the ADS failure, and provide reliable prevention strategies, so that their probability of occurrence is substantially reduced. There may be a greater potential of early containment failure with these hazards, than it may be with a few tonnes of oxidic and metallic melt discharged, at very low pressure, into the water pool surrounding the vessel.

The ULPU experiments conducted by Professor Theofanous and co-workers have provided definitive data on the CHF for the external surface of the vessel; and the CYBL experiments, conducted by Chu et.al., have provided the visual evidence for the substantial heat removal rate at the very bottom of the reactor vessel external surface. I believe, that the heat removal aspect of the IVR is quite well assured, and the uncertainties are low, except for the actual AP600 physical design details. In particular, as the authors state, the physical design has to allow sufficient area for the steam produced from the cavity to flow to the containment dome; and the insulation on the vessel has to allow a steady access of the water to the vessel external surface. The flow area and the water access have to be assured throughout the life of the plant and, thus, may be subject to the maintenance and in-service inspection regimens conducted on the plant.

The authors have performed an excellent job on defining the thermal loading on the internal surface of the vessel, however, the situation is not as clean, as it is, for the heat removal on the external surface of the bottom head. The basic misgiving, in my mind, is that the authors have assumed an end state of the melt pool and, thereby, an independence from the core melt scenario, which ignores the intermediate and the transient states, which may impose greater thermal loading on the vessel inner surface. I accept the authors' argument that the maximum thermal loading due to a purely oxidic pool would be scenario independent. However, when a metallic layer on top of the oxidic pool provides "focusing"; the authors, themselves, have identified an intermediate state with a 1.18 meter deep oxidic pool and a 0.22 meter metallic layer, which results in larger thermal loading than the assumed end states of the oxidic pool (1.5 m to 1.6 m depth) and the metallic layer (0.9 to 1.0 m high).

I have sorely missed an appendix, or a section, on the core melt progression assumed. Clearly, the knowledge-base on the later phases of the melt progression is poor and some assumptions have to be made. If I follow the relatively better known scenario, the first discharge of the melt to the lower plenum (full of water) would be like that in TMI-2 i.e. in the range of 20 to 40 tonnes, (Appendix H actually assumes 47 tonnes, while section 8 assumes 22 tonnes). Next, if it is assumed that a certain fraction of the melt jet fragments and the steam generation leads to lowering of the water level, and to greater melting in the core region, there could be a release of metallic constituents from the core bottom, followed by the release of the remaining oxidic material from the melt pool established within the original core boundary. Now, this is not an unlikely scenario, which could result in an intermediate state of a three-layered pool (less than 1.4 m depth) with a metallic layer sandwiched between two oxidic layers. This may lead to the condition, investigated by the authors, in which the thin metallic layer has an adiabatic boundary condition at the top surface; which was found to result in much greater thermal loading. Another point is that a fraction of the Zirconium metal released may be in the form of U-Zr eutectic, which may generate some decay heat in the metallic layer.

The role of water in the lower plenum in quenching the melt discharges, the timing of its complete evaporation, and the subsequent remelting and layering of the pool, are all undefined. I would also prefer to leave them undefined, if I would be certain that the thermal loading during the intermediate states is always less than that in the end state. The authors have established thermal margins of approximately 100% for the most probable end state; perhaps, some scenario dependence could be considered and thermal margins investigated for some plausible intermediate states.

The thermal loadings on the internal wall of the vessel have been determined for the final stable state of the melt pool natural convection. Nature is quite kind in the final stable state, since the stratification in the lower levels of the melt pool reduces the convective heat flux to that transmitted by conduction. In the transient states leading to the final stable state the stratification may not be fully established and the heat fluxes near the pool bottom may be higher. This has been recognised by the authors as an "open issue", on page 5-10, and it primarily affects the thermal margins established for the lower reaches ($\theta \leq \pm 15°$) of the vessel. An evidence of this is also in the Figure D.5, for Vi/V near zero and near 0.06, where the ACOPO quasi static (along the cooling transient) dimensionless pool temperatures show a variation of a factor of ≈ 2.

Perhaps, further investigation of this "open issue" could be performed through a perusal of the data from the COPO and the UCLA experiments; and also through calculations of the transient natural convection states leading to the steady state.

The technique used in the mini ACOPO experiments, and to be used in the ACOPO experiments, is unique, since the experiments related to IVR, performed by all the other investigators have employed volumetric heating. The ACOPO technique makes the experiment very simple and if it is valid, it really advances the state of the art of the experimentation in this area. I believe, the data obtained has been expressed in the form of correlations developed for the volume-heated experiments by using the cooling rate as equivalent to the heating rate. It seems to follow the same correlations as did the volume-heated experiments, except, perhaps, there may be some differences. The mini ACOPO experiment, having a reasonable volume, seems to reach a stable state within minutes, whereas in the COPO and the other earlier volume-heated experiments, it took much longer time. Figure D-9 shows that in the cool-down pool, the upward heat fluxes in the center half of the pool are approximately 20% higher than those in the outer half of the pool. Such spatial profiles were not measured in the internally-heated pools. Instead, unsteady wave-form and dynamically changing upward heat fluxes were measured. Perhaps, the natural convection system with volume-heating is much stiffer than without it, and it may be that the transient nature of the cool-down experiments, driven only by the boundary conditions, is different than the unsteadiness of the internally-heated turbulent liquid pool. Periods of unsteadiness in internally- heated pools are in range of 3-10 minutes and it may take many periods before the flow structures shown in Figure 5.2 are established. Do such flow structures get established in the cool-down pool within the few minutes needed to reach the steady state? A demonstration of the cool-down pool natural circulation, as the same as that in the internally-heated pool, could be through the measurement of the flow structure in the cool-down pool.

On page 5-3 of the report, it is stated that the natural circulation in a pool, with no volumetric internal heating, obeys the correlation $Nu = F(Ra, Pr^m)$. Perhaps, the results of the cool-down experiment could be correlated through this correlation; and the upwards and downwards heat fluxes obtained compared with those obtained through Equations (5.12), (5.28) and (5.30). I do not know whether this is a fruitful approach, however, it may provide some insight.

The heat transfer correlations obtained in the document do not have any dependence on Pr number, and the experiments performed for fluids having Pr number between 2.6 and 10.8 confirm that. (Cf Figure 5.4) Calculations performed recently by Dinh et.al., to be reported in the NURETH-7 meeting, show that the heat fluxes do not change significantly for Pr numbers between 2.0 and 10.0, but at Pr = 0.6, the downward heat flux increases considerably, while the upwards heat flux decreases slightly. This calculated result is for the laminar natural convection pool ($Ra=10^{11}$) and its applicability to highly-turbulent pool is not assured. However, there may be merit in investigating the regime of Pr number below 2.6. The stably stratified flow patterns near the bottom of the vessel may be different for the low Pr number fluids, and that may change the heat flux to the very bottom regions ($\theta \leq \pm 15°$) of the reactor vessel.

The attack of the vessel by the impingement of a melt jet has been discussed in section 8 and in the Appendix H, with different approaches. The section 8 approach employs Saito's correlation and derives a curve for the vessel ablation depth vs. jet diameter. It uses a melt volume of 2.5 $m^3 \approx 20$ tonnes and for a jet diameter of 10 cms obtains the ablation depth of 12.6 cms. The Appendix H, on the other hand, uses a melt mass of 47 tonnes and melt jet diameter of 4.8 cms to arrive at the ablation depth of 12.4 to 13.6 cms. If the section 8 analysis is redone with 47 tonnes melt mass and melt jet diameter of 4.8 cms, the ablation depth will be larger than the vessel wall thickness and no pool will form in the lower head.

Both the section 8 and the Appendix H evaluations assume the formation of an oxidic crust on the vessel wall. Thus the ΔT, for the heat transfer, is respectively 200 and 165 K. This is correct if the crust formed is stable and not swept out by the jet action. The jets are highly-turbulent, with Reynolds numbers in the range of 3 to $5x10^5$, and the survival of the crust in this regime may not be easy. The crust existence could be estimated by comparing the characteristic times for the conduction-controlled crust growth, the remelting of the crust and the convection-controlled residence. The remelt time at the heat flux of 6 MW/m^2 may be much longer than the crust growth time, however, the convection-controlled residence time may be less than 0.01 sec. Perhaps, the crust may exist at the peripheral parts of the jet impingement zone, but not at its center.

I believe that both the Section 8 and the Appendix H evaluations of the ablation depth, due to melt jet impingement, are overly simplistic and, perhaps overly conservative by not considering the presence of water. It is true that large scale data on this type of configuration is non-existent and the estimates made can not be validated. Nevertheless, the estimates made in Section 8 and the Appendix H are so close to the vessel wall thickness that one is left wondering about the seriousness of the jet impingement hazard, inspite of the fact that in the TMI-2 accident 20 tonnes of oxidic melt having a substantial superheat did not damage the vessel.

The authors have not considered phase change in their evaluation of heat fluxes, particularly where crust or vessel wall melting may occur. This certainly will complicate the evaluation, however, many times the phase change reduces the heat transfer, due to the needed heat of fusion, and the changes in viscosity that may occur at the melting surface. Perhaps, an estimate of this effect could be made.

Comments on Specific Items

Section 5, Pages 5-8 and 5-9

The Kelkar calculated correlations of $Nu_{up} = 0.18\, Ra'^{0.237}$ and $Nu_{dn} = 0.1\, Ra'^{0.25}$, both under-predict the values of the Nu numbers at $Ra'=10^{10}$, when no turbulence model should be invoked. Kelkar correlation gives $Nu_{up} = 42$ and $Nu_{dn} = 32$, while the Steinberner-Reineke measured correlation provides $Nu_{up} = 74$ and Eq 5.22 provides $Nu_{dn} = 55$. I believe, there is something wrong with the Kelkar calculation. It does not matter that at $Ra'=10^{15}$, the values of Nu_{dn} from the Kelkar and Mayinger correlations are only 2% different. I believe, the calculated "correlations" should not be put in the same "pot" as the measured data. In fact, I believe, that discovering the correct turbulent eddy-diffusivity model, which will be valid for the experimental and the prototypical conditions (melts, geometrics etc.) would be a great achievement.

Section 5, Pages 5-16 and 5-17

Specialising Eq. (5.35) to two boundaries with equal temperature drop, one would obtain

$$h = 0.059 \cdot 2^{\frac{1}{3}} \cdot \left(\frac{g\beta}{\alpha\nu} \cdot \Delta T' \right)^{\frac{1}{3}} = 0.074 \cdot \left(\frac{g\beta}{\alpha\nu} \cdot \Delta T' \right)^{\frac{1}{3}}$$

which is different from Eq. (5.41). This probably is a typo, or I do not understand the text before Equation (5.41).

Appendix H

In this appendix, Table 2 provides Reynolds numbers for the melt jet as 260,000 to 480,000, which signify that the jets are turbulent. However, the correlation of Swedish used for determining the Nu number is for laminar jets. If Martin's correlation $Nu=0.606\, Re^{0.547}\, Pr^{0.42}$, appropriate for turbulent jets, is employed, the value of Nu number for the second case in the Table 2 would be 776 instead of 560, which would lead to an even greater vessel ablation rate.

I believe the impingement time in Table 2 is too long. The analysis does not consider ablation of the hole of 4.8 cm through which 47 tonnes of melt is being poured into the vessel. The hole size will increase by factor of 5 or more, increasing the jet size, reducing the impingement time and the vessel ablation.

Appendix L

This is a very valuable compilation of the relevant thermophysical properties. The viscosities shown for $U0_2$ and $Zr0_2$, and the rules for the mixtures, are apparently valid only for the liquidus state i.e., above the melting temperature. Is there any data or equation to evaluate the viscosities for temperatures between the solidus and liquidus. The boundary conditions at all the inside surfaces of the vessel are in that uncertain temperature range between the solidus and liquidus, where the properties will affect the heat transfer rates.

The densities for the metallic mixtures are not too different from those for the corium. Once the natural circulation starts, it may be difficult to separate out the metallic components from the oxidic components in the corium and have them join up with the metallic layer on top.

Appendix N

In the run no A-2 in Table N-2, T_{li} should be 75.6 instead of 25.6. This is a typo, I am sure.

On page N-5, it is not clear which two equations were solved for T_b and T_{li}.

Summary

I believe, Professor Theofanous and his colleagues have written a very beautiful document on the subject of in-vessel retention of core melt in the vent of a severe accident. I believe, IVR is a very important issue for future nuclear plants in which accident management should be directly integrated in the system design.

Professor Theofanous and colleagues have considered most every aspect of the in-vessel core melt retention issue and have endeavoured to address the phenomena that are active in the process of retaining the melt in the vessel. Their emphasis is on providing data and models which illuminate and describe the physics of the various processes occurring and then integrating all the various sub processes to emerge with the assessment of the margins. This is the essence of the ROAAM approach, and in this case it actually is much more straight forward than in the case of the issues of the BWR Mark I liner melt-through and the Zion PWR direct containment heating (DCH) loading, which Professor Theofanous helped resolve earlier through USNRC sponsored research efforts. The experimental backing for the correlations and the models employed, in this document, to arrive at the thermal loading, and the maximum heat fluxes allowed, is also much more extensive than it was for the Mark-I liner melt-through issue. Professor Theofanous and colleagues have themselves performed original research and provided key data, on the CHF at the vessel external surface and on the heat fluxes on the vessel internal surface.

I have made several comments on the evaluations employed in the document. I believe, some of the questions asked are important in providing greater depth and validity to this document for the resolution of the IVR issue. My major question is about the possibility of getting a smaller thermal margin in some intermediate and transient state before reaching the final stable state, where there is an ample margin to accommodate the thermal loading imposed. I have also asked some questions about the ACOPO experimental technique, which I believe is unique and ingenious, however, should be qualified by, perhaps, measurement of the natural convection flow patterns. The evaluations of the jet impingement thermal loadings, and the vessel ablation-depth estimates, are not as complete as one would wish and, perhaps, the authors could strengthen those analyses. Some other points have also been raised, e.g., the Pr number dependence of the downward thermal loadings, the effect of the phase changes at the boundaries on the heat fluxes etc.

Finally, I believe, that the authors have based their case for the high thermal margins available during the in-vessel core melt retention for the AP600, primarily on the data measured in the

ULPU and ACOPO facilities. It would be highly instructive for the reviewers to observe a key experiment, or two, in each of these facilities and examine the instrumentation and the experimental procedures. This will lend much greater confidence to the peer-review process.

COMMISSARIAT A L'ENERGIE ATOMIQUE
DIRECTION DES REACTEURS NUCLEAIRES

CENTRE D'ETUDES NUCLEAIRES DE GRENOBLE

ADRESSER LA CORRESPONDANCE A :

J.M. Seiler
CEN-G
DTP/STR/LETC
17 rue des Martyrs
38054 GRENOBLE CEDEX 9
Tél. : 76.88.30.23
Fax : 76.88.52.51

Dr. L.W. Deitrich,
Director Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue, Argonne, Illinois 60439
USA

N/Réf. : G/STR/LETC/95-2/JMS/mo

January 12, 1995

Ref: Review of the document *"In Vessel Coolability and retention of a Core Melt"* prepared by T.G. Theofanous, S. Additon, C. Liu, O. Kymäläinen, S. Angelini, T. Salmassi

Dear Dr. Deitrich

Please find enclosed the review of the paper in reference which I performed with some of my colleagues.

We found that it was a very interesting work with many pertinent and new results and I enjoyed particularly reading this report, especially some sentences from appendix A.

We had not the time to look at all the parts of this report and we concentrated on thermalhydraulic aspects.

I hope that this will help to strengthen in some way this work.

Please receive also my best wishes for the new year.

Best Regards,

J.M. Seiler

S-83

Copy (letter + P.J.): Prof Theofanous
Copies (P.J.): D. Grand, S. Rouge, J.M. Bonnet

Comments on "In-Vessel Coolability and retention of a Core-Melt" by T.G. Theofanous, C. Liu, S. Additon, S. Angelini, O. Kymäläinen, T. Salmassi

DOE/ID-1046

This is clearly a very important work containing very pertinent and new data.

Comments will address different parts of the work.

I) Comments concerning Scenario examinations:

In the document, 2 scenarios are considered in the "thermal regime": the stratified pool and the melt jet impingement.

It is considered that the stratified pool is the worst (i.e.: the most conservative) situation, without any discussion. In fact other situations may be emphasised and should be, at least, discussed to rule them out.

I-1) A first kind of (different) situation may be linked to the existence of a debris bed with molten metals within it. The assumed scenario is the following:

a) The melt (oxydes+metals) flowing from the core is quenched in the water present in the lower head. The quenched melt forms a debris bed with rather large particles in it (say between a few millimetres to a few centimetres).

b) The residual water is evaporated and debris begin to remelt.

c) The materials which remelt first are metals (mainly Stainless Steel & Zr).

d) These molten metals may migrate within the debris bed and accumulate in the lower part of the debris. Only the porosities are filled with the liquid metal, the oxydic debris staying as solid debris within the molten metal (higher density). Thus rather large heights of such "porous pools" may be emphasised with a rather low metal inventory.

e) The decay heat produced by the metals and the oxydics debris is transported to the boundaries by natural convection of the metal throughout the porous medium. The temperature of the molten metal is expected to stay rather low and depends on the composition of the metal (say less than 1800°C). At such low temperature it may be expected that the dissolution of the oxides by the metals is low (Stainless Steels does, indeed, not dissolve ZrO_2 or UO_2 at these temperature levels).

f) The oxides situated above (but out of the molten metal-solid oxides pool) remelts later (due to their higher melting temperature). The molten oxides will enter into contact with the low temperature "porous metal-oxides pool" and form a crust at the interface which relies on the solid oxydic debris situated below.

g) Under these (inversed) conditions part of the power dissipated in the overlying high temperature oxydic pool may deverse downwards into the low temperature "porous pool". Thus this lower pool will have to evacuate not only the decay heat dissipated within it but also part of the power dissipated in the overlying oxydic pool.

h) Under these conditions, two flux peaks may appear; the first near to the upper surface of the "porous pool", the second later (in time) and above (in height) due to the oxydic pool.

Such a situation has been observed in the in pile SCARABEE experiments. It does not seem to me to be unrealistic for LWR accident situations.

It is not clear to me whether this situation is enveloped by the situation considered in the report.

I-2) One may also emphasise scenarios leading to debris beds (with water present in the lower head) and with local remelting producing localised hot spots onto the lower head. The heat fluxes to the vessel will be, of course, much lower than the heat fluxes related to a molten pool situation. It seems from the TMI 2 VIP investigations that the mechanical loads induced by the hot spots on the lower vessel head do not endanger the vessel integrity. This may also be true for AP 600 but should at least be mentioned.

II) Stratified Pool situation:

II-1) A presence fraction of more than 50 % in mass of ZrO_2 in the oxydic phase would inverse the stratification if the metal is mainly Stainless Steel; This would correspond to 100% oxidation of Zr and less than 30 tons of molten UO_2 (less than 40 % of core inventory). Has this situation to be considered ?

II-2) No heat flux profile has been considered for the metallic pool. This should be justified since the margin to critical heat flux is relatively low in some cases (fig 7.16 and 7.15 for adiabatic conditions).

II-3) Physico-chemical reactions between the metallic pool and the vessel may lead, potentially, to low interfacial temperatures with the vessel wall in the metal pool layer. This may increase the lateral heat flux when there is no metallic crust formed at the surface of the pool or in the presence of a thermal resistance at the surface of this pool. It may perhaps be argued that the interface temperature is not expected to drop below 1500°K (which is considered as boundary condition for the calculations) considering that the mole fraction of Zr in the metallic layer does not exceed 50% (according to phase diagram presented in fig 6.1).

II-4) Nothing is said concerning the evacuation of the heat flux released at the top of the pool.

This heat flux is expected to melt a variable part of the in-core structures; but what happens afterwards ? Is this power diverted to the upper part of the vessel ?, What would be the related heat flux distribution ? May the heat flux discontinuity at the metal layer surface induce unexpected ***buckling*** of the vessel ?

II-5) Presence of aerosols may decrease the heat transfer by radiation from the pool surface. But this is bounded by the adiabatic conditions.

II-6) The correlations presented in section 5-1 of the report are qualified on experimental results coming from COPO and mini-ACOPO. In the report describing the COPO experiments (appendix B) it is indicated that a thin layer (0,1 mm) of Teflon is used as electrical insulator over the cooled walls. This layer represents a thermal resistance of, about, 4E-4 m^2K/W. The thermal resistance due to the boundary layer flow in water is estimated to be, about, E-3 m^2 K/W. This means that the Teflon layer represents, about, 30% of the total thermal resistance. (This is an order of magnitude as the local thickness of Teflon may vary). Thus the validation of the correlations against these experimental results is questionable within an uncertainty range of, about, 30% which is quite important and which weakens some other considerations (for instance concerning the way the physical properties must be estimated)
Furthermore, the electrical insulation of the top cooling plates is made of alumina which has a high thermal conductivity at low temperatures. Thus, the thermal resistance related to this alumina insulation layer may be lower than the thermal resistance due to the Teflon layer. This may lead to a non prototypical increase of the heat transfer to the top and, consequently, to a decrease of the lateral heat fluxes. May these effects be quantified and included in the uncertainties ? What would then be the consequences on the lateral heat fluxes in the reactor situation ? (a little increase of the lateral heat flux in the region of the oxydic pool may not endanger the vessel and a decrease of the power diverted to the metallic layer may increase the safety margins ?)

II-7) Heat transfer in the metallic layer:

The correlations presented in the report (pages 5-16 and 5-17) are valid for fluids having a Prandtl number higher than 1 and have been validated on water experiments MELAD (Pr about 5 to 10). We also know from the work on LMFBRs that correlations valid for low Prandtl numbers (sodium, Pr about 0,005) are based on the adimensional group $GrPr^2$ rather than on GrPr. Steel has a Prandtl number which is intermediate (about 0,1). Thus we ask about the validity of the correlations used for the metallic layer and we are not convinced that experiments performed with water are representative. But the main question concerns the heat flux *distribution,* and a different choice of correlation may perhaps not affect this distribution. Could a sensitivity study be performed to check this point ?

Mini-ACOPO:

II-8) The definition of the Ra' number based on the transient approach is not given. From the text we understand that this number is based on the thermal inertia of the liquid and on the cooling rate ?

II-9) The internal Rayleigh number (Ra') is much more sensitive to the scale (power 5) than to the temperature difference (power 1). Thus it may be expected that small scale experiments privilege laminar boundary layer flows on the side walls which are not prototypical of reactor conditions.

II-10) For high temperature differences, how are estimated the physical properties which are involved in the Adimensional numbers ? Are these properties also estimated at "film" temperature ?

II-11) I am not sure that the transient approach is representative of all cases with internal heating. For instance in the situation of a homogeneous pool with an adiabatic upper boundary we have observed an overshoot in the pool temperature nearby the adiabatic surface in the BAFOND experiments (volume heated)(Ref 1). Overshoot means that the temperature increases much just below the adiabatic surface due to the stagnation condition. This temperature increase may induce heat flux peaking at the top of the cooled sidewalls. Such effect is specific to volume heating conditions and may not be observed in a transient pool experiment.
==> *I would suggest that an analysis of the representativity of transient cooldown experiments and related quantitative scaling should be included in the paper (also in relation with remarks II-7 and II-8).*

II-12) Figures D-12 and D-17 from appendix D suggest that the heat flux distribution is not uniform in the upper isothermal (as suggested by Fig D.15) region. This has not been observed on the COPO experiments (at least no strong effect was observed). Is this related to a scale effect ? (usual heat transfer correlations for turbulent boundary layers suggest that the heat exchange coefficient does not depend on the distance).
If this observation is extrapolated to the metal layer have we thus to consider a heat flux profile in this layer ?(see also remark II-2) (This would reduce the margins to failure).

(Reference 1: Seiler J.M., Cooling of molten materiallic liquid pool submitted to volumetric heating; new correlations for various cooling conditions; Int conf on thermal reactor safety ENS/ANS October 2-7 1988 Avignon France)

III) Thermal loads under jet impingement:

III-1) Only oxydic jets are considered. Why have metallic jets coming from the core been outruled ? Are such jets not credible ? ***Metallic jet would much more endanger the vessel integrity*** The EROS tests at KfK have shown very fast ablation for Iron jets impacting on a Steel plate.

III-2) The calculations presented in the report for oxydic jets make the implicit hypothesis that the crust which forms on contact with the vessel is stable. The stability of the crust has been observed in the tests performed by Saïto with Salt and Tin plates. ***However there is no general agreement, to my knowledge, on this point*** (the durations of the tests performed with real materials have not been sufficient to come to a clear conclusion). The stability of the crust may depend on several parameters such as:

- the *temperature* of the oxydic material (we estimate that the crust may survive several seconds for a 100°C overheat but less than 0.15 second for a 500°C overheat)
- the *inclination* of the wall (the FARO BLOKKER test n°1 (molten UO2 jet on an inclined plate 5° from vertical) has shown ablation of the plate).

III-3) The inclination of the wall would also impede the occurrence of the "pool effect" (accumulation of molten material in the eroded cavity inducing a reduction of the heat transfer).
==> Thus, I am not convinced that the analysis presented in the document is complete.

IV) Thermal failure and vessel bottom coolability:

The set of experiments presented (ULPU, CYBL) provides important results.

The most important experiments are the ULPU experiments. The approach which is used supposes that the CHF depends on the local heat flux, on the local two-phase flow conditions, on wall effects and on local pressure. Two-phase flow conditions depend on the overall recirculation path and on 2D local effects.

IV-1) Local Two-Phase flow conditions are expected to be represented if local superficial velocities are represented. This is one of the similarity criteria (the other is the level of the local heat flux). The theory, valid for saturated conditions, includes also the implicit assumption that the local *thickness* of the Two-Phase Boundary Layer is identical in the experiment (constant width) and the reactor (pie segment). This assumption is not demonstrated but may perhaps be assumed as realistic since size and inclination effects are represented. This should be discussed.

IV-2) The geometry effect is compensated by a heat flux profile defined on the basis of previous similarity arguments. The upstream (from the investigated location) compensation procedure is quite clear. The interest of the downstream compensation is not very clear to me.
For the inner region (angle between 0° and 10°), the heat flux is constant. This should provide conservative CHF conditions in this region.

IV-3) It is shown that an increase of the subcooling and of the recirculation mass flow rate has a great effect on the CHF (increase from 0,30 MW/m² to 0,50 MW/m² at the bottom, increase from 1 MW/m² to 1,6 MW/m² at the side top location). This is clearly very interesting. However the contribution of each effects (subcooling or mass flow rate) is not quantified and nothing is said about the representativity of the flow path in the ULPU experiments.
In other words the CHF results depend not only on the angle (as suggested by figure E-12) but also on the subcooling and on the recirculation mass flow rate. There is no indication in the text concerning the evolution of the recirculation mass flow rate for the different CHF tests performed at different angles.

==> *Thus one must be cautious when using the results presented on Figure E-12 and correlations E1 and E2.*

An optimisation of the flow path (as suggested in appendix K) may lead to an increase of the liquid flow at the bottom of the vessel. Thus, even higher CHF levels may be obtained, locally, than presented on figure E-12.

On the contrary, bad recirculation conditions (flow restrictions, ...) may lead to lower CHF levels. However it seems that the results obtained for Configuration I hold as a lower bound for CHF (0,3 MW/m² at the bottom and 1 MW/m² at the side top).

These remarks are important in regard to the large heat fluxes which are computed in the metal layer under some assumptions (1 MW/m² in fig. 7.14 (page 7-15)and 1.4 MW/m² in fig. 7-16 (page 7-17)) or for applications to other reactors.

Future work may thus be oriented both on:

- a better knowledge of the contribution of each effects on CHF (pressure, recirculating mass flow rate, subcooling, ..),
- an optimisation of the flow path (as proposed under appendix K) for a maximisation of the recirculating mass flow rate, since heat fluxes higher than 1 MW/m² cannot be excluded.

IV-4) It is also mentioned that all results have been obtained with a copper wall and that experiments with steel will be performed. It seems essential to perform the tests with steel since the elevated thermal conductivity of copper may have an effect on CHF. It may be suspected that the oscillatory behaviour of boiling at low inclinations induces periodic dry patches which act as initiators of dry-out. The rewetting of these dry patches may be related to the maximum temperature reached on these surfaces during the dry phase. The maximum temperature in these patches is reduced in the case of a copper wall (when compared to a steel wall) due to heat flux redistribution towards the surrounding wetted zones. This suggests that a better understanding of the mechanisms of initiation of dry-out, if possible under these particular conditions, would be welcome.

PAUL SHEWMON

2477 Lytham Rd, Columbus, OH 43220
Tel. 614-457-4378 e-mail: SHEWMON.1@OSU.EDU

December 21, 1994

To: L.W. Deitrich, Director Reactor Engr. Div., ANL

REVIEW OF 'IN-VESSEL COOLABILITY AND RETENTION OF CORE MELT', by Theofanous, et al., DOE/ID 1046

This report treats primarily, almost exclusively, the case in which all of the core, core-internals, and lower support structure have melted, and a steady-state has been attained. This molten material fills the lower head with the dense oxide of the core and a layer of molten metal floats on top of it. Convection brings the heat to the top and side surfaces where it is carried away by conduction thru the steel and radiation upward. With water surrounding the vessel, heat can be removed so effectively from the external surface thru nucleate boiling of water that the external surface remains well below the heat flux required for dryout, and vessel failure. I have read the report carefully, and sought other scenarios that might lead to vessel failure. Provided the reactor cavity is flooded in a timely manner, I believe that a molten core could be contained and adequately cooled inside the pressure vessel.

You asked me to pay particular attention to Chapt. 4, Structural Failure Criteria. The authors basically consider net section collapse as the most probably failure mode. The net load acting on the wall in the situation considered in this report is extremely low, due to the combination of buoyancy forces and the weight of the internal melt. Only a fraction of a millimeter of steel would be sufficient to support this load. The other significant stress acting in the wall is thermal stress. It is greater than the yield strength, but such stresses are self-limiting and thus relieved with a minor amount of strain. The only way the vessel could fail is by the eating away of essentially all of the wall thickness. The authors show that when the wall is thinner than 2.5 cm (one inch) the water on the outside is sufficient to keep the wall from thinning, i.e. melting, any more.

I feel it is important to emphasize one other thing. It is a given in this problem that there will always be water surrounding the exterior of the pressure vessel. However, the cavity is not normally flooded in an operating plant and someone will have to make the decision to flood the reactor cavity, and do it in a timely manner. It is important to emphasize that this should not be put off until 'the last minute'. If the molten core redistributes before the cavity is flooded, and with minimal water going into the vessel, the vessel will fail long before one gets to the steady-state whose analysis the report dwells on. I realize that assuring timely flooding is more a regulatory matter than a technical one. But, I wish to stress that timely flooding is essential if the plant is ever to reach the situation of retention analyzed herein.

Paul Shewmon

Paul Shewmon

ARGONNE NATIONAL LABORATORY

9700 South Cass Avenue, Argonne, Illinois 60439

February 16, 1995

Dr. L. W. Deitrich, Director
Reactor Engineering Division
Argonne National Laboratory
9700 S. Cass Avenue
Argonne, IL 60439

Dear Dr. Deitrich:

Reference: T. G. Theofanous, et. al., "In-Vessel Coolability and Retention of a Core Melt," DOE/ID-1046, November 1994.

I have reviewed various chapters and appendices of the referenced document in my areas of expertise. My comments are contained in the attachment.

Thank you for inviting me to participate in the review of this very important work. Please don't hesitate to call upon me for additional assistance if needed.

Yours truly,

Bruce W. Spencer

BWS:ljo

Attachment

cc: (w/attachment)

Dr. L. Baker, Jr.

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Review Comments on DOE/ID-1046

Bruce W. Spencer

Argonne National Laboratory

The report "In-Vessel Coolability and Retention of a Core Melt" by Prof. T. G. Theofanous, et. al., is an excellent synergism of preexisting data plus new data in support of deterministic models, together with a rational methodology for addressing parameter ranges, to address the viability of in-vessel retention in a core melt accident scenario for AP600. This reviewer agrees with the approach and methodology used in the report. Caveats pertaining to key AP600 features and future design decisions are clearly presented and are important in assessing the basis of applicability for the AP600 system.

The report considers the melt-relocation-related jet impingement heat flux as one of the regimes producing limiting thermal loads. In the two sections of the report that address this thermal loading mechanism, Chapter 8 and Appendix H, the relocating melt mass amounted to ~0.3 and ~0.6 of the core fuel mass.

[1. The report should state the basis for selecting an amount of melt used in the jet ablation calculations.]

A key basis of the second regime, the pool natural convection regime, is that limiting loads are scenario-independent and are bounded by the thermal loads to the wall in the final steady state.

[2. This fundamental basis of the report should be strengthened via a few selected examples involving particle bed heatup and remelting. It is recommended to investigate the downward heat flux i) during the pool formation process when the convecting pool may be contained by thick crust and upward heat transfer may be small, and ii) for the case that steel melts into a fuel particle bed and permeates to the bottom, facilitating heat transport to the vessel bottom.]

It is clear that the upward/downward split of heat transport from the corium pool is crucial to the overall problem, including the presence of a steel top layer wherein radiation heat loss and sideways heat transfer participate in the integral processes. The analyses based on existing database yield the distribution of loads which are shown in the report to be removable from the walls with considerable margin; i.e., in terms of remaining wall thickness in relation to wall load bearing requirement (for fully depressurized system) and in terms of the polar variation in heat flux in relation to CHF limitation.

[3. The additional work the authors list in Chapter 9 to strengthen the report basis should be pursued.]

[4. The extent of the key AP600 assessment results cited in Chapter 7 should be broadened. Key results are presented in terms of the ratio $q(\theta)/q(\theta)_{CHF}$. Other key representative results should also be given such as pool and metal layer bulk temperatures, crust thicknesses, wall thicknesses, and pool and metal layer energy splits.]

[5. The database used for the analyses should be extended to include real reactor materials involving realistic temperature levels, boundary conditions, and crusting effects, and real melt behavior in the superheat range as well as slurry range between T_{sol} and T_{liq} for the $UO_2/ZrO_2/Zr$ system. The authors themselves have devised an excellent approach to achieve this data via the ACOPO pool approach wherein high Ra' data is obtained for Nu_{up}, Nu_{dn}, and $Nu_{dn}(\theta)$ using large melt heat capacity in a cooling mode in lieu of internal heat generation. A few reactor material tests should be performed analogous to ACOPO at 1/2 scale, including in some cases the integral effect of an overlying steel layer.]

The report does not address the likely length of time that pool natural convection cooling would be relied upon if this regime were entered in an accident. It could be days or even weeks. The IVR assessment has included structural and thermal loads assessments, but the treatment of chemical processes which may effect head integrity over prolonged time is treated minimally.

> [6. A thorough examination of interfacial chemical processes should be undertaken involving not only the Fe/Zr mixture but also including other potential constituents of the corium including absorber materials, control rod materials, and fission products to address any possible chemical-related attack on the wall integrity at the temperatures and time duration of interest.]

> [7. For a ground-breaking safety approach as important for AP600 as IVR, it is warranted to perform a large-scale, integral test to demonstrate the viability of the integral processes over a lengthy duration. Real reactor materials, real vessel head material, and internal heat generation are required for such a demonstration test. The experiment technology is readily available to utilize a slice geometry analogous to the authors' own COPO experiments. A representative AP600 corium composition should be employed with the wide range of relevant materials as included in (6) above. The test may start from particle bed form.]

Other comments:

8. The report should clarify the scenario for the 3BE sequence considered to be of main interest to IVR. This sequence involves a large or medium size pipe break. Figure M1 seems to indicate that if cavity flooding is achieved, much of the RCS piping will be covered with water. Is water reflood of the vessel via the break a part of the 3BE sequence? What effect would water reflood have on the accident scenario?

9. In Appendix M, it would be better to refer to the cavity flooding valves as "remote actuated, motor operated valves" rather than "manual valves".

10. Pg. 5-7, 2nd line, believe Ra' exponent should be 14 rather than 16.

January 9, 1995

Dr. L.W. Deitrich, Director
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439

Dear Dr. Deitrich

Thank you for offering the possibility to participate in the Peer Review of the ARSAP report on "In-Vessel Coolability and Retention of a Core Melt" prepared by T.G. Theofanous et al. Please find enclosed my review report. As you asked in your letter of November 10, 1994, I first concentrated on Approach and Assessment. The discussion continues then with a number of more detailed comments.

Yours Sincerely

Harri Tuomisto

Harri Tuomisto

IVO International Ltd
FIN-01019 IVO, Finland

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January 9, 1995

Review of "In-vessel Coolability and Retention of Core Melt"
by T.G. Theofanous, C. Liu, S. Additon, S. Angelini, O. Kymäläinen, T. Salmassi
ARSAP Report DOE/ID-10460, November 1994

General remarks

In-vessel retention by external flooding is an effective means to reduce thermal and energetic challenges to the containment integrity during core melt accidents. If the concept is applied as a basic severe accident management strategy, it is really an essential task to assess the overall feasibility and reliability. The report makes a remarkable *synthesis of the thermal regime of the in-vessel retention by external flooding.*

The in-vessel retention concept was introduced to the severe accident management considerations in the end of 1980's. The technical feasibility was initially demonstrated for the Loviisa Nuclear Power Plant by Prof. Theofanous (see Ref. 39 of the Report). Since that time, plenty of new research has been performed to confirm the first demonstration. New information has been generated to a large extent in support of the Loviisa and the current ARSAP program. In these studies, the ROAAM approach has been applied to evaluate the risk of failing in the thermal regime.

The report and the problem treatment as a whole has been organised in an excellent way. It has been a great pleasure to have an opportunity to read it and to find the beauty of such developments as the idea behind the ACOPO experiments, and the thermal treatment of the metallic layer.

In the following, Approach and Assessment applied in the report are discussed. This is followed by some detailed remarks, which are meant for obtaining further clarification of certain aspects of the thermal regime and for the overall resolution of the in-vessel retention concept.

Approach and Assessment

For evaluation of the approach and the assessment, there are two questions to be answered:

- Is the approach sufficiently consistent and comprehensive to allow the overall assessment?
- Is it justified to say that the issue is principally and practically solved, and that only confirmatory research is necessary any more?

Fortunate to the reviewer, the applied ROAAM approach itself makes it possible to answer these subjective questions.

The ROAAM approach has been developed to deal with phenomenological uncertainties in complex physical and technical problems. The ROAAM has reached a mature state of development and application. Appendix A to the report is very essential for understanding the principal methodology. The starting point is to create the quantification framework by dividing the problem to such pieces which can be treated in the physically meaningful way. One of the most powerful features is that all new developments of the subject can be easily integrated into the framework and into the quantification.

The quantification framework needed for the in-vessel retention has turned out to be comparatively simple. First of all, the simplicity reflects that large margins are available for the heat transfer from the heat generating oxidic pool itself. Therefore, the framework concentrates on unfavorable conditions of the metallic layer. On the other hand, the simplicity can be understood to imply that the developmental stage is mature enough.

The available experimental results and theoretical considerations support the conclusion that the modelling uncertainties are very small in comparison to the margins. In terms of the ROAAM, I have no difficulty to agree that the assessment approach is of Grade B type and the maturation status (Phase IV) is reached upon completion of the peer review.

As shown in Fig. 1.1 of the report, the in-vessel retention issue will include the FCI Regime and the Steam Explosion Regime in addition to the Thermal Regime treated here. The final feasibility can be demonstrated after the separate report of melt-coolant interactions is available.

Concerning the practical design and Severe Accident Management measures, i.e. ensuring free water flow on the vessel and assuming low pressure conditions, some comments are included later. Notwithstanding, my answer to the above questions is positive: the treatment is consistent and comprehensive, and it is justified to state that the thermal regime is resolved to the point where only confirmatory research and practical design solutions are necessary.

Viscous effects

The corium pool heat transfer experiments have employed water and freon as a working liquid. Corium itself behaves in a different way on the pool boundaries where crust is formed: the increase of viscosity takes place gradually in corium. There is a not a sudden jump from the solid to the liquid phase. On the other hand, the validation calculations for the pool heat transfer take plenty of effort when trying to solve the heat transfer in the turbulent boundary layer.

It would be interested to obtain the authors' opinion on the influence of increasing viscosity to the heat transfer distribution, particularly whether it could increase heat transfer in the upwards direction. What are the authors' recommendations for the future fluid dynamics calculations?

The influence of the metallic layer

The problem definition in Chapter 2 defines that the thermal load to the lower head is maximized when the debris pool has reached a steady state, the heat generating debris volume has been maximized and the thermal resistance along the upward thermal radiation path has been maximized. Maximizing the debris volume creates some confusion with the "focusing effect" of the metallic layer. The most significant parameter by far is the height of the metallic layer on the top of the oxidic pool. As an extreme parametric study of Chapter 7 demonstrates, the limiting case presupposes only partial relocation of the oxidic part. Could the partial relocation cases make a nonnegligible increase in the failure risk?

The amount of steel in the metallic layer has been explained from the inner structures and their melting during the accident. Only schematic structural drawings of the reactor vessel and its internals have been given. For the readers' own judgment, a detailed drawing of the reactor core, internals and vessel would be useful.

In the Grenoble Workshop on "Large Molten Pool Heat Transfer" in March 1994 the question of steel boiling was brought up and was also mentioned in the Workshop Summary. The authors' response on the possibility of this phenomenon to increase to the metallic layer heat transfer would be desirable.

Low pressure sequence

The report assumes that high pressure core melt sequences can be practically excluded. Since the high pressure sequence Case 1A turned out to have rather high contribution in the AP600 PRA, some additional aspects are needed.

To show that the contribution of high pressure sequences is negligible, very high reliability requirements are provided for the system, particularly to show that negligible contribution to the in-vessel retention can be excluded. Naturally, this should be done in context with the available time for required operator actions. In case that depressurization by pressurizer surge line failure is argued, it would need quantification.

Blocking of the flow paths

In addition to ensuring availability of the flow paths by proper insulation and cavity exit design, the flow paths must be protected against all debris possibly flowing with water. The sources of such debris are the piping and vessel insulation (mineral wool, glass wool, thin metal sheets), rust, paints, concrete dust etc. Particularly, the narrow flow paths out from the cavity might be subject to clogging. The current research for the containment sump clogging can be utilized for the final design.

Fouling of the vessel wall

Another potential problem related to the water chemistry, impurities and all the small size debris flowing with water may be the fouling of the pressure vessel external surface during boiling heat transfer. At the beginning the fouling could have some advantage in increasing the surface wetting properties, but in the long term it might create an insulating layer. The possibility to study the fouling effect e.g. in the next phase ULPU-2000 experiments could be considered.

Thermal shock of the vessel

External flooding brings two potential problems to the vessel integrity due to thermal shock.

The first concern is an inadvertent flooding of the cavity that may bring a problem of the pressurized thermal shock to the vessel material (and to the weld if existing on the core area) exposed to the fast neutron fluence. This is not directly concern of the in-vessel retention concept, but any adverse effects for the safety of the vessel under design basis conditions should not be caused. The potential for inadvertent flooding should be checked under normal operating and overcooling transient conditions. The cracks located on the outside surface of the vessel may start propagating, since the outside cooling temperature is very low. Low initial and end-of-life brittle transition temperature of the vessel and weld material can minimize the risk.

Secondly, the relocation of core material onto the lower head causes a severe thermal shock to the vessel bottom. Before relocation, the inner surface temperature of the vessel may be about 100 °C and the external surface temperature equals to that of the flooding water. The contact with hot corium creates very steep temperature gradient in the wall. Now the cracks cannot propagate through the vessel wall, because they will stop in the heated part. However, it should be checked that the cracks are not so long and deep that they could cause the failure of the vessel (global rupture of the bottom), after partial melting of the wall thickness.

Harri Tuomisto

Harri Tuomisto, Dr.Tech.

AEA

AEA Technology

Facsimile

To	Dr Lou Baker Argonne National Laboratory
Fax	010-1-708 252 4780
From	Dr Brian Turland Reactor Safety Studies Department 236/A32, Winfrith Dorchester, Dorset, DT2 8DH, United Kingdom
Telephone	44 1305 251888 Ext 3029
Facsimile	44 1305 202508
e-mail	brian.turland@aeat.co.uk
Date	~~7 December 1994~~ 27 February 1995
Total number of pages	10

Review of In-Vessel Coolability and Retention of a Core Melt by T G Theofanous et al.

Dear Lou,

Following your fax of 24 February, I attach a draft of my comments on the Theofanous report. As you can see they were prepared some time ago, but were delayed because we did not have a contract. I do not anticipate making significant changes in the final version, but formally they have to go through our QA process!

I will mail you a copy of the final report with the correct signatures on at a later date.

Yours sincerely,

Brian D Turland

Brian Turland

DRAFT

REVIEW OF

In-Vessel Coolability and Retention of a Core Melt

(T G Theofanous, C Liu, S Additon, S Angelini, O Kymäläinen and T Salmassi)

Reviewer: B D Turland

AEA Technology
A32 Winfrith
Dorchester
Dorset DT2 8DH
England

(Phone 44-1305-251888 ext 3029; facsimile 44-1305-202508
e-mail: brian.turland@aeat.co.uk)

4 January 1995

1. INTRODUCTION

This review concentrates on Natural Convection, covered in Chapter 5 of the report, and the overall approach and assessment covered in Chapters 2, 6, 7, 8 and 9 and therein referenced appendices as requested in the letter from L W Deitrich dated 10 November 1994. The review is organised as follows: Overall comments are given in Section 2, while Section 3 contains the detailed comments on Chapter 5 (natural convection), and technical comments on the other nominated chapters are given in Section 4. An appendix contains details of typos found during this review. Below, the word 'authors' refers to the authors of the original study (Theofanous et al).

2. OVERALL APPROACH

The authors make clear that the AP-600 design is favourable to in-vessel debris retention by cavity flooding. I support this view, particularly because of the absence of lower head penetrations and the ability to get water into the cavity. However the information given in Table 7.3 (accidents contributing to the core damage frequency), the PRA information given in Appendix M (cavity flooding unsuccessful in 20% of the core damage cases), and the discussion of the thermal insulation (Appendix K) all indicate that even if one had complete confidence in the analysis presented in the report, there are still likely to be circumstances in which debris would not be retained in the lower head. In my view, these PRA and engineering related issues deserve priority, because the report does make a strong technical case for in-vessel retention provided the constraints of prior depressurisation and action to initiate cavity flooding are met. This may require system enhancements to obtain the necessary degree of assurance.

Like the authors (apart from more specific comments on Chapter 2, below), the reviewer will assume that the above prior conditions have been met, and concentrate on the heat transfer aspects of demonstrating that the debris will be retained in the vessel. The reviewer's observations are based, in part, on an unpublished study performed in the UK for a large (1.3 GWe) PWR. Apart from penetrations, which are present in this design but of no concern for AP-600, and the somewhat larger inventory of fuel, which reduces margins, the major threat to vessel integrity was identified as being due to the focusing effect of the metal layer, consistent with the sensitivity described in Section 7.3 of the report. In the absence of penetrations, this independent study also concurred with the authors that the greatest challenge to vessel integrity is at high polar angles, not close to the pole of the lower head.

Turning to technical matters, the allowable uncertainties from the in-vessel distribution of the heat flux are high because of the high critical heat fluxes found in the ULPU experiments, particularly in Configuration II (Appendix E.2). These were obtained in a full water loop, without significant obstructions and opportunities for vapour accumulation. Confirmatory tests with the chosen thermal insulation and more prototypic flow paths are desirable.

Note that most of the above points are covered in Chapter 9 by the authors.

3. CHAPTER 5: NATURAL CONVECTION

The authors assume (page 5-2) that, in the presence of unoxidised Zr, all the uranium remains as UO_2. Powers [Chemical Phenomena and Fission Product Behaviour during Core Debris/Concrete Interactions in 'Proc. of the CSNI Specialists' Meeting on Core Debris-Concrete Interactions, EPRI NP-5054-SR; February 1987] has queried this assumption, and notes that only about 5 atom percent uranium in the metal phase is sufficient to make the metal phase of core debris more dense than the oxide phase. As far as I know this question is unresolved. Given the large steel inventory assumed, it may not be possible to achieve inversion of the densities of the phases, and other possible configurations of debris (including partitioning of the decay heat source) may prove less of a challenge to the vessel, but I would like to see these points addressed for completeness.

The authors assume (page 5-2) that the crusts impose a uniform temperature boundary condition at the melt liquidus. Elsewhere (eg the CORCON code) it is assumed that the melt solidus temperature provides the external boundary condition for the melt pool. Evidence from the ACE Phase C experiments and the associated determinations of liquidus and solidus indicate that 'melt' temperatures beneath the liquidus are possible. Further the equations for crust growth are consistent with the solidus assumption, with the deposited crust having the composition associated with the solidus and a concentration gradient in the melt phase close to the crust. In some circumstances compositions in this region may be such as to give nucleation in the boundary region (or encourage the growth of dendrites). Unless the zirconium is fully oxidised, allowing oxides of iron to become part of the oxidic layer, this question is largely academic as far as the crust is concerned - both the solidus and liquidus will be significantly above the steel melting temperature. However, equilibrium phase diagrams indicate that, for the compositions anticipated, UO_2 will preferentially be deposited in the crust and that UO_2 should precipitate near the cool boundaries leaving a liquid richer in the less dense ZrO_2. Should this happen, convection, which depends on local density differences might be modified.. This effect was demonstrated in simulant experiments at low Rayleigh numbers [S B Schneider and B D Turland: Experiments on Convection and Solidification in a Binary System in 'Proc. Workshop on Large Molten Pool Heat Transfer, Grenoble, March 1994; NEA/CSNI/R(94)11], but was found to disappear for the simulants used at much lower Rayleigh numbers than those expected in a reactor melt pool. Confirmatory experiments with real materials are desirable, and should be performed as part of the OECD/Russian RASPLAV project. A paragraph should be added discussing possible multi-component effects on natural convection.

As the primary factor driving convection is the temperature difference, and the system response is determined by the heat flux, I would expect length independence to imply that

$$Nu \sim (Ra')^{0.25}$$

not as Ra' to the 0.2 power as indicated by equation 5.10; I note this is consistent with Cheung's analysis referred to on page 5-6.

I note that the apparent transition in the COPO experiments (page 5-5; fig B.3, page 5-9) at high Ra corresponds to the sets of experiments with different pool depths.

Given the margins that seem to exist, the statement on page 5-6 that 'even a 30% discrepancy could be potentially rather significant to our conclusions' seems rather strong for the AP600 application. However, I am pleased to see this issue has been addressed further in the mini-ACOPO experiments and the planned follow-on larger scale tests.

The following comments apply to Appendix D, describing the mini-ACOPO experiments in more detail:

> The basic idea behind these experiments - to obtain data in proper geometry using cool-down rather than internal heating - is to be commended. The major question is whether the data should be applied directly, or used to benchmark a model that is then applied to the internal heating case. As the authors note,

mathematically a spatially uniform cool-down rate is equivalent to an equilibrium with internal heating. They go on to show that over most of the volume the temperature is close to being uniform, so the cool-down rate is also spatially uniform in this region. However this does not apply in the lower part of the pool, where the 'effective volumetric heating' will be appreciably lower than in the bulk. It is not sufficient to demonstrate self-similarity to claim uniformity of heating. Indeed the curves showing self-similarity are quite constrained - they must asymptote to 1 and average to 0, thus it is not surprising that the largest discrepancies are at small values of V_i/V. A better indicator of the uniformity of the heating would seem to be

$$(T - T_w) / (T_{max} - T_w)$$

where T_w is the wall temperature. It should also be noted that the bottom 10% of the volume corresponds to about one-quarter of the pool depth, so the effective heating close to the pole of the vessel may be significantly reduced. The consequence of this will be to bias the results somewhat to lower downward heat fluxes (particularly near the pole). For completeness, I would prefer these effects to be taken account of in a model of the pool (not a full CFD simulation) although I do not expect them to invalidate the conclusions the authors draw from these experiments.

The assumption that the pool reaches a quasi-equilibrium configuration is justified by the experimental data, and the observation that the pool suffers no major internal adjustments during the cooldown is also important in justifying the experimental method.

It is noticeable that there is a stronger dependence of downward Nusselt number on Ra' than the correlation lines shown in Figs D.10 and D.11. Extrapolations of these data to reactor-size pools will give more equal downward and upward heat transfer correlations, in contrast with the 2-dimensional COPO data referred to in Appendix B. (This is covered in the main text of Chapter 5).

The maximum wall peaking factor of two seems to be well-founded.

The claim on page 5-12 that the UCLA data (for downward heat transfer) indicates an intermediate behaviour is not consistent with the plotting on Fig 5-7. While only a modest extrapolation is necessary, it is not justified to refer to the extrapolations as bounds - can say 'are expected to bound'.

The treatment of the metal layer appears reasonable and conservative.

The simplified model (pages 5-17 to 5-24) is interesting. However, the figures 5.9 to 5.11 expect a lot of work from the reader. I suggest only one set of curves per figure, which can then be labelled appropriately. Of the assumptions made for this model, I suspect the energy radiated from the surrounding cavity to the layer (assumption 4, page 5-18) may not always be negligible (equivalently the view-factor is reduced from 1 to allow for sidewalls close to the melting point of the steel).

Overall, this chapter presents a balanced account and the conclusions drawn are consistent with the current experimental database for convection in ideal simulant liquids. Too little account is taken of effects that might come into play with real materials, and no mention is made of the somewhat contradictory results that have been obtained with UO_2 melts in the past (Argonne experiments by L Baker et al and the SCARABEE-N test). However, there is a lot of margin available, provided the ULPU critical heat flux curve is appropriate, and it is difficult to see any circumstances in which non-ideal fluid effects would lead to vessel failure.

4. OTHER CHAPTERS: TECHNICAL COMMENTS

Chapter 2

It is right to emphasise that full primary system depressurisation is being assumed (the validity of this can presumably be obtained from the Level 2 PSA referred to in Appendix M) and that pre-flooding of the lower head has taken place prior to debris relocation. Appendix M, while indicating that there is sufficient inventory of water and sufficient flow paths, does suggest that more needs to be done to guarantee that water will be delivered in a timely manner, assuming that cavity flooding is adopted as a primary severe accident management operation. Likewise, my reading of Appendix K suggested that there is no difficulty in principle with designing insulation to allow effective flooding around the vessel, but this also needed further consideration by the plant designers in conjunction with the information obtained from the ULPU tests. As the information is presented here, it still looks that cavity flooding is an 'add-on', as it has to be for existing designs, rather than something built into the plant design. I assume there is regular testing of the valves in the drain lines from the IRWST. Overall I agree that the AP600 is an attractive design for the implementation of cavity flooding.

Without a full appreciation of the geometry and the assumptions that have been made on crust behaviour (eg are lower ceramic blockages, formed initially on a metallic blockage sufficient to retain the debris pool) it is not possible to underwrite the statements made on melt relocation: that discharge will occur at the core side and will involve a substantial fraction of the core. With the highest heat fluxes near the top of a molten pool, it seems to me more likely that the initial discharge by this route might be limited to, say, 30% of the core.

I support the view expressed on page 2-2 that what has been mentioned as a slow approach to steady state is really attributable to the thermal capacitance of the pool rather than unsteadiness in the natural convection process. However the following statement that the thermal loads are bounded by the steady state is untrue, as the discussion of the metal layer shows: as this layer grows the heat flux to the vessel wall reduces.

The term 'sizeable fraction of the core' on page 2-5 is undefined, but no evidence is produced to indicate that it is anything close to 100%. This is significant given the later arguments over the depth of the steel layer.

Chapter 3

This provides a reasonable overview of the data, largely generated at UCSB, on the critical heat flux. It seems reasonable to assume that the vessel is cooled sufficiently during depressurisation *from the inside* to guarantee nucleate boiling when cavity flooding is initiated.

Chapter 6

In practice a significant amount of the decay heat may be generated in the metallic layer (see page 6-1), if metallic fission products are able to migrate there. However, I do not expect this to affect the conclusions drawn by the authors.

As noted above, the solidus is appropriate for considerations of crust behaviour (an effective solidus, based on the temperature at which more than, say, 40% of the debris is liquid may be appropriate for melt convection considerations). For the metal layer, the implication of the phase diagram (Fig 6.1) is that attack on the steel wall may be possible at temperatures below the steel melting point, depending on the mole fraction of Zr.

The reality of the thin crust between the oxidic and metallic layer is not questioned in the report. If it is unstable, it may lead to an augmentation of upward heat transfer.

The model described in this chapter is suitable for the purpose envisaged. If it were to be developed further, the radiation sink(s) should be treated in a more discretised manner as the temperature will vary with distance from the debris.

Chapter 7

The use of the whole UO_2 inventory may not be bounding. If only 80% of the core relocates (eg leaving remnants of low rated assemblies) then the oxidic pool will be beneath the lower support plate, the metal layer may be thinner and the focusing effect more pronounced (particularly as the surroundings will be close to the melting point of steel). Such a configuration could occur before a final 'equilibrium' state is reached. In this configuration smaller amounts of metal are possible.

The ranges for the amount of metal involved seem rather narrow, and the text does not seem that consistent; the first part of the second paragraph (on page 7-5) implies that 105 tons of metal are expected, whilst this is way out of range of the probability distribution, presumably as all the core barrel is not expected (is the reflector attached to the core barrel?). Molten metal from the lower head should also be included. I would make this distribution broader in both directions. This would broaden the distribution for the height of the metallic layer (Fig 7.6).

It would be useful to have Sienicki's material in an Appendix. However, the timings seem reasonable.

The statement (page 7-11) that 'the zirconium oxidation is clearly quite independent of τ' is rhetoric. I would only say there is no obvious correlation.

I do not regard "the limits to failure" case (page 7-16) to be as 'extreme' as the authors do. However, the results are encouraging, when allowance is made for likely lateral temperature gradients in the (relatively) thin metal layer.

I note that no uncertainty has been allowed in the application of the ULPU critical heat flux data.

Chapter 8

It is stated in the second paragraph that 'The fundamental consideration is that molten oxide cannot exist next to a steel boundary even under strongly convective conditions..', however equation 8.2 has $T_j - T_{w,m}$ as the driving temperature difference, not the melt superheat.

The argument (page 8.3) against a small diameter jet seems relatively weak, given that a local failure is expected: as the pour continues one may expect the melt to erode downwards. Again it would be useful to see Sienicki's work. Water in the lower head would play a mitigative role, and, as smaller diameters are considered for the jet, the coherency of its impact is more difficult to maintain. If impingement is on the cylindrical section, is this thicker than 15 cm?

Chapter 9

The absence of lower head penetrations should be added to the key features that lead to the favourable conclusion for the AP600-like design (penetrations may not preclude melt retention in the lower head, but they make the analysis more difficult).

This chapter provides a fair summary of the assessment, and I support the conclusions drawn and the recommendations made. I would also add (i) development of an analytic model for interpretation of the ACOPO tests, and (ii) a limited series of tests with real materials to support the conclusions drawn from simulant or simplifying model assumptions. The OECD/Russian RASPLAV project should address (ii).

Having retained the debris as a high temperature melt, a decision would be needed as whether this was an acceptable configuration over a period of many days. I assume this issue will be addressed when late reflood is considered in the subsequent report.

APPENDIX 1: TYPOS IN THE DRAFT REPORT

The following typos were noted in reviewing the report:

Chapter 4

Page 4.3, 5 lines from end: σ_2 ---> σ_z

Chapter 7

Page 7-9 line 2: it is can be ----> it is

Page 7-9 line 4: (due to moderated rate at coolant loss) ----> (due to lower rate of coolant loss) ????

Chapter 8

Page 8-1 6 lines from end: all the data that could be obtained

Appendix D

Page D-19 line 14: but is proceeds with an abrupt ????

Appendix M

Figure M3: The mass scale should be 1000s of tons.

ARGONNE NATIONAL LABORATORY

9700 South Cass Avenue, Argonne, Illinois 60439

November 10, 1994

Dr. Harri Tuomisto
Thermal Hydraulics and Nuclear Safety
IVO International Ltd
SF-01019 IVO
Finland

SAMPLE for summary of assignments see Attachment

Dear Dr. Tuomisto:

Thank you for agreeing to serve as a reviewer of

In-Vessel Coolability and Retention of a Core Melt

prepared by

T. G. Theofanous, S. Additon, C. Liu, O. Kymäläinen,
S. Angelini and T. Salmassi

The purpose of this document is to demonstrate the effectiveness of "in-vessel retention" as a severe accident management concept for a reactor like the AP600. The intent of this review is to assess whether this purpose has been achieved to a sufficient degree for the results to be of use in the regulatory/licensing arena. To help us in this very important task, we have assembled an international panel of 17 distinguished experts in various technical areas covered in the report.

To ensure a complete and thorough coverage, each expert is being asked to concentrate his review in one or more areas of primary responsibility. We ask that you concentrate on Approach and Assessment, covered in Chapters 2, 6, 7, 8. 9 and therein referenced Appendices. Of course, you are welcome to offer comments on any other aspect of the report as you see fit. We estimate that you will need no more than 24 hours for this review and preparation of written comments. If you feel now, or if during the course of the review that you require more time, please let me know.

I wish to emphasize that we are interested only in your own technical judgment and opinions; while you are free to consult any of your associates or colleagues, the final product of your review will be considered to be yours. A signed copy of your review letter will be bound, together with the authors' responses, in the published version of the report.

After some discussion, we have decided on a modified review process. While initially we contemplated a meeting of the whole panel with the authors at ANL, prior to receiving review comments, we have now decided to ask for written comments in advance of any panel meeting. In this way, we hope to improve the efficiency of the review process by dealing with as many comments as possible by correspondence. We will reserve the possibility of holding a meeting to resolve major issues and/or accommodate reviewers' desires to see the experimental facilities.

The schedule for the review is envisioned as follows:

1. Report sent out to reviewers .... November 17, 1994
2. Reviewer comments in to ANL .... December 20, 1994
3. Authors responses to ANL .... January 15, 1995
4. Determination of need for meeting .... February 1, 1995
5. Meeting (if required) .... In February or March 1995
6. Any follow up activities to follow as appropriate until final resolution.

Contractual and financial matters relating to your participation will be handled by the UCSB Contracts Office:

Mr. Randy Stoskopf
Contracts and Property Administrator
Business Services
University of California, Santa Barbara
Santa Barbara, CA 93106

(a) a letter indicating your availability to do this review job, under the independent verifications activity of the ROAAM program at UCSB, and your hourly rate, and

(b) some document (usually a payment receipt is just fine) verifying this rate.

Within one week UCSB has committed to send you a purchase order for your services. To ensure that this procedure does not interfere with your review schedule, I hope you will be able to send the required materials to UCSB as soon as possible.

Please let me or Lou Baker (708-252-8349) know if you have any questions on any of the procedural matters described above, or any others. Also, during the course of your review, please feel free to contact us if you need any clarifications on technical matters. We will help in this regard as coordinators of this independent review effort.

Thank you again for your cooperation and willingness to participate.

Sincerely,

L. W. Deitrich, Director
Reactor Engineering Division

ATTACHMENT

Summary of Assignments

Cheung, F-B. (Penn State)	Critical Heat Flux	Chapter 3 and
	Natural Convection	Chapter 5
Chu, T.Y. (SNL)	Critical Heat Flux	Chapter 3
Dhir, V.K. (UCLA)	Critical Heat Flux	Chapter 3
Epstein, M. (FAI)	Approach and Assessment	Chapters 2, 6, 7, 8, 9
Henry, R.E. (FAI)	Critical Heat Flux	Chapter 3 and
	Approach and Assessment	Chapters 2, 6, 7, 8, 9
Kress, T.S. (ORNL)	Approach and Assessment	Chapters 2, 6, 7, 8, 9
Levy, S. (Levy and Associates)	Approach and Assessment	Chapters 2, 6, 7, 8, 9
Mayinger, F. (U. Munich)	Approach and Assessment	Chapters 2, 6, 7, 8, 9
Nickell, R.E. (AST)	Structural Behavior	Chapter 4
Olander, D.R. (UC Berkeley)	Structural Behavior	Chapter 4 and
	Approach and Assessment	Chapters 2, 6, 7, 8, 9
Schmidt, R.C. (SNL)	Natural Convection	Chapter 5
Sehgal, B.R. (RIT)	Approach and Assessment	Chapters 2, 6, 7, 8, 9
Seiler, J.M. (CEN-G)	Critical Heat Flux	Chapter 3
Shewmon, P. (OSU)	Structural Behavior	Chapter 4
Spencer, B.W. (ANL)	Approach and Assessment	Chapters 2, 6, 7, 8, 9
Tuomisto, H. (IVO)	Approach and Assessment	Chapters 2, 6, 7, 8, 9
Turland, B.D. (AEA)	Natural Convection	Chapter 5 and
	Approach and Assessment	Chapters 2, 6, 7, 8, 9

APPENDIX T

EXPERT COMMENTS AND AUTHORS' RESPONSE

TABLE OF CONTENTS

General Comments and Point-by-Point Responses to:

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

T.1. Response to Cheung, F-B. (Penn State) — Specific Assignment: Chs. 3,5

General Comment and Highlights

This reviewer raises four criticisms, only one of which is identified by the reviewer as having conceivably an adverse impact on our conclusions. We disagree with all of them, and wish to rebut them point by point.

Point-by-Point Responses

1. *It is a pleasure to participate in the review of the above-referenced report. As you requested, I have concentrated my review in the areas of natural convection and critical heat flux covered in the document.*

The various chapters and appendices that address natural convection and critical heat flux in relation to lower head integrity are generally well written. They provide a detailed description of the major findings of the work performed by the authors and a concise summary of others' past and on-going research efforts. Overall, the information presented in the report appears to be quite convincing and complete. There are, however, several important technical points that are not well substantiated by experimental evidence and/or sound theoretical arguments. These technical points, which need to be further evaluated, are discussed below.

* *

2. *1. Configuration Dominated by Natural Convection Phenomena*

The partition of thermal energy flow by natural convection presented in Chapter 5 and the formulation of thermal loads under natural convection presented in Chapter 6 were based on the steady-state configuration shown in Figure 2.2. This specific configuration represents the final state that would actually be realized in any in-vessel retention scenario. However, as explained below, this steady-state configuration may not bound all intermediate states and thus, it can not be solely based upon in assessing the natural convection problem at hand.

Following the initial, major relocation event but before the attainment of a final steady state, a transient situation could arise within the lower head in which a region of the molten pool developed a large local internal heat generation rate due to a concentration of the larger burnup portion of the uranium oxide fuel and fission products. This non-uniform, highly concentrated, volumetric energy source could cause a period of very intense heat transfer from the core melt to the *local vessel wall. During this period, the downward heat fluxes in the local region could be considerably higher than those observed under steady-state conditions. Because of this intense, localized heating of the wall, a hot spot could develop in the lower head. This hot spot could lead to wall thinning and jeopardize the lower head integrity. However, the presence of a large localized heat source would induce strong convective currents in the local region, resulting in rapid dispersion and dilution of the fuel rich material. Once the fuel concentration becomes more uniform (i.e., diluted), it no longer would cause a high heat flux in the local vessel wall and the hot spot would diminish. This transient situation, which involves the development of a hot spot, is apparently* not bounded *by the enveloping configuration depicted in Figure 2.2.*

It should be noted that a localized hot spot covering an elliptical region of approximately 1 m by 0.8 m was found to exist for about 30 minutes in the reactor lower head during the TMI accident. Results of the TMI-2 Vessel Investigation Project indicated that the hot spot was not caused by impinging molten corium jets. Rather, it was caused by a large localized heat source arising from sustained heat loading from the debris on the lower head. Conceivably, the transient situation described above could arise under certain circumstances and thus, it can not be excluded in risk analysis.

The TMI vessel was *not* cooled on the outside, and there is no need for much imagination (or need to invoke a "highly concentrated volumetric energy source" as the reviewer does) to understand that with 20 tons of melt on the lower head wall heating was inevitable. Rather, the mystery continues to be why the vessel did not fail! In fact, as explained already in the report (and further elaborated in a new Appendix O), the radial reflector in the AP600 is rather massive, and would require a prolonged thermal attack before it would fail. During this time the pool grows both radially and axially within the core region, being well mixed by natural convection. To have a realistic perspective on how much more (than the average) concentrated the volumetric energy source could be, one needs to look at the core power shapes, as demonstrated in Appendix O (Figure O.2). One can see a rather uniform distribution, and this, of course, is not an accident—it is obtained through fuel management.

3. *2. Dependence of the Surface Heat Fluxes on the Length Scale of the Melt Pool*

For a volumetrically heated pool, the heat removed from the boundaries of the pool must exactly balance the energy generated within the pool under steady-state conditions. This is the case for the oxidic pool illustrated in Figure 5.2. Assuming a uniform volumetric heat generation rate, the energy generated in the *pool is a monotonically increasing function of the pool depth. It follows that the surface heat fluxes at the pool boundaries must also increase with the pool depth (although the "up" to "down" energy flow split may either increase or decrease). Otherwise, a steady-state natural convection process can not be maintained in the pool. This is true no matter the natural convection flow regime is laminar or turbulent (see discussion on the turbulent flow regime in the next paragraph). Physically, the steady-state surface heat fluxes from a volumetrically heated pool can not be independent of the pool depth. In view of this, the arguments of length scale independence or small length scale dependency of the surface heat fluxes discussed in Chapter 5 and Appendix B are not physically meaningful. In conducting experimental studies of natural convection in a volumetrically heated pool, the geometry and the size of the pool are always among the key features that need to be correctly simulated.*

For highly turbulent natural convection flow (i.e., at sufficiently high internal Rayleigh numbers), the convective heat transfer is expected to be independent of the physical dimensions of the pool. This is because the fine scales of turbulent mixing in the well-mixed region are considerably less than the pool depth. It follows that the Nusselt number - Rayleigh number relationship should be given by a correlation of the form

$$Nu \sim Ra'^{0.25} \quad for \quad Ra' \to \infty$$

which is consistent with the limiting behavior of the Nusselt number given by equation (5.15). Note that the product, $\dot{Q}H$, in equation (5.1) is proportional to the total heat generated in the pool per unit area of the upper surface. This product term always appears together and should not be separated. For highly turbulent flow, the upper surface heat flux is expected to vary linearly with the product term, with the remaining terms being independent of the length scale. To be physically meaningful, the index of 0.2 in equation (5.10) should be replaced by 0.25.

The first paragraph is attributed inappropriately, while the second one issues an inappropriate attribution that is also of no consequence.

First. There is nothing in the report to indicate (as implied by the reviewer) that "surface heat fluxes" are independent of length scale or exhibit small length scale dependency. In the same vein, we do not know where the last sentence of the first paragraph is coming from—especially given the fact that in this report we introduced the *first and only* large-scale experiment, in this field, with the proper geometry.

Second. As explained below Eq. (5.10), the point about little or no length scale dependence of heat transfer coefficient (*not* heat flux) was made to indicate the utility of a much broader data base, at the upper boundary, than that available from hemispherical geometries. This is true whether the exponent is 0.2 (as in our Eq. 5.10) or 0.25 (as in the reviewer's equation for $\mathrm{Ra}' \to \infty$). In fact, the experimental data show an intermediate exponent of 0.233 to be valid for Ra′ up to $3 \cdot 10^{13}$, which was further extended up to the near prototypic values of $7 \cdot 10^{14}$ by the mini-ACOPO data in the present work. So, in practical terms there is no basis, nor consequence whatsoever, for replacing the 0.2 exponent with 0.25, as the reviewer suggests.

But, even on fundamental grounds the point cannot be taken well, as it was already discussed in conjunction with the reviewer's own result (Cheung, 1980), Eq. (5.15) of the report. Note the following:

(a) Our interest is for intermediate values of the Pr number and finite values (*not* infinite) of the Ra′ number.

(b) For such intermediate values, Eq. (5.15) shows an intermediate exponent of 0.227.

(c) Also, it should be noted that the "turning over" in a $\ell n\,(\mathrm{Ra}'^{1/4}/\mathrm{Nu})$ vs $\ell n\,\mathrm{Ra}'$ plot predicted by Eq. (5.15), towards the asymptotic regime $\mathrm{Nu} \sim \mathrm{Ra}'^{1/4}$, is not supported by present data (see Figure T.1). Note in particular that while the previously available Kulacki-Emara data stopped just short of the turn-over region, with the mini-ACOPO data we are well into it. As a consequence neither the Pr nor the Ra′ number asymptotic dependencies of Cheung (1980) can be considered as verified or appropriate at this time. Further clarification of this point is expected in the near future, through the use of the ACOPO data.

4. 3. Simulation of the Divergent Effect and the Three-Dimensional Aspects of the Two-Phase Boundary Layer

The local flow structure on the external surface of the pie-segment geometry described in Appendix E.1 can not be fully simulated by using the constant-width test Section of the ULPU facility. Although the local heat flux may be matched by using the power-shaping approach, the detailed hydrodynamic behavior of the two phase boundary layer flow can not be fully simulated. For the Pie-segment geometry, the cross-sectional flow area is not constant but increases downstream in the flow direction. The local power levels in the lower Part (i.e., upstream

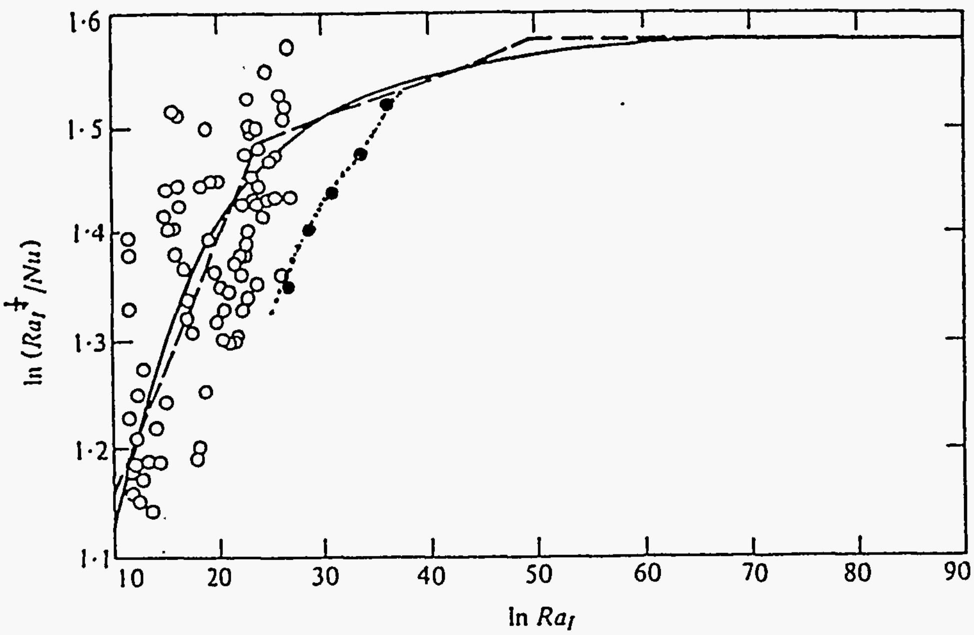

Figure T.1. The present results (• · — • · — •) plotted in the manner suggested by Cheung (1980).

portion) of the pie-segment geometry are considerably higher than the corresponding values for the constant-width test section. Thus, the bubble activities in the upstream locations are more intensive for the pie-segment geometry than for the constant-width test section. As a result, more vapor per unit surface area will be produced upstream in the pie segment. The population of the vapor phase, however, tends to diverse downstream as they flow upward along the pie segment owing to the increase in the cross-sectional area. This divergent effect, which may strongly influence the boiling process and thus the critical heat flux, is absent altogether in the constant-width test section.

Not true. The whole purpose of the power shaping principle in ULPU is to represent what the reviewer calls here the "divergent [sic] effect." This paragraph, and in particular the second sentence ("Although the local heat flux may be matched by using the power-shaping approach, the detailed hydrodynamic behavior of the two-phase boundary layer can not be fully simulated") lead us to believe that the reviewer did not fully understand the power-shaping principle and its implications. As shown in Figure E.3 of Appendix E.1, there is a sufficiently close approach of the upstream boundary layer for matching angles (θ_m) as small as 10°, and the simulation keeps getting better for larger angles. Only the region $\theta_m < 10°$ is, strictly speaking, deficiently (but conservatively) simulated in this respect, by using a uniform heat flux, but extensive sensitivity-type experiments indicate that the effect of this deficiency is negligible.

5. *Besides the divergent effect, the constant-width test section of the ULPU facility can not simulate the three dimensional aspects of the boundary layer boiling process that takes place on the external bottom surface of a AP600-like reactor. The superficial vapor velocity represents only one of the several requirements that need to be satisfied in simulating the boundary layer boiling process. Other flow parameters including the local void fraction, characteristic bubble size, bubble growth-and-departure frequency, and the divergence of the vapor bubble population in the flow direction need to be matched in the simulation. These flow parameters may have important effects on the boundary layer boiling process and the local critical heat flux distribution. Note that as a result of the boundary layer flow effects, the dynamics of the two-phase flow may vary significantly along the curved and diverging heating surface. Conceivably, matching the superficial vapor velocity alone is not enough in simulating the actual 3-D process, as the superficial velocity represents only a necessary condition but not a sufficient condition for the simulation.*

Our idea is that the independent variable here is local superficial velocity, and that as long as there is a reasonable upstream development length (within which the superficial velocity in ULPU is close to that in the reactor—again, please refer to Figure E.3 of Appendix E.1) all other multiphase aspects are automatically simulated. Again, the statement "Note that as a result of the boundary layer flow effects, the dynamics of the two-phase flow may vary significantly along the curved and diverging heating surface," lead us to believe that the reviewer hasn't fully understood this idea on which ULPU is based. The validity of this idea is further buttressed by the insensitivity of the results to both upstream and downstream power shapes, and in fact even to the natural circulation flow rate (see the new Appendix E.3).

6. *With the divergent and the three-dimensional effects, higher vapor velocities can be accommodated without exceeding the CHF limit. Thus more heat can be removed from the heating surface by nucleate boiling. This means that the local CHF values measured in the ULPU tests represent a conservative estimate (rather than the best estimate) of the actual situation. In the actual 3-D case, higher local critical heat fluxes can be anticipated.*

Based on our responses to the above two points we will have to disagree with the reviewer's conclusion that the ULPU results are conservative, except perhaps in a small region around $\theta_m \sim 0°$. But even for this region non-sensitivity to power shapes and mechanistic consideration (see new Appendix E.4) indicate that the effect is small, if not negligible for our purposes.

7. 4. Simulation of the Subcooling Effect due to the Gravity Head

In a fully flooded cavity, the water in the vicinity of the lower head would have $\sim 14\,^{\circ}C$ subcooling as a result of the gravity head. Thus it is necessary to properly simulate the phenomenon of subcooled boiling on the external bottom surface of the reactor vessel. However, exactly how this was done using the power-shaping method in the ULPU facility is not immediately clear.

For saturated boiling, the superficial velocity at a given downstream location can be uniquely related to the accumulated power generated in the upstream portion of the test section. Thus matching of the local superficial vapor velocity can be conveniently accomplished by using the power-shaping approach. For subcooled boiling, however, the superficial power vapor velocity at a given downstream location can not be uniquely related to the accumulated power generated upstream. This is due to the fact that condensation of the vapor phase would take place within the boundary layer in the presence of subcooling. The accumulated amount of vapor that is condensed before reaching a given downstream location depends on the size of the vapor bubbles, the local vapor velocity, the vapor population density, the cross-sectional flow area, and the degree of subcooling. None of these parameters except the degree of subcooling can be simulated in the constant-width test section. It does not appear to be feasible to match the superficial vapor velocity in the ULPU test using the power-shaping approach for the case with subcooling.

As noted already in Appendix E.1, "the same results can be obtained . . . if one considers the total energy (sensible plus latent) flow per unit width . . . reflecting the fact that the convected sensible heat is also important in the local behavior of the two-phase boundary layer." In the lower portion (say $\theta_m < 30°$) stratification is strong enough to make the approach immediately tangible to the power-shaping principle. In the upper portion (say $\theta_m > 45°$) sensible energy diffusion away from the two-phase boundary layer may be substantial, but still relatively small compared to the total energy flow within the boundary layer, and the degree of divergence (due to geometry) is small, thus allowing the power shaping principle to be applicable in this case as well. Further perspectives can be obtained from the new data (see new Appendix E.3) on the effect of flux shapes and throttling of the natural circulation flow at the inlet to the test section.

8. A more detailed description of the power shape used in the experiments for Configuration II should be given in the report.

All the power shapes used in Configuration II have been supplied in Appendix E.2, so there is no more detail to provide.

T.2. Response to T.Y. Chu (SNL) — Specific Assignment: Ch. 3

General Comment and Highlights

We have some difficulty discerning the reviewer' position relative to the simulating power of the ULPU experiment. Also, it is not clear to us to what extent this reviewer agrees with our conclusions about AP600. On the one hand he states that "because the margin to failure is fairly significant the reviewer feels that despite the inaccuracies involved, the critical heat flux data is sufficient for the present purpose," but on the other hand he opens up the whole issue by requiring a more "detailed justification" of Configuration II.

It is interesting that this reviewer is able to find all sorts of detailed aspects to question the ULPU approach, yet he does not hesitate to use the Cheung and Haddad *quenching* experiment as the standard for judging the appropriateness of the ULPU CHF values at $\theta \sim 0°$, or to compare Vishnev's correlation from a lab-scale experiment in pool boiling, with our Configuration II data. We believe that both are so removed from the phenomena we are interested in, that in the absence of any mechanistic hypotheses such comparisons are not only unjustifiable, they may even be misleading (i.e., create a false sense of security that there is confirmation from multiple sources).

We hope the enlarged data base (new Appendices E.3 and E.4) will help the reviewer better appreciate the simulating power of ULPU and to understand our statement about "other" experiments in the previous paragraph. The nature and potential significance of the "inaccuracies" that he is referring to is addressed in a point-by-point fashion.

Point-by-Point Responses

1. *I. Comments on Critical Heat Flux*

The review covers the material in Chapter 3 and Appendix E entitled The ULPU Experiments. The experiment appears to be well designed and executed within the constraints of the assumptions made.

The review will be presented from two points of view:

A. Does the ULPU experiment simulate the three-dimensional boiling process on the exterior of the reactor vessel?

B. The application of ULPU data to in-vessel core retention.

* *

2. *A. The two criteria: (1) matching superficial velocity at and beyond the point of interest, and (2) a gradual build-up of superficial velocity up to the point*

> *of interest, are reasonable; however, by no means guarantee that the flow fully simulates the actual 3-D flow outside of a reactor vessel. For example, there is no flow divergent effect in the strip and the velocity development is certainly different in the ULPU case due to the difference in the superficial velocity upstream of the point of interest. Furthermore, as pointed out in the report, the dynamic aspect of the flow and condensation effects are not properly taken care of by the criteria. Physically, the shape of a wedge cut from a hemisphere takes a* $\sin\theta$ *profile, since* $\sin\theta$ *varies rather slowly near 90°, the CHF data is likely to be accurate near the equator. However, in the bottom center region, the strip geometry does not adequately simulate the 3-D axisymmetric two-phase boundary layer flow. A comparison of the data in Figure E.12 (Appendix E.2) and the recent data of Cheung and Haddad (Proceedings WRSM 22 October 1994), Figure 1, shows that the CHF values obtained in ULPU might be too low near* $\theta = 0°$. *It is interesting to note that away from the bottom center area, the two sets of data have similar trends.*

We do not agree that the Cheung and Haddad data are, at this time, qualified to be compared with ULPU, or applied to reactors. But since a comparison has been attempted, we must be very clear about it. So, let us look at Table E.2, that contains all the data points. First, runs UF-5-0 and UF-6-0 show a critical heat flux just around the 400 kW/m^2 data point of Cheung and Haddad shown in reviewer's Figure 1. Note, however, that as demonstrated by the other data points, values as low as ~300 kW/m^2 (25% lower) were obtained by various heat flux shapes and by allowing long sampling intervals at a given power level. None of these effects were investigated in the quenching experiment cited by Chu, which, in fact, involves a very rapid "traverse" past the peak heat flux, once surface wetting occurs! We believe that taking the conservative envelope of our data is appropriate, and that nothing much should be made from the *apparent* agreement of the *peak heat flux measured in the quenching experiment and some of our critical heat flux values.*

The flow divergence inherent in the geometry can be seen in Figure E.2, which is now supplied also with a scale. We can see that divergence is strong only near $\theta \sim 0°$, and certainly negligible for $\theta > 45°$. As explained in the report, the region $0 < \theta < 22.5°$ is rather uninteresting from a failure point of view. This is to put the reviewer's criticism into perspective, independent of whether the ULPU data are realistic or conservative in this small region.

Furthermore, the statement: "... as pointed out in the report, the dynamic aspects of the flow and condensation effects are not properly taken care of by the criteria" has been incorrectly attributed. On condensation (subcooling) effects on p.E.1–6 we noted that "the same results can be obtained under moderate subcoolings, as is the case of interest here, if one considers the total energy (sensible plus latent) flow per unit width, which in turn provides a more generalized similarity criterion, reflecting the fact that the convected sensible heat is also important in the local behavior of the two-phase boundary layer." On the other hand, on p. 3–4 we indicate that "loop flow and dynamic effects" are to be addressed when the thermal insulation design becomes available. This is now the case (see addendum to Appendix K), and the results can be found in new Appendices E.3 and E.4.

> 3. *What criterion is used to determine "For θ_m as small as 10°, the simulation is* deemed *to be acceptable, (p. E.1–6)?" The use of passive voice without giving a justification is not informative.*

Statement is based on qualitative judgment comparing the "upstream length" for which $\Delta J/J <$ 20% to the flow regime structures (see also new Appendix E.4).

> 4. *Unless there are good reasons to discard the UF-6-0 and UF-5-0 data, they should be included in Figure E.18. These values are not far from the Cheung and Haddad data.*

The data were *not* disregarded. They were not shown in Figure E.18 because they are not relevant to the lower envelope correlation presented there. The "similarity" to the quench peak fluxes is irrelevant, as explained above.

> 5. *The data presented in Figure E.16 suggest that there is considerable lateral gradient in the heating block. If this is not the case, a new plot should be used.*

The few degrees difference is indicative of thermocouple error, without calibration adjustment. See also Appendix E.4.

> 6. *The large axial conduction correction for Configuration 1 is disconcerting. What would happen, if the experiment is run with the heating zone around the point of interest twice as wide? Or more generally, does the width of the heating zone influence the measured CHF values?*

In p. E.1-16, we show that the conduction correlation factors are 85%, 89%, and 95% at the three different locations respectively. It is not understood why the reviewer finds such a 5 to 15% correction "disconcerting." Moreover, these are *not* errors, but accounting for a well-understood and quantifiable phenomenon.

7. *CYBL can be operated to 400 kW/m^2 as currently designed.*

The statement in the report referred to "demonstrated" capability, as the follow-up sentence indicates.

8. *B. Because the margin to failure is fairly significant (as shown in Chapter 7), the reviewer feels that despite the inaccuracies involved, the critical heat flux data is sufficient for the present purposes, provided the following clarifications are made:*

As discussed under highlights, the meaning of this sentence is not clear, in light of what is brought up immediately below it.

9. *1. There is a substantial increase of CHF in Configuration II, due to the natural circulation loop. The data from Configuration II is used to demonstrate the large thermal margin. Therefore, the authors must provide a more detailed justification that the natural circulation observed is prototypic, in terms of flooding level, dimension of rise and downcomer, and the correspondence between the strip geometry and the axi-symmetric geometry in the integral sense. The arguments made in the power shaping principle are largely based on reproducing the local condition at the measurement location of interest.*

On the "integral sense" we already explained that we observe the key aspects, including height, vapor flow, and riser dimensions. They provide the correct void fraction, including flashing effects, and hence the correct driving head for natural circulation. In addition, we now have data on the effect of inlet throttling and heat flux shapes (Appendix E.3) that show the robustness of our thermal failure criterion. Further systems effects have been examined in Configuration III that includes the proper exit restriction, as described in Appendix E.4.

10. *2. The experimental methodology stresses "the determination of the critical heat flux ... under the constraint of a specific power shape. (p.E.1-5)" Under this methodology and specifically the power shaping principle, the results presented in Figure E.12 (section E.2) are only valid for the power shape in Figure E.11. (section E.1). Therefore, there is a contradiction in principle, to apply the CHF curve to the assessment of different power shapes in Chapter 7, Figures 7.13 to 7.16. To borrow an expression from thermodynamics, one needs to answer the question of whether CHF is a point function or a path function. It is entirely likely that CHF is only a weak path function. But justifications (which*

may require sensitivity experiments) must be made to smooth out this apparent contradiction.

We now have tested all relevant flux shapes (see new Appendix E.3), and have shown that CHF is a very weak function of flux shape. However, for highly peaked situations, as the arbitrary parametric in Chapter 7, with the 20 cm-thick metal layer, the CHF is found to be higher. As expected, our "reference" CHF results are conservative.

11. 3. The authors repeatedly stress the importance of aging the surface; however, there apparently is no attempt to characterize the surface. At lease a simple sessile drop observation or a SEM should be provided. This is especially important in the upcoming tests with the painted steel test section. How does the paint age under the test conditions? Should only data with new paint (never boiled) be used for in-vessel core retention assessment? How does the paint age in service? How can the test data be applied to the "real" accident conditions?

See Appendix E.4.

12. 4. It is interesting that the Vishnev correlation (Vishnev et al., "Study of heat Transfer in Boiling helium on Surfaces with Various orientations," Heat Transfer-Soviet Research, vol. 8, no. 4, p. 104–108) derived from laboratory scale experiments and using helium as a working fluid, actually predicts the ULPU data trend to within 10% (Figure 1). The Vishnev correlation specialized to the nomenclature of the present report is:

$$\frac{CHF_\theta}{CHF_{180}} = \left(\frac{10+\theta}{190}\right)^{0.5}$$

Where $\theta = 0°$ corresponds to horizontal downward-facing, and $\theta = 180°$ corresponds to horizontal upward-facing.

Interesting curiosity, but nothing more! See response under General Comment and highlights.

13. CHF phenomenology is still a mystery.

See Appendix E.4.

14. II. Comments on mini-ACOPO Experiments

The authors should be congratulated for making a conceptual breakthrough in simulating natural convection in pools with internal heat generation. This problem has puzzled experimentalasts for the last twenty years. However, for the experimental results to be applicable in a local sense, more detailed justifications will be needed than presented in the report. The energy equation for the problem of interest is (taken from Kelkar et al., 1993):

$$\frac{\partial(\rho C_p \phi)}{\partial t} + \overline{\nabla} \cdot (\rho C_p \overline{U} \phi) = \overline{\nabla} \cdot \left(\left(k + \frac{\mu_t C_p}{\sigma_\phi} \right) \bar{\nabla}_\phi \right) + S$$

The authors' contention is that by assuming quasi-steady states during a cooldown experiment, the variation of the bulk stored energy (temperature) with time:

$$\frac{\partial(\rho C_p \phi_b)}{\partial t}$$

can be considered to be the internal heat generation rate S. This argument is reasonable in an integral sense. However, if one were interested in local behaviors such as local heat transfer coefficients, it might be necessary to show explicitly that the local (X_f) variation of the stored energy in the fluid

$$\frac{\partial[\rho C_p \phi(X-f)]}{\partial t}$$

is everywhere uniform because the problem of interest is for spatially uniform heat generation. This type of data should be available from the interior thermocouples. While these data are not accessible to the reviewer, the discussions of self-similar profiles, Figures D.4 and D.5 in the report suggest that perhaps the bottom 10% of the volume may follow a different decay history. If this observation were true, local heat transfer coefficients from $\theta = 0°$ and $40°$ could be in error. Another location of interest would be the $\theta = 80°$ or $90°$ region where there is large difference between the mini-ACOPO result and the UCLA result.

We appreciate the favorable remarks, and the cause expressed in the form of several questions is well taken. The reviewer's interpretation of the ACOPO concept is similar but superseded by that of Schmidt's, who went into it at much greater depth. Please refer to our response to Schmidt for a complete treatment of this issue.

15. The effect of boundary conditions should also be examined. Isothermal boundary conditions will promote mixing (uniform thermal response) but an adiabatic upper boundary may be more problematic. Again, these are observations based on incomplete information, but the reviewer feels that the authors need to examine the data carefully before extracting local information and apply the information to the assessment of in-vessel core retention.

As can be understood from the report, adiabatic boundary conditions are not of interest to this work. The one run reported in Appendix D, was in an effort to better understand the UCLA experiment.

16. There are other related issues the reviewer will not cover here. However, all these suspected uncertainties can perhaps be tested in a temperature decay experiment designed to reproduce the Kulacki-Emara data. Although, it must be recognized that a horizontal layer configuration is more likely to promote a uniform interior behavior.

In effect, we have done that, and the result was successful (see Figure 5.3). As explained in the report, we would not expect to find anything different in a rectangular test section. However, the main issue of similarity hinges on the formation of, and sensitivity to, the stratification observed in the lower part of the hemisphere, and it would remain. Our strategy for addressing this issue is by using a very large scale experiment (as the ACOPO at 1/2-scale) as explained in our response to Schmidt.

17. III. Comments on Metal/Oxide Phase Separation

According to the analyses in the report, the location with the least thermal margin is near the equator of the hemisphere. The main reason for this behavior is due to the steel layer floating on top of the oxide melt. However, according to an analysis by Dana Powers (Dana Powers, "Chemical Phenomena and Fission Product Behavior During Core Debris/Concrete Interactions, Proceedings of the Committee on the Safety of Nuclear Installations (CSNI) Specialists' Meeting on Core Debris-Concrete Interactions, NP-5054-SR, Compiled by R.L. Ritzman, EPRI, September 3–5, 1986), the presence of metallic zirconium can lead to the formation of uranium metal and resulting in a denser metal phase. An experiment by Park et al. is quoted in the paper to illustrate this possibility. Since phase separation is associated with the location of least margin, the authors may want to look into the possible existence of a heavier metal phase.

We have examined this mechanism and are skeptical that we can take credit for it. Even if it was operative there would be transient aspects associated with sufficient uranium getting dissolved, and then the crusts should be dealt with before one can see this metal sinking to the bottom. Also, it should be made clear that the reviewer's second sentence ("The main reason . . . on top of the oxide melt") applies only to a couple of limiting parametric evaluations (Figs. 7.15 and 7.16). In the base case, as well as all other parametrics the least thermal margin is at high elevations, but still in regions in contact with the oxidic pool (Figs. 7.10 to 7.14).

T.3. Response to V.K. Dhir (UCLA) — Specific Assignment: Ch. 3

General Comment and Highlights

This reviewer hesitates to accept the report conclusions on critical heat flux. Besides seeking a number of clarifications, there seem to be two main reasons for this. First is his assertion (his point #2) that "in the reactor cavity, counter current type of flow simulation will occur rather than that of a natural circulation loop (co-current)". As shown in Figure 3.1 and discussed in Appendix M, this is *not* correct. The flow enters the cavity near the lower head elevation through a tunnel connecting the cavity to the steam generator compartment. Most likely, the reviewer was misled by Figure M.1 (one of the "standard pictures of the AP600 compartment") which represents a cut that is not through this tunnel. The same is true in Figures K.1 and K.2, and we regret that. Also, the confusion may be due to our not showing the insulation in Figure 3.1. The actual geometry can be understood by visualizing the tunnel shown in Figure 3.1, superposed on Figure K.2. To eliminate the chance of such a misunderstanding in the future, we have added a statement to that effect in the caption of Figure K.2.

Second, the reviewer asserts in his closing paragraph, "... at this point, the information is incomplete and it is not possible to conclude that boiling heat flux on the outer surface of the vessel will be below the local critical heat flux under all types of heat fluxes imposed on the inner wall of the vessel." In the intervening few months since the report went out for review, we continued work with ULPU (Configuration II) to better understand the behavior and hopefully zero in on the mechanism. In particular, we now have data under a much wider "spectrum" of flux shapes and natural circulation flow rates that reveal as insensitive behavior. Moreover, we were able to "visualize" the boiling crisis and obtain data on systems effects simulating the actual thermal insulation design, including the exit flow restriction. All this material is provided in new Appendices E.3 and E.4, and we believe fully addresses this concern of the reviewer.

Point-by-Point Responses

1. *In Chapter 3, the authors discuss the coolability of the reactor vessel with emphasis on the heat fluxes that can be accommodated under nucleate boiling conditions on the outer surface of the vessel. Local and global aspects of boiling on the vessel outer surface are discussed. Two sets of critical heat flux data have been obtained (Appendices E.1 and E.2) on a one dimensional full length representation of the reactor vessel. In the first set, the data are obtained under pool boiling conditions with heat supplied to only the lower portion; covering angular position from* $+30$ *to* $-30°$. *In the experiments liquid was saturated with angular position of the lower stagnation point being* $0°$ *and that of the equator being* $90°$. *A*

correlation for the critical heat flux obtained from these data is reported. In the second set of experiments, a natural circulation loop was established. Heat flux distribution on the test surface was established to simulate a reference heat flux. The reference heat flux was obtained from an earlier study of Theofanous et al. The heated region spanned from 0 to 90°. Because of the hydrostatic head difference in the natural circulation experiments, a liquid subcooling of about 10 °C existed near the lower edge. The critical heat fluxes obtained in natural circulation experiments are found to be higher then those obtained under pool boiling conditions. Again, the data have been correlated with angular position. The authors have done careful experiments and have obtained nearly full scale simulation of this prototype. They should be complimented for it.

* *

2. *1. The authors claim that their full length representation affords an essentially perfect full scale simulation. I cannot agree with this statement. At the stagnation point of a sphere, the behavior of the vapor bubbles at departure will be different than that for a plane surface.*

This point is true, strictly speaking, at the neighborhood of the stagnation point ($\theta \sim 0°$); but in a practical sense, it can be said that the ULPU representation in this area is conservative. Moreover, from the data trends found in angles away from the stagnation region (say $\theta \geq 15°$), for which the ULPU simulation by power shaping is quite adequate, we can say that, in fact, this conservatism is not quantitatively significant. Finally, it should be kept in mind that, as discussed in the report, the stagnation region is the least interesting from the point of view of lower head failure.

3. *2. In the reactor cavity, counter current type of flow simulation will occur rather than that of a natural circulation loop (co-current). Hence, I believe that the configuration shown in Figure E.4 is more appropriate. Data for this configuration have been obtained when the heated region spanned $-30° \leq \theta \leq 30°$. It is important that data be obtained for this configuration when the heated region spans $0° \leq \theta \leq 90°$. The critical heat flux in this configuration will be lower than that for the natural circulation case.*

This is not correct, as discussed under highlights. The caption of Figure K.2 was supplemented to prevent such misunderstanding in the future.

4. *3. Some flashing of the superheated liquid is expected to occur in the upper region ($\theta \sim 90°$). The authors do not report any such observation. A discussion*

of the effect of flashing in the local critical heat flux in the upper region is also needed.

The important effect of flashing is in flow oscillations, and we hinted on that around the middle of page 9–1, by reference to the "dynamic behavior of the two-phase natural circulation flow." The subject is now explicitly discussed in Appendix E.4.

> *5. 4. The heat flux imposed on the inner wall is obtained from the earlier work of Theofanous et al. I do not know if the imposed heat flux distribution represents an upper limit for all types of molten pool scenarios that can be envisioned. This includes partially filled lower vessel heads as well.*

This point is well taken. We now have data for a much wider range of flux shapes, including those found in the parametric and sensitivity studies of Chapter 7 (see also Appendix P). The results demonstrate the cumulative effect of upstream power, especially for the important higher elevations, such that the more peaked the profile is the higher the critical heat flux.

> *6. 5. It would have been interesting and informative if the authors had compared their steady state critical heat flux data under pool boiling conditions with the data reported in the literature from small scale (a few centimeters in length) test sections. It should also be noted that most of the data reported in the literature on small scale test sections were obtained under transient conditions.*

We are strongly against such data comparisons. As discussed in the report already, the small scale experiments, besides having been obtained under transient conditions, physically have nothing to do with the problem at hand, and any agreement, or disagreement, with these data is bound to cause confusion. This can only change if a mechanistic connection between ULPU and these small scale experiments is found.

> *7. 6. To isolate the effect of global versus local conditions, it would have been valuable if the authors had reported the critical heat flux obtained at a given location when all of the regions upstream of the given location are heated and when heating is provided only locally.*

We now have such data (see Appendix E.3).

> *8. 7. The actual heat flux profiles on the heated block surface were obtained by numerically solving the two dimensional conduction equation with appropriate*

boundary conditions. No information is given as to what those boundary conditions were. Also, we are given little information on the progression of the dryout front from zone to zone after occurrence of critical heat flux conditions at a given location.

The block surface in contact with the water was assigned the measured temperature (~130°C), while all other surfaces were kept at a zero heat flux. The calculated temperatures on the back face (opposite to the wetted one) was in very good agreement with the measured values. This point is now made in Appendix E.4. This appendix also contains data on the spreading of the dryout region.

9. 8. It is stated that the annular gap in the prototype is 20 cm. From the information given in the report, I cannot ascertain if the hydraulic diameter in configuration 1 of ULPU is scaled properly with respect to the prototype.

The inlets of the U-tube in Configuration I have a diameter of 15 cm. This has been added in the description (p. E.1–12). The annular gap in the reactor geometry is also 15 cm.

10. Finally, I believe that the authors have obtained very valuable data. However, at this point, the information is incomplete and it is not possible to conclude that boiling heat flux on the outer surface of the vessel will be below the local critical heat flux under all types of heat fluxes imposed on the inner wall of the vessel.

We are confident that the greatly enlarged data base in Appendix E.3 and interpretations of it will satisfy the reviewer's concerns.

T.4. Response to M. Epstein (FAI) — Specific Assignment: Chs. 2.,6,7,8,9

General Comment and Highlights

This reviewer raised a valid point, about the flux distribution in metal-layer-wall interface (due to boundary layer development). It was an oversight on our part not to be explicit about it. A relevant discussion has now been added at the end of Chapter 5. We also appreciate his point about jet-diameter behavior and the role of metallic jets in the impingement analysis of Chapter 8. These points are addressed in the new Appendix O.

Point-by-Point Response

1. *I have read with care the chapters of the above-references report that were assigned to me, namely Chapters 2, 6, 7, 8, and 9. I felt compelled to also read Chapter 5 in order to gain the required background for Chapter 6.*

Overall I find the authors' version of in-vessel retention to be a scrutable and believable one. In particular, I liked the authors accident scenario-independent treatment of the subject. Moreover, I feel that the report will serve as a handy reference source for the pertinent, recent literature on natural convection in volumetrically heated pools, downward boiling, and thermophysical properties of high-temperature materials.

I only have two major comments with regard to the technical content of the report, both of which are aimed at strengthening the authors' already good case for in-vessel retention. These comments are listed below and are followed by several additional, but relatively minor comments that the authors may wish to consider.

* *

2. *(1) It is not clear to me that the authors have provided a conservative treatment of the melt layer, as stated in Section 5.2. My understanding is that Churchill and Chus' free convection heat-transfer correlation, Eq. 5.39, gives the average heat flux along the vertical segment of the reactor vessel wall in contact with the molten metal layer. I would anticipate a considerable variation of the local heat flux along this segment with a peak heat flux achieved just beneath the surface of the metal layer that may be of the order of a factor of two greater than that predicted with Eq. 5.39. Perhaps the authors feel that they have incorporated or compensated for "heat flux peaking" when they speak of the "focusing effect" and lateral eddy diffusion limitations in the bulk (on page*

5-17). Unfortunately I have difficulty in following these arguments or pinpointing where in Appendix N that these arguments are confirmed. Perhaps I am wrong, but my feeling is that the only major limitation to the lateral flow of heat is the laminar sublayer adjacent to the vessel wall and that, in order to properly assess the maximum heat flux from the metal layer to the vessel wall analytically, the appropriate coupling (thermal and mechanical) must be made between the upward flowing free stream just outside the side-wall free-convective boundary layer and the downward flow within the boundary layer itself. Alternatively, the heat flux variation along the side wall can be obtained by experiment, perhaps with a modified version of the apparatus described in Appendix N.

Valid point. See addendum at the end of Chapter 5, where we show that this "entrance effect" is more than compensated for by 2D conduction through the wall.

3. (2) I think the authors can provide a more convincing jet impingement analysis (argument) than the one presented In Section 8. In particular, I believe more information is needed to justify the lower bound jet diameter of 10 cm. It seems to me that a breach on the core-side boundary may first appear as a small opening (pin-hole or crack). Thus the early stages of the core draining process may occur via a narrow, high-impingement heat-transfer jet. Of course, the jet heat flux will decrease with time owing to the enlargement of the breach. An analysis of this process should appear in Section 8, and apparently such an analysis is available (Sienicki, 1995). More detail regarding Turland's (1994) work should also be included. In other words, all the available arguments that put the jet impingement issue to rest should be spelled out in Section 8. Also, something should be said about the unlikelihood of molten metal jet impingement during core relocation.

See new Appendix O.

4. (3) The authors may wish to reference Epstein and Fauske (Nuclear Technology 87, 1989, 1021–1035), as they were the first to suggest the core relocation picture illustrated in Fig. 2.3 (for TMI) and to my knowledge they were the first to examine heat loads from in-vessel molten-core material pools by using a methodology that is very similar to the one used in the subject report.

Reference added in p. 2-5; but this kind of approach goes back to the LMFBR days.

5. *(4) Is there any experimental data that supports the last sentence of the paragraph that follows Eq. (H.8) in Appendix H (page H-6)? I believe that this sentence should read "when the stream diameter becomes sufficiently small compared to the <u>boundary layer thickness</u> ahead of the ablation front ...". It would seem to me that the head thickness is not an important parameter with respect to the erosion rate, as long as melt is removed from the cavity formed by the jet as the jet erosion process proceeds.*

But this is the point. The melt is *not* completely removed from the cavity (see Saito et al., (1990).

6. *(5) I was particularly interested and impressed by the experimental work reported in Appendix D. I might mention that we (FAI) proposed the idea of quasi-steady cool down experiment to simulate steady-state turbulent natural convection with volumetric heating some time ago (verbal and written solicitations to ARSAP and EPRI, respectively, June 1992 through February 1993). I was pleased to learn by reading the report that the method works and I hope it will be utilized to once and for all settle the issue of the heat transfer split in hemispherical segment pools at "infinite" Rayleigh number.*

Our program for this project under ARSAP began in January 1993, and we were not planning to conduct original work in this area. We realized the need near the end of 1993, and conceived the ACOPO idea in February 1994. The mini-ACOPO was built in May 1994. We were not aware that FAI has proposed a cool down experiment previously.

7. *(6) The inequality $Ra < 10^{12}$ on the top of page 5.17 bothers me. Given the form of the correlation (Eq. 5.39) 1 am sure that there is a lower Rayleigh number below which this correlation is invalid.*

This is the way the range was specified in the reference. Since it covers the transition regime, the actual lower limit would be much lower than 5×10^9, which is the lower limit used in our analysis.

8. *(7) Typos: (i) Page C-17, change ragid to rigid in figure caption for Fig. C.6 and (ii) Page N-5, 4 lines from bottom: "furtitious"?*

Corrections made.

T.5. Point-by-Point Response to R.E. Henry (FAI) — Specific Assignment: Chs. 2,3,6,7,8,9

General Comment and Highlights

While this reviewer agrees with the technical positions and conclusions of the report, he requests several "enhancements" to be made, in the interest of clarity. These, and their disposition, are discussed point by point.

Point-by-Point Response

1. *As requested, I reviewed the report entitled, "In-Vessel Coolability and Retention of a Core Melt". I agree with the general approach taken in the report, the formulation of the analyses for the molten pool, the relative distribution of heat fluxes from the pool and the conclusions of the report. While I believe some additions need to be made to the report, which are discussed below, this report can be used as a document which assembles the major works performed in this area and provides sufficient justification for the conclusion that external cooling of the reactor pressure vessel lower head and cylinder can prevent failure of the structures even when molten core debris exists in the lower head.*

* *

2. *1. The discussion with respect to the molten pool is focused on a fully molten pool with a rigid boundary at the melting temperature. Certainly this is the case for experiments such as the COPO and UCLA tests. However, as discussed in the report, the core debris in the lower head would be expected to have different temperatures for the solidus and liquidus states. The report clearly specifies the temperature that should be used to characterize the heat transfer from the molten pool, i.e. the liquidus temperature. However, there is no discussion on the influence of a "slush layer" between the fully molten pool and the rigid frozen crust on the vessel inner surface when there is a significant difference between the solidus and liquidus. How would this be expected to influence the correlations that have been developed from pools in which the solidus and liquidus temperatures are equal, i.e. a single melting temperature? My intuition is that this would tend to decrease the downward heat transfer and increase the upward heat transfer. If this is the case, the use of the correlations by the authors for fully molten pools tend to be a conservative representation of the reactor system. Some discussion should be included with respect to the importance of this slushy layer between the pool and the crust and the general influence this would have*

on the calculated results. The details of this behavior are relatively complex, but likely not of first order importance. However, the qualitative influences of this difference should be considered in the report.

The "slush layer" is a thin region all around the inner crust boundary that allows the transition from the liquidus to the solidus (at the slush-crust interface). From the point of view of natural convection the pool "sees" the inner boundary of this layer, it being isothermal, at the liquidus. This, then, controls convection; only this! The resulting local fluxes (together with the thermal resistance of the remaining path to water, including any "gap" between the crust and the lower head) determine the thickness of this layer and of the crust behind it. But, we are not interested in the details of this split. Rather, we lump the two together in an effective crust. The "approximation," then is only to the extent that the thermal conductivity of the slush layer differs from that of the crust—truly a second order effect. One might think that in regions of low convection, i.e., at $\theta \sim 0°$, the slush layer would tend to buildup. However, this is self-limiting in that conduction alone cannot provide sufficient cooling, and the upper portion is heated to above liquidus to the extent necessary for a stable behavior. This stable behavior is the solution provided from our equations. Since this point was brought up by another reviewer as well, we have added a short explanation in page 6-3, and make reference to the response provided here for more detail.

3. 2. As discussed in the report, the sequences which are considered are generally those in which the RPV lower plenum is full, or almost full of water, at the time that molten core debris enters the lower head. Experience with such situations indicates that there could be a non-trivial contact resistance develop between the crust and the wall when this occurs. Such a contact resistance is not considered in the analysis presented in the report. Neglecting such a resistance is a conservatism in the analysis for the downward energy transfer to the RPV lower head. Conversely, this increases the upward heat transfer to the remainder of the RPV and therefore the heat flux transferred to these other puts of the reactor vessel. Estimates from the available information suggest that the contact resistance could be the equivalent of conduction through a few centimeters of UO_2*. Here again, the details of the analyses do not have to be included; rather, the influence of such behavior should be discussed and perhaps included as part of the sensitivity analyses at the end of the report.*

As discussed above, the effect of any gap would be to decrease the crust thickness and hence the conductive component of the heat going through the vessel wall. The effect would be maximum at $\theta \sim 0°$, and ignoring it there is certainly conservative; by how much can be deduced from the crust thickness distributions shown in Appendix Q. As far as effect on the global behavior, it is negligible, because by the condition of Eq. (6.8), the *total* crust is less than a few percent of the oxidic mass (and hence of the decay heat). The point is clarified by reference to this response in page 6-3. Also see new Appendix P.

4. 3. There is discussion with respect to the influence of a boil-up level in the gap between the insulation and the reactor vessel cylinder. The inleakage of water through the gaps in the insulation must be considered as a two-way street. Water certainly can readily ingress into the insulation, but the boil-up level can also tend to leak out through the gaps in the insulation thereby decreasing the influence of such a boiled-up situation. This should be discussed in terms of both behaviors.

The flow rates are quite high for any such out leakage to be significant.

5. 4. The bottom line to the integral evaluation is discussed in Section 6. Since this documents the integral analysis, I recommend that this discussion be expanded to make several of the central elements of the analysis more clear. For example,

a. Equation 6.6 described the heat flux into the wall. Does $\delta_{cr}(\theta)$ include the power generated in a "slushy layer" dictated by the temperature difference between the liquidus and solidus conditions?

Yes. See response to point #1.

6. b. The upward radiation calculation described in 6.10 assumes one characteristic temperature for the steel internal structures and therefore does not need to consider the respective view factors to individual parts of the rector vessel, i.e. the downcomer and the upper internals. If the discussion is only focused on the integrity of the lower head, this is sufficient. Conversely, if the intent is to describe the potential for in-vessel core debris retention, then it is important to justify that the upward energy flux does not cause the vessel to fail at some other location between the metal layer and the vessel support location, i.e. the hot legs and cold legs. To accomplish this, the analysis should be somewhat more detailed than that which was represented by Equations 6.10 and 6.12.

The reason there is no concern for the side wall, and hence for a detailed radiation network-type calculation, is because the process is overwhelmed by the large surface available to dissipate the heat radiated off the top of the pool. For example, the heat flux leaving the top of the metal layer is limited by $\epsilon_s \sigma T_{\ell,0}^4$. With $\epsilon_s = 0.45$ and $T_{\ell,0} = 1712$ K, which corresponds to the condition in Figure 7.16, this heat flux is 220 kW/m^2. Actual fluxes through the vessel wall will be much lower than this value due to the much larger surface area available.

> 7. *d. The solution scheme for Section 6.12 discusses using T_s as an iteration parameter By deduction it appears that this is the average temperature between T_{si} and T_{so}. However, I could not find this stated in the discussion. Since the upward heat flux from the pool and the dissipation to the respective parts of the reactor vessel and its internals are equally as important for in-vessel retention as the behavior of the lower head, the specific details of how this solution is determined and the respective split between upward and downward energy transfer should be displayed in this section. This needs to be done to justify the conclusion that "thermally-induced failure of an externally flooded AP600-like reactor vessel Is physically unreasonable."*

As defined in the nomenclature, T_s is the radiative sink temperature. It is *not* the average between the T_{si} and T_{so}. The solution to Eqs. (6.9) to (6.12) is *unique* and we find it in the manner described just below Eq. (6.12). This solution does not affect the upward to downward energy split. It determines the upward to sideways (the metal layer to vessel wall) energy split. Detailed results such as T_{so}, T_s, etc., are collected in Appendix Q.

> 8. *As mentioned above, I believe that this report provides the necessary foundation for documenting the case for in-vessel retention using the numerous attractive features of the AP600 design. However, to provide this foundation, several of the discussions in the report should be enhanced such that the approach and conclusions are clear.*

Done as described above.

T.6. Response to T. S. Kress (ORNL) — Specific Assignment: Chs. 2,6,7,8,9

General Comment and Highlights

The main point, and concern, of this review was summarized in the closing statement: "The defense of the case, in my mind, strongly rests on justifying the choice of decay heat value." Having cited the reference, Schnitzler (1981), we did not elaborate on the bases of the calculation. Further, we now realize that the short explanation at the bottom of p.7–1, may mislead one to think that all we are accounting for is the relative decay of volatiles, to obtain an effective non-volatile power fraction. We welcome the opportunity to clarify this point here.

The reference (Schnitzler, 1981) describes models developed for use in the SCDAP-RELAP code in order to properly reflect the time-dependent decay heating in disrupted fuel regions. Our Figure 7.2 curve is one of four given for a typical PWR core evaluated at equilibrium (33,800 MWd/MTU), under various heatup assumptions—we chose the one representing the highest decay heat values in the time range of interest (i.e. after 3 to 4 hours). The calculations were carried out with the ORIGEN2 code, using the time-dependent removal rates shown in Table T.1, where the various radionuclide groups are identified in Table T.2. As can be seen in Table T.1, no releases are assumed to occur after 16 minutes, so all subsequently formed volatile products are retained in the melt. Reviewed recently for our present purpose (Osatek, Personal Communication, 1995) the removal rates in Table T.1 were found to be reasonably conservative, except for Groups 6 and 7 (Ba and Sr). However, the *total* contribution of these radionuclides was found to be under 0.4%, which is too small to warrant that an actual correction be made.

Point-by-Point Response

1. *Introduction:*

In this review, I considered the key items that would influence the ability of external cooling to prevent vessel failure to be:

1. *Quantity of melt*
2. *Composition of melt*
3. *Decay heat level*
4. *Internal pool heat transfer*
5. *Radiation heat transfer off top surface*
6. *Boiling heat transfer on outside of vessel*

Table T.1. Element Group Release Rate Constants for Rapid Heatup and Quench (Table 4 of Schnitzler, 1981)

Interval After Fuel Failure (Minutes)	(Minute^{-1}) Group Number 1	Group Number 2	Group Number 3	Group Number 4	Group Number 5	Group Number 6	Group Number 7	Group Number 8	Group Number 9	Group Number 10
0–1	0.1000	0.0100	0.0100	0.0100	1×10^{-6}	0.0	0.0	0.0	0.0	0.0
1–2	0.0060	0.0060	0.0060	0.0015	0.0004	8×10^{-5}	1×10^{-5}	1×10^{-5}	1×10^{-6}	1×10^{-5}
2–3	0.0250	0.0250	0.0250	0.0066	0.0025	0.0007	0.0001	6×10^{-5}	5×10^{-6}	1×10^{-6}
3–4	0.1000	0.1000	0.1000	0.0300	0.0150	0.0060	0.0015	0.0004	6×10^{-5}	1×10^{-5}
4–10	0.0250	0.0250	0.0250	0.0066	0.0025	0.0007	0.0001	6×10^{-5}	5×10^{-6}	1×10^{-6}
10–11	0.7000	0.5000	0.3000	0.1000	0.0400	0.0100	0.0040	0.0008	6×10^{-5}	1×10^{-5}
11–16	0.5000	0.3000	0.1000	0.0300	0.0200	0.0070	0.0020	0.0005	6×10^{-5}	1×10^{-5}
>16	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0

Table T.2. Fission Product Element Group Membership for Release Calculations (Table 2 of Schnitzler, 1981)

Group	Elements
1	Kr, Xe
2	Br, I
3	Cs, Rb, Se
4	Te, Ag
5	Sb
6	Ba
7	Sr
8	Zn
9	Ru, Rh, Pd, Mo, Tc
10	Y, La, Ce, Pr, Nd, Pm, Sm, Eu, Zr, Nb

7. *Integration of Items 1–6 (resultant wall temperatures, wall thinning, ability of wall to carry loads, and treatment of uncertainties).*

My review comments that follow are ordered as above and are intended to address the adequacy with which each of these were dealt.

* *

2. 1. Quantity of Melt and 2. Composition:

The analysis included all of the oxidic core, all of the Zr available, the lower support plate, the reflector, the lower supports, and some portion of the core barrel. The fraction of Zr oxidized was treated probabilistically in three ranges:

– most likely range...	*.4 to .6*	*(probability of P)*
– unlikely range...	*.6 to .7*	*(P/10)*
– highly unlikely range...	*.7 to .9*	*(P/100).*

Comments:

The greater the quantity of ZrO_2 added to the melt, the more dilution effect you will have (that is, you will reduce the effective volumetric heat generation rate). In addition, putting more of the Zr into the melt as the oxide reduces the thickness of the metallic layer overlying the fuel melt. Thus, I would expect higher values of ZrO_2 fraction to be non-conservative with respect to this problem.

I think the probability density function for the fraction of Zr oxidized should have included some relatively high probability that it would be less than .4.

As explained, we expect that Figure 7.3 is already quite conservative. The sensitivity presented in Figure 7.11 (compared to Figure 7.10) shows that shifting the distribution to the right by 10% has an essentially negligible effect. The nature of the physics is such that the effect of the ZrO_2 quantity is continuous, hence we would expect a similarly low sensitivity if we were to shift the distribution to the left. We confirmed this by carrying out the calculation, and the result is shown in Appendix P. Note that increasing the metal layer thickness reduced the peak heat flux in that region.

3. Similarly, when one adds the amount of steel in the lower support plate, the reflector, and the lower supports one gets a total of 77 tons without adding in any of the core barrel. I would have expected to see the "likely probability" range for the steel mass in Figure 7.5 to extend upwards to beyond 80 tons instead of the 72 tons shown.

As explained in the report, Figure 7.5 was specified so as to represent a conservative distribution. By extending the distribution to higher values, as suggested by the reviewer, we actually diminish the thermal loads. But the results show clearly that the metal layer is not limiting (it is thick enough), so increasing the quantity further would produce a slight and inconsequential decrease also.

> 4. *With the ROAAM procedure, I worry about cliff effects. An abrupt and severe change in the probability between ranges could mask a strong sensitivity in the region near the abrupt change. Because of the focussing effect of the metallic layer, the content of steel might be such an area to expect such a strong sensitivity.*

One needs always, with or without ROAAM, to worry about cliff effects. This is why we run extensive parametrics, and are open to reviewers' suggestions for even more. It is important to understand, however, that this is (should) not be a random exercise, but rather guided by the physics of the situation, and the results obtained already—as illustrated, for example, in the previous two questions. For example, we explored the focusing effect and its asymptotic implications in general (see Chapter 6), before finalizing the parameter ranges and sensitivities considered, to ensure that no credible near-cliff was overlooked. This sort of approach is essential to the proper application of the ROAAM process. Here it is suggested by the reviewer that because of the focusing effect, the steel content might present a strong sensitivity. In fact, at the end of Chapter 5, we provided a detailed evaluation of the point (i.e., at what metal depth the focusing effect sets in), and even provided general quantitative results in an easy-to-use graphical form. These, and the results in Chapter 7, show that in AP600, we are far from conditions where focusing concerns could arise. Further, focusing cannot be obtained by adding steel, as suggested by the previous question. Moreover, even when a condition leading to focusing was *contrived* (Figure 7.16), failure could not be obtained. See also Appendix O.

> 5. *3. Decay Heat Level (volumetric heat generation rate):*
>
> *The report chose to look at a bounding sequence (3BE) as being "of main interest to IVR". According to the MAAP code, this sequence gives the fuel melt in the vessel bottom head at about 4 hours after shutdown. To get the decay heat level at that time, the procedure was to multiply the total decay heat by the fractional contribution due to the non-volatile fission products.*

Comments:

I have some concerns about the above procedure. The choice of bounding sequence appears to be well founded. I would not be comfortable, however, in relying on only one codes calculation to determine the timing. I recognize that Figure 7.12 results from shifting this timing to one hour sooner and that this is an appropriate manner to address the sensitivity to this. Nevertheless, I see some strong sensitivity in the calculated $q_w(\theta)/\dot{q}_{chf}(\theta)$ to this shift although the decay heat increase was small.

Actually, we do not know where the reviewer sees the "strong sensitivity." Careful examination of Figure 7.1 will show that the increase in decay heat is ~10%, while Figure 7.12 shows an increase in thermal load by 10% also! Again, from the physics we can expect this proportional dependence rather than a "strong sensitivity."

6. My concern stems from concern about the validity of the decay heat value. The overall decay heat curve (that includes all nuclides) looks reasonable for a ~2000 MW th reactor compared to what I am familiar with for higher power reactors. (The 2000 MW value is my guess for the AP600. The report is remiss in not giving the real value or the source of its decay power curve). The modification to account for the loss of volatiles could be in error. The correct procedure would be to remove the appropriate volatiles at the initial time and redo the ORIGIN-type calculation that includes the decay schemes to determine the evolution of decay heat versus time. I am concerned that the process used may underestimate the decay heat because the decay schemes may build in additional volatiles not correctly accounted for by the procedure and which would remain in the pool to contribute their decay heat.

Actually, this is what was done (as explained under Highlights). The rated power of the AP600 is 1933 MW_t, and this was obtained from the AP600 SAR.

7. In addition, core melt accidents do not necessarily release all the volatiles before the melt enters the lower head. Estimates I have seen range as low as 50% released for the Iodine and Cesium and as low as 10% for the Te and Sb. Generally, even some small amounts of the Xe and Kr are assumed to remain with the melt. The conservative approach would have been to retain some portion of the volatiles within the melt.

As noted under highlights only the Ba and Sr numbers would need to be revised downwards in light of present understanding, but their total contribution to the decay power is less than 0.4%. Everything else is state of the art.

8. The report is remiss in not defining exactly what nuclides it considers to be volatiles and in not defining what fraction of these are assumed to be removed from the melt. This is all wrapped up in Figure 7.2 which, incidentally, looks suspect to me. I do not believe the fractional contribution of the non-volatiles approaches 1 immediately after shutdown.

As can be seen in Table 4 no significant release is assumed to occur in the first minute or so, and this is why the decay power fraction in Figure 7.2, begins from 1, and starts decreasing only after ~1 to 2 minutes.

9. 4. Internal Heat Transfer Coefficients:

Equation 5.28 was basically used for the pool-to-wall heat transfer coefficients as corrected for local distribution by Eqs. 5.30a and 5.30b. For the upward heat transfer to the overlying metallic layer, the Steinberner-Reineke correlation (Eq. 5.12) was used. Each of these was validated (or derived) via the Mini-ACOPO experiments as discussed in Appendix D. For heat transfer within the metallic layer, an existing literature correlation (Globe- Dropkin) was modified to allow separate application to heat transfer from the pool crust through the bottom boundary layer in the metal and from the metallic layer through the upper boundary layer to the top surface. For the "sideways" heat transfer from the metallic layer to the vessel wall, another existing correlation (Churchill-Chu, Eq. 5.35) was used which, coincidentally, gave a heat transfer coefficient approximately 1/2 that of the modified Globe-Dropkin correlation. The MELAD experiments reported in Appendix N were conducted to demonstrate the validity of the correlations for the metallic layer.

* *

10. Comments

The internal heat transfer aspects of this problem are, in general, well done and acceptable. The Mini-ACOPO experiments appear to be well founded and well conducted. The results from the 1/8 scale facility should be applicable to the full scale. I have one major comment and then a number of minor comments on this part of the evaluation.

The major concern I have here is with the use of the Churchill-Chu correlation for the sidewards heat transfer from the metallic layer. I see no good reason why this heat transfer coefficient should be so much less than that for the bottom and top surfaces. The MELAD experiments reported in Appendix N appear to validate the proposed use but these were conducted in a significantly different geometry from that of the disc shape in the reactor case. I would like to see some additional theoretical analyses to justify these results.

Neither of the two correlations can be doubted, because both are supported by extensive data obtained with various fluids and by numerous investigators. To understand the difference between them simply consider the direction of the buoyancy-induced motions, in relation to the orientation of the boundary. On top, the process looks a little like "nucleate boiling," while on the side it is more like "film boiling." Incidentally, we cannot understand the comment about the MELAD experiment geometry. The issue discussed here is one of principle, and surely behavior could not be affected by the cross sectional shape of the pool.

11. There is a need to better describe in the report the thermocouple locations in the Mini-ACOPO experiments.

The *exact* position of all thermocouples were clear in Figure D.3 of the report. It is not clear what additional information is requested here.

12. More justification is needed for the use of transient experiments to model steady-state conditions. This was addressed by Runs A4 and A5 in Appendix D. However, some comparisons of characteristic times would be helpful to completely close this issue.

For a more complete discussion of this issue see our response to Schmidt.

13. Figures D4 and D5 should identify the various data points shown at a given value of V_i/V (I assume they are for different times during the transient- but we are not told).

This can be (and was) done for Figure D.4 which contains data from one run only. The idea of Figure D.5 was to show that even with five runs included the dimensionless stratification trend is very similar to that of a single run. See also the addendum to Appendix D.

14. There is no figure showing that lateral temperature gradients are negligible as claimed on page D-11. An oversight?

Actually, the variation is so small that cannot be shown well in a figure. So, we simply added the statement that the maximum deviation between the wall and centerline readings (at any elevation) was less than 5% of the overall $\Delta T = T_{\max} - T_i$.

15. The report should do a better job of defining "Φ". It does not appear on the Figures or in the Nomenclature.

Actually, this was a typo. Φ should be θ_p, and it was corrected.

16. 5. Radiation Off Top Surface:

Radiation off the top surface of the metallic layer was treated in a standard manner that includes back radiation from the sink which was given a single constant temperature (to be solved for from the equations that include the total heat upward through the top surface, radiation, conduction through the heat sink, radiation off the back side of the heat sink to the vessel internal wall, and conduction through the vessel wall essentially to the water temperature). An emissivity of .45 was used and a sensitivity analysis was done for higher emissivity values.

Comments:

The procedure used is appropriate and acceptable. Nevertheless, I would have liked for the sensitivity study to have included lower emissivity values if only as an artificial means to try to enhance the "focussing" effect. I don't know whether or not the metallic layer has a crust on the top surface. A newly formed frozen layer of metal may have a low emissivity value.

The parametrics in Figures 7.13–7.15 were run to provide further perspectives on the effect of additional resistances on top of the metallic layer. The adiabatic case corresponds to an emissivity of zero. All three cases show that the effect is negligible. Hence it does not appear worthwhile to explore in-between values. Moreover, it is well known, and our data demonstrate that a solid surface, even if just-solidified, has much higher emissivity values than the liquid. We do not think that it is appropriate to use emissivities lower than 0.45 in the extreme (and purely hypothetical) case of Figure 7.16. However, to fully respond to this question, we have run this case with an emissivity of 0.35. The results are shown in Appendix P, together with all other additional parametrics. We find that the peak flux increases by only 10%, and with the new critical heat flux correlation specialized for this highly peaked shape we do not obtain failure. It should also be kept in mind that in this highly contrived case, as well as for the case of Figure 7.16 in the report we used a decay power of 1.4 M/m^3, which as we can see in Figure

7.8, is truly the upper bound. Since there was so much interest in this case, we have run, for it, a more "full" calculation, using the decay power as a parameter. The results are shown in Appendix P.

17. 6. Boiling Heat Transfer on Outside of Vessel:

The objective here was to determine the distribution of critical heat flux on the bottom head submersed in a water bath. This was accomplished experimentally by the use of the innovative 1-D ULPU test facility that had the following characteristics:

- *full length/correct curvature*
- *a "slice" geometry*
- *power input varied with position to match the distribution of heat transfer from the pool side as measured in Mini-ACOPO*
- *an "aged" copper surface.*

I found the description of the experiment procedure in Appendix E to be somewhat obtuse. With persistence, however, you can figure out what was done.

I believe the experiment procedure to be valid (i.e. determining the local CHF as a function of angular position by matching the steam flow into the local region that would be obtained as produced in upstream areas for the total heat required to produce the local CHF. It is recognized that a 2-D prototype is modelled by 1-D tests. I believe this is conservative because the 2-D streamlines are divergent whereas the 1-D streamlines in the test are parallel. This should result in a slightly lower measured CHF than one would expect in the real case.

I believe when these tests are validated for the surface material, this will be sufficient to determine the distribution of CHF on the external surface of the bottom head.

* *

18. 7. Integration to Determine Resultant Wall Temperatures, Wall Thicknesses, Loads, and the Ability to Carry the Loads:

Mostly, deterministic calculations were used. However, the ROAAM procedure was used with assigned probability distributions for

- *decay power*

- *quantity of Zr oxidized*
- *quantity of steel in metallic layer, and*

some sensitivity studies were also made.

Comments:

I commented earlier on the probability ranges for the above parameters. I also believe the sensitivity studies should have included variations in the opposite directions to those made. For example,

- *a lower value of emissivity*
- *an overprediction of the downward heat flux (rather than Mayinger's correlation which underpredicts the downward heat flux)*
- *a shift of the fraction of Zr oxidized to the left rather than to the right.*

All of these were responded to above. None was found to have a significant impact on the conclusions.

19. For the "thermal jets" issue, the use of only 1/3 of the fuel volume and a jet diameter of 10 cm need better justification. Figure 8.1 shows that even with $V_r = 1/3$ of the fuel volume and $D = 10$ cm, you get a total ablation depth of 12.5 cm – perilously close to the wall thickness of 15.24 cm. It doesn't take much more fuel or a much smaller jet diameter to ablate through.

The calculations in Chapter 8 and in Appendix H were done at such extreme conditions that it is not appropriate to take, as the reviewer does, the results as something that might actually happen (see also Appendix O). To leave 3 cm of metal under such a calculation we interpret as comfortable margins to failure, rather the "perilously close" of the reviewer. In reality it is extremely unlikely that (a) 1/3 of the core can come out as a coherent pour, and (b) remain as a single release point with a diameter as small as 10 cm. Moreover, the calculation assumed a normal impingement and a continuous removal of both melts.

20. Final Comments:

This was indeed a comprehensive and competent piece of work to address this issue. I checked all of the equations presented and could find no errors.

The report itself suffers, I believe, from including too much peripheral material put there for "perspective". I think the report would have been better if it *focused more on what was actually done and on the correlations actually used in the analyses.*

We put in the report nothing more than necessary to explain and support our case. In the overall perspective of the 17 reviews obtained this is really a surprising comment.

21. The defense of the case, in my mind, strongly rests on justifying the choice of decay heat value. The comments I made earlier in this review on the content of volatiles, the timing, and the appropriate modification of the curve for loss of volatiles are very important.

See General Comment and Highlights.

T.7. Response to S. Levy (Levy and Associates) — Specific Assignment: Chs. 2,5,6,7,8,9

General Comment and Highlights

This review is highly critical on *the* whole range of topics covered in the report. Not a single thing seems to have satisfied the reviewer, and not even a few things can be selected as *the* major criticisms. Accordingly, we are reluctant to provide here any highlights. The review and our responses have to be read in their entirety. We found nothing in this review that would alter our conclusions.

Point-by-Point Response

1. As per Argonne National Laboratory (ANL) request of November 10, 1994, I have reviewed the subject report and I wish to first command [sic] the authors for their extensive analytical and experimental work in support of the concept of "in-vessel retention" in the AP600 passive nuclear Pressurized Water Reactor (PWR). However, I have several concerns about the studies and I have attempted to group them by specific topic areas to help the authors prepare responses to my comments:

* *

2. A. Boiling Crisis or Critical Heat Flux (CHF)

DOE/ID-1046 relies upon data from Figure 3.3 for CHF as a function of position on the lower head for quantifying the thermal failure criteria. These data were taken under full submergence and natural convection in the ULPU facility. My concerns are as follows:

1. Natural convection enhances the CHF condition. This is clearly visible by comparing the results of Figure 3.3 with those of Figure 3.2 obtained for pool boiling. The increase in CHF is 67% at the zero degree angle position and 36% at the 90 degree angle. This means that the natural circulation in the tests must simulate accurately the flow behavior in the AP600. It should be noted first that in Figure E.1 the cold water is returned at the bottom of the cavity rather than "draining into the reactor cavity through a tunnel at the compartment floor elevation which spills into the cavity at the elevation of the top of the lower head" when the IRWST drain valves are actuated (see page M-4 and Figure M-2). Subsequently during "passive reflux to the cavity" (which is being simulated by the ULPU tests), water "would enter in the outlet nozzle region and drain down through the octagonal portion of the cavity" (see page K-4). During this mode *of operation steam water flow will rise in a counter flow mode to the returning water in the cavity annulus. This countercurrent flow will produce less natural circulation flow than in the ULPU tests and also it most likely will impact the subcooling of the water reaching the bottom head.*

The counter flow mode described is incorrect. The actual flow path was shown clearly in Figure 3.1, and ULPU simulates it faithfully.

3. 2: The authors have recognized that their tests do not include reactor pressure vessel insulation. The insulation is bound to interfere with the natural circulation flow not only by reducing the size of the annular gap but also by providing increased resistance for the water to reach the vessel outer surface. An allowance should be provided for this reduction until tests with prototypic insulation can be carried out.

We disagree with this point too. It is highly inappropriate to "make an allowance" for something you know little about. We took the position that to rely on "leaky" insulation is inappropriate. We wish to have "free" access of water towards the pole of the lower head; we explained that clearly, and in Appendix K we even indicated one possible approach to accomplish that. Meanwhile, Westinghouse has developed an insulation concept along these lines (see addendum to Appendix K), so the point is mute now.

4. 3. The tests were performed with thick highly conducting walls. Past CHF tests have shown that such circumstances will increase the local critical heat flux. While the reactor pressure wall thickness is large to start with, it could thin down significantly during the course of the severe accident and tests with less conduction might be appropriate.

The test section thickness is 5 cm, and only in the most extreme parametric scenarios considered do we not find sufficient melting to reduce the wall to such an extent. Not only is 5 cm conservative, once you go beyond a few centimeters there is not much effect anyway.

5. 4. The AP600 reactor vessel standoff insulation concept depicted in Figure K.1 shows narrow (about 2.5cm) flow passages between the vessel and the insulation panels. Even in the alternative insulation concept of Figure K.2, the flow passage is about 5 cm. (The concept in Figure K.2 will create strong cavity air recirculation along the reactor vessel wall, which will reduce the effectiveness of the insulation and increase the temperature of the reactor cavity concrete). Such *insulation configurations will not only reduce the natural circulation flow rate but they would encourage the steam to flow along the narrow spacing between the reactor vessel and the insulation. Therefore, they would tend to approach conditions found in thin rectangular channels submerged in saturated liquid. A significant amount of CHF data has been obtained in thin vertical channels and they show a drop in pool boiling CHF as the ratio of length to width of the channel increases. At atmospheric pressure and a length to width ratio of about 30 the CHF drops to 32 percent of the accepted pool boiling value (see M. Mode et al, Critical Heat Flux During Natural Convective Boiling in Vertical Rectangular Channels Submerged in Saturated Liquid, ASME Transactions, Journal of Heat Transfer, Vol. 104, pp 300-303, May 1992). Some similar and strong negative impact due to the presence of insulation is expected in the AP600 configuration and its magnitude will depend upon the final design of the insulation. Still, an allowance needs to be provided at this time.*

Much of this is speculative, and again we disagree with an approach based on "allowances." An insulation concept along the lines indicated by the ULPU experiments, has been developed by Westinghouse designers (see addendum to Appendix K). There is no penalty for increased air natural convection during normal operation, there is no significant resistance to inflow, and the minimum clearance is 23 cm which is larger than in the ULPU Configuration I test section. As promised in the report, confirmatory experiments will be run, in Configuration III ULPU, now that the geometry is known (see Appendix E.4). The reference cited is irrelevant to our situation.

6. 5. The potential impact of the accident upon the insulation is noted in the report. However, if the large LOCA break takes place within the cavity, one can expect significant damage to the insulation and potential flow blockages in the cavity outlet nozzle region.

The only LOCAs considered possible in the cavity are from failure of the direct vessel injection line. Any failure at this line is unlikely and only failures localized very near the vessel would impact the insulation. Such localized failures are highly unlikely if not below the screening frequency level. The insulation panels are well supported (so failure could not occur far from the break), and the material is reflective steel, to preclude the kinds of blockages that might otherwise occur with fibrous insulation. The only fibrous insulation is used above the steam vents which provide the exit flow path.

7. 6. Because the water refilling the cavity is borated, its boiling will deposit boron on the reactor vessel surfaces and its impact upon CHF was not considered.

Also, the water reaching the reactor cavity will contain dirt and dust and it will accumulate in a reactor cavity which cannot be expected to be clean and may contain paint flaking off the vessel. This lack of water purity conditions needs to be recognized.

Yes, indeed. ULPU was run under highly pure as well as under highly contaminated conditions—rust, pieces of plastic, paint flakes, etc. No effect was found. Any deposits on the surface will increase the wetting property of it and hence the critical heat flux capability. Because of the strongly convective flows we cannot expect to have boron enrichment in the cavity, and as one of the final confirmatory tests we plan to run long term with the proper boron concentration in the water to examine the rate of buildup due to boiling precipitation.

8. 7. In view of the preceding comments, significant degradation in the CHF values of Figure 3.3 are anticipated (possibly by as high a factor as 2 to 3). It is remarkable, therefore, that no sensitivity study of this important parameter was included in Section 7.3 and it is recommended that it be added.

As described above, any reduction of CHF from those found in ULPU are unfounded, and no such sensitivity studies are warranted.

9. B. Subdivision into Regimes and Lack of Analysis of Intermediate Transient States

DOE/ID-1046 is limited to the long term natural convection-dominated thermal regime conditions depicted in Figure 1.2. There are several statements in the report that "this approach is conservative" (see page 2-1) and that "the thermal loads to the pool boundaries throughout the time period of a heat-up transient are bounded by the thermal loads in the final steady state" (see page 2-2) but very little basis and proof are offered for such positions. A few examples are given below to show that it might not be the case:

1. The proposed long term pool configuration depicted in Figure 1.2 shows an oxidic pool surrounded by an oxidic crust with a metallic layer above it. According to the report, most of the metallic layer comes from the melt out of stainless steel structures in the lower plenum and, during the heat up transient, the steel must rise through the oxidic pool before reaching the top layer.

Not true. One can see in Table 7.2 that the lower supports (i.e., the lower plenum steel) amount to only 2 tons. This quantity is minuscule compared to the 65 tons from the reflector and the core support plate.

10. The temperature of the steel because of its high conductivity will approach that of the oxidic pool and during the melt out phase of the lower plenum it would be superheated and could reach temperatures above 2900 K. Such using superheated molten material will have several negative impacts, including:

(a) As it reaches the vessel, it could lead to CHF conditions on the outside surface of the vessel.

(b) It could lead to failure of the vessel wall because superheated metallic material will attack and erode the vessel at an accelerated rate.

(e) It would not allow the formation of a top oxidic pool crust as depicted in Figure 2.1.

(d) It would radiate to top structural components and cause their melt and failure. Such top components would fall within the pool and disturb the natural circulation patterns as well as possibly produce cracking of the crust layer separating the oxidic pool from the reactor vessel.

In this reviewer's opinion, failure of the reactor vessel during this transient heat-up by the metallic layer or other causes (e.g. falling components) may be a dominant mode of failure and it is not considered in DOE/ID-1046 at the present time.

All these are highly speculative situations given the small quantity of lower plenum steel, as described in the previous passage. The effects of falling masses into the pool would be to mix it up and to cause a temporary reduction in superheat. The impact of a uniform temperature pool has already been accounted for in the mini-ACOPO tests, and ignoring the temporary reduction in superheat is clearly conservative. Note, however, that the two effects would be mutually counteractive.

11. 2. On the top of page 2-2 it is noted that the report is restricted to "scenarios in which failure to supply coolant into the reactor vessel persists indefinitely". On page 1-3 it is stated that "energetic interactions concerning late water injection are relatively benign due to the prevailing stratified configuration" and the "integrity in the early potentially energetic, steam explosion regime, can be assessed against the full lower head capability". Addition of water on top *of a rising superheated metallic layer will not be benign and may approach the steam explosion regime particularly if it contains between 10 to 65 percent by weight of molten zirconium (see Table 1.2). It will not be benign even with a stratified layer. Furthermore, before such energetic interactions occur the reactor vessel wall would be thinned down by impingement of a molten jet and by erosion by the hot oxides and metals and the full structural capability would not be available.*

The quotes given are out of context. This report addresses retention in the absence of water in the reactor vessel. Fuel coolant interactions potentially arising from late water addition will be assessed in a separate report, as noted already. In the introduction of the present report we only tried to lay out the overall approach, and draw the distinction between the energetics from the relocating event (premixed explosion) and the potential interaction event from late water addition (stratified configuration). There is little value in speculating about the outcome, before the work is even presented.

12. 3. On page 3-3,it is stated that "partially flooded conditions are of limited interest, as discussed in Appendix M". In fact, in Appendix M, it is reported that "the PRA concludes that flooding was unsuccessful in 20% of the core damage cases" and this is high enough to justify dealing with a partially flooded reactor vessel. Under such conditions, the radiation would decrease to the vessel walls but it would increase to the top components and enhance their chance to fail and participate in the scenario. Also, there would be a sharp discontinuity in the vessel wall temperature much closer to the top of the metallic layer. Finally, the degree of water subcooling outside the reactor vessel would be lowered and so will the CHF condition.

Yes, but the report also states that flooding reliability will increase to meet screening levels as needed, if the accident management scheme is to be taken seriously. This was done (see updated Appendix M).

13. 4. There is no reason not to expect the partial melting material configuration depicted in Figure 2.1 to progress to that of the complete meltdown of Figure 2.2. If this is the case, the melt impingement produced by partial meltdown could erode the reactor vessel steel by as much as 12 to 14 centimeters (see page H-7). The corresponding weakening of the reactor vessel is not considered in the Structural Section 4.

Again, materials from different parts of the report are taken, and combined, out of context. The assessments in Chapter 8 and Appendix H are for massive releases of already accumulated melt quantities. This applies to the initial relocation event following meltthrough of the core reflector and core barrel. The configuration in Figure 2.1 will evolve to that of Figure 2.2, of course (this is why we put it there!), if the accident is allowed to proceed without water addition (as assumed here); however, as explained already, any subsequent relocations will be gradual and subject to decay power limitations. It is completely erroneous to apply the results of Chapter 8 (and Appendix H) to this situation.

> *14. It is therefore recommended to reassess the conclusion on page 2-5 that there are only two specific configurations to be considered because they "bound the thermal loads on the lower head with respect to any other intermediate state that can be reasonably be expected". Other configurations, scenarios, and transient intermediate states need to be included and shown to not impact the results.*

This reviewer has not proposed (as discussed above) any scenarios that are not covered already by those in the report. Other reviewers did, and they are discussed in Appendix O. No impact on our conclusions resulted from these additional considerations.

> *15. C. <u>Overstylized Pool Configuration</u>*
>
> *The pool configurations shown in Figures 1.2 and 3.1 are very stylized and some of the presumed simplifications are expected to impact the predictions in DOE/ID-1046:*
>
> *1. The pool configurations and the heat transfer results are predicated on the existence of a crust (or solid interface) separating the oxide pool from the metallic layer. As noted under comment B.1, there can be no crust as the molten material from the bottom stainless steel structure rises through the oxidic pool. Even under long term conditions, it is difficult to visualize how a strong crust could form "naturally" above the oxidic pool and support 67 to 72 tons of metallic material over the large reactor vessel span of the AP600. Without a crust/solid interface the heat transfer at that surface could be higher because there would be a wavy interface produced by two counter flowing fluids. Also, the temperature at that location would be higher and above the specified oxidic components liquidus temperature of 2973 K and so will the bulk metallic layer temperature.*

The crust is formed upon contact, and really represents a thermal boundary condition between the two liquids. In particular, it *does not* have to carry *any* loads—it simply transmits them to the liquid below. The report shows that the formation of such a crust is inevitable. Much of the reviewer's difficulty derives (as evidenced in Lev9 above) from visualized large quantities of steel melting within a UO_2 pool in the lower plenum and producing a counter-current flow situation. As indicated in our response to Lev9, this is not true. Moreover, in the new Appendix O, we show that the major metallic components are added on the top of a substantial in size oxidic pool.

> *16. 2. A single molten bulk temperature is used in the oxidic pool and the metallic layer. Physically, one can expect stratification vertically and radially in the oxidic and metallic pools. The temperatures should rise away from the cooled walls in the radial direction. Also, vertical stratification due to gravity will lead to increased temperatures vertically. Such maldistribution of temperatures can be expected to have an impact upon crust formation, natural circulation currents and upwards and downwards split in heat transfer. For example, with a reduced temperature towards the bottom of the vessel, the viscosity will increase (particularly if some solids become present) and the downwards heat transfer will drop. In contrast, the upwards heat transfer will rise which tends to strengthen the reviewees concern about reactor vessel failure at the oxidic-metallic interface or above it.*

In this passage, the reviewer tries to predict a very complex heat transfer program with words. And the statements made are contrary to the experiments and analyses presented in the report. There are also factual errors that misrepresent what was shown in the report. Consider the following:

(a) The thermal structure of the oxidic pool is anything but uniform. It was measured experimentally, discussed extensively, and modelled as such (see Appendix D).

(b) The lateral gradients in the metal layer were also discussed and investigated experimentally (see Appendix N). In the model they were ignored, because we had no reliable way to account for them. It is obvious that this is conservative.

(c) There cannot be vertical stratification in the metallic pool. It is heated from below and cooled from above!

(d) We find that the oxidic pool near the top is superheated by $\sim$150 °C. Solids precipitation is not expected until the temperatures drop below the liquidus, and this occurs only within the thermal boundary layer all around the pool boundary. Moreover, in volumetrically heated pools there cannot be any stagnant regions that are significant in size.

> *17. 3. The report considers only two phase diagrams: an uranium dioxide (UO_2) – Zr oxide (ZrO_2) phase diagram and an iron (Fe) – zirconium (Zr) phase*

diagram. According to NUREG/CR-5869, several Zr, stainless steel (SS), UO_2, and ZrO_2 eutectics were formed in melting experiments at Oak Ridge in 1987 (Nucl. Eng. Des., 121, 324-337, 1990) and they are listed in Table 18.3 in Attachment 1 [[please see original letter]] *taken from NUREG/CR-5869. Furthennore, there can be a large number of other material species involved as illustrated from Table 18.4 in Attachment 1 for a BWR bottom pool. They come from the species present in stainless and control rod materials which are also present in the AP600. It is also worth noting from Table 18.3 in Attachment 1 that the Zr-SS eutectic has a melting temperature of 1723 K (150 K above the metallic melting point of iron-zirconium used in DOE/ID-1046). There is also a strong possibility for the formation of a Zr-SS-UO_2 eutectic with a melting point of 1873 K (300 K above the metallic melting point used in DOE/ID-1046). This eutectic has the added complication of being able to produce some initial heat generation. There is no question that the phase diagrams in the reactor case will be much more complicated than those in the presumed overstylized pool and the presence of additional eutectic mixtures with higher liquidus temperatures and their potential formation of solid particles must be recognized.*

As with every real problem one can find complexities forever. The real question is: What is important to the conclusions? First, Table 18.4 is irrelevant, as our interest here is a PWR. Second, the Zr-SS eutectic cited indicates that we may be 150 K conservative in our treatment of the metallic layer. The impact is that for the limiting flux considered in Chapter 4 we would have a $\sim$1 cm thicker wall. The margins to structural failure for the CHF failure criteria are so great that this increment is really of no consequence. The Zr-SS-UO_2 eutectic is irrelevant in the presence of the Zr-SS one. Thus, we find all these "complications" really of no interest to the problem at hand.

18. D. <u>Natural Convection in Oxidic Pool</u>

DOE/ID-1046 relies upon pool natural convection correlations and the mini-ACOPO data to predict the heat transfer in the oxidic pool. There remain several concerns about this approach:

1. Some concerns yet to be resolved are listed in the report:

(a) Timewise variation of the stratification pattern within the pool (see page 5-10) and the relationship of the final, truly steady state to the sequence of transient states leading up to it (see page 5-3).

These are incredibly out of context citations! Pages 5-3 and 5-10 present introductory material, leading up to the genesis of the ACOPO experiment. They provide the rationale for the need for the experiment. Just in case this could be missed, on page 5-11 we begin with: "The mini-ACOPO experiment mentioned above (and described in detail in Appendix D) was built and operated to address this different set of issues as well."

> *19. (b) Dependence upon Prandtl number. All the data in the report have been taken at Prandtl number of 7 (Kulacki-Emara, Jahn and Reinecke, and Steinberger-Reinecke), at a Prandtl number of 8 (UCLA) at Prandtl numbers of 2.6 to 10.8 (mini-ACOPO). The Prandtl numbers are higher than those anticipated in the reactor case. In 1955, the reviewer used integral methods to predict natural convection flows (see Attachment 2) and it was clearly shown that for laminar flow the Nusselt number was dependent upon the Grashof number times the square of the Prandtl number for low Prandtl numbers instead of the Grashof number times the Prandtl number. Also, there was an extra dependence found upon the Prandtl number in turbulent flow (this is also true in Eq. (5.39).*

(a) Attachment #2 makes use of velocity/temperature profiles introduced by Eckert and Jackson (E.R.G. Eckert ad T.W. Jackson, NACA TN 1015, 1950). The result then is also the same, predicting a 2/5th power law on the Ra number, in contradiction to experimental data that exhibit a 1/3 power law (i.e., Churchill and Chu, 1975). It is now understood that this discrepancy is due to the poor choice of the temperature profile (R. Cheesewright, ASME Journal of Heat Transfer, Vol. 90, 1968). Consistent results with experiments can be obtained by recognizing the existence of a two-layer boundary layer structure, as discussed by George (W.K. George, Jr., Proceedings of the 6th International Heat Transfer Conference, Toronto, 1978, pp. 1-6) and George and Capp (W.K. George, Jr. and S.P. Capp, International Journal of Heat and Mass Transfer, Vol. 22, 1979, pp. 813-826).

(b) Laminar flow is of no interest here

(c) The Pr numbers of interest to our problems are 0.13 and 0.6, for the metal and oxide pools respectively, and these are quite close to normal fluids, as compared to those usually referred to as "low" Pr number fluids ($Pr < 10^{-3}$).

(d) The key aspect of the behavior in low Pr number fluids is that the thermal boundary layer extends well beyond the hydrodynamic boundary layer. In reviewer's Attachment 2, we find that this key aspect was ignored, by assuming that the two boundary layers are of equal thickness. This alone would be sufficient to explain the erroneous trend predicted by the equation in Attachment #2.

(e) Finally, coming to the case with volumetric heating (the oxidic pool), the reviewer is really remiss in not recognizing that ours was the first serious attempt to raise and look into this "extra" Pr number issue. In fact, we believe a value of 0.6 is close enough to unity, and together with the data that show no measurable effect over a four-fold of change (2.6 to 10.8), leaves little doubt about the validity of our formulation.

20. (c) There is considerable scatter among the available data. This is illustrated in Figures 5.7 and 5.8. The scatter certainly exceeds the "30% discrepancy which could be potentially rather significant to our conclusions due to the importance of the upwards heat flux on the behavior of the steel layer" noted on page 5-6. Similarly, the exponent on the Rayleigh number exhibits considerable variation. This becomes all the more important at the very high Rayleigh numbers anticipated in the oxidic pool.

Figure 5.7 contains results of only two experiments, and only 2 points from one of them (the UCLA one). The bounds shown around the line for run A16 are ±15%. We cannot interpret this as "considerable scatter". Mayinger's line was based on a numerical model, and so are the points shown for Kelkar et al. Same thing on Figure 5.8. In this figure the only non-negligible discrepancy is at the high angles between UCLA and mini-ACOPO. This was discussed in detail in the report. The parametric and sensitivity studies in Chapter 7 more than amply cover any perceived uncertainties from these figures.

21. Here again, it is worth noting that Attachment 2 shows that the Grashof and Rayleigh number exponent varies for a laminar boundary layer from 0.2 for a horizontal plate facing upwards to 0.25 for a vertical plate which explains the range of exponents shown in Eqs. (5.10) to (5.17),(5.19),(5.20), and (5.22) and (5.23). In the case of turbulent flow along the entire boundary layer, the exponent on the Grashof number according to Attachment 2 is found to vary from 0.36 for a horizontal plate facing upwards to 0.4 for a vertical plate. These turbulent predictions give partial support to the exponents in Eqs. (5.27) and (5.28), particularly if one takes into account the initial buildup of a laminar boundary layer. Also, the change in behavior observed in the mini-ACOPO data at a Rayleigh number of $3(10^{13})$ may be due to a local transition from laminar to turbulent flow.

Given what went into the formulation in Attachment 2 we cannot see how it can be applied to plate orientations that are near horizontal. Boundary layer separation (or "lifting") is not even mentioned in Attachment 2, while boundary layer separation (or "lifting") is the dominant mechanism in horizontal plate configurations. We do not agree with the interpretations offered here.

> 22. *2. All the tests have been performed with a pool completely liquid and with small temperature differences from the bulk to the heat transfer surface. The use of a film temperature to calculate the heat transfer is questionable, particularly in view of the large temperature differences expected in the reactor core, the great number of eutectics, and the presence of solids discussed under comment C.3.*

We raised the issue of the magnitude of $(T_{max} - T_i)$ and of the effective "film" temperature at the bottom of p. 5-3, and addressed it by the mini-ACOPO experiment that attained ΔT's up to ~100 °C, which is quite comparable to the reactor values of 90 to 160 °C. Remarkably, the reviewer presents the first half of it only, here, as a criticism.

> 23. *It is hoped that the ACOPO experiments being performed presently will help resolve some of the concerns noted above. However, it is important to note that the ACOPO tests are non prototypic of the reactor case because they cannot account for the presence of several eutectics and their solidification at different temperatures or for a metallic layer in direct contact with the oxidic pool.*

* *

> 24. *E. DOE/ID-1046 relies upon the Globe and Dropkin correlation to predict the heat transfer within the metallic laver. This correlation was supplemented by the use of a Churchill and Chu correlation to predict the heat transfer on the vertical wall of the metallic layer. The combination was justified by a simple simulant experiment (MELAD) described in Appendix N. Several concerns with this approach have already been noted and they are reproduced here for completeness purposes:*

* *

> 25. *1. There will be no crust between the metallic layer and the oxidic pool. There will be direct contact between these two fluids at a wavy interface and the rates of heat transfer will be different and higher from those obtained from the Globe and Dropkin correlation.*

Wrong. See above.

26. 2. In order to take into account conduction within the fluid the Globe and Dropkin should be modified by adding 1.0 to the right hand side of Eq. (5.34).

As shown just below Eq. (5.34) the data range for Globe-Dropkin is for $0.02 < \mathrm{Pr} < 8750$.

27. 3. The Churchill and Chu correlation does not agree with the equations proposed in Attachment 2 and this may deserve further examination.

Equation (5.39) is based on an extensive data base from many sources. Perhaps Attachment 2 needs further examination, but not by us (see for example our response to item #19).

28. 4. The use of film temperature is questionable again particularly close to the metallic layer-oxidic pool interface where the wavy interface could produce a much higher and oscillating temperature.

This presupposes 1 above, which is incorrect. Incidentally, the presence of waves by no means invalidates the use (or need for) a film temperature.

29. 5. The energy balance equation (5.43) lacks a radiation term to account for reflected energy from the receiving surfaces. The right hand side of the equation should have a negative term which contains the emissivity of the receiving surface and its absolute temperature raised to the fourth power. This term could have a significant impact on the results presented in DOE/ID-1046.

See assumption #4 just above the equation. The complete model, including back radiation, is described in Chapter 6. This complete model is used in the calculation of Chapter 7. The simplification in Chapter 5 was made to obtain the universal solution shown in Figures 5.9 through 5.12. Comparisons with the full model, in Chapter 6, shows that the error due to this and the other three assumptions listed is negligible.

30. F. Other Comments

1. The reviewer spent little time on the structural aspects of the report except to note that:

(a) An impulse methodology is utilized in Figure 1.1 to determine the potential for the structural failures. As mentioned on the top of page 1-4, "this is illustrative of global considerations; the actual assessment is likely to require

additional details, such as the space-time distribution of the loads" as well as the space distribution of vessel wall thickness and temperature.

(b) There will be discontinuities in vessel wall thickness and temperatures due to the initial melt impingement on the bottom reactor vessel head (see B.4) or due to different erosion rates at the oxidic pool-metallic layer interface, or due to partially flooding the reactor vessel. Stress concentration factors need to be applied to take such discontinuities into account.

The study will consider whatever conditions are appropriate and important.

31. 2. The thermophysical properties derived in Appendix L utilize iron (Fe) rather than stainless steel. Stainless steel has about half the thermal conductivity of iron and similar variations are expected for other properties. This needs to be corrected.

The vessel wall is not stainless, and the proper thermal conductivity for it was utilized. Internal components are all stainless, but for these the melt properties are of interest, and they are dominated by the properties of iron. The sensitivity of thermal conductivity of various iron alloys to composition is due to solid state microstructural effects.

32. Also, as noted under comment C.3, stainless steel zirconium and $U0_2$ can form several eutectics with higher melting temperatures. With the anticipated weight percent of Zirconium (10 to 65 percent), it is not clear why the Zr-SS-UO_2 (0.3/0.6/0.1) eutectic would not play a dominant role and possibly produce a multilayered configuration.

See above response to item #17.

33. 3. An important assumption made in DOE/ID-1046 is that the heat generation is uniform and confined to the oxidic pool. With the suggested stratification and temperature maldistribution discussed in comment C.2, it is anticipated that $U0_2$ will tend to favor the upwards portion of the pool and that the heat generation per unit volume could be much higher in that region. Also, note that the SS-Zr-UO_2, eutectic could be present in the metallic layer and provide some limited heat generation.

This presupposes comment C.2, which is incorrect (see response to item #16). A sensitivity on the fraction of decay heat deposited directly into the metal layer can be found in Appendix P.

34. 4. In Table 7.3 which tabulates the accidents contributing to the AP600 core damage frequency (CDF) from a Level I Probabilistic Risk Assessment (PRA), vessel rupture is shown to account for 23 percent of CDF and it is considered not relevant to in-vessel retention (IVR). This is not fully correct because it means that IVR cannot be effective on 23% of the accidents contributing to the CDF.

This is exactly what we mean—if the vessel fails there cannot be in-vessel retention. The reason vessel failures show as significant contributors is the increased reliability of the passive AP600 design to prevent severe accidents. Moreover, Table 7.3 has been updated according to the revised PRA submitted to the NRC on March 1995. In the update vessel rupture was given more attention based on its earlier relative importance. The contribution of accident sequences lumped into this class is now estimated at 4.1% (of a total core melt frequency of $2.5 \cdot 10^{-7}$). This value should still be conservative, but such improbable events are difficult to quantify in any case.

35. 5. I continue to remain confused by the use of the Risk Oriented Accident Analysis Methodology and the judgments used to formulate probability density functions in Section 7 but I have decided to defer on this topic to other reviewers more familiar with probabilistic and risk assessment techniques.

* *

36. 6. It is recommended that the report title be limited to the specific case of the AP6OO concept. It requires depressurization of the vessel, its lowerhead to be fully submerged and a low power density. The combination of such characteristics is found today in only the AP600.

The data and techniques can be, and they are being, used for other reactor designs. The AP600 is just the first illustration.

37. In summary, at the present time I cannot support the conclusion on top of Section 9 that "thermally-induced failure of an externally flooded AP600-like reactor vessel is physically unreasonable". There is no question that the chances of in-vessel retention have been improved but the conclusion that failure is physically unreasonable will require dealing with the comments provided herein and particularly with the need of prototypic CHF tests and natural circulation tests with prototypic corium and metallic pools.

We found nothing in the above comments that would alter our conclusions. Also, it is not clear to us what is meant by "There is no question that the chances of in-vessel retention have been improved". Improved compared to what?

38. I hope that these comments are useful to you and I appreciate the opportunity to participate in the review. Before closing, let me reiterate that my negative comments are not meant in any way to detract from the progress made about in-vessel retention by the investigators participating in DOE/ID-1046.

* *

T.8. Response to F. Mayinger U. Munich — Specific Assignment: Ch. 5

General Comment and Highlights

We deeply appreciate the favorable remarks and insights offered by this reviewer. We agree completely with his comments and contributions to the issues discussed in the report, based on his own work.

Point-by-Point Responses

1. *Introduction*

In spite of the experience from the TMI-accident, where several tons of core melt were retained coolable in the lower plenum of the pressure vessel, most of the severe accident studies assume that the melt penetrates the pressure vessel and the only way of retention would be a core catcher, integrated into the concrete of the containment. In the face of this opinion of many specialists, it is a great service to a realistic assessment of nuclear reactor safety, that the U.S. Department of Energy initiated and sponsored a study on In-Vessel Coolability and Retention of a Core Melt, which was performed by T.G. Theofanous and co-workers and which is subject of review here.

The study was concentrated on the future concept of the AP600-nuclear power plant, however many general conclusions can be drawn for other types, also for nuclear reactors being in operation. Therefore the report deserves general consideration in the nuclear community.

The capability of the pressure vessel to retain the molten core is a function of the heat transfer coefficient at the inner side of the pressure vessel wall (between corium and wall), of the heat conductivity in the material of the wall and the heat transfer at the outer side of the wall,(between wall and boiling water). In case, that the heat conduction in the wall would be the limiting parameter one has to check, whether in a melting attack, the wall thickness is so much reduced, that it cannot carry the weight of the molten core any more, even being supported by the buoyancy force of the surrounding water-vapour mixture.

⋆ ⋆

2. *Heat transfer between corium and wall of the pressure vessel*

The heat transfer between the corium and the wall, as well as the fluiddynamic conditions in the corium, which consists of an oxidic pool and an overlaying *metallic layer, were very carefully studied in the report and the results are clearly presented in chapter 5. The authors compared own measurements with experimental and theoretical data from the literature and found agreement to such an extend, that they were able to predict the Nusselt-number for the heat transfer between the oxidic pool and the wall as average value, as well as in the form of local data versus the circumference of the lower hemisphere of the pressure vessel. Especially at high Rayleigh-numbers (in the order of 10^{15}), which are representative for the situation in a real molten pool, the agreement of the data is good, which means that the heat transfer coefficient can be reliably predicted.*

The temperature in the oxidic pool, however, is not only a function of the heat sources and the heat transfer from the melt to the wall, but it is also influenced by the metallic layer, which is superimposed to it. In the metallic layer the density of the heat production by decay heat is much smaller, than that in the oxidic pool. Therefore in a first approximation it was assumed in the report, that pure Benard convection exists, which has a different flow pattern from that of the convection with inner heat sources.

The fluiddynamic behaviour and the heat transfer in a cavity with Benard conditions and the heat transfer to the wall of rectangular cavities are well studied and also understood in the literature. The authors compared data from the literature and by assuming, that the convection in the metallic layer with its cylindrical surroundings can be treated like that in an rectangular cavity, they could derive reliable data for solving their problem. The simplification in the assumption for the geometry can be certainly justified.

If the layer is of pure metallic nature, then one can certainly assume, that there are no or at least neglectable heat sources in it. It is a metallurgical question, whether there could be dissolved some $U0_2$ in this metallic liquid. Then the situation would be a little more complicated to handle it.

There is a report in the literature, dealing with the thermal interaction between a lower oxidic pool and an upper metallic layer/1/, which however is a little hidden, because it can be only purchased from the "Gesellschaft für Reaktorsicherheit" (GRS) at Köln. It is not classified and therefore freely available. In this report Steinberner and Mayinger studied the heat transfer in two layers systems by using the holographic interferometry. In Fig. I an example of the interferograms *measured in the two layers are presented. This figure is taken from the above mentioned report.*

The aim of these experiments was to study the heat transfer at the phase-interface between the two layers and also the heat loss at the upper free surface of the metallic layer. From this one gets the temperature in the metallic layer.

The temperature distribution in both layers is a strong function of the heat transport from the oxidic to the metallic melt and of the heat transfer at the metallic surface. Of course in addition the heat sources in both layers play an important role. Fig. 2 shows three characteristic cases for the temperature distribution in these layers. The dotted lines in this figure represent the temperature distribution, if no heat transfer between the layers would exist.

For the case, that there are no heat Sources in the upper metallic layer, Steinberner and Mayinger/1/ developed simple correlations for predicting the heat flux from the lower boundary of the oxidic pool to the wall of the cavity. When the density of the heat source is given and the Rayleigh-numbers are known. These correlations have the following form.

$$(1-\eta)^{0.87} = 0.172 \cdot Ra'^{0.226}(\eta^2 - 2\theta) \tag{1}$$

$$\eta = -\frac{0.104 \cdot \left[-\frac{1}{0.172\ Ra'^{0.226}} - 2 \cdot \theta\right]^{1.305}}{2.47 \cdot\ Ra'^{(-0.305)} + 0.104 \cdot \left[-\frac{1}{0.172 \cdot Ra'^{0.226}} - 2 \cdot \theta\right]^{1.305}} \tag{2}$$

The symbols in these correlations are defined as follows:

$$\begin{aligned} \eta &= \frac{q_u}{q_i \cdot L} \\ \mathrm{Ra}' &= \frac{g \cdot \beta \cdot q_i \cdot L^5}{\nu \cdot a \cdot \lambda} \\ \theta &= \frac{(T_o - T_u) \cdot \lambda}{q_i L^2} = \frac{Ra_E}{Ra'} \\ Ra_E &= \frac{g \cdot \beta \cdot (T_o - T_u) \cdot L^3}{\nu \cdot a} \end{aligned} \tag{3}$$

Equations (1) and (2) go back to a proposal by Baker et. al./2/ and contain also ideas, which Kulacki et. al. proposed in /3/. The solution of these equation is presented in Fig. 3 in a graphical form.

Please note, that the equations (1) and (2) and the results in Fig. 3 were elaborated for horizontal fluid layers with a flat bottom. They cannot give information about the heat flux at the side wall (90°) of a spherical bottom of a Pressure vessel containing two layers of fluid, the lower one with and the upper one without internal heat sources.

In Fig. 3 and in the equations, being the basis of this Fig. q_u *stands for the heat flux density at the flat bottom of a cavity and* $q_{i\ell}$ *represents the heat source density in the heated fluid layer. The detailed derivation of the equation (1) and (2) can be found in /1/.*

The heat from the upper surface of the metallic layer is transported by radiation mainly. Radiative heat transfer is a strong function of the temperature (T^4) *and one has to also take into account the heat, which is reflected or radiated from the top of the pressure vessel to the metallic layer. This heat exchange strongly influences the temperature in both layers, the metallic and the oxidic one. With very high temperatures of the fluids the wall of the pressure vessel may start to melt (especially at the side-parts) instead of forming an insulating crust as partially assumed in the DOE-report.*

The authors of the DOE-report deliberately do not take into account the very first period of the pool-convection, when the jet of the flowing down melt penetrates the fluid layer and is impinging onto the bottom of the pressure vessel. They argue, that the period of filling up the lower plenum of the vessel is short compared to the time, when the molten pool is exposed to purely free convection. This statement is certainly correct.

There is another argument for this assumption of the authors. As Steinberner/4/ proved in his Ph.D. Thesis, the Nusselt-number at the impinging point of the jet is usually similar or smaller, than that one, which exists at the side wall (90°) with free convection, driven by internal heat sources. Only with very low pool heights these Nusselt-numbers are higher than those at the side wall. Fig. 4, taken from Steinberners work, shows the boundary conditions at a different pool height, and also the relative Nusselt-numbers. Low pool heights exist only for a short time, when the melt-down process starts. In most accident- cases water would be still present in the lower plenum of the vessel during this very first period, which changes the situation completely and which produces a preliminary quenching of the melt.

In Fig. 4 also interferograms of the temperature distribution in the pool during jet impingement are presented. The black and white fringes can be read as isotherms.

This difference in the pattern of the isotherms between free convection and under jet conditions can be clearly seen in Fig. 5, where the upper interferogram gives the situation without and the lower one with an impinging jet. Comparing the boundary layer at the impinging point and at the 90° position, one realizes, that the temperature gradient and by this the heat flux are similar, which can be deduced from the densely packed pattern of the isotherms.

So generally speaking one can draw the conclusion, that chapter 5 of the DOE-report precisely and reliably describes the heat transfer from a molten pool — with and without internal heat sources — to a spherical and cylindrical wall. The results presented there are a very good basis for analysing possibilities of retention of a core melt.

* *

3. Heat conduction in the wall of the vessel

To calculate heat conduction in a solid wall is a very simple task, if the transport properties — especially thermal conductivity — are given at the relevant temperatures and if the boundary conditions — heat transfer coefficience and temperatures — are known. There is enough information in the literature and also in the DOE-report about the transport properties. However the boundary conditions at the outer and the inner side of the wall are more complicated to handle.

The heat transfer coefficient at the inner side of the wall is very well described in chapter 5 of the DOE-report, as already mentioned. Also the heat transfer at the outer side or the guarantee, that DNB will not be exceeded, is well documented in the report, as discussed a little later. An open question seems to be, whether at the inner side of the vessel also at the positions of highly convective flow (90°), a crust is formed or whether the material of the wall is eroded by the hot melt. The report presents data on the thermal conductivity of the steel up to 1500 K (appendix L) and also deals with creep considerations for the lower head (appendix G).

Including all the other informations in the DOE-report, it is possible to describe the stress in the wall of the lower head during meltdown and during the free *convection of the melt. To do this one needs a small computer code, correlating the feedback control between boundary conditions at the inner and at the outer side of the wall, the heat conduction of the wall and the wall thickness. There are some deliberations in the report about this subject, however I missed detailed calculations of this problem.*

A very simple estimation may demonstrate this subject. Let us assume, that the temperature at the outside of the wall is 373 K (nucleate boiling) and that the temperature on the inner side must not exceed 1400 K, then the thermal conductivity varies between 40 and 30 W/Km, with a minimum of 25 W/Km at 1100 K, as can be seen in Fig. L-3 (page L-21 of the report). Furthermore we take a heat flux of 500 kW/m^2 *from the melt to the side wall. With a very simple application of Fourier's law, we then end up at a maximum wall thickness of 6 cm for these assumptions.*

A parametric study of the temperature situation in the wall at various boundary conditions would still give more confidence to the final and certainly correct conclusion of the DOE-report, namely, that the pressure vessel of the AP 600 can retain a pool of molten core, just by flooding the cavity between the pressure vessel and the shielding concrete.

In the calculations discussed in Chapter 7, we accounted for the temperature variation of thermal conductivity, as given in Appendix L, and allowed for wall melting as dictated by the local heat flux (see Eq. (6.6) and Eq. (6.7) and the next few lines of text that follow). The reviewer is correct that we failed to emphasize these, which, to a large degree, is due to the abbreviated reporting of the results in Chapter 7 of the report. To remedy this we have added a new Appendix Q that gives detailed results for all the calculated parameters, including wall thicknesses, for the base case and the most limiting parametric case examined. Also, following Eq. (6.6), we have indicated that the wall thermal conductivity used is the effective value accounting for temperature dependence. Because in the case of inner wall melting the inner and outer temperatures are fixed, it is possible to come up with a single effective value, applicable to all calculations. This is true approximately only when there is no melting, but these are uninteresting regions from the point of view of potential failure.

4. <u>Heat transfer from the wall to the flooding water</u>

Very sophisticated and detailed experiments are reported in the DOE-report, dealing with the subject of critical heat flux at facing-down surfaces and at vertical *walls with free convective bubbly flow. This experimental data, together with the nice experiments on free convection heat transfer at the inner side of the vessel wall, proof very reliably, that a safety margin with a factor of 2 exists against critical heat flux, even at positions with very high thermal loads. So one can be sure, that the heat transfer from the wall to the water is negotiated by nucleate boiling, which has very high heat transfer coefficients, as is well known. This means, that the temperature difference between the outer side of the wall and the bulk of the water is very small—in the order of a few Kelvin.*

* *

5. Conclusion

The report DOE/ID-10460 is a very fine and reliable document on the coolability of a core melt in the pressure vessel of a medium-sized nuclear reactor and proofes, that a hypothetical core melt situation can be managed and that the debris can be safely retained in the pressure vessel.

Questions rising in connection with that problem are carefully discussed and satisfactory answers are given to all issues, being linked with the thermo- and fluiddynamic phenomena under core melt conditions. The report makes a great and very valuable step forward in the risk assessment of nuclear power plants, especially of nuclear reactors of future design. I would like to congratulate the authors to their work.

A few minor additions to the report – as mentioned in this review – would probably be of interest to the reader, who is not an expert in heat transfer and could improve the value of the report still more.

* *

T.9. Response to R.E. Nickell (AST) — Specific Assignment: Ch. 4

General Comment and Highlights

The main thrust of this review is to express a concern, namely, the possibility of ductile tearing due to longitudinal bending stresses, especially due to "discontinuities" of mechanical loading and thermal conditions along the longitude of the lower head. In fact, we considered this failure mechanism and discarded it as one of secondary importance. We do recognize, however, that its explicit consideration in the report is appropriate, and it has, therefore, been included as an addendum to Chapter 4. In doing so, we also took up some of the more detailed suggestions made by the reviewer, and we trust he will find our response complete and satisfactory.

Point-by-Point Response

1. *Thank you for the opportunity to review and comment on the referenced report (Reference 1). Specifically with respect to the experimental and analytical investigations of ex-vessel heat transfer phenomena for a submerged reactor vessel lower head following a severe (core melt) event, this report is very comprehensive. An excellent case is made for the bounding values of heat flux through the vessel wall. (These heat flux limits are referred to erroneously as "thermal loads" in the report, a term that should be reserved for the product of thermal expansion and structural stiffness.) However, my assignment was to review the structural implications of the report, concentrating on Chapter 4 (Structural Failure) and Appendix G (Creep Considerations for the Lower Head). Other portions of the report were examined for context. My comments on the structural sections of the report are provided below.*

We disagree that our term "thermal loads" is erroneous as applied in the report. As explained in the report, heat fluxes above certain limits can cause failure by a mechanism quite distinct from those due to thermally-induced stresses discussed in Chapter 4 and Appendix G. Thus, we have a generalized "load," and a usage quite common in thermohydraulics. Note that to distinguish between the two we use "thermal loads" versus "thermal stresses."

2. *Chapter 4 contains an argument that the vessel lower head, in the submerged condition, will not fail absent a boiling crisis on or near its external surface. The structural failure criterion is not given explicitly but, from a close examination of the argument, appears to be based on a tensile membrane stress limit equal to the yield strength of the vessel material at an appropriate metal temperature. At the bottom of page 4-1, the required membrane wall thickness*

of 0.15 mm, when multiplied by a tensile yield strength of 355 Mpa and a vessel circumference of about 12 meters, gives a membrane resultant force of 71 tons. This required wall thickness is then compared to a minimum wall thickness of 1.1 cm that is kept sufficiently cool by the convective heat transfer in the external pool to maintain its strength.

It is crucial to remember that the condition of heat flux examined here is a very high value that corresponds to the thermal failure criteria, as explained in Chapter 3. In Chapter 7 we find that there is a thermal margin of $\sim$100%, which means that the 1.1 cm value considered here has an *additional* 100% margin to structural failure in the membrane stress mode.

3. This argument is intended to address the stresses due to dead weight less buoyancy forces from displaced water in the pool, with the dead weight inclusive of the weight of the core melt that accumulates at the bottom of the head. The thermal expansion stresses due to temperature gradients across the vessel wall are treated in a similar, simplified manner by recognizing the longitudinal bending stress caused by the gradient (and the differential thermal expansion), but then limiting the discussion of the compressive (inside) and tensile (outside) bending stresses to regions away from any geometric or loading discontinuities.

The simple analysis was provided to make the membrane stress argument quite transparent. Actually, due to axial conduction in the vessel wall, regions of discontinuity do not exist as such. A complete analysis is given in an addendum to Chapter 4.

4. These stresses were not identified in the report as longitudinal bending stresses, and this omission is unfortunate. The report also does not discuss longitudinal bending that might be caused by either a non-uniform distribution of the core melt weight, nor is the effect of non-uniform buoyancy force considered. A stress analyst would expect the deformation of the bottom head and cylindrical side wall to be non-uniform in the radial direction, reflecting the non-uniform distribution of weight, temperature, and buoyancy force, let alone the geometric discontinuity represented by the changes in curvature at the junction between the spherical lower head and the cylindrical side wall. The vessel would be expected to "pinch in" at some points around the longitude, relative to the outward radial motion elsewhere. This does not mean that the net radial displacement would be inward; it means that some portions of the vessel would have greater radial displacement than the inner surface of the vessel. One might suspect that one *location of reversed curvature would be at the very bottom of the head, as the result of slightly greater buoyancy forces that cause the head to "dent." Another possibility is at the junction between the head and the cylindrical shell where the meridional curvature changes.*

The finite element model shown in Figure 4.5 could be used to study these longitudinal bending effects, provided that the mesh layout in the radial direction (across the shell thickness) is sufficient bending stiffness, in addition to membrane stiffness.

Following the suggestions made here, we carried out additional finite element calculations (with sufficient bending stiffness and distributed loads due to the weight of the melt inside and the buoyancy outside). The results are presented in an addendum to Chapter 4. These demonstrate the existence of significant margins to failure.

5. In an effort to determine whether the longitudinal bending effects would be significant, this reviewer searched the other chapters and appendices of the report for: (1) any discussion on the distribution of dead weight (or distribution of equivalent internal pressure), as a function of the meridional coordinate, θ; (2) distribution of the buoyancy forces, as a function of θ; and (3) distribution of temperature, even for approximately the same gradient, as a function of θ. Some estimates of the variation in temperature are available (see Figure C.6), showing that the temperature at $\theta = 0$ will be lower than that at $\theta = 90$ degrees, with perhaps a 20 to 25 % variation, irrespective of heat flux.

Actually, Figure C.6 is for an experiment, and the temperatures in it have nothing to do with the reactor. The inside wall temperatures in the reactor can be obtained from the local fluxes presented in Chapter 7, and an outside wall temperature of 130 °C (nucleate boiling). Better yet, the fluxes should be imposed in a 2D conduction calculation to obtain the smoothing due to conduction along the wall. This was done in the new calculation that includes the effects of distributed dead weight and buoyancy as already mentioned above. We found these effects to have negligible impact on the results.

6. In order to complete this study with respect to the potential for structural failure of the vessel lower head or cylindrical side wall, the following steps should be taken. **First**, *real structural failure modes and structural failure criteria must be considered. Real structural failure modes include such phenomena as ductile rupture, ductile tearing, brittle fracture, low-cycle fatigue, corrosion fatigue,* *buckling, creep rupture, and creep fatigue. The report currently addresses ductile rupture, on a partial basis, and uses the value of membrane tensile stress (and its comparison to tensile yield strength) as the failure criterion. Ductile tearing at the inside surface of the vessel, caused by reversed longitudinal bending, with either a strain limit or a peak stress limit, would also seem plausible. Creep rupture has been addressed in Appendix G, again for a simplified state of membrane tensile stress. The other failure modes do not apply to this loading and environmental situation.*

We agree with the suggestion that ductile tearing needed further consideration. This was done as described below.

7. **Second**, *in order to determine the probable state of stress and deformation in the vessel as the result of the core melt event, the ABAQUS analysis reported in Chapter 4 should be revisited. The effects of longitudinal bending and potential reversed curvature caused by changes or discontinuities in the geometry or loading should be considered. Of particular importance is the effect of distributing the melt content weight, the temperatures, and the buoyancy resistance in the longitudinal direction. The buoyancy resistance will have an effect similar to a change in vessel stiffness; changes in wall thickness and in radii of curvature will also affect vessel stiffness. The existing ABAQUS model may be too crude, or the applied loadings may have been inappropriate, to detect these longitudinal bending effects.*

Third, *the calculated stresses and strains from any revised ABAQUS model should be subjected to a sensitivity study over a range of temperature distributions, wall thickness changes, etc., in order to scope out the worst case situations Then,* **fourth**, *the stresses and strains for these worst cases can be compared to real failure criteria. A basis for the latter was prepared by Teledyne Engineering Services for the Electric Power Research Institute (EPRI) some years ago, following the TMI-2 event. The relevant pages from Reference 2 are provided as an attachment.*

Done as described in an addendum to Chapter 4. These results show that the thermal failure criterion is, as used in the report, is appropriate.

8. *I hope that these comments are constructive, and will enable the excellent work done to date to be placed in a proper context. Once again, thank you for the opportunity to review and comment on this report.*

* *

T.10. Response to D. R. Olander (UC Berkeley) — Specific Assignment: Chs. 2,3,6,7,8,9

General Comment and Highlights

Four critical points are made in this review. Of these, we found that three are based on physically inconsistent arguments, and speculations that are not born out by the design specification for the AP600. The fourth point brought up has some basis, but turns out not to be important to the conclusions. In summary form here they are:

(a) An additional heat source in the metallic pool due to steam-Zr reaction. Rather than mass transfer rate limited, this reaction is limited by the available quantities of steam in the reactor vessel. Under water injection there is no such limitation; however, now the cooling rate by far exceeds the additional energy source. Moreover, even without such cooling the additional power could be more than compensated by an increase in emissivity producing no significant impact on vessel wall heat fluxes.

(b) The reviewer takes our focusing effect and makes it a threat to integrity by simply postulating that the quantity of metallic melt is reduced "perhaps by as much as a factor of ten." The report already contained some key arguments as to why this is not possible We provide additional bases for these arguments in the point-by-point response, and in Appendix O.

(c) The reviewer assumes that the thermal stresses will induce a plethora of cracks on the outside cold region of the vessel wall, so that even arrested in the inner hotter regions, such cracks would jeopardize structural integrity. We supply material specifications for the AP600 design that show this to be physically unreasonable. Moreover, even if cracks could develop they would be arrested well before the inner, unstressed, core material discussed in Chapter 4.

(d) The reviewer asserts that a significant fraction of the decay heat source would be found in the metallic layer. This may be plausible, but actually considering contact configurations and mass transfer limitations, it has to be rather unlikely. We use the reviewer's numbers to show that this effect would bring the heat flux in the metallic layer region up to about the same CHF margin existing previously at the upper edge of the oxidic layer. The margin remains at 100%.

Point-by-Point Response

1. I Chemical Phenomena

(a) Reaction of the metallic melt with steam

The report clearly indicates (pp 1-1 and 1-3) that the cavity above the metal pool is filled with steam. The metallic melt contains ~50% of the core's Zr in elemental form. It is impossible for steam and zirconium to remain unreacted during hours of contact at temperatures of ~1600 K. Reaction of steam and zirconium was responsible for the development of the accident in the first place. In the metallurgical industry, addition of small quantities of Zr to molten steel during the steelmaking process is used as a deoxidizing procedure. Contrary to oxidation of solid Zry, buildup of a coherent ZrO_2 layer on the upper surface of the metal pool is unlikely because the substrate is a liquid in turbulent flow.

The kinetics of steam reaction with Zr in the Fe-Zr liquid alloy is not known. It is probably very rapid because of the absence of a protective oxide scale. A conservatively high estimate of the reaction rate (and the corresponding heat release rate) can be made by assuming complete conversion of steam to hydrogen at the surface with the overall rate controlled by mass transfer in the gas phase adjacent to the pool surface. Mass transfer is by natural convection, driven by both the unstable temperature gradient and by the reduction of the gas density at the surface that accompanies conversion of H_20 to H_2. Using the Sherwood number in place of the Nusselt number for the turbulent natural convection correlation for heated plates facing upward, the mass transfer coefficient is given by:

$$k_g = 0.14D\left[\frac{g(\Delta\rho/\rho_f)}{\nu^2}Sc\right]^{1/3} \tag{1}$$

where, for an ideal gas,

$$\frac{\Delta\rho}{\rho_f} = \frac{\Delta T}{T_f} + \frac{\Delta M}{M_f} \tag{2}$$

$\Delta T = T_{1,0} - T_{b,g}$ *and* $T_{b,g}$ *is the bulk steam temperature, taken as 1000 K.* $\Delta M = M_w - M_H$ *is the difference in the molecular weights of water and* H_2. T_f *and* M_f *are the mean values of these two properties. With these values,* $\Delta\rho/\rho_f \sim 2$.

D is the diffusion coefficient of the H_2O/H_2 *system. It is calculated from the correlation given in the appendix of Ref. 2 to be ~11* cm^2/s *at* $T_f = 1300$ *K and* *a total pressure of 1 atm. The viscosity of a 50 mole % steam-hydrogen mixture at T_f is $\sim 4 \times 10^{-4}$ g/cm-s and the mass density of this mixture is $\sim 8 \times 10^{-5}$ g/cm^3. Substituting these values into Eq (1) gives $k_g \sim 5$ cm/s.*

The flux of water vapor to the upper surface of the metal layer is:

$$J = k_g S_{up} (\rho_f / M_f) (y_{b,g} - y_{surf}) \tag{3}$$

For an oxide pool volume of 10 m^3, $S_{up} = 12$ m$^2 \cdot y_{b,g}$ is the mole fraction of steam in the bulk gas and y_{surf} is the value in the steam at the surface. These are taken as 1 and 0, respectively. Equation (3) gives a water vapor flux to the surface of ~5 moles/s. At this rate, all of the Zr in the Fe-Zr alloy pool is consumed in ~12 hours (assuming 50% of available Zr in the metal pool).

The heat released by the steam-metal reaction is calculated from the enthalpies of formation of ZrO_2 and $H_2O(g)$ (Ref. 3, Appendix) to be 293 kJ/mole H_2O. The chemical heat release at the surface of the metal pool is $5 \times 293 \times 10^{-3} = 1.5$ MW. This is a significant addition to the ~13 MW from decay heat in the oxide pool. The metal layer surface heat source due to chemical reaction is ~120 kW/m^2.

The implication made here can be discounted at three levels.

(i) Inconsistency in steam supply. It is not possible to remove 5 moles/s of hydrogen from the boundary layer (as the reviewer does), if the bulk concentration is zero (as the reviewer assumes). In fact, any continuing supply of steam, as assumed, has to come from the containment atmosphere, together with a *lot of air*! The $\Delta M / M_f$ in Eq. (2) would then be significantly off, and the driving force $(y_{b,g} - y_{surf})$ could be overestimated in Eq. (3) by a factor of 3 or more.

(ii) A good way to have an unlimited supply of steam to the melt is through water addition. But then we have to consider also enhanced heat transfer due to film boiling, and much enhanced radiation loss from the melt due to the increase of emissivity (of the oxide). These by far would outweigh any reasonable chemical energy source and, in fact, even the 120 kW/m^2 number proposed by the reviewer.

(iii) Finally, it is interesting to consider the impact (actually non-impact) of the reviewer's number, even without the two exceptions outlined above. The total decay power considered is 14 MW, and of it ~7 MW goes into the metal layer, so that the 1.5 MW would represent an increase by ~20%. However, this behavior would be accompanied by a substantial increase in emissivity. Using a value of 0.8, as suggested by the review in the next point, we find that the net effect is to increase the sideward heat flux by only 4%. This can also be determined by using Figure 5.11.

2. (b) Metal pool emissivity

The report takes the emissivity of the upper surface of the metal pool to be 0.45, which is reasonable for a clean metal surface. This value was measured by the experiment described in Appendix I of the report. However, if steam had been mixed with the pure argon used in this experiment, the surface of the Fe-Zr liquid would have been oxidized and the emissivity would probably have been ~0.8. In the model, this would have increased the radiant heat loss from the pool upper surface and reduced the heat flux to the vessel wall. Credit should be taken for this reduction.

Yes, to be consistent we must, as described above.

3. (c) Extraction of uranium from the oxidic pool by the metal alloy

It is well established that molten cladding dissolves UO_2 pellets to produce a melt that contains up to 40 wt% uranium on an oxygen-free basis(4). Therefore, the elemental Zr in the metal pool should also extract uranium from the oxidic pool. The melts from the TMI 2 core contained small quantities of uranium(5). This process will reduce the eutectic temperature of the metal pool from that of the Fe-Zr binary to that of the U-Fe-Zr ternary alloy. A pseudo-binary phase diagram of this alloy can be approximated by averaging the Fe-Zr and Fe-U phase diagrams.

The extraction process considered here would require extensive contact between liquid cladding and UO_2. Here we have a predominantly stainless steel melt, with some Zr dissolved in it, in contact with solid UO_2. The eutectic temperature of such a system (0.3 – 0.6 – 0.1 mole fractions) is 1873 K or ~250 K *higher* than the temperature used in our calculation to attack the vessel wall in contact with the metallic layer. On the other hand, a Zr stainless steel eutectic (0.193 – 0.807 mole fraction) is found at 1723 K, still 100 K higher than the temperature used in the report. Recognizing the huge margins in the structural evaluation, and the conservative choice of the eutectic temperature, further elaboration in this area is not considered to be fruitful.

4. (d) Vessel wall melting temperature

The report used the eutectic temperature of the Fe-Zr binary for the melting temperature of the wall ($T_{l,m} = 1335°C$). This is correct only if the melt composition *is $x_{Zr} = 0.088$ mole fraction zirconium. For $x_{Zr} \neq 0.088$, the appropriate value for $T_{l,m}$. is the liquidus temperature in the phase diagram shown in Fig. 6.1 of the report. For $x_{Zr} \leq 0.088$, this can be approximated by:*

$$T_{l,m}(°C) = 1536 - 2284x_{Zr} \tag{4}$$

The steady-state heat flux balance at the metal melt-vessel wall interface is:

$$h(T_b - T_{l,m} = k(T_{l,m} - T^{**})/\delta_s \tag{5}$$

where $h = A(T_b - T_{l,m})^{1/3}$ [Eq(5.41)] with A given by Eq(5.47) and δ_s is the thickness of the vessel wall adjacent to the metal layer. It is in general not equal to the as-fabricated value ($\delta_{s0} = 5$ cm) because iron may precipitate on the wall or the wall may dissolve in the liquid to give a thickness that satisfies Eq(5) for the specified value of T_b. The wall thickness relative to the as-fabricated value calculated from Eq(5) is:

$$\frac{\delta_s}{\delta_{s0}} = \frac{T_{l,m} - T^{**}}{T_b - T_{l,m}} \frac{1}{Bi} \tag{6}$$

where

$$Bi = h\delta_{s0}/k \tag{7}$$

is the Biot number. Using the value $A = 2764$ given in the example on p. 5-19 of the report, $\delta_{s0} = 0.05$ m, and $k = 25$ W/K-m(Table 7.1):

$$Bi = 5.5(T_b - T_{l,m})^{1/3}$$

and Eq(6) is:

$$\frac{\delta_s}{\delta_{s0}} = 0.18\frac{T_{l,m} - T^{**}}{(T_b - T_{l,m})^{4/3}} \tag{8}$$

An example of this effect is given in Table 1 using the bulk metal temperature given in the example on p. 5-19 of the report ($T_b = 1405$ °C) and $T^{**} = 100$ °C.

Table 1 Thickness of vessel wall opposite metal layer

x_{Zr}	$T_{l,m}$ (°C)	δ_s (cm)
0.05	1421	*
0.065	1387	25
0.088	1335	4

**bulk temperature is less than the liquidus temperature; Fe-Zr cannot exist as a single-phase liquid*

The table shows that the wall thickness is very sensitive to the mole fraction of Zr in the metal melt. In the model developed in the report, Eq(4) above should be used for $T_{l,m}$ in the last term of Eqs(5.42) and (6.9). Equation(5) above needs to be added to the set of equations to determine the vessel wall thickness.

If $x_{Zr} > 0.088$, the phase that precipitates on the wall is Fe_2Zr and Eq(4) is replaced by the liquidus joining the eutectic point and the melting point of Fe_2Zr in Fig. 6.1 of the report.

First of all, the as-fabricated value of δ_{s0} is *not* 5 cm, but rather 15 cm. This is explained clearly in the report. Second, we used the eutectic because such composition is conservative, and the subject (as described above) is not worthy of extensive elaboration, especially recognizing that the composition cannot be specified with any degree of accuracy.

5. *(e) <u>Melting temperature of the oxidic pool</u>*

The melting temperature of the oxidic pool given in Table 7.1 of the report is too high. Because of the addition of transition-metal oxides to the ceramic melt, a melting temperature of ~2700 K is suggested (p. 84 of Ref. 5). Other investigators suggest that the high-melting ceramic may flow as a solid carried like a slurry in the molten spinel (ref. 5, p. 187 and ref. 6). The spinel is $Fe(A\ell,Cr,Ni,Zr)_2O_4$, and may be present at levels as high as 10% in the oxidic material. The oxidic pool may not be a single phase liquid as assumed in the report (see also bottom of p. 5).

The melting temperature of the oxide in Table 7.1 is 2973 K. The reviewer prefers 2700 K. Actually, the absolute temperature level, within a few hundred degrees, does not matter at all. What is important is the melt superheat, and the correct use of properties for the superheated melt (Appendix L). The superheat is obtained from the energy balance, i.e., the melt will superheat sufficiently to allow the boundary fluxes, with the appropriate heat transfer coefficients to just balance the decay power source in the volume.

6. *(f) <u>Location of the decay heat source</u>*

The report assumes that the decay heat source is where the uranium is. However, the decay heat is due to the fission products, not the uranium. This fact was partially recognized by the authors of the report when they allowed for loss of volatile fission products (they need to state which fission products are volatile). However, a significant fraction of the fission products may be present in the metal *layer. The presence of the noble metals (Ru,Rh, Pd) in the metallic phases of the TMI-2 core debris has been verified (Ref. 5, p. 91). Te is likely to follow elemental Zr in the metal layer. Zr fission product will distribute in the same manner as the structural Zr. Some Cs is found in the debris. The oxides of Mo have higher standard free energies of formation than UO_2 or ZrO_2 and Mo probably is more stable in the metal phase(6). Table 2 shows a possible partitioning of all fission products in one of three locations: volatilized and escaped; retained in the oxide; dissolved in the Fe-Zr metal layer.*

Table 2 Distribution of fission products in core debris

Fission product	Released	in oxide pool	in metal layer
Zr,Nb*	0	0.15	0.15
Mo	0	0	0.24
noble metals	0	0	0.25
Cs	0.15	0.04	0.04
rare earths	0	0.53	0
Ba,Sr	0	0.15	0
Xe,Kr	0.25	0	0
others+	0.03	0	0.01
Total	0.43	0.87	0.70

* *assuming 50% of Zry from core in each phase*
\+ *Te in metal phase*

The sum of the numbers in the row for each fission product group is the elemental yield of this group from fission. The sum of the Total row is 2.

If the total fission product decay heat source is 13 MW, the above table suggests that it is divided into 7.2 MW in the oxidic pool and 5.8 MW in the metal layer. This heat source in the metal layer should be considered in the report's model.

Using the reviewer's numbers the thermal load in the steel layer would be ~10 MW, as compared to 7.41 in our calculations; that is an increase of ~25%. The impact of this increase can be seen directly from Figure 5.11 (using approximately the extra power to increase q_{up}). The H_ℓ/R of interest here is ~0.5, the corresponding line is "flat" and therefore the wall thermal loading should increase by the same amount, i.e., 25%. Referring then to Figure 7.10, we find that the critical flux ratio in the metal layer increases from 0.4 to 0.5; that is, it reaches the peak value found (previously) in the oxidic pool. Now, of course, the oxidic pool is by comparison only slightly loaded. The margin to failure is still 100%! See also parametric evaluations, carried out with the full model, in Appendix P.

Having said that, we do not agree that in a meltdown scenario there is sufficient opportunity and contact for the "extraction" process contemplated by the reviewer here. Especially, we consider it even more unlikely that this is possible to occur in a transient state with a small quantity of steel in the metal layer. Further perspectives on this topic are provided in Appendix R.

7. II Amounts and composition of the liquids in the lower head

The report justifies the large amount of steel in the metal pool (~72 tons) with the claim that an oxidic pool height of 1.5 m would touch the core lower support plate. This, in consequence, would melt, and along with it, substantial portions of the core barrel and the reflector. The 1.5 m height is based on the assumption that all of the fuel in the core is relocated to the lower plenum. However, in TMI-2, a larger reactor, only 20 tons of core debris reached the lower head, and consideration must be given to the possibility that the initial oxidic pool height is less than 1.5 m and does not contact the lower support plate. The smaller quantity of oxidic material than the entire fuel loading would reduce the heat fluxes q_{up} and q_{dn} because the surface-to-volume ratio of the pool would increase. However, counteracting this is the probability that the fuel that did melt and reach the lower head would have a higher volumetric heating rate because it came from high-burnup regions of the core, near the center.

The TMI accident was terminated! The TMI accident retained considerable quantities of water in the vessel that *quenched* the debris. The molten fuel mixes well by natural convection, and the pool has to extend to the radial edges of the core and remain there long enough before the reflector and core barrel are breached. The pool will be of significant size, and the decay heat in this mass *should not* be considered as coming from the core center and having higher decay power levels. See also response to Cheung item #2, and Appendix O.

8. *The most profound consequence of melting appreciably less than the entire fuel contents of the core is the reduced quantity of molten steel in the metal layer. If large portions of the core barrel, the lower support plate, and the reflector remained in place, the Fe concentration of the metal layer and the height of this phase would be greatly reduced, perhaps by as much as a factor of ten.*

On page 7-16 we explain that a minimum bound on the quantity of steel on top of the oxidic pool is 17 tons. This corresponds to a metal layer depth of 22 cm. This case, under a maximum thermal load (70% of the core), was shown to not yield failure. The "perhaps by as much as a factor of ten" is simply hypothetical and cannot be considered credible without some key considerations of how such a large quantity of core debris can get to the lower plenum without metal in the first place. See Appendix O for an explanation of why this is not consistent with physical behavior. Hence the consequences of it, as given by the reviewer below, are of no interest.

9. The consequences of this are:

1. The heat flux to the vessel wall from the metal layer would be more focused, thus increasing $q_{l,w}$ (Fig. 6.3).

2. The composition of the Fe-Zr alloy would be in the Zr-rich region of the phase diagram, which has a lower eutectic temperature than the eutectic in the Fe-rich region which is assumed in the report.

3. Because of the small height-to-diameter of the metal layer, it could no longer be characterized by a single bulk temperature, T_b. There would be some radial bulk temperature gradient in this layer.

4. The remaining core support plate above the metal layer would act as an additional radiation shield and reduce the radiative heat loss from the upper surface of this layer.

These "consequences" are of no interest, as explained above.

10. 5. With a greatly-reduced quantity of steel melted, the metal/oxide system would more closely resemble that of the TMI 2 core debris than the neatly separated liquid phases on which the report is based. Examination of the TMI 2 rock samples was extensively reported in Ref. 5. These studies suggest that the metal and oxide phases were never fully separated. Instead, the metallic phases were interspersed with oxide phases to form a slush that one report characterized as wet sand (Ref 5, p 187); another study (6) suggested that the $(U,Zr)O_2$ is transported to the lower plenum as a solid with the spinel phase acting as a lubricant. Relocation of core material to the lower head probably resembles a pour of wet concrete more than a clean flow of fully-liquid phases. It is even possible that distinct oxidic and metallic phases never separate in the core debris but remain in a dispersed state like oil and vinegar salad dressing. The analysis would then have to deal with a single composite medium with heat transfer through the connected liquid metallic phase and the heat source (at least part of it) in the dispersed solid oxide phase.

Again the reviewer is reminded that the debris in TMI quenched by itself, even though the vessel was not cooled from the outside. The train of thought employed here is fundamentally flawed when applied to accidents that progress sufficiently to produce significant thermal loads in the presence of external cooling. We have to examine here a melt pour that is much more severe than wet sand, because the radial core reflector in the AP600 provides an effective obstacle to relocation (see Appendix O). Significant melt superheating must develop to produce a breach, and a significant length of time is required to do so (see Appendix O). At that time the in-core pool has macroscopic dimensions, and we expect it to contain an oxidic melt, as described in Appendix O.

11. **III Mechanical Aspects**

(a) Wall loading by internal pressure

The report considers two sources of stress generation in the vessel wall: dead-weight loading and thermal gradients. To these two should be added pressure loading, which may be important in high-pressure accident scenarios. The yield strength of the vessel steel drops sharply above 900 K. On p. 4- 1, the wall thickness that retains full strength ($\sigma_Y = 355$ MPa) is given as $\delta = 1.1$ cm. The internal pressure needed to achieve an equivalent stress in a thin-wall spherical shell that is equal to the yield stress is

$$p_{tot}(at\ yielding) = 2\delta\sigma_Y/R$$

Using the above figures and $R = 2$ m gives a pressure for yielding of ~4 MPa. This total pressure (or greater) is encountered in some accident scenarios.

The report makes it clear that we are not interested in pressurized scenarios, even though the capability for such may be there. In fact, the capability would be more than 4 MPa, because the 1.1 cm used by the reviewer (taken from Chapter 4) was for the limiting case of fluxes close to CHF. In the report we show that actual fluxes would be as low as half of that, and the pressure carrying capability correspondingly as high as twice the 4 MPa value.

12. *(b) Wall failure by fracture*

In the report, high thermal stresses are accommodated by yielding and by creep. However, the outer surface of the vessel wall is held at 100 °C by boiling water, so the possibility of brittle behavior should be considered. The problem is not unlike pressurized thermal shock, in which cold water contacts a hot wall resulting in temperature gradients and thermal stresses. In the present case, a hot liquid contacts a cold wall.

i) In Fig. 4.4 of the report, a significant fraction of the wall thickness on the outside surface is at stresses larger than the yield stress. This is also the region that is coldest. It is possible that the stress intensity factor (K_I) exceeds the fracture toughness (K_{Ic}) and cracks develop on the outer surface, propagating inward until the crack arrest fracture toughness (K_{Ia}) is reached. Although through-wall cracking is not possible, the outer surface of the vessel could develop a population of cracks that render this region unable to sustain thermal stresses.

ii) The vessel wall above the metal layer is relatively cold throughout its thickness. Because the bottom of the vessel is hot, its thermal expansion places the upper vessel walls in tension. Again, K_I could exceed K_{Ic} and fast crack growth may occur.

iii) The temperature gradients developed during the initial thermal transient when the oxide liquid first pours into the lower head are steeper than those that prevail at steady state. As in the case of pressurized thermal shock, the transient behavior of the temperature distribution leads to crack propagation early in the event. Thermal stress distributions early in the core relocation to the lower head should be computed as well as the steady-state distributions treated in the report.

The minimum water temperature in the IRWST is 55 °C, and the end-of-life RTNDT of the AP600 vessel wall material is specified as 20 °C at the vessel belt-line welds. The only welds of interest here (i.e., subject to severe thermal stresses) are two circumferential welds on the lower head, one at the junction with the cylindrical section, and the other at some lower angle. These welds are expected to suffer minimal irradiation damage as compared to those in the belt-line regions, so the 20 °C temperature is conservative. Given that the vessel is depressurized (no primary loads) we do not see a concern with brittle fracture at all. These considerations are now briefly summarized in an addendum to Chapter 7.

13. (c) Stability of the crust on the pool upper surface

The report makes a point that the crust separating the oxidic pool and the metal layer is very thin. Yet this crust, which is ceramic, sustains a sizable temperature gradient (leading to thermal stresses in it) and is bounded on both sides by moving liquids (which probably produce waves much as shown in Fig. 1.2 of the report). It is very difficult to imagine that such a crust would be mechanically stable in this environment. Instead, it would probably be broken into pieces which sink into the oxidic pool because the solid density is greater than the liquid density.

The crust would continually reform, but its mechanical disruption would render its thermal resistance much less than if it were a coherent slab as assumed in the report. If this were so, the boundary condition $T = T_m$ at the upper pool surface would no longer be valid, and q_{up} would greatly exceed q_{dn}.

The crust forms upon contact, and it would be sufficient to establish the thermal boundary condition considered, even if it was unstable.

14. **IV Miscellaneous**

(a) Information

The report should contain summary tabular or graphical information on the reactor vessel which is the subject of its analysis. Even as basic a piece of information as the vessel wall thickness is only casually mentioned in the text and on the abscissa of some figures. Useful vessel information should include:

- *geometry, including instrument penetrations (if any) of lower head*
- *composition of wall steel*
- *plot of yield strength Vs temperature*
- *thermal expansion coefficient*
- *elastic and creep properties*
- *fracture toughness properties as functions of temperature*

This information as an appendix would be much more useful than the series of appendices describing the various heat transfer experiments. These contribute little to the tenor of the report and could simply be referred to in their original documentation. Appendix D describes an experiment that is not even built.

(a) There are no penetrations on the lower head. The geometry was described in Figure 2.1 and Table 7.2.

(b) Composition of wall steel was described and discussed in Appendix L.

(c) The yield strength vs temperature can be found in Chapter 4 and Appendix G.

(d) The thermal expansion coefficient along with all other properties were given in detail in Appendix L and in summary form in Table 7.2.

(e) Elastic and creep properties were presented in detail in Appendix G.

(f) The fracture toughness is provided in the final paragraph of Chapter 7.

(g) The heat transfer experiments constitute the heart of the case, and the experiment in Appendix D was not only built, it provided unique and essential data!

15. (b) Verification of numerical examples in the text

i) Starting with Eq(5.33) of the report with $S_{up} = \pi H(2R - H)$ *and* $S_{dn} = 2\pi RH$ *(instead of hemisphere values) and* V *given by Eq(6.1), Eq(5.34) is:*

$$q_{dn} = \dot{Q}FR \quad \text{where} \quad F = \frac{\gamma(1-\gamma/3)}{(2-\gamma)R' + 2} \quad \text{and} \quad \gamma = \frac{H}{R}$$

For the example given on p 5-15, $\dot{Q} = 1.3$ *MW/m*^{3}*,* $V = 10$ *m*^{3} *and* $R = 2$ *m. These values give* $H = 1.45$ *m, and from the above equation for* $R' = 1.31$*,* $q_{dn} = 391$ *kW/m*^{2} *instead of the value of 313 given in the text.*

The problem is that the 10 m^3 volume is not enough to fill a whole hemisphere. We used this value because it is typical for our interest, and Eq. (5.34) (or 5.33$'$ in the present version) is only approximately applicable. This was a bad choice for a numerical example. In fact, Eq. (5.33), which in general should be used, and the reviewer's number is correct. As additional perspective using the complete numerical calculation, we obtain 357 and 296 kW/m^2 for the two values of R' respectively. This point is clarified by means of a small addendum at the end of Section 5.1.

16. ii) The value of $\tilde{q}$ *given in the example on p 5-19 should be* 9.1×10^5

Using $q_{up} = 600$ *kW/m*^{2} *and* $A = 2764$ *in Eq(5.44) results in a difference between* $T_{l,i}$ *and* T_b *of* $\sim$*0.4°C. This does not seem to be physically reasonable. However, using* $T_{l,i} = T_b + 0.4 = 1678.4$ *K and* $\dot{Q} = 1.5$ *MW/m*^{3}*,* $q_{up} = 600$ *kW/m*^{2} *in Eq(6.14) gives* $\delta_{cr} = 7$ *cm, and the group* $\delta_{cr}\dot{Q}/q_{up} = 0.17$*, which violates the condition given by Eq(6.15).*

These three points made here, and our responses, are as follows:

(a) Our 9.1×10^8 value is correct.

(b) Using the q_{up} and A values of the reviewer, $T_{\ell,i} - T_b = 54$ °C, and *not* 0.4.

(c) This then makes $T_{\ell,i} = 1736$ K, which with $\dot{Q} = 1.5$ MW/m^2 and $q_{up} = 600$ kW/m^2, gives $\delta_{cr} = 1.1$ cm. Then the criterion $\delta_{cr}\dot{Q}/q_{up} \sim 0.027$ and *not* 0.17. Actually, there was a typo error in Eq. (6.15), which is corrected (0.03 instead of 0.01).

We are not sure what the reviewer's errors are due to, but it appears that he is forgetting the 10^3 factor between kW and W. In any case, we greatly appreciate this opportunity to recheck our numbers.

T.11. Response to R.C. Schmidt (SNL) — Specific Assignment: Ch. 5

General Comment and Highlights

We appreciate the favorable comments regarding the ACOPO experiment concept, and the suggestions made to further justify and improve the validity/usefulness of it. There is, however, an interesting variation (between the reviewer and us), at the conceptual level, on what ACOPO really represents and why it can be expected that it is adequate for our purposes. We believe the reviewer's interpretation is more restrictive, but, as he shows, even that is sufficient for our purposes at the ACOPO 1/2-scale (only marginal at the mini-ACOPO scale). Since the data from ACOPO are imminent, this variation does not give rise to any real issue. On the contrary, this should be very helpful in convincing Prof. Sehgal (of the validity of the ACOPO approach) who actually has questioned it. With this in mind, the two interpretations are discussed further under the point-by-point response.

Point-by-Point Response

1. The review comments presented here are organized into four parts: (1) General Comments on Chapter 5, (2) General Comments on the ACOPO experiments (described in both Chapter 5, and Appendix D), (3) Miscellaneous comments that cover all the sections that I read, and (4) A brief technical note the Validity of the ACOPO Experiments for Natural Convection in Hemispherical Enclosures at High Raleigh Numbers.

Comments on Chapter 5.

My comments here will be restricted to the discussion of heat transfer in the oxidic pool region.

<u>Section 5.1</u>

The fundamental goal of this section is to obtain the best estimates possible for heat transfer in the oxidic pool to the top and bottom surfaces, and the local beat flux variation on the curved surface. I have carefully reviewed this section and have the following comments.

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2. *Upper (Flat) Surface Heat Transfer:*

In the paper cited for Eq. (5.11) the correlation is given as 0.403 $Ra^{0.226}$*. Also, the correlation for Eq. (5.13) is given as 0.233* $Pr^{0.239}$ $Ra^{0.233}$*. For clarity it would be useful to point out that the constants have been adjusted in this report to account for the different Rayleigh number definition.*

Clarification added in the text.

3. *I believe it more accurate to say that there are three (instead of two) correlations that are typically cited when considering the upper surface heat transfer. In addition to the two mentioned, the well known correlation of Jahn and Reineke for semicircular geometries* ($Nu_{up} = 0.36\ Ra^{0.23}$) *is often used, and in fact has been (in the past) the most commonly used correlation in severe accident codes.*

We stand corrected that Steinberner and Reineke (1978) only verified and extended the data base for the earlier Jahn and Reineke (1974) correlation. It may be semantics, but this extension was crucially into the turbulent region where it has its own merit. On the other hand, we do not believe it would be appropriate to use it for downward heat transfer (semicircular vs hemispherical geometries). See also response to item #6.

4. *In addition to mentioning the Jahn and Reineke correlation, the discussion in 5.1 does not adequately point out the differences in the experiments from which the correlations cited are developed, and could be strengthened by doing so. The Kulacki and Emara study considered a plane fluid layer (rectangular cavity) where only the top surface was cooled. Steinberner and Reineke considered three different thermal boundary conditions. However, the only case for which upper surface data was taken is the one with adiabatic sidewalls, with cooled (isothermal) top and bottom surfaces. The Jahn and Reineke correlation is for semicircular geometries. As can be seen from Fig. 5.2, none of these three situations is exactly the same as the problem of interest, i.e., a hemispherical pool with isothermal surfaces at all boundaries (top, bottom, and side). The remarkable thing to note is that despite major differences in geometry and thermal boundary conditions, the correlations are all relatively close. This provides some confidence that the upper surface heat transfer in hemispherical pools with isothermal surfaces should be similar.*

The text following Eq. (5.10) to the end of the paragraph actually was intended to address this point, but from a slightly different perspective. We do *not* think it is remarkable that all these experiments agree for heat transfer to the upper plate. It would be remarkable if they did not agree! This is why we did not emphasize the geometries and thermal conditions at the upper boundaries. Still, the reviewer's clarification is useful to help sharpen the point, one way or another, and we welcome it.

> *5. Overall, I basically concur with authors conclusions about the results for the heat transfer to the curved surface. However, to be "conservative, but not overly so", on the upper surface, I think the authors should consider the use of Equation (11) instead of Equation (12) as the reference correlation for the upward heat transfer. I have prepared a figure, (Fig. 1 below) to illustrate why I think this is so. Shown on this figure are the data from the mini-ACOPO experiments (taken from Fig. 5.3 in the report), the Asfia Dhir data (Appendix C), and the three experimentally derived correlations mentioned above. Considering the current uncertainty in the mini-ACOPO data, together with the results of Asfia and Dhir, it seems to me that an appropriately conservative approach (at least for the present) is to use the Kulacki and Emara correlation, not the Steinbemer and Reineke correlation.*

We disagree with the suggestion made on several grounds, and this is important! *First*, lower heat transfer to the upper surface is *not* necessarily conservative. In fact, as discussed already in the report, looking for margins to failure one first finds them where the steel layer is in contact with the wall, which means the higher value is more conservative. *Second*, with the range $2 \cdot 10^4 < \mathrm{Ra}' < 4.4 \cdot 10^{12}$ vs $10^7 < \mathrm{Ra}' < 3 \cdot 10^{13}$ in the Steinberner–Reineke data, the Kulacki–Emara correlation would appear to be too heavily weighted to the laminar/transition regime. We feel the Steinberner–Reineke correlation, essentially confirmed by Jahn and Reineke, besides covering a narrower/higher range it extends it by one order, and hence is preferable. *Third*, with only two data points, one of them significantly lower than all correlations, and both obtained with data only over a portion of the upper wall the Asfia–Dhir data certainly cannot be considered as supporting the Kulacki–Emara correlation. This is not meant as criticism, but only to make sure it is understood that the Asfia–Dhir experiments were focused on the lower boundary. *Fourth*, the mini-ACOPO experiments extend the support of the Steinberner–Reineke correlation by nearly two orders of magnitude, and are in excellent agreement with it in the data overlap region. We take this agreement to be a clear demonstration of the validity of the ACOPO concept and the mini-ACOPO experimental and data reduction techniques. In other words, this *is* the demonstration test that the reviewer wishes to have. And this is the fundamental reason we do not wish to concede this point.

However, it is appropriate to consider using Kulacki–Emara in a parametric/sensitivity calculation, and such was performed. It is reported in Appendix P, together with parametrics suggested by other reviewers.

6. <u>*Lower (Curved) Surface Heat Transfer*</u>

I basically concur with the conclusions drawn by the author concerning the heat transfer to the lower surface. Figure 5.7 was particularly useful in illustrating the data which leads them to choose Equations (5.28) and (5.22) as representative of the spread in the current data base. However, it should be noted that the correlation of Jahn and Reineke (Eq. 5.21) was not included in Fig. 5,7. This correlation predicts much lower Nusselt numbers at these high Rayleigh numbers (For example, at Ra = 10^{15}*, 270 vs about 6OO). It would probably be more complete if the authors directly discuss why they choose not to use this data. My experience leads me however to concur with the apparent judgement of this report and discount these predictions as too low.*

This is a fundamental point, too! The Jahn–Reineke correlation cannot be put on this plot, because it is for semicircular (as opposed to hemispherical) geometry. In fact, Jahn's data are shown in Figure 5.8, renormalized to a hemispherical geometry. We can see in this figure that they are entirely consistent with the mini-ACOPO data. In fact, the average value produced through this area-weighting process is in good agreement with trends in Figure 5.7. This means that the upper portion of the curved wall controls heat transfer, so that the convergence effect that is present in the lower-most portion of the hemispherical geometry is not so important, and the local heat transfer values actually agree. We had neglected to mention all this before, but now a remark is added as a footnote to Figure 5.8.

7. *The constant shown in Fig. 5.7 for the Mayinger et al. correlation should be changed from 0.54 to 0.55.*

Typo was corrected.

8. <u>*Heat Flux Distribution on the curved Surface:*</u>

I feel that the review of the data was sufficiently complete and that the base correlation used (Eq. 5.30) is adequate for this study. However, the use of the UCLA data (which shows a more peaked distribution) was definitely needed to bound the uncertainty in the current data.

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9. *General Comments on mini-ACOPO experiment (Section 5, and Appendix D)*

My primary comments relative to the ACOPO experiment are contained in the last section, which provides a more technical review of how to validate the ACOPO approach. However, some general comments are appropriate here. First, I feel that the authors should be congratulated for developing and exploring a novel approach to solving a very difficult experimental problem. The approach taken is a variation of the approach used by Chow and Akins for studying convection in Spheres (as well as a number of subsequent numerical studies by others). I am very favorably impressed with the approach, and as a result of this review I am now a strong supporter of this method as being a good one. I none-the-less have some concerns about the strength of the validation arguments the authors have chosen to present. Furthermore, I might comment that within the context of the report (Chapter 5 in particular), I get a strong sense that the authors have a great deal of confidence in the results of the mini-ACOPO experiments. This is not wrong, and my assessment tends to confirm the validity of the approach, but further work needs to be done before the uncertainty level of the mini-ACOPO data can be clearly determined. Thus, I might recommend a somewhat higher sense of caution (for the present) then is reflected in the tone of the current report.

At the beginning of section D.5, the authors state that "the key point" validating the experimental concept is the establishment of a self similar stratification pattern during the cooldown. They define a local dimensionless temperature in Eq. (D.1), and plot the data for these temperatures in Figures D.4 and D.5. The claim is that because a "well defined, self similar temperature gradient exists in the intermediate 10% to 50% of the pool volume" that the approach is validated. I do not think that this is a correct path to validation. Even if quasi-static behavior is assumed, thermal profiles would be expected to change as the system moves from a high Rayleigh number to a lower Rayleigh number. To my knowledge, there is no basis for expecting the thermal profiles plotted using their dimcnsionless temperature to be exactly the same at say Ra= 10^{16} *as they are at say* 10^{14}. *The argument is better made that because the range of Rayleigh numbers is not very great, and the pool is in the fully turbulent regime, that the normalized thermal profiles would not be expected to change very much. But this is quite different from claiming that "self similar" temperature profiles can be shown to exist at different Rayleigh numbers. Furthermore, I do not see how one can know that the profiles would not show approximately similar stratification patterns if* *the system was not at quasi-steady states. As mentioned earlier, I think there are better ways to argue the experimental validity, and I have outlined them in the last section.*

This is addressed in conjunction with discussing the technical note, at the end.

10. I think the data in Figures D.4 and D.5 should be plotted as a function of depth, not as a function of normalized fluid volume. It would be easier for the reader to relate to and just as relevant.

Not really. As seen in Figure 5.8, and as discussed above, the heat flux near the bottom is very low and its distribution very flat—nothing much happens there. A big part of the reason is that there is too little fluid in, and circulating through, it. This is why the way the results were plotted is physically much more meaningful.

11. On pg. D-11, top of the page, first complete sentence after Eq. (D.1): This sentence somewhat confused me. It states that lateral temperature gradients are always negligible, which is an important piece of information, but no data is actually shown to support this statement. The authors should show the data in some form.

It is difficult to show in a figure when the data are on top of each other. Rather, we have added a statement on p.D-11 that the agreement is within 5% of the overall ΔT. Part of the text from 3rd to 8th lines below Eq. (D.1) was scrambled, and this added to the confusion. This was also corrected.

12. Msc. Editorial type comments, minor corrections and questions

Pg. 2-1. Next to last sentence: What is a "Grade B approach"? A reader such as I has no idea what is meant here.

See Appendix A.

13. Pg. 2-2. Second Paragraph, last sentence: I suggest replacing the terms "production and dissipation" with "heat input and heat loss." The terms production and dissipation are more commonly used in terms of turbulence production and dissipation as compared to energy or heat transfer.

Clarification added, although content leaves no room for misunderstanding.

14. Pg. 2-2. Last Paragraph. 2nd and third sentences ("What occasionally . . . " and "In Particular . . . ".): These statements seem out of place and confuse the *point of the paragraph. Furthermore, they are technically confusing and I don't think they're needed. Steady state is approached slowly with the time constant of the system being a function of both the pool thermal capacitance and the flow strength. Who has suggested otherwise? Appendix D does discuss the presence of strong boundary layers but only makes a conjecture that this impacts the quasi-steady state assumption.*

See discussion on the technical note at the end.

15. *Fig. 7.3, Pg. 7-4:* *Why are there irregular wiggles on the flat portions of the probability density function plotted?*

They are due to the finite sample interval and total sample size.

16. *Page 7-11 3rd to last sentence:* *I might suggest changing "contrary to popular opinion" to "contrary to what might have been expected."*

Disagree. The "might have been expected" would include the author in the expectation crowd, while our expression does not. We mean the latter.

17. *Fig. 7.9,. pg 7-12:* *I cannot distinguish which curves correspond to which values of θ in this plot. Could something be done to correct this problem?*

As the caption in the figures says, "the fluxes increase monotonically with angle (all solid lines)." So all one needs to do is count the lines.

18. *Pg. 10-2. Ref. 19:* *The Kelkar et al. reference title should be changed to "Computational Modeling ... ", instead of "Computer Modeling ... ".*

Typo was corrected.

19. **"On the Validity of the ACOPO Experiments for Natural Convection in Hemispherical Enclosures at High Rayleigh Numbers"** *(see Technical Note in the original — Appendix S)*

As noted under "highlights," this effort by the reviewer is highly appreciated and most welcome. However, in the spirit of continuously deepening the understanding, we offer the following discussion.

The reviewer approaches the problem of quasi-steadiness in a strict/narrow sense. In this sense the cooling rate (he calls it Q) and the bulk-to-wall temperature difference (he calls it ΔT) must remain constant, and if so one has an exact reproduction of the original mathematical problem—i.e., the approach provides an exact analogue. The hope then is that when constancy is approximately observed, the analogue would also be approximately valid. How approximately? We do not know—in a mathematical sense—the problem is non-linear and mathematics abandons us—or rather we must abandon mathematics here—but we are already in a frame of mind that may not put us on the most advantageous path. Both Q and ΔT are varying with time (Figure 1), and we are too insistent on constancy and we find ourselves pushed to operate to a small subregion, near the end of the transient. But here is where data acquisition gets most tricky, errors creep in, and above all how can we be happy applying data obtained with a ΔT of $\sim 2°$C to a case where ΔT's are 100 to 150 °C? Or, could we operate at 5°C, and what error would we be committing? At the beginning of Section 2 the ΔT ~const. is introduced as a "different" constraint (from that of Q ~const.), but as can be seen in 2.1 and Figure 1, the two are, in fact, quite corresponding. So really, the crux of the argument is *judging*, in some way or another, what constitutes a quasi-steady state. We thought that on a surface-to-volume basis alone by going from 1/8- to 1/2-scale experiment we alter the "transient" by a factor of 1/4. The reviewer reasons that instead of $1/R$ an $\sim 1/R^2$ dependence is appropriate (q/R^2), which means a 1/16 improvement. Whichever is the case, the change is very significant, thus providing a reliable test of the quasi-steady state assumption. Thus we formulated the mini-ACOPO/ACOPO strategy, and we will know the results in the near future.

The reviewer judges quasi-steady state by comparing a pool-internal time constant (t_σ) to the time taken to cover a certain Ra$'$ range (ΔRa$'$) in ACOPO. The t_σ is taken as the time needed to reach a new steady state after a step change in Ra$'$ number in an experiment performed with volumetric heating—an equation from Kulacki-Emara was utilized for estimating t_σ. In this experiment (planar layer) only the top was cooled, and the t_σ is time required for the fluid in the bulk to lose (or gain) the excess temperature. But this is a "bulk" process, and has very little to do with the boundary layer that controls heat transfer. This is what we mean in p.2-2, Last paragraph, second and third sentences, which has been questioned as "confusing" by the reviewer. Namely, that the pool-internal time constant is *misunderstood* as representing some sort of transient effect on the heat transfer *behavior*. Which brings us to our definition of quasi-steady state, as one that the cooldown "should be slow enough to allow the process to pass through a series of quasi-steady states that approximate corresponding steady-states with heating rates equal to the instantaneous cooling rates in the experiments." This means that the *boundary layer* time constant is short compared to the pool cooldown time, and that we can *use* the heat transfer coefficients (determined from a cooldown experiment run under conditions that satisfy quasi-steady state—in our definition of it) evaluated on the instantaneous pool temperature to *predict* the transient response of a steady-state pool subjected to a sudden change in Ra$'$ number. It should be clear, now, that t_σ grossly exaggerates the fundamental characteristic time against which pool cooldown time should be compared to judge quasi-steady state. So we come to the conclusion that the mini-ACOPO is not marginal but amply meets quasi-steady state, and that the ACOPO will do even more so.

Note: The boundary layer time constant can be taken roughly as a few characteristic residence times of the fluid in it. For example, using a mean velocity of ~5 mm/s (see Steinberner and Reineke data) and a length of ~40 cm (as in mini-ACOPO), we find a residence time of ~1 minute, and expect a proper value of t_σ to be only ~2 minutes.

T.12. Response to B.R Sehgal (Royal Institute of Technology) — Specific Assignment: Chs. 2,6,7,8,9

General Comment and Highlights

This review is very thoughtful and comprehensive. Even though it is favorable in many respects, it also raises questions about some of the most fundamental aspects of the treatment. Namely, the scenario-independence (bounding nature of the final, steady-state pool), the validity of the ACOPO experiment, and the existence of crusts, as a boundary condition, in the jet-impingement region. The scenario independence was raised by several other reviewers as well. It is addressed in Appendix O. The validity of the ACOPO concept has been questioned by means of several specific technical points, including transient effects on stratification, and on pool response in general, heat flux distribution on the upper surface, and independent Pr number effects. These are all addressed point by point below. Additional light will be shed by the main ACOPO experiment, which is on the way, although we regard it as confirmatory. Finally, about the crust, the basic idea is that the molten oxide solidifies upon contact with molten steel, for any reasonably relevant level of turbulence, and the effective thermal boundary condition is not dependent on crust stability—or growth and remelting. Details are provided in the point-by-point response.

Point-by-Point Response

1. Professor Theofanous and coworkers should be congratulated for writing a beautiful report on the subject, which almost reads like a text book. I honestly enjoyed reading it and appreciate it very much, since the treatment is comprehensive and logical and I learnt a lot. They have performed much original work during the process of resolving this important issue. I do have a number of comments, which will be described in the following text. I will begin by providing general comments on the core melt in-vessel retention (IVR) concept and continue with comments on the document. This will be followed by comments on some specific items in the document and I will state my conclusions in a summary.

General Comments on the In-Vessel Core Melt Retention Concept

Ever since light water reactor severe accidents stepped into the consciousness of the reactor safety physicists and engineers, they are considered synonymous with molten core on the floor (core melt-down), Certainly, there are other detours, e.g. a steam explosion, or a high pressure melt ejection (HPME), which could distribute the core in particulate form all over the containment. Since the severe accident was considered as "fiction" during the years when the currently installed

PWRs and BWRs were designed; the designers just threw up their hands, said "So be it! we will just make the containment strong and let us forget about this melt-down accident".

Since 1979, when the severe accident assumed a little larger reality, the nations of the World, possessing light water nuclear power reactors, have been spending millions of dollars each, every year, on severe accident research to prove (or show with high assurance) that, (1) severe accidents are very rare indeed and (2) even if we get one, and the molten core lands on the floor of the containment, our strong containments (designed with foresight) will hold up long enough to, (a) reduce the radioactive emissions significantly and (b) enable evacuation of the near surroundings. The money spent has produced good results and, except for one or two remaining issues, the case has been made to the satisfaction of most technically-knowledgeable observers, if not to the satisfaction of the public at large.

In the last few years, accident management has come to the fore as a concept to upgrade the safety of the existing and the future plants. Accident management measures for the existing plants have varied from one country to another; e.g. Sweden has installed filtered vents on all of its plants and inerted the BWR containments, while U.S. has only inerted its BWR Mark I and II containments. Accident management has been brought into the design process for the future plants. In particular, the designs of the U.S. passive plants, and of the European pressurized power reactor (EPR), are incorporating accident management features which, hopefully, will provide substantial additional safety margins, so that even the need for public evacuation is virtually eliminated.

The concept of retaining the molten core within the vessel in the event of a severe accident, should be very appealing to both the operators of the plant and the public. Certainly, keeping the radioactivity confined to a smaller volume and not having to cope with an extensive clean-up and decontamination operation, is a very worthwhile goal. The U.S. passive advanced PWR design, the AP600 has adopted this accident management concept, and establishing its feasibility and reliability is the aim of this document. If it is successful in achieving this aim for the AP600, it will open the door for considering this concept for other plants: present and future. Thus, the effort here is a milestone and should be so treated.

I believe, nature is quite partial to the in-vessel core melt retention concept. It has been found that the heat removal, with boiling water on the external surface of the reactor pressure vessel, varies the same way, as a function of the polar angle, as the thermal loading imposed on the inside surface of the vessel by a naturally circulating core melt. Additionally, the maximum heat removal rate at the very bottom of the spherical vessel is substantial, due to the particular boiling mechanism that nature prefers there. If these two natural occurrences were not so disposed, the concept of in-vessel core melt retention could not materialize into reality.

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2. *Comments on the Document*

I must start my comments on the document with Table 7.3. I was surprised to find that only 30% of the accidents contributing to core damage frequency (CDF) are relevant to IVR. Again, I was surprised to note that 23% of the CDF is caused by vessel rupture for which no accident management can be provided and 18% of the CDF is related to high pressure melt ejection (HPME). Perhaps, the ARSAP program should also target these two events, i.e. vessel rupture and the ADS failure, and provide reliable prevention strategies, so that their probability of occurrence is substantially reduced. There may be a greater potential of early containment failure with these hazards, than it may be with a few tonnes of oxidic and metallic melt discharged at very low pressure, into the water pool surrounding the vessel.

Actually, the numbers in Table 7.3 should be understood in the perspective of a core melt frequency of $2.5 \cdot 10^{-7}$ per year. This exceedingly small value is a consequence of the passive emergency core cooling design, and many other attractive safety features of the AP600. That the order of magnitude of the other core melt classes has approached that of the vessel rupture is an indication that prevention of core melt has reached its "natural" limits. In any case the high pressure scenarios appear to have been exaggerated in the original PRA, and have been revised significantly downwards in the most recent version, submitted to the NRC on March 10, 1995. On the basis of this new information, Table 7.3 and the discussion of IVR scenarios are revised in an addendum to Chapter 7.

3. *The ULPU experiments conducted by Professor Theofanous and co-workers have provided definitive data on the CHF for the external surface of the vessel;*

and the CYBL experiments, conducted by Chu et.al., have provided the visual evidence for the substantial heat removal rate at the very bottom of the reactor vessel external surface. I believe, that the heat removal rate aspect of the IVR is quite well assured, and the uncertainties are low, except for the actual AP600 physical design details. In particular, as the authors state, the physical design has to allow sufficient area for the steam produced from the cavity to flow to the containment dome; and the insulation on the vessel has to allow a steady access of the water to the vessel external surface. The flow area and the water access have to be assured throughout the life of the plant and, thus, may be subject to the maintenance and in-service inspection regimens conducted on the plant.

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4. The authors have performed an excellent job on defining the thermal loading on the internal surface of the vessel, however, the situation is not as clean, as it is, for the heat removal on the external surface of the bottom head. The basic misgiving, in my mind, is that the authors have assumed an end state of the melt pool and, thereby, an independence from the core melt scenario, which ignores the intermediate and the transient states, which may impose greater thermal loading on the vessel inner surface. I accept the authors argument that the thermal loading due to a purely oxidic pool would be scenario independent. However, when a metallic layer on top of the oxidic pool provides "focusing"; the authors, themselves, have identified an intermediate state with a 1.18 meter deep oxidic pool and a 0.22 meter metallic layer, which results in larger thermal loading than the assumed end states of the oxidic pool (1. 5 m to 1.6 m depth) and the metallic layer (0.9 to 1.0 m high).

Actually, this case was identified as "arbitrary parametric" and was used only to provide some perspective on the extreme limit of the focusing effect. Moreover, even at this extreme, the actual margin is more than illustrated in Figure 7.16. In any case, more consideration to transient scenarios can now be found in a new Appendix O.

5. I have sorely missed an appendix or a section, on the core melt progression assumed. Clearly, the knowledge-base on the later phases of the melt progression is poor and some assumptions have to be made. If I follow the relatively better known scenario, the first discharge of the melt to the lower plenum (full of water) would be like that in TMI-2 i.e. in the range of 20 to 40 tonnes, (Appendix H actually assumes 47 tonnes, while section 8 assumes 22 tonnes). Next, if it is *assumed that a certain fraction of the melt jet fragments and the steam generation leads to lowering of the water level, and to greater melting in the core region, there could be a release of metallic constituents from the core bottom followed by the release of the remaining oxidic material from the melt pool established within the original core boundary. Now, this is not an unlikely scenario, which could result in an intermediate state of a three-layered pool (less than 1.4 m depth) with a metallic layer sandwiched between two oxidic layers. This may lead to the condition, investigated by the authors, in which the thin metallic layer has an adiabatic boundary condition at the top surface; which was found to result in much greater thermal loading.*

As explained in Appendix O, failure of the lower blockage within the time frame of the core relocation scenario is not possible.

6. Another point is that a fraction of the Zirconium metal released may be in the form of U-Zr eutectic, which may generate some decay heat in the metallic layer.

See Appendix P, Olander's item #6 and our response, and Appendix R.

7. The role of water in the lower plenum in quenching the melt discharges, the timing of its complete evaporation, and the subsequent remelting and layering of the pool, are all undefined. I would also prefer to leave them undefined, if I would be certain that the thermal loading during the intermediate states is always less than that in the end state. The authors have established thermal margins of approximately 100% for the most probable end state; perhaps, some scenario dependence could be considered and thermal margins investigated for some plausible intermediate states.

See Appendix O.

8. The thermal loadings on the internal wall of the vessel have been determined for the final stable state of the melt pool natural convection. Nature is quite kind in the final stable state, since the stratification in the lower levels of the melt pool reduces the convective heat flux to that transmitted by conduction. In the transient states leading to the final stable state the stratification may not be fully established and the heat fluxes near the pool bottom may be higher. This has been recognised by the authors as an "open issue", on page 5-10, and it primarily *affects the thermal margins established for the lower reaches ($\theta \leq \pm 15°$) of the vessel. An evidence of this is also in the Figure D.5, forVi/V near zero and near 0.06, where the ACOPO quasi static (along the cooling transient) dimensionless pool temperatures show a variation of a factor of $\equiv 2$.*

The four items enumerated on p. 5-10 are establishing the *need for* the ACOPO experiment, and *not the consequence* of having it. Thus, in particular, item 4 (transient effects) is first identified here as an "open issue," and in subsequent pages it is addressed by the mini-ACOPO test. It will be further addressed by the ACOPO test, but as mentioned already, this is only confirmatory. As far as the variation exhibited in mini-ACOPO, it is now addressed in an addendum to Appendix D. A footnote to Figure 5.8 was also added so that this addition is not missed.

9. Perhaps, further investigation of this "open issue" could be performed through a perusal of the data from the COPO and the UCLA experiments; and also through calculations of the transient natural convection states leading to the steady state.

The UCLA data are shown (on Figure 5.8) to be well bounded by our data. The COPO data (shown in Figure B.7a,b) show that the local fluxes go to zero as $\theta \rightarrow 0°$, so they also are bounded. However, *none of these* data sets provide information on transient states, *while the mini-ACOPO does.*

10. The technique used in the mini ACOPO experiments, and to be used in the ACOPO experiments, is unique, since the experiments related to IVR, performed by all the other investigators have employed volumetric heating. The ACOPO technique makes the experiment very simple and if it is valid, it really advances the state of the art of the experimentation in this area. I believe, the data obtained has been expressed in the form of correlations developed for the volume-heated experiments by using the cooling rate as equivalent to the heating rate. It seems to follow the same correlations as did the volume-heated experiments, except, perhaps, there may be some differences. The mini ACOPO experiment, having a reasonable volume, seems to reach a stable state within minutes, whereas in the COPO and the other earlier volume-heated experiments, it took much longer time.

There is a fundamental problem with the reviewer's interpretation here. We have anticipated such difficulty and tried to help it with a note near the bottom of p. 2-2 ("What occasionally has been mentioned as a slow approach to steady state is really attributable to the thermal capacitance of the pool rather than to unsteadiness in the natural convection process. In particular, in Appendix D we demonstrate that boundary layer effects dominate, so that the behavior of such pools is readily predictable even under non-stationary conditions. What this means is that the thermal loads to the pool boundaries throughout the time period of a heat-up transient are bounded by the thermal loads in the final steady state.") In fact, comparing the time needed for steady states in volume heated pools, to the time constant of the natural convection process is like comparing apples and oranges! See also our response to Schmidt—Item #19.

> 11. *Figure D-9 shows that in the cool-down pool the upward heat fluxes in the center half of the pool are approximately 20% higher than those in the outer half of the pool Such spatial profiles were not measured in the internally-heated pools.*

It is not clear what the reviewer is referring to by "internally-heated pools." Only the average value is available from COPO, and only one value, on the outer portion of the upper boundary is available from the UCLA experiment. On the other hand, the difference between the inner and outer regions in Figure D-9 is *only 15%* or less (not 20%). This cannot be considered very significant for the kind of process/problem considered here.

> 12. *Instead, unsteady wave-form and dynamically changing upward heat fluxes were measured. Perhaps, the natural convection system with volume-heating is much stiffer than without it, and it may be that the transient nature of the cool-down experiments, driven only by the boundary conditions, is different than the unsteadiness of the internally-heated turbulent liquid pool. Periods of unsteadiness in internally-heated pools are in range of 3–10 minutes and it may take many periods before the flow structures shown in Figure 5.2 are established. Do such flow structures get established in the cool-down pool within the few minutes needed to reach the steady state? A demonstration of the cool-down pool natural circulation, as the same as that in the internally-heated pool could be through the measurement of the flow structure in the cool-down pool.*

This is speculative, and not consistent with available data. The reviewer refers to the oscillations in Figure C.10 which are *minuscule*, and would be smaller still at the higher Ra' numbers of interest here. Some oscillations are actually expected as plumes form and detach from the boundary layer, and some oscillations are in fact seen in mini-ACOPO. These are all, however, second order effects, and the data themselves show that the first order flow patterns are established in a matter of seconds. Indeed, the flow structure in the mini-ACOPO pool is directly evident from the stratification patterns already shown in Figures D.4 and D.5. It was an omission to not include "time" in these figures. We have now done so in an addendum to Appendix D. Also, it should be noted that the first few minutes marked "unstabilized flow" in the energy balances refers to the transient behavior of the cooling jacket itself, and *not* to the natural convection process within the pool. To avoid a chance of misunderstanding, we have now made that explicit in the caption of these figures as well. Using the bypass flow, this was minimized in run A16, and as shown in Figure D.8, the data are consistent from the first minute on. The time-wise development of stratification is now added as Figure D.21, and should help the reader appreciate how rapidly the internal patterns develop.

13. On page 5-3 of the report, it is stated that the natural circulation in a pool, with no volumetric internal heating, obeys the correlation $Nu = F(Ra, Pr^m)$*. Perhaps, the results of the cool-down experiment could be correlated through this correlation; and the upwards and downwards heat fluxes obtained compared with those obtained through Equations (5.12), (5.28) and (5.30). I do not know whether this is a fruitful approach, however, it may provide some insight.*

We do not think this is a fruitful approach, since it would be recasting the same information.

14. The heat transfer correlations obtained in the document do not have any dependence on Pr number, and the experiments performed for fluids having Pr number between 2.6 and 10.8 confirm that. (Cf Figure 5.4) Calculations performed recently by Dinh et.al., to be reported in the NURETH-7 meeting, show that the heat fluxes do not change significantly for Pr numbers between 2.0 and 10.0, but at Pr = 0.6, the downward heat flux increases considerably, while the upwards heat flux decreases slightly. This calculated result is for the laminar natural convection pool ($Ra = 10^{11}$*) and its applicability to highly-turbulent pool is not assured. However, there may be merit in investigating the regime of Pr number below 2.6. The stably stratified flow patterns near the bottom the vessel may be different for the low Pr number fluids, and that may change the heat flux to the very bottom regions (*$\theta \leq \pm 15°$*) of the reactor vessel.*

This is speculative also, and not supported by the data and what we know about convective heat transfer. First of all, as shown in Eq. (5.5), the Ra' number actually incorporates the Pr number. There is no apparent a priori reason to expect an independent (additional) Pr number dependence, and what we know from limited experiments with liquid metals ($Pr < 10^{-3}$) such dependence is extremely weak ($n < 0.1$). We felt compelled to conduct a special investigation, as a caution due to Eq. (5.13). We showed that a five-fold decrease in Prandtl number has no effect. The reviewer feels that an additional four-fold decrease (from 2.5 to 0.6, which is the value for corium) can have a significant effect. There is no physical reason to expect such a sudden change in trend. This is consistent with the final version of the paper cited by the reviewer (apparently a newer version than the one available at the time of the review), depicting less than a 20% effect on local fluxes, near the bottom, for a Pr number change from 7 to 0.6 (semicircular geometry, $Ra' = 10^{10}$). The effect is already negligible for our consideration, and it would be even less for the highly turbulent flow of interest in our case ($Ra' \sim 10^{15}$).

15. The attack of the vessel by the impingement of a melt jet has been discussed in section 8 and in the Appendix H, with different approaches. The section 8 approach employs Saito's correlation and derives a curve for the vessel ablation depth vs. jet diameter. It uses a melt volume of 2.5 $m^3 \equiv 20$ tonnes and for a jet diameter of 10 cms obtains the ablation depth of 12.6 cms. The Appendix H, on the other hand, uses a melt mass of 47 tonnes and melt jet diameter of 4.8 cms to arrive at the ablation depth of 12.4 to 13.6 cms. If the section 8 analysis is redone with 47 tonnes melt mass and melt jet diameter of 4.8 cms, the ablation depth will be larger than the vessel wall thickness and no pool will form in the lower head.

Appendix H was provided for some additional perspectives *on the margins to failure*, and not to have its arbitrary use of 47 tons be combined with the much more limiting analysis of Chapter 8.

16. Both the section 8 and the Appendix H evaluations assume the formation of an oxidic crust on the vessel wall. Thus the ΔT, for the heat transfer, is respectively 200 and 165 K. This is correct if the crust formed is stable and not swept out by the jet action. The jets are highly-turbulent with Reynolds numbers in the range of 3 to 5×10^5, and the survival of the crust in this regime may not be easy. The crust existence could be estimated by comparing the characteristic times for the convection-controlled crust growth, the remelting of the crust and the convection-controlled residence. The remelt time at the heat flux of 6 MW/m^2 may be much longer than the crust growth time, however, the convection-controlled residence time may be less than 0.01 sec. Perhaps, the crust may exist at the peripheral parts of the jet impingement zone, but not at its center.

This is speculative also, and counter to existing data. The reviewer is referred to Saito et al. (1990), already referenced in the report, and to Epstein et al. (1980), referenced by Saito. In the report we discussed also the applicability of these data (i.e., Re up to $3 \cdot 10^5$, Pr$\sim$ 1). The key point is that by crust we do not require the macroscopic existence of the crust, nor any thermal resistance associated with such macroscopic crusts. Rather, the rule is to impose a thermal boundary condition, at the melt liquidus, and for this a dynamic creation and washout (by the ablating melt beneath) of microscopic crust pieces is sufficient. Basically, what happens is that the melt freezes on contact, and it is totally unimportant that the resulting crust is washed out moments later, as a new one forms immediately. The data show that *very clearly*! And, as noted in the report already, with turbulent convection (with 10 cm and 5 m/s jet diameter and velocity respectively) even a hot steel substrate ($\sim$1200 K) requires an oxidic melt temperature of over 4400 K before the regime changes to one without crusts.

> *17. I believe that both the Section 8 and the Appendix H evaluations of the ablation depth, due to melt jet impingement, are overly simplistic and, perhaps overly conservative by not considering the presence of water. It is true that large scale data on this type of configuration is non-existent and the estimates made can not be validated. Nevertheless, the estimated made in Section 8 and the Appendix H are so close to the vessel wall thickness that one is left wondering about the seriousness of the jet impingement hazard, inspite of the fact that in the TMI-2 accident 20 tonnes of oxidic melt having a substantial superheat did not damage the vessel.*

First of all, Appendix H does consider the effect of water. Second, the whole idea of both Chapter 8 and Appendix H was to provide two complementary perspectives of how hard it is to see real physics doing the job of melting through the wall, even under some extreme conditions of impingement. This, of course, agrees with TMI and makes it hard to accept the "simplistic" characterization applied to these analyses by the reviewer. We accept the "perhaps overly conservative" characterization, but then, why is the reviewer concerned that the results obtained "are so close to the vessel wall thickness"?

> *18. The authors have not considered phase change in their evaluation of heat fluxes, particularly where crust or vessel wall melting may occur. This certainly will complicate the evaluation, however, many times the phase change reduces the heat transfer, due to the needed heat of fusion, and the changes in viscosity that may occur at the melting surface. Perhaps, an estimate of this effect could be made.*

This point is not relevant to the evaluation. At steady state, as analyzed here, there are no phase change effects on the heat fluxes. This may be conservative during the transient, but the time constant of these processes is much shorter than the time scale of the accident scenario, steady state will certainly be reached, and therefore its consideration does not constitute an undue conservatism.

19. Comments on Specific Items

Section 5, Pages 5-8 and 5-9. The Kelkar calculated correlations of $NU_{up} = 0.18$ $Ra'^{0.237}$ and $Nu_{dn} = 0.1\ Ra'^{0.25}$, both under-predict the values of the Nu numbers at $Ra' = 10^{10}$, when no turbulence model should be involved. Kelkar correlation gives $Nu_{up} = 42$ and $Nu_{dn} = 32$, while the Steinbemer-Reineke measured correlation provides $Nu_{up} = 74$ and Eq. 5.22 provides $Nu_{dn} = 55$. I believe, there is something wrong with the Kelkar calculation. It does not matter that at $Ra' = 10^{15}$, the values of Nu_{dn} from Kelkar and Mayinger correlations are only 2% different. I believe, the calculated "correlations" should not be put in the same "pot" as the measured data. In fact, I believe, that discovering the correct turbulent eddy-diffusivity model, which will be valid for the experimental and the prototypical conditions (melts, geometrics etc.) would be a great achievement.

While it is not our job to defend the Kelkar et al. calculation, it is the only one around, and it would be an omission to not include it with all other relevant information on Figure 5.7. We think the comparison is interesting, and we did point out the discrepancy of the same calculation with the flux data in the upper boundary. In the same vein, we should not forget also that the Mayinger correlation was derived from calculations, and it suffers from the same difficulties in the upper boundary. All this may point to the fact that near-vertical boundaries are easier to calculate than horizontal or near-horizontal—see also comparisons of the shapes in Figure 5.8. This makes sense physically also if one thinks about the more intricate nature of turbulence source/sink terms near such boundaries.

20. Section 5. Pages 5-16 and 5-17. Specialising Eq. (5.35) to two boundaries with equal temperature drop, one would obtain

$$h = 0.059 \cdot 2^{1/3} \cdot \left(\frac{g\beta}{\alpha v} \cdot \Delta T'\right)^{1/3} = 0.074 \cdot \left(\frac{g\beta}{\alpha v} \cdot \Delta T'\right)^{1/3}$$

which is different from Eq. (5.41). This probably is a typo, or I do not understand the text before Equation (5.41).

No. Equation (5.41) is correct. There is a 2 also that comes from the left-hand-side (for h write $q/\Delta T = q/2\Delta T'$).

> 21. *Appendix H. In this appendix, Table 2 provides Reynolds numbers for the melt jet as 260,000 to 480,000, which signify that the jets are turbulent. However, the correlation of Swedish used for determining the Nu number is for laminar jets. If Martin's correlation* $Nu = 0.606\ Re^{0.547}\ Pr^{0.42}$*, appropriate for turbulent jets, is employed, the value of Nu number for the second case in the Table 2 would be 776 instead of 560, which would lead to an even greater vessel ablation rate.*

Actually, for turbulent jets the correct correlation (supported by relevant data) is Saito's, as used in Chapter 8. We do not know where the reviewer found Martin's equation, but it is not appropriate for this problem. Also note that at a Re number of $5 \cdot 10^5$ Martin would produce a non-conservative result by more than a factor of 2. The assumption in Appendix H is that the melt jet exits the pool under insufficient shear to create any appreciable level of turbulence. Again, Appendix H is to provide a complementary perspective to Chapter 8, and the 47 tons of discharge utilized in it are way too much material for any realistic accident scenario—See also Appendix O. Moreover, as the next question and response indicate, there is a huge conservatism on the time duration of the pour.

> 22. *I believe the impingement time in Table 2 is too long. The analysis does not consider ablation of the hole of 4.8 cm through which 47 tonnes of melt is being poured into the vessel. The hole size will increase by factor of 5 or more, increasing the jet size, reducing the impingement time and the vessel ablation.*

This is indeed correct, and all the more reason to appreciate that penetrating the wall with an oxidic jet is physically unreasonable. See also Appendix O.

> 23. *Appendix L. This is a very valuable compilation of the relevant thermophysical properties. The viscosities shown for* $U0_2$ *and* ZrO_2*, and the rules for the mixtures, are apparently valid only for the liquidus state i.e., above the melting temperature. Is there any data or equation to evaluate the viscosities for temperatures between the solidus and liquidus. The boundary conditions at all the inside surfaces of the vessel are in that uncertain temperature range between the solidus and liquidus, where the properties will affect the heat transfer rates.*

This is not of real interest to the natural convection process. The slurry layer allows for the temperature to go from liquidus to solidus, and the slurry layer exists all around the boundary.

The natural convection process "sees" the liquidus as an isothermal boundary condition. The slurry layer can be seen as a largely immobile thin region, and together with the crust makes up an effective crust. See also the response to Henry item #2.

> *24. The densities for the metallic mixtures are not too different from those for the corium. Once the natural circulation starts, it may be difficult to separate out the metallic components from the oxidic components in the corium and have them join up with the metallic layer on top.*

A naturally convecting pool is not the same thing as one participating in a corium concrete interaction. Also, steel cannot remain suspended in a superheated oxidic pool, as it will have to boil away, if not separated.

> *25. Appendix N. In the run no A-2 in Table N-2, T_{li} should be 75.6 instead of 25.6. This is a typo, I am sure.*

Yes. Typo corrected.

> *26. On page N-5, it is not clear which two equations were solved for T_b and T_{ij}.*

The first two, Equations (N.3) and (N.4). Clarification made in text.

> *27. Summary*
>
> *I believe, Professor Theofanous and his colleagues have written a very beautiful document on the subject of in-vessel retention of core melt in the event of a severe accident. I believe, IVR is a very important issue for future nuclear plants in which accident management should be directly integrated in the system design.*
>
> *Professor Theofanous and colleagues have considered most every aspect of the in-vessel core melt retention issue and have endeavoured to address the phenomena that are active in the process of retaining the melt in the vessel. Their emphasis is on providing data and models which illuminate and describe the physics of the various processes occurring and then integrating all the various sub processes to emerge with the assessment of the margins. This is the essence of the ROAAM approach, and in this case it actually is much more straight forward than in the case of the issues of the BWR Mark I liner melt-through and the Zion PWR direct containment heating (DCH) loading, which Professor Theofanous helped resolve earlier through USNRC sponsored research efforts. The experimental backing for*

the correlations and the models employed, in this document, to arrive at the thermal loading, and the maximum heat fluxes allowed, is also much more extensive than it was for the Mark-I liner melt-through issue. Professor Theofanous and colleagues have themselves performed original research and provided key data, on the CHF at the vessel external surface and on the heat fluxes on the vessel internal surface.

* *

28. I have made several comments on the evaluations employed in the document. I believe, some of the questions asked are important in providing greater depth and validity to this document for the resolution of the IVR issue. My major question is about the possibility of getting a smaller thermal margin in some intermediate and transient state before reaching the final stable state, where there is an ample margin to accommodate the thermal loading imposed. I have also asked some questions about the ACOPO experimental technique, which I believe is unique and ingenious, however, should be qualified by, perhaps, measurement of the natural convection flow patterns. The evaluations of the jet impingement thermal loadings, and the vessel ablation-depth estimates, are not as complete as one would wish and, perhaps, the authors could strengthen those analyses. Some other points have also been raised e.g., the Pr number dependence of the downward thermal loadings, the effect of the phase changes at the boundaries on the heat fluxes etc.

(a) We hope the additional discussion on transient effects provided above, and of intermediate scenarios in Appendix O, has fully addressed this area of the reviewer's concern.

(b) We explained that the questions raised about the ACOPO technique are not valid, and that the stratification patterns measured provide a good indication of the natural convection flow patterns, and their appropriateness to the problem at hand. More confirmation will be obtained from the 1/2-scale ACOPO in the next few months.

(c) The analyses for melt impingement were purposely made such as to clearly bound the behavior and provide an overall perspective on the margins to failure—which are very large. As explained above, we do not agree with the reservations expressed.

(d) All these other points have been addressed above too.

29. Finally, I believe, that the authors have based their case for the high thermal margins available during the in-vessel core melt retention for the AP600, primarily *on the data measured in the ULPU and ACOPO facilities. It would be highly instructive for the reviewers to observe a key experiment, or two, in each of these facilities and examine the instrumentation and the experimental procedures. This will lend much greater confidence to the peer-review process.*

This is a reasonable request and we plan to invite all those reviewers wishing to see the facilities in the near future.

T.13. Response to J. M. Seiler (CEN-G) — Specific Assignment: Ch. 4

General Comment and Highlights

We appreciate the comprehensiveness and depth of this review. There is general agreement, but a number of issues are raised as well, especially on the intermediate states (transient aspects of scenario), for which several suggestions are made, and the impingement of metallic jets during relocation. These issues are addressed in Appendix O. Several other points of a more detailed nature are discussed in the point-by-point response. We trust these will be found adequate.

Point-by-Point Response

1. *This is clearly a very important work containing very pertinent and new data. Comments will address different parts of the work.*

* *

2. **I) Comments concerning Scenario examinations:**

In the document, 2 scenarios are considered in the "thermal regime": the stratified pool and the melt jet impingement.

It is considered that the stratified pool is the worst (i.e.: the most conservative) situation, without any discussion In fact other situations may be emphasised and should be, at least, discussed to rule them out.

I-1) A first kind of (different) situation may be linked to the existence of a debris bed with molten metals within it. The assumed scenario is the following:

a) *The melt (oxydes+metals) flowing from the core is quenched in the water present in the lower head. The quenched melt forms a debris bed with rather large particles in it (say between a few millimetres to a few centimetres).*

b) *The residual water is evaporated and debris begin to remelt,*

c) *The materials which remelt first are metals (mainly Stainless Steel & Zr).*

d) *These molten metals may migrate within the debris bed and accumulate in the lower part of the debris. Only the porosities are filled with the liquid metal, the oxydic debris staying as solid debris within the molten metal (higher density). Thus rather large heights of such "porous pools" may be emphasised with a rather low metal inventory.*

e) *The decay heat produced by the metals and the oxydics debris is transported to the boundaries by natural convection of the metal throughout the porous medium. The temperature of the molten metal is expected to stay rather low and depends on the composition of the metal (say less than 1800° C). At such low temperature it may be expected that the dissolution of the oxides by the metals is low (Stainless Steels does, indeed, not dissolve ZrO2 or U02 at these temperature levels).*

f) *The oxides situated above (but out of the molten metal-solid oxides pool) remelts later (due to their higher melting temperature). The molten oxides will enter into contact with the low temperature "porous metal-oxides pool" and form a crust at the interface which relies on the solid oxydic debris situated below.*

g) *Under these (inversed) conditions part of the power dissipated in the overlying high temperature oxydic pool may deverse downwards into the low temperature "porous pool". Thus this lower pool will have to evacuate not only the decay heat dissipated within it but also part of the power dissipated in the overlying oxydic pool.*

h) *Under these conditions, two flux peaks may appear; the first near to the upper surface of the "porous pool", the second later (in time) and above (in height) due to the oxydic pool.*

Such a situation has been observed in the in pile SCARABEE experiments. It does not seem to me to be unrealistic for LWR accident situations.

It is not clear to me whether this situation is enveloped by the situation considered in the report.

The metals circulating through a porous oxidic matrix is possible in the presence of a large quantity of metals in the lower plenum, as in BWRs, and, in fact, it has been considered previously in such an application (Theofanous et al., 1991, NUREG/CR- 5423). This is not the case for the AP600, and as explained in Appendix O, the relocation will be basically oxidic. Interestingly enough, we also predict a "layered" situation during the transient evolution, however, under conditions that would prohibit the formation of highly loaded (thermally) thin metal layers. As always, the thermal loads from the oxidic pool are modest.

Also, we should draw attention to the aspect of the reviewer's scenario involving downwards loading from the upper oxidic pool onto the lower "porous pool." Such downwards heat transfer has to be basically conduction-controlled (through the lower crust of the upper pool). As such, while it may be significant at small scales (such as the SCARABEE tests), it is totally unimportant at reactor scales.

3. *I-2) One may also emphasise scenarios leading to debris beds (with water present in the lower head) and with local remelting producing localised hot spots onto the lower head. The heat fluxes to the vessel will be of course much lower than the heat fluxes related to a molten pool situation. It seems from the TMI 2 VIP investigations that the mechanical loads induced by the hot spots on the lower vessel head do not endanger the vessel integrity. This may also be true for AP 600 but should at least be mentioned.*

The TMI reference is especially to be emphasized, because it occurred in the *absence* of external cooling. The consideration offered by the reviewer is welcomed for completeness.

4. **II) Stratified Pool situation:**

II-1) A presence fraction of more than 50% in mass of ZrO2 in the oxydic phase would inverse the stratification if the metal is mainly Stainless Steel. This would correspond to 100% oxidation of Zr and less than 30 tons of molten U02 (less than 40% of core inventory). Has this situation to be considered ?

No. 100% oxidation is not credible, and in combination with only 30 tons (out of 75) of UO_2 in the lower plenum, is impossible. See Appendix O.

5. *II-2) No heat flux profile has been considered for the metallic pool. This should be justified since the margin to critical heat flux is relatively low in some cases (fig 7.16 and 7.15 for adiabatic conditions).*

This is a valid point, and an omission on our part. It is discussed now in an addendum at the end of Chapter 5. No significant impact results.

6. *II-3) Physico-chemical reactions between the metallic pool and the vessel may lead, potentially, to low interfacial temperatures with the vessel wall in the metal pool layer. This may increase the lateral heat flux when there is no metallic crust formed at the surface of the pool or in the presence of a thermal resistance at the surface of this pool. It may perhaps be argued that the interface temperature is not expected to drop below 1500°K (which is considered as boundary condition*

for the calculations) considering that the mole fraction of Zr in the metallic layer does not exceed 50% (according to phase diagram presented in fig 6.1).

Yes. This is how we picked the 1335 °C eutectic, as explained on page 6-1.

7. II-4) Nothing is said concerning the evacuation of the heat flux released at the top of the pool.

This heat flux is expected to melt a variable part of the in-core structures; but what happens afterwards ? Is this power diverted to the upper part of the vessel ? What would be the related heat flux distribution ? May the heat flux discontinuity at the metal layer surface induce unexpected buckling *of the vessel ?*

The heat flux released from the top of the pool is treated by the conduction/radiation path described in the original report at the bottom of p.6-2 and top of p.6-4. The resulting temperatures in that upper region are now displayed as part of "detailed results" summarized in Appendix Q. Rather than "buckling," the proper term here is "ductile tearing"; it is now considered in an addendum to Chapter 4.

8. II-5) Presence of aerosols may decrease the heat transfer by radiation from the pool surface. But this is bounded by the adiabatic conditions.

Yes, and moreover, natural convection and precipitation mechanisms would help keep the atmosphere clear.

9. II-6) The correlations presented in section 5-1 of the report are qualified on experimental results coming from COPO and mini-ACOPO. In the report describing the COPO experiments (appendix B) it is indicated that a thin layer (O.1 mm) of Teflon is used as electrical insulator over the cooled walls. This layer represents a thermal resistance of, about, 4E-4 m^2 K/W. The thermal resistance due to the boundary layer flow in water is estimated to be, about, E-3 m^2 K/W. This means that the Teflon layer represents, about, 30% of the total thermal resistance. (This is an order of magnitude as the local thickness of Teflon may vary). Thus the validation of the correlations against these experimental results is questionable within an uncertainty range of, about, 30% which is quite important and which weakens some other considerations (for instance concerning the way the physical properties must be estimated).

Furthermore, the electrical insulation of the top cooling plates is made of alumina which has a high thermal conductivity at low temperatures. Thus, the thermal *resistance related to this alumina insulation layer may be lower than the thermal resistance due to the Teflon layer. This may lead to a non prototypical increase of the heat transfer to the top and, consequently, to a decrease of the lateral heat fluxes. May these effects be quantified and included in the uncertainties ? What would then be the consequences on the lateral heat fluxes in the reactor situation ? (a little increase of the lateral heat flux in the region of the oxydic pool may not endanger the vessel and a decrease of the power diverted to the metallic layer may increase the safety margins ?)*

The COPO experiment, with a slice of torospherical shape, was not used in any direct way in the analyses presented in the report. Rather, it provided important background support on the group of correlations by Mayinger and co-workers, which, in turn, provided the *entré* to the correlations employed in conjunction with the mini-ACOPO data for use in our work. It should be noted, however, that the effect of the Teflon resistance was compensated by *local* adjustment of water flow rates in each of the colling units, so as to obtain an isothermal boundary within the tolerances discussed in the paper. So, the 30% "error" quoted above is *not* appropriate.

10. II-7) Heat transfer in the metallic layer

The Correlations presented in the report (pages 5-16 and 5-17) are valid for fluids having a Prandtl number higher than 1 and have been validated on water experiments MELAD (Pr about 5 to 10). We also know from the work on LMFBRs that correlations valid for low Prandti numbers (sodium, Pr about 0.005) are based on the adimensional group $GrPr^2$ rather than on GrPr. Steel has a Prandtl number which is intermediate (about 0,1). Thus we ask about the validity of the correlations used for the metallic layer and we are not convinced that experiments performed with water are representative. But the main question concerns the heat flux distribution, and a different choice of correlation may perhaps not affect this distribution. Could a sensitivity study be performed to check this point ?

Actually, the correlations discussed on pages 5-16, 5-17 encompass the Prandtl numbers of interest here. Namely, Eq. (5.34) is reported to be valid for $0.02 < Pr < 8750$ and the Eq. (5.39), which we use, is valid for *all* Pr numbers. The water experiments were performed to demonstrate the separation of the Globe-Dropkin correlation into 2 boundary layers and the integration with Churchill and Chu for an integral model of the pool. With this background we wouldn't be able to imagine what an appropriate sensitivity study would be, but we are open to suggestions.

11. **Mini-ACOPO:**

II-8) The definition of the Ra′ number based on the transient approach is not given. From the text we understand that this number is based on the thermal inertia of the liquid and on the cooling rate ?

Yes.

12. *II-9) The intemal Rayleigh number (Ra′) is much more sensitive to the scale (power 5) than to the temperature difference (power 1). Thus it may be expected that small scale experiments privilege laminar boundary layer flows on the side walls which are not prototypical of reactor conditions.*

Agreed. This is why we are running the 1/2-scale ACOPO, too. But we expect results to be confirmatory, because the Ra′ numbers in the mini-ACOPO were large enough already ($Ra' \sim 10^{15}$), to place it well above the transition ($Ra' \sim 10^{12}$).

13. *II-10) For high temperature differences, how are estimated the physical properties which are involved in the Adimensional numbers ? Are these properties also estimated at "film" temperature ?*

Yes. This was discussed at the bottom of p.5-3.

14. *II-11) I am not sure that the transient approach is representative of all cases with internal heating. For instance in the situation of a homogeneous pool with an adiabatic upper boundary we have observed an overshoot in the pool temperature nearby the adiabatic surface in the BAFOND experiments (volume heated)(Ref 1). Overshoot means that the temperature increases much just below the adiabatic surface due to the stagnation condition. This temperature increase may induce heat flux peaking at the top of the cooled sidewalls. Such effect is specific to volume heating conditions and may not be observed in a transient pool experiment.*

→ I would suggest that an analysis of the representativity of transient cooldown experiments and related quantitative scaling should be included in the paper (also in relation with remarks II-7 and II-8).

A top adiabatic boundary leads to a fundamentally different behavior. See, for example, flux shapes in Figure D.19. The key behaviors in volumetrically heated pools, with isothermal boundaries, are quite well known, and mini-ACOPO reflects those key features accurately. More information on this point is given in the comments of Schmidt, and our responses to them (item #19).

15. II-12) Figures D-12 and D-17 from appendix D suggest that the heat flux distribution is not uniform in the upper isothermal (as suggested by Fig D.15) region. This has not been observed on the COPO experiments (at least no strong effect was observed). Is this related to a scale effect ? (usual heat transfer correlations for turbulent boundary layers suggest that the heat exchange coefficient does not depend on the distance).

If this observation is extrapolated to the metal layer have we thus to consider a heat flux profile in this layer ?(see also remark II-2) (This would reduce the margins to failure).

From Figure B.6a,b it is hard to discern any trends in COPO due to the rather large data scatter. With values ranging up to almost a factor of 2 it is not immediately clear how to compare with Figure D.15. Moreover, the mini-ACOPO boundary is not vertical even at high angles, and scale could also play a role. We expect to resolve these points with the large ACOPO. The steel layer, as already mentioned, is now discussed in this respect in an addendum to Chapter 5.

16. **III) Thermal loads under jet impingement:**

III-1) Only oxydic jets are considered. Why have metallic jets coming from the core been outruled ? Are such jets not credible ? Metallic jet would much more endanger the vessel integrity The EROS tests at KfK have shown very fast ablation for Iron jets impacting on a Steel plate.

This is a valid point, and an omission on our part not to discuss it. It is now discussed in Appendix O.

17. III-2) The calculations presented in the report for oxydic jets make the implicit hypothesis that the crust which forms on contact with the vessel is stable. The stability of the crust has been observed in the tests performed by Saito with Salt and Tin plates. However there is no general agreement, to my knowledge, on this point (the durations of the tests performed with real materials have not been sufficient to come to a clear conclusion). The stability of the crust may depend on several parameters such as:

- *the temperature of the oxydic material (we estimate that the crust may survive several seconds for a 100° C overheat but less than 0.15 second for a 500 ° C overheat)*
- *the inclination of the wall (the FARO BLOKKER test n°1 (molten U02 jet on an inclined plate 5° from vertical) has shown ablation of the plate).*

Even if the crust were unstable, the instantaneous freezing would establish the thermal boundary condition, and this is what matters. The 100 °C superheat rather than the 500 °C one is pertinent to this problem. The FARO BLOKKER tests were all rather benign. The 5° one showed slight ablation only. No analysis was presented that contradicts our, or Saito's, approach.

18. III-3) The inclination of the wall would also impede the occurrence of the "pool effect" (accumulation of molten material in the eroded cavity inducing a reduction of the heat transfer).

→ Thus, I am not convinced that the analysis presented in the document is complete.

The pool effect was not included in the analysis. See Saito's paper.

19. **IV) Thermal failure and vessel bottom coolability:**

The set of experiments presented (ULPU, CYBL) provides important results.

The most important experiments are the ULPU experiments. The approach which is used supposes that the CHF depends on the local heat flux, on the local two-phase flow conditions, on wall effects and on local pressure. Two-phase flow conditions depend on the overall recirculation path and on 2D local effects.

IV-1) Local Two-Phase flow conditions are expected to be represented if local superficial velocities are represented. This is one of the similarity criteria (the other is the level of the local heat flux). The theory, valid for saturated conditions, includes also the implicit assumption that the local thickness of the Two-Phase Boundary Layer is identical in the experiment (constant width) and the reactor (pie segment). This assumption is not demonstrated but may perhaps be assumed as realistic since size and inclination effects are represented. This should be discussed.

We think this was discussed already. First, we emphasized the importance of matching the two-phase boundary layer, upstream, at, and downstream from the point at which boiling crisis is being simulated That led us to the full-length test section as a basic requirement. Next, we discussed the upstream length within which the vapor flow is close (i.e., within some appropriate tolerance) to that in the reactor (pie) case. The point being that there is enough development length so the boundary layer has no memory that it was not generated "exactly" with the same history as in the reactor. Knowing the boundary layer behavior, from ULPU testing (this is now discussed in Appendix E.4), and having a large amount of sensitivity runs on flux shapes and recirculation flow rates (see new Appendix E.3), help to gain further perspective on these similarity issues. Moreover, we believe that these similarity arguments are applicable in the presence of gravity-induced subcooling.

20. IV-2) The geometry effect is compensated by a heat flux profile defined on the basis of previous similarity arguments. The upstream (from the investigated location) compensation procedure is quite clear. The interest of the downstream compensation is not very clear to me.

This is to preserve the overall gravity head due to voids and hence any internal recirculation flow patterns. See also above (item #19) and new Appendix E.4.

21. For the inner region (angle between 0° and 10°, the heat flux is constant. This should provide conservative CHF conditions in this region.

Yes, but based on our observations and data, even at the pole we think the ULPU results are realistic (conservative but not significantly so).

22. IV-3) It is shown that an increase of the subcooling and of the recirculation mass flow rate has a great effect on the CHF (increase from 0,30 MW/m^2 to 0,50 MW/m^2 at the bottom, increase from 1 MW/m^2 to 1,6 MW/m^2 at the side top location). This is clearly very interesting. However the contribution of each effect (subcooling or mass flow rate) is not quantified and nothing is said about the representativity of the flow path in the ULPU experiments.

In other words the CHF results depend not only on the angle (as suggested by figure E-12) but also on the subcooling and on the recirculation mass flow rate. There is no indication in the text concerning the evolution of the recirculation mass flow rate for the different CHF tests performed at different angles.

→ Thus one must be cautious when using the results presented on Figure E- 12 and correlations El and E2.

See new Appendix E.3. The correlation E2, as used in the report, is appropriate.

23. An optimisation of the flow path (as suggested in appendix K) may lead to an increase of the liquid flow at the bottom of the vessel. Thus, even higher CHF levels may be obtained, locally, than presented on figure E-12.

On the contrary, bad recirculation conditions (flow restrictions, ...) may lead to lower CHF levels. However it seems that the results obtained for Configuration I hold as a lower bound for CHF (0,3 MW/m^2 at the bottom and 1 MW/m^2 at the side top).

These remarks are important in regard to the large heat fluxes which are computed in the metal layer under some assumptions (1 MW/m^2 in fig. 7.14 (page 7-15) and 1.4 MW/m^2 in fig. 7-16 (page 7-17)) or for applications to other reactors.

Future work may thus be oriented both on:

- *a better knowledge of the contribution of each effect on CHF (pressure, recirculating mass flow rate, subcooling, ...),*
- *an optimisation of the flow path (as proposed under appendix K) for a maximisation of the recirculating mass flow rate, since heat fluxes higher than 1 MW/m^2 cannot be excluded.*

Yes, on both items. Please see new Appendix E.3. Note that pressure in the containment will be low and cannot be conveniently increased. Recirculating mass flow rate is found not to be very important. Subcooling is, but, again, this cannot be conveniently increased beyond that due to gravity head. We think if higher critical heat fluxes are required (i.e., large reactors), optimization of flow paths and the possibility of fin structures could be examined.

24. IV-4) It is also mentioned that all results have been obtained with a copper wall and that experiments with steel will be performed. It seems essential to perform the tests with steel since the elevated thermal conductivity of copper may have an effect on CHF. It may be suspected that the oscillatory behavior of boiling at low inclinations induces periodic dry patches which act as initiators of dry-out. The rewetting of these dry patches may be related to the maximum temperature reached on these surfaces during the dry phase. The maximum temperature in these patches is reduced in the case of a copper wall (when compared to a steel wall) due to heat flux redistribution towards the surrounding wetted zones. This suggests that a better understanding of the mechanisms of initiation of dry-out, if possible under these particular conditions, would be welcome.

Please see Appendix E.4.

T.14. Response to P. Shewmon (OSU) — Specific Assignment: Ch. 4

General Comment and Highlights

The reviewer agrees with our approach and conclusions but is concerned that "someone will have to make the decision to flood the reactor cavity, and do it in a timely manner," and emphasizes that this should not be put off until "the last minute." We certainly agree, and perhaps did not emphasize enough that clear procedures and operator training are tantamount to removing this from the realm of someone's arbitrary decision process. This is now emphasized in the updated analysis of the cavity flooding system and its reliability (see addendum to Appendix M).

Point-by-Point Responses

1. *This report treats primarily, almost exclusively, the case in which all of the core, core-internals, and lower support structure have melted, and a steady-state has been attained. This molten material fills the lower head with the dense oxide of the core and a layer of molten metal floats on top of it. Convection brings the heat to the top and side surfaces where it is carried away by conduction thru the steel and radiation upward. With water surrounding the vessel, heat can be removed so effectively from the external surface thru nucleate boiling of water that the external surface remains well below the heat flux required for dryout, and vessel failure. I have read the report carefully, and sought other scenarios that might lead to vessel failure. Provided the reactor cavity is flooded in a timely manner, I believe that a molten core could be contained and adequately cooled inside the pressure vessel.*

* *

2. *You asked me to pay particular attention to Chapt. 4, Structural Failure Criteria. The authors basically consider net section collapse as the most probably failure mode. The net load acting on the wall in the situation considered in this report is extremely low, due to the combination of buoyancy forces and the weight of the internal melt. Only a fraction of a millimeter of steel would be sufficient to support this load. The other significant stress acting in the wall is thermal stress. It is greater than the yield strength, but such stresses are self-limiting and thus relieved with a minor amount of strain. The only way the vessel could fail is by the eating away of essentially all of the wall thickness. The authors show that when the wall is thinner than 2.5 cm (one inch) the water on the outside is sufficient to keep the wall from thinning, i.e. melting, any more.*

* *

3. *I feel it is important to emphasize one other thing. It is a given in this problem that there will always be water surrounding the exterior of the pressure vessel. However, the cavity is not normally flooded in an operating plant and someone will have to make the decision to flood the reactor cavity, and do it in a timely manner. It is important to emphasize that this should not be put off until 'the last minute'. If the molten core redistributes before the cavity is flooded, and with minimal water going into the vessel, the vessel will fail long before one gets to the steady-state whose analysis the report dwells on. I realize that assuring timely flooding is more a regulatory matter than a technical one. But, I wish to stress that timely flooding is essential if the plant is ever to reach the situation of retention analyzed herein.*

The human factors aspects of cavity flooding reliability are now discussed in an addendum to Appendix M.

T.15. Point-by-Point Response to B.W. Spencer ANL — Specific Assignment: Chs. 2,6,7,8,9

General Comment and Highlights

Our perception of this review is that while there appears to be no specific disagreements with the methodology and technical bases of the report, there is no concurrence with the conclusions either. It is implicit, but clear enough, that to obtain concurrence on the conclusions we must address the seven specific recommendations made. We have no problem with four of these recommendations, and believe they have been fulfilled in the manner described in the point-by-point response. We disagree, however, with the remaining three, and provide what we believe to be appropriate and adequate rebuttal.

Briefly (for details see point-by-point below) all three points relate to prototypic material experimentation. Specifically, the reviewer proposes: 1/2-scale ACOPO-like experiments with prototypic materials (his item #5), examination of chemical-related attack using prototypic materials, including fission products (his item #6), and "integral" tests with prototypic materials run over long time periods (his item #7). Our position is as follows:

(a) We agree that the technology for carrying out ACOPO-like prototypic-material experiments is available, and agree that such tests would be a good idea. In fact, we originally proposed these tests to ANL/DOE, once our data from mini-ACOPO confirmed the principle of the technique—we called them PACOPO (for prototypic ACOPO). However, our reasoning for having suggested these tests is totally different from the reviewer's, and we definitely do not believe they are *needed* to reach firm conclusions about the AP600.

(b) We certainly agree about the importance of in-depth understanding of chemical-related attack phenomena, and have continued the work along the lines described in Appendix J. Besides Zr, this work, which included control rod materials (such as silver), is described in an addendum to Appendix J. We did not expect, nor did we find any major surprises. The fundamental point is that because of the very steep temperature gradient in the wall, the remaining thickness and wall integrity are not sensitive at all to any reasonable uncertainties in the wall eutectic-melting temperature. With this additional work we believe we have reached an adequate level of confirmation about IVR in the AP600.

(c) Having addressed chemical attack, as in (b) above, and creep as already done in the report, we see no merit in the long-term testing proposed. These are the only phenomena with potentially long-term effects, and unless a specific shortcoming is pointed to in what has been provided, such tests would be of little or no value.

Point-by-Point Response

1. The report "In-Vessel Coolability and Retention of a Core Melt" by Prof. T. G. Theofanous, et. al., is an excellent synergism of preexisting data plus new data in support of deterministic models, together with a rational methodology for addressing parameter ranges, to address the viability of in-vessel retention in a core melt accident scenario for AP600. This reviewer agrees with the approach and methodology used in the report. Caveats pertaining to key AP600 features and future design decisions are clearly presented and are important in assessing the basis of applicability for the AP600 system.

* *

2. The report considers the melt-relocation-related jet impingement heat flux as one of the regimes producing limiting thermal loads, in the two sections of the report that address this thermal loading mechanism, Chapter 8 and Appendix H, the relocating melt mass amounted to ~0.3 and ~0.6 of the core fuel mass.

> *[1. The report should state the basis for selecting an amount of melt used in the jet ablation calculations.]*

The basis reason was already expressed in the report; namely, relocation through a side-failure. The 30% shown in the main report was chosen as a reasonable upper value for a coherent relocation event. The 60% value shown in Appendix H was arbitrary and chosen only to show the huge margins to failure. The actual details of this relocation are much more interesting for in-vessel steam explosion considerations and therefore are being addressed in Theofanous et al. (1995). For a prelude, see Appendix O.

3. A key basis of the second regime, the pool natural convection regime, is that limiting loads are scenario-independent and are bounded by the thermal loads to the wall in the final steady state.

> *[2. This fundamental basis of the report should be strengthened via a few selected examples involving particle bed heatup and remelting. It is recommended to investigate the downward heat flux i) during the pool formation process when the convecting pool may be contained by thick crust and upward heat transfer may be small, and ii) for the case that steel melts into a fuel particle bed and permeates to the bottom, facilitating heat transport to the vessel bottom.]*

(i) As long as crusts exist (isothermal boundary) the energy flow split depends only on pool geometry, and it favors upwards and sideways heat transfer as compared to downwards. In a transient heatup situation (a thick crust next to the lower head), some of the decay power goes into heating up the crust, and only a portion is conducted away to thermally load the lower head. Maximum thermal load is obtained under steady state conditions. The maximum crust thickness possible is when the crust receives no heat from the melt. This thickness is sim10 cm, and the heat flux delivered to the lower head by it (for the power density of 1.4 MW/m^3), is 120 kW/m^2. If the melt delivers some heat flux to the crust, its thickness has to decrease, so it can accommodate the heat flow with the available (fixed) temperature difference. If there is a gap conductance, the thickness has to be decreased still further. It is very clear from the perspective of the various cases examined in the report that the lower head cannot be endangered by solid crusts.

(ii) The metal relocation process, postulated by the reviewer, is self-limiting as a heat transport mechanism. That is, even if the oxidic debris porosity were largely penetrable by the high surface tension metallic melt (which is, in fact, highly unlikely), it would quickly fill up and, being stably stratified, as it should be, reduce the process back to conduction (for the lowermost nearly flat part of the lower head). More details on the whole issue of the transient meltdown aspects of the lower head thermal-load process can be found in a new Appendix O.

> *4. It is clear that the upward/downward split of heat transport from the corium pool is crucial to the overall problem, including the presence of a steel top layer wherein radiation heat loss and sideways heat transfer participate in the integral processes. The analyses based on existing database yield the distribution of loads which are shown in the report to be removable from the walls with considerable margin; i.e., in terms of remaining wall thickness in relation to wall load bearing requirement (for fully depressurized system) and in terms of the polar variation in heat flux in relation to CHF limitation.*

> *[3. The additional work the authors list in Chapter 9 to strengthen the report basis should be pursued.]*

From the additional work described in Chapter 9, only the item referencing the lower head surface characteristics, and perhaps thermal properties, is germane to concluding the assessment for the AP600. This work has now been completed and is reported in Appendix E.4. The results, as expected, confirm the conclusions reached previously. The other items mentioned in Chapter 9 may be pursued in conjunction with the European Passive Advanced PWR design (a scaled-up version of the AP600).

5. *[4. The extent of the key AP600 assessment results cited in Chapter 7 should be broadened. Key results are presented in terms of the ratio $q(\theta)/q(\theta)_{CHF}$. Other key representative results should also be given such as pool and metal layer bulk temperatures, crust thicknesses, wall thicknesses, and pool and metal layer energy splits.]*

This is a very good suggestion, and for completeness, we provide a complete set of results for the base case, and for the most limiting parametric case (of Chapter 7), in Appendix Q.

6. *[5. The database used for the analysis should be extended to include real reactor materials involving realistic temperature levels, boundary conditions, and crusting effects, and real melt behavior in the superheat range as well as slurry range between T_{sol} and T_{liq} for the $U0_2/ZrO_2/Zr$ system. The authors themselves have devised an excellent approach to achieve this data via the ACOPO pool approach wherein high Ra' data is obtained for Nu_{up} Nu_{dn} and $Nu_{dn}(\theta)$ using large melt heat capacity in a cooling mode in lieu of internal heat generation. A few reactor material tests should be performed analogous to ACOPO at 1/2 scale, including in some cases the integral effect of an overlying steel layer.]*

This has been addressed under "general comments and highlights." As far as we can see, the only "technical" component of this expressed need is concerning the "slurry" range between T_{sol} and T_{liq}. But it is well-known that the inner boundary of the "slurry range" will be at the melt liquidus, and this is all that matters as far as natural convection is concerned. This is the approach taken in the report. Again, the PACOPO experiments would be interesting, but their role should be viewed as strictly confirmatory, and in fact not really necessary in the order of priorities for the AP600.

7. *The report does not address the likely length of time that pool natural convection cooling would be relied upon if this regime were entered in an accident. It could be days or even weeks. The IVR assessment has included structural and thermal loads assessments, but the treatment of chemical processes which may effect head integrity over prolonged time is treated minimally.*

> *[6. A through examination of interfacial chemical processes should be undertaken involving not only the Fe/Zr mixture but also including other potential constituents of the corium including absorber materials, control rod materials, and fission products to address any possible*

chemical-related attack on the wall integrity at the temperatures and time duration of interest.]

We do not see the concern about time frames for chemical attack and thus do not concur that tests such as these "should be undertaken". We use equilibrium thermodynamics that presuppose "infinite" contact anyway. However, the demonstrations, along the lines offered originally in Appendix J, have been expanded and are presented in an addendum to it.

8. [7. For a ground-breaking safety approach as important for AP600 as IVR, it is warranted to perform a large-scale, integral test to demonstrate the viability of the integral processes over a lengthy duration. Real reactor materials, real vessel head material, and internal heat generation are required for such a demonstration test. The experiment technology is readily available to utilize a slice geometry analogous to the authors' own COPO experiments. A representative AP600 corium composition should be employed with the wide range of relevant materials as included in (6) above. The test may start from particle bed form.]

This has been addressed under general comments. The "lengthy time duration," again, seems to drive this question, and again, we cannot identify a technically-based concern.

9. The report should clarify the scenario for the 3BE sequence considered to be of main interest to IVR. This sequence involves a large or medium size pipe break. Figure M1 seems to indicate that if cavity flooding is achieved, much of the RCS piping will be covered with water. Is water reflood of the vessel via the break a part of the 3BE sequence? What effect would water reflood have on the accident scenario?

Since the issuance of the report, Westinghouse has changed the design of the cavity flooding lines from the 4- and 10-inch lines to two 6-inch lines. The new cavity flooding behavior is discussed in an addendum to Appendix M. The relative timing of cavity flooding to the melt progression is discussed in Appendix O, and shown in Figure O.10.

10. In Appendix M, it would be better to refer to the cavity flooding valves as "remote actuated, motor operated valves" rather than "manual valves".

This editorial change has been incorporated.

11. Pg. 5–7, 2nd line, believe Ra' exponent should be 14 rather than 16.

Typo has been corrected.

T.16. Response to H. Tuomisto (IVO) — Specific Assignment: Chs. 2,6,7,8,9

General Comment and Highlights

We appreciate the favorable comments and the insightful questions raised about several pertinent aspects that we overlooked in preparing the report. Having these questions raised at this time affords us the opportunity to enhance significantly the completeness of the treatment. We believe our response will solidify the reviewer's agreement with our approach and conclusions.

Point-by-Point Responses

1. *General remarks*

In-vessel retention by external flooding is an effective means to reduce thermal and energetic challenges to the containment integrity during core melt accidents. If the concept is applied as a basic severe accident management strategy, it is really an essential task to assess the overall feasibility and reliability. The report makes a remarkable synthesis of the thermal regime of the in-vessel retention by external flooding.

The in-vessel retention concept was introduced to the severe accident management considerations in the end of 1980's. The technical feasibility was initially demonstrated for the Loviisa Nuclear Power Plant by Prof Theofanous (see Ref. 39 of the Report). Since that time, plenty of new research has been performed to confirm the first demonstration. New information has been generated to a large extent in support of the Loviisa and the current ARSAP program. In these studies, the ROAAM approach has been applied to evaluate the risk of failing in the thermal regime.

The report and the problem treatment as a whole has been organised in an excellent way. It has been a great pleasure to have an opportunity to read it and to find the beauty of such developments as the idea behind the ACOPO experiments, and the thermal treatment of the metallic layer.

In the following, Approach and Assessment applied in the report are discussed. This is followed by some detailed remarks, which are meant for obtaining further clarification of certain aspects of the thermal regime and for the overall resolution of the in-vessel retention concept.

Approach and Assessment

For evaluation of the approach and the assessment, there are two questions to be answered:

- *Is the approach sufficiently consistent and comprehensive to allow the overall assessment?*

- *Is it justified to say that the issue is principally and practically solved, and that only confirmatory research is necessary any more?*

Fortunate to the reviewer, the applied ROAAM approach itself makes it possible to answer these subjective questions.

The ROAAM approach has been developed to deal with phenomenological uncertainties in complex physical and technical problems. The ROAAM has reached a mature state of development and application. Appendix A to the report is very essential for understanding the principal methodology. The starting point is to create the quantification framework by dividing the problem to such pieces which can be treated in the physically meaningful way. One of the most powerful features is that all new developments of the subject can be easily integrated into the framework and into the quantification.

The quantification framework needed for the in-vessel retention has turned out to be comparatively simple. First of all, the simplicity reflects that large margins are available for the heat transfer from the heat generating oxidic pool itself. Therefore, the framework concentrates on unfavorable conditions of the metallic layer. On the other hand, the simplicity can be understood to imply that the developmental stage is mature enough.

The available experimental results and theoretical considerations support the conclusion that the modelling uncertainties are very small in comparison to the margins. In terms of the ROAAM, I have no difficulty to agee that the assessment approach is of Grade B type and the maturation status (Phase IV) is reached upon completion of the peer review.

As shown in Fig. 1.1 of the report, the in-vessel retention issue will include the FCI Regime and the Steam Explosion Regime in addition to the Thermal Regime treated here. The final feasibility can be demonstrated after the separate report of melt-coolant interactions is available.

Concerning the practical design and Severe Accident Management measures, i.e. ensuring free water flow on the vessel and assuming low pressure conditions, some comments are included later. Notwithstanding, my answer to the above questions is positive: the treatment is consistent and comprehensive, and it is justified to state that the thermal regime is resolved to the point where only confirmatory research and practical design solutions are necessary.

* *

2. Viscous effects

The corium pool heat transfer experiments have employed water and freon as a working liquid. Corium itself behaves in a different way on the pool boundaries where crust is formed: the increase of viscosity takes place gradually in corium. There is a not a sudden jump from the solid to the liquid phase. On the other hand, the validation calculations for the pool heat transfer take plenty of effort when trying to solve the heat transfer in the turbulent boundary layer.

It would be interested to obtain the authors' opinion on the influence of increasing viscosity to the heat transfer distribution, particularly whether it could increase heat transfer in the upwards direction. What are the authors' recommendations for the future fluid dynamics calculations?

This is also referred to as the "mushy" or "slurry" layer. For the present system this can be thought to exist next to *all* boundaries. The thickness of it depends on the intensity of local convection and hence of local heat flux. The higher the flux the thinner the layer. The existence of this layer is to allow the transition, required by thermodynamics, from the liquidus, on the melt-side face of it, to the solidus, on the inside face in contact with the crust. As a pool boundary temperature we use the liquidus, and as driving force for heat transfer, the pool superheat. Thus, it can be said that in our treatment the crust *and* slurry layer are lumped together in an effective crust. Thus, there is no change on heat flux distribution to be found if the slurry layer were to be treated explicitly. An explicit treatment would allow us to determine how the thermal resistance of our effective crust is split between a real crust and a slurry layer, but this is an complex problem whose solution would require consideration of convection and its effect on the slurry layer thickness and properties. To a first, but adequate in our opinion, approximation in areas of strong convection (which are all *except* the lowermost fluid region exhibiting the strongest stable stratification—see mini-ACOPO data) the mushy layer will be thin and hence of minor interest. In the lowermost region this mushy layer might build up some more, reducing somewhat the local fluxes; however, this is such a small area (compared to the total) and with such already low flux level, that any change in it would not perceptibly change the rest of the heat fluxes, including, in particular, the one in the upwards direction. On this basis, regarding future fluid dynamics calculations we would recommend a similar approach as this, i.e., using a liquidus temperature as the boundary condition and lumping the mushy layer with the crust into one effective crust.

3. *The influence of the metallic layer*

The problem definition in Chapter 2 defines that the thermal load to the lower head is maximized when the debris pool has reached a steady state, the heat generating debris volume has been maximized and the thermal resistance along the upward thermal radiation path has been maximized. Maximizing the debris volume creates some confusion with the "focusing effect" of the metallic layer. The most significant parameter by far is the height of the metallic layer on the top of the oxidic pool. As an extreme parametric study of Chapter 7 demonstrates, the limiting case presupposes only partial relocation of the oxidic part. Could the partial relocation cases make a nonnegligible increase in the failure risk?

The extreme parametric case considered in Chapter 7 is bounding because it uses the minimum amount of steel possible and the maximum amount of oxide that can physically exist with it. More oxide will produce contact with the lower support plate and a melt-in process that would drastically increase the metal layer thickness, while less oxide would produce, clearly, lower thermal loads. Even this extreme case cannot quite produce failure, and the margin is somewhat greater than shown in Figure 7.16. This is because the very sharp local peaking is combined with much lower upstream fluxes (or vapor flow), a situation that according to new ULPU data yields higher critical heat fluxes. These data, which were obtained with flux shapes appropriate to the parametric and sensitivity studies are summarized in Appendix E.3. In any case, some further consideration of partial relocation is given in Appendix O.

4. *The amount of steel in the metallic layer has been explained from the inner structures and their melting during the accident. Only schematic structural drawings of the reactor vessel and its internals have been given, For the readers' own judgment, a detailed drawing of the reactor core, internals and vessel would be useful.*

A figure with key components and dimensions is provided in the new Appendix O.

5. *In the Grenoble Workshop on "Large Molten Pool Heat Transfer" in March 1994 the question of steel boiling was brought up and was also mentioned in the*

Workshop Summary. The authors' response on the possibility of this phenomenon to increase to the metallic layer heat transfer would be desirable.

This is addressed in the new Appendix O.

6. Low pressure sequence

The report assumes that high pressure core melt sequences can be practically excluded. Since the high pressure sequence Case IA turned out to have rather high contribution in the AP600 PRA, some additional aspects are needed.

To show that the contribution of high pressure sequences is negligible, very high reliability requirements are provided for the system, particularly to show that negligible contribution to the in-vessel retention can be excluded. Naturally, this should be done in context with the available time for required operator actions. In case that depressurization by pressurizer surge line failure is argued, it would need quantification.

The data in Table 7.3 have been revised on the basis of additional work on the interplay between PRA and design. It is not planned to argue the case on the basis of pressurizer line failure. A new table, and the implications on the IVR scenarios, including cavity flooding and related human factor aspects, are presented in addenda to Chapter 7 and Appendix M.

7. Blocking of the flow paths

In addition to ensuring availability of the flow paths by proper insulation and cavity exit design, the flow paths must be protected against all debris possibly flowing with water. The sources of such debris are the piping and vessel insulation (mineral wool, glass wool, thin metal sheets), rust paints, concrete dust etc. Particularly, the narrow flow paths out from the cavity might be subject to clogging. The current research for the containment sump clogging can be utilized for the final design.

Because the natural convection flows are rather strong, and the characteristic dimensions macroscopic, we have no mechanism for clogging similar to containment sump grids. This is further addressed in an addendum to Appendix K discussing the insulation design and related flow paths.

8. Fouling of the vessel wall

Another potential problem related to the water chemistry, impurities and all the small size debris flowing with water may be the fouling of the pressure vessel *external surface during boiling heat transfer. At the beginning the fouling could have some advantage in increasing the surface wetting properties, but in the long term it might create an insulating layer. The possibility to study the fouling effect e.g. in the next phase ULPU-2000 experiments could be considered.*

Because of the very strong convection we do not believe fouling to be a serious problem. However, the suggestion is well taken in the confirmatory sense offered, and it has been added in Chapter 9 of the report (under future work).

9. Thermal shock of the vessel

External flooding brings two potential problems to the vessel integrity due to thermal shock.

The first concern is an inadvertent flooding of the cavity that may bring a problem of the pressurized thermal shock to the vessel material (and to the weld if existing on the core area) exposed to the fast neutron fluence. This is not directly concern of the in-vessel retention concept but any adverse effects for the safety of the vessel under design basis conditions should not be caused. The potential for inadvertent flooding should be checked under normal operating and overcooling transient conditions. The cracks located on the outside surface of the vessel may start propagating, since the outside cooling temperature is very low. Low initial and end-of-life brittle transition temperature of the vessel and weld material can minimize the risk.

See addendum to Chapter 7. The vessel is designed to withstand one inadvertent flooding during normal operation.

10. Secondly, the relocation of core material onto the lower head causes a severe thermal shock to the vessel bottom. Before relocation, the inner surface temperature of the vessel may be about 100 °C and the external surface temperature equals to that of the flooding water. The contact with hot corium creates very steep temperature gradient in the wall. Now the cracks cannot propagate through the vessel wall, because they will stop in the heated part. However, it should be checked that the cracks are not so long and deep that they could cause the failure of the vessel (global rupture of the bottom) after partial melting of the wall thickness.

No cracks would be expected to propagate because of the extremely good quality of steel (RTNDT of 20° at end-of-life at belt-line, even lower in the lower head region) and the relatively high temperature of the flooding water (IRWST at least of 50 °C).

T.17. Response to B. Turland (AEA) — Specific Assignment: Chs. 2,5,6,7,8,9

General Comment and Highlights

We appreciate this deeply thoughtful and comprehensive review. Broad agreement on the methods and particular aspects of the study and agreement with the conclusions have been noted. Many points of clarification and constructive requests for completeness have also been made. We find ourselves in good agreement throughout, and have attempted to respond in the spirit requested. There are only two areas where there appears to be some difference of opinion. The one is the most severe parametric case in the report, which we call "extreme"; the reviewer considers it to be "not as extreme" as we do. The other is in prototypic material experiments. It is not exactly clear to us how strongly the reviewer feels about this comment, but he has mentioned it a couple of times. We believe that although such experiments would be quite welcome, they are not necessary to support our conclusions for the AP600. The reason is that natural convection is a pretty basic process, and once the correct range of the governing dimensionless group (Ra′) has been reached there is nothing really to wonder about. Moreover, we wish to express caution, as such tests will inevitably be attempted, that due care and attention be given, so as not to be lead astray from what we know already, due to distortions forced upon us by experimental difficulties.

Point-by-Point Response

1. *1. INTRODUCTION*

This review concentrates on Natural Convection, covered in Chapter 5 of the report, and the overall approach and assessment covered in Chapters 2, 6, 7, 8 and 9 and therein referenced appendices as requested in the letter from L.W. Deitrich dated 10 November 1994. The review is organised as follows: Overall comments are given in Section 2, while Section 3 contains the detailed comments on Chapter 5 (natural convection), and technical comments on the other nominated chapters are given in Section 4. An appendix contains details of typos found during this review. Below, the word 'authors' refers to the authors of the original study (Theofanous et al).

* *

2. *2. OVERALL APPROACH*

The authors make clear that the AP-600 design is favourable to in-vessel debris retention by cavity flooding. I support this view, particularly because of the absence of lower head penetrations and the ability to get water into the cavity.

However the information given in Table 7.3 (accidents contributing to the core damage frequency), the PRA information given in Appendix M (cavity flooding unsuccessful in 20% of the core damage cases), and the discussion of the thermal insulation (Appendix K) all indicate that even if one had complete confidence in the analysis presented in the report, there are still likely to be circumstances in which debris would not be retained in the lower head. In my view, these PRA and engineering related issues deserve priority, because the report does make a strong technical case for in-vessel retention provided the constraints of prior depressurisation and action to initiate cavity flooding are met. This may require systems enhancements to obtain the necessary degree of assurance.

Agreed. Please see addenda to Appendices M and K and revised Table 7.3 in the addendum to Chapter 7.

3. Like the authors (apart from more specific comments on Chapter 2, below), the reviewer will assume that the above prior conditions have been met, and concentrate on the heat transfer aspects of demonstrating that the debris will be retained in the vessel. The reviewer's observations are based, in part, on an unpublished study performed in the UK for a large (1.3 GWe) PWR. Apart from penetrations, which are present in this design but of no concern for AP-600, and the somewhat larger inventory of fuel, which reduces margins, the major threat to vessel integrity was identified as being due to the focusing effect of the metal layer, consistent with the sensitivity described in Section 7.3 of the report. In the absence of penetrations, this independent study also concurred with the authors that the greatest challenge to vessel integrity is at high polar angles, not close to the pole of the lower head.

* *

4. Turning to technical matters, the allowable uncertainties from the in-vessel distribution of the heat flux are high because of the high critical heat fluxes found in the ULPU experiments, particularly in Configuration II (Appendix E.2). These were obtained in a full water loop, without significant obstructions and opportunities for vapour accumulation. Confirmatory tests with the chosen thermal insulation and more prototypic flow paths are desirable.

Please see new Appendix E.3, addendum to Appendix K, and implications of this additional information in an addendum to Chapter 3.

5. *Note that most of the above points are covered in Chapter 9 by the authors.*

* *

6. *3. Chapter 5: NATURAL CONVECTION*

The authors assume (page 5-2) that, in the presence of unoxidised Zr, all the uranium remains as UO_2*. Powers [Chemical Phenomena and Fission Product Behaviour during Core Debris/Concrete Interactions in 'Proc. of the CSNI Specialists' Meeting on Core Debris-Concrete Interactions, EPRI NP-5054-SR; February 1987] has queried this assumption, and notes that only about 5 atom percent uranium in the metal phase is sufficient to make the metal phase of core debris more dense than the oxide phase. As far as I know this question is unresolved. Given the large steel inventory assumed, it may not be possible to achieve inversion of the densities of the phases, and other possible configurations of debris (including partitioning of the decay heat source) may prove less of a challenge to the vessel, but I would like to see these points addressed for completeness.*

The points raised here are appropriate to be discussed, for completeness. We can approach such a discussion from a couple of standpoints.

(a) We agree with the reviewer that this question is unresolved, while the author (Powers) writes that "These analyses must be considered speculative until a definitive analysis of the thermochemistry of oxidic core debris can be obtained".

(b) Powers was concerned with corium-concrete interactions, where due to bubbling the metallic and oxidic components of the melt found themselves in good contact (promoting chemical reactions and mass transfer between them). In the present situation most of the zirconium is tied up in the lower blockage, near the core support plate; it would be the last to remelt, together with a large quantity of steel (the plate itself), and thus would be added on top of the oxidic pool (see Appendix O). Such a stratified geometry separated with oxidic crusts is not favorable for extensive chemical reactions between the metallic and oxidic components. So, most of the uranium present in the metallic layer will be the quantity dissolved during the first clad melting and relocation period, and this was estimated at no more than $\sim$5% of the zirconium involved (D. Olander, Nuclear Engineering and Design, 1995, (to appear)). Since the zirconium itself is only a small fraction of the iron present in the metallic layer, we can see that we are far from the 5% uranium composition needed, according to Powers, to obtain inversion.

7. *The authors assume (page 5-2) that the crusts impose a uniform temperature boundary condition at the melt liquidus. Elsewhere (eg the CORCON code) it is assumed that the melt solidus temperature provides the external boundary condition for the melt pool. Evidence from the ACE Phase C experiments and the associated determinations of liquidus and solidus indicate that 'melt' temperatures beneath the liquidus are possible. Further the equations for crust growth are consistent with the solidus assumption, with the deposited crust having the composition associated with the solidus and a concentration gradient in the melt phase close to the crust. In some circumstances compositions in this region may be such as to give nucleation in the boundary region (or encourage the growth of dendrites). Unless the zirconium is fully oxidised, allowing oxides of iron to become part of the oxidic layer, this question is largely academic as far as the crust is concerned — both the solidus and liquidus will be significantly above the steel melting temperature. However, equilibrium phase diagrams indicate that, for the compositions anticipated, UO_2 will preferentially be deposited in the crust and that UO_2 should precipitate near the cool boundaries leaving a liquid richer in the less dense ZrO_2. Should this happen, convection, which depends on local density differences might be modified. This effect was demonstrated in simulant experiments at low Rayleigh numbers [S B Schneider and B D Turland: Experiments on Convection and Solidification in a Binary System in 'Proc. Workshop on Large Molten Pool heat Transfer, Grenoble, March 1994; NEA/CSNI/R(94)11], but was found to disappear for the simulants used at much lower Rayleigh numbers than those expected in a reactor melt pool. Confirmatory experiments with real materials are desirable, and should be performed as part of the OECD/Russian RASPLAV project. A paragraph should be added discussing possible multicomponent effects on natural convection.*

Again, this is a point well taken in the spirit of completeness. Basic molecular diffusion consideration lead to the conclusion that such multicomponent effects could not affect natural convection in any significant way, under the turbulent flow conditions at the high Rayleigh numbers of interest here. We feel this was amply demonstrated by Schneider and Turland (1994) and there is no need for confirmation by prototypic material tests. The reference has been added to the report.

8. *As the primary factor driving convection is the temperature difference, and the system response is determined by the heat flux. I would expect length*

independence to imply that

$$Nu \sim (Ra')^{0.25}$$

not as Ra′ to the 0.2 power as indicated by equation 5.10; I note this is consistent with Cheung's analysis referred to on page 5-6.

Equation (5.10) is presented as an order-of-magnitude to make the argument of weak, or no scale dependence, so we can use results from horizontal layers for the upper boundary, as discussed. This is certainly true whether one uses Eq. (5.10) or the exponent 0.25 in it. In fact, for finite values of the Ra′, the exponent falls in between 0.2 and 0.25. This point is further discussed in response to Cheung's item #3.

9. I note that the apparent transition in the COPO experiments (page 5-5; fig B.2, page 5-9) at high Ra corresponds to the sets of experiments with different pool depth.

Given the margins that seem to exist, the statement on page 5-6 that 'even a 30% discrepancy could be potentially rather significant to our conclusions' seems rather strong for the AP600 application. However, I am pleased to see this issue has been addressed further in the mini-ACOPO experiments and the planned follow-on larger scale tests.

We agree. This statement was written before we had the mini-ACOPO and the rest of our analysis together.

10. The following comments apply to Appendix D, describing the mini-ACOPO experiments in more detail:

The basic idea behind these experiments - to obtain data in proper geometry using cool-down rather than internal heating - is to be commended. The major question is whether the data should be applied directly, or used to benchmark a model that is then applied to the internal heating case. As the authors note, mathematically a spatially uniform cool-down rate is equivalent to an equilibrium with internal heating. They go on to show that over most of the volume the temperature is close to being uniform, so the cool-down rate is also spatially uniform in this region. However this does not apply in the lower part of the pool, where the 'effective volumetric heating' will be appreciably lower than in the bulk. It is not sufficient to demonstrate self-similarity to claim uniformity of heating. Indeed the curves showing self-similarity are quite constrained - they must asymptote to

1 and average to 0, thus it is not surprising that the largest discrepancies are at small values of V_i/V. A better indicator of the uniformity of the heating would seem to be

$$(T - T_w)/(T_{max} - T_w)$$

where T_w is the wall temperature. It should also be noted that the bottom 10% of the volume corresponds to about one-quarter of the pool depth, so the effective heating close to the pole of the vessel may be significantly reduced. The consequence of this will be to bias the results somewhat to lower downward heat fluxes (particularly near the pole). For completeness, I would prefer these effects to be taken account of in a model of the pool (not a full CFD simulation) although I do not expect them to invalidate the conclusion the authors draw from these experiments.

The points made here are well taken, and we are working on a model. Also, please see the addendum to Appendix D and Schmidt's Technical Note (part of his comments) and our response to it.

11. The assumption that the pool reaches a quasi-equilibrium configuration is justified by the experimental data, and the observation that the pool suffers no major internal adjustments during the cooldown is also important in justifying the experimental method.

It is noticeable that there is a stronger dependence of downward Nusselt number on Ra' than the correlation lines shown in Figs D.10 and D.11. Extrapolations of these data to reactor-size pools will give more equal downward and upward heat transfer correlations, in contrast with the 2-dimensional COPO data referred to in Appendix B. (This is covered in the main text of Chapter 5).

The maximum wall peaking factor of two seems to be well-founded.

The correlation line shown in Figures D.10 and D.11 is Eq. 5.22 (Mayinger) The somewhat stronger dependence of data on the Ra' number was reflected in Eq. (5.28), which is the one used in the analyses.

12. The claim on page 5-12 that the UCLA data (for downward heat transfer) indicates an intermediate behaviour is not consistent with the plotting on Fig 5-7. While only a modest extrapolation is necessary, it is not justified to refer to the extrapolations as bounds - can say 'are expected to bound'.

Strictly speaking, the reviewer is correct. Really, one could not extrapolate from the 2 UCLA data points. Our statement that "trends of the UCLA data . . . indicating an intermediate behavior" derives from the observation that they are somewhat higher than the Mayinger correlation, while the UCLA authors have interpreted them (see Appendix C) to be in essential agreement with it—i.e., same trend.

13. The treatment of the metal layer appears reasonable and conservative.

* *

14. The simplified model (pages 5-17 to 5-24) is interesting. However, the figures 5.9 to 5.11 expect a lot of work from the reader. I suggest only one set of curves per figure, which can then be labelled appropriately. Of the assumptions made for this model, I suspect the energy radiated from the surrounding cavity to the layer (assumption 4, page 5-18) may not always be negligible (equivalently the view-factor is reduced from 1 to allow for sidewalls close to the melting point of the steel).

Strictly speaking, the point is correct, but as we show in Chapter 6, using the complete model, that includes back radiation, the effect is negligible. We revisited Figures 5.9 through 5.11 regarding clarity, and we agree with the reviewer that they require too much work from the reader. The main reason for this, we think, is the faulty caption in the figures. Rather than expanding the volume with more figures, we decided to remedy this by making the use clearer in the captions.

15. Overall, this chapter presents a balanced account and the conclusions drawn are consistent with the current experimental database for convection in ideal simulant liquids. Too little account is taken of effects that might come into play with real materials, and no mention is made of the somewhat contradictory results that have been obtained with UO_2 melts in the past (Argonne experiments by L Baker et al and the SCARABEE-N test). However, there is a lot of margin available, provided the ULPU critical heat flux curve is appropriate, and it is difficult to see any circumstance in which non-ideal fluid effects would lead to vessel failure.

It is very difficult to conduct experiments with prototypic materials, and harder yet to obtain basic information from such. The ANL tests were run with pure UO_2 in a cubic, 10 cm on the side, test section with an essentially adiabatic boundary condition at the upper surface. They found that the downward heat flux was close to that on the side walls, in contradiction with what could be expected from basic natural convection physics. This uniformity in behavior was attributed to the dominance of an internal radiation heat transfer mechanism (i.e., transparency of pure UO_2 to the infrared). This is not expected to be the case (L. Baker, 1995, Personal Communication) for the reactor oxidic melt, due to ZrO_2 and many impurities in it. Also due to the smallness of the test section, the thermal radiation path was rather short, compared to reactor scale.

16. 4. OTHER CHAPTERS: TECHNICAL COMMENTS

Chapter 2

It is right to emphasise that full primary system depressurisation is being assumed (the validity of this can presumably be obtained from the Level 2 PSA referred to in Appendix M) and that pre-flooding of the lower head has taken place prior to debris relocation. Appendix M, while indicating that there is sufficient inventory of water and sufficient flow paths, does suggest that more needs to be done to guarantee that water will be delivered in a timely manner, assuming that cavity flooding is adopted as a primary severe accident management operation. Likewise, my reading of Appendix K suggested that there is no difficulty in principle with designing insulation to allow effective flooding around the vessel, but this also needed further consideration by the plant designers in conjunction with the information obtained from the ULPU tests. As the information is presented here, it still looks that cavity flooding is an 'add-on', as it has to be for existing designs, rather than something built into the plant design. I assume there is regular testing of the valves in the drain lines from the IRWST. Overall I agree that the AP600 is an attractive design for the implementation of cavity flooding.

Please see updates to Appendices M and K, with more detailed information on cavity flooding and insulation design. For the AP600 we wish that they be considered as part of this package rather than as "add-ons".

17. Without a full appreciation of the geometry and the assumptions that have been made on crust behaviour (eg are lower ceramic blockages, formed initially on a metallic blockage sufficient to retain the debris pool) it is not possible to underwrite the statements made on melt relocation; that discharge will occur at the core side and will involve a substantial fraction of the core. With the highest heat fluxes near the top of a molten pool, it seems to me more likely that the initial discharge by this route might be limited to, say, 30% of the core.

Yes, by substantial we mean something like 30% as an upper limit, as considered in Chapter 8. Also see Appendix O.

18. I support the view expressed on page 2-2 that what has been mentioned as a slow approach to steady state is really attributable to the thermal capacitance of the pool rather than unsteadiness in the natural convection process. However the following statement that the thermal loads are bounded by the steady state is untrue, as the discussion of the metal layer shows: as this layer grows the heat flux to the vessel wall reduces.

At this stage the focusing effect of the metal layer has not been introduced yet, so our statement refers to the thermal loads from the oxidic pool only. The focusing effect is discussed and treated separately in Chapter 7.

19. The term 'sizeable fraction of the core' on page 2-5 is undefined, but no evidence is produced to indicate that it is anything close to 100%. This is significant given the later arguments over the depth of the steel layer.

By "sizeable" we mean "not small," i.e., ~30%. We have added this to the text.

20. Chapter 3

This provides a reasonable overview of the data, largely generated at UCSB, on the critical heat flux. It seems reasonable to assume that the vessel is cooled sufficiently during depressurisation from the inside to guarantee nucleate boiling when cavity flooding is initiated.

* *

21. Chapter 6

In practice a significant amount of the decay heat may be generated in the metallic layer (see page 6-1), if metallic fission products are able to migrate there. However, I do not expect this to affect the conclusions drawn by the authors.

* *

22. As noted above, the solidus is appropriate for considerations of crust behaviour (an effective solidus, based on the temperature at which more than, say, 40% of the debris is liquid may be appropriate for melt convection considerations). For the metal layer, the implication of the phase diagram (Fig 6.1) is that *attack on the steel wall may be possible at temperatures below the steel melting point, depending on the mole fraction of Zr.*

* *

23. The reality of the thin crust between the oxidic and metallic layer is not questioned in the report. If it is unstable, it may lead to an augmentation of upward heat transfer.

We do not see significant mechanisms for sustained instability. Moreover, the oxide freezes upon contact with the metal, which is sufficient to establish the thermal boundary conditions.

24. The model described in this chapter is suitable for the purpose envisaged. If it were to be developed further, the radiation sink(s) should be treated in a more discretised manner as the temperature will vary with distance from the debris.

* *

25. Chapter 7

The use of the whole UO_2 inventory may not be bounding. If only 80% of the core relocates (eg leaves remnants of low rated assemblies) then the oxidic pool will be beneath the lower support plate, the metal layer may be thinner and the focusing effect more pronounced (particularly as the surroundings will be close to the melting point of steel). Such a configuration could occur before a final 'equilibrium' state is reached. In this configuration smaller amounts of metal are possible.

This is the case actually examined as a parametric. The likelihood for it is really a matter of judgment, especially since the final stages of core relocation cannot be defined with any degree of confidence. The dominant factor against focusing, as explained already, is the large steel inventory in the lower internals and the reflector. We do not believe it is possible to have more than, say, 60% of the core relocated without having melted most, if not all, of the reflector. It is on this basis that we call the particular parametric "extreme." Actually, the margins for this case are greater than shown in Figure 7.16. The reason is that with such local peaking the CHF is greater than that used in Figure 7.16. These new results can be found in Appendix E.3. More on the "focusing" problem can be found in new Appendix O.

26. The ranges for the amount of metal involved seem rather narrow, and the text does not seem that consistent; the first part of the second paragraph *(on page 7-5) implies that 105 tons of metal are expected, whilst this is way out of range of the probability distribution, presumably as all the core barrel is not expected (is the reflector attached to the core barrel?). Molten metal from the lower head should also be included. I would make this distribution broader in both directions. This would broaden the distribution for the height of the metallic layer (Fig 7.6).*

No, the reflector is not attached to the core barrel; it rests on the core support plate which is hung from the core barrel (with secondary support from the lower head). Perhaps our expression about the core barrel mass was not clear enough. We refer to the "lowest portions" only, while the *total* mass is 40 tons. Thus the quantity of melt expected would be somewhat more than 67 tons, the "more" depending on what fraction of the 40 tons is to be considered. By comparison to these quantities the amount supplied by lower head melting would be indeed negligible. So we do not find any real reason to broaden the distribution of the melt mass.

27. It would be useful to have Sienicki's material in an Appendix. However, the timings seem reasonable.

* *

28. The statement (page 7-11) that 'the zirconium oxidation is clearly quite independent of τ' is rhetoric. I would only say there is no obvious correlation.

We disagree on this point, and our reasoning has been stated. If the independence is questioned, it puts in question the calculation procedure. If we accept that a correlation exists, but we do not know what it is, the problem should be considered as a splinter, and the treatment should afford the most adverse type of dependence.

29. I do not regard "the limits to failure" case (page 7-16) to be as 'extreme' as the authors do. However, the results are encouraging, when allowance is made for likely lateral temperature gradients in the (relatively) thin metal layer.

This was discussed above, item #25.

30. I note that no uncertainty has been allowed in the application of the ULPU critical heat flux data.

Actually this was discussed near the bottom of the text in Figure 3.2 (page 3-5): "From an evaluation of uncertainties, as discussed in Appendix E, we expect this result to be good within a few percent, so that CHF can be excluded outside a rather narrow range around it." We did not elaborate on this point in the calculations (Ch. 7) themselves, and the presentation of results because the wide margins shown dwarf completely such effects. With the much larger data base now available from ULPU (new Appendix E.3), the accuracy and realistic-lower-bound nature of it is made much more clear.

31. Chapter 8

It is stated in the second paragraph that 'The fundamental consideration is that molten oxide cannot exist next to a steel boundary even under strongly convective conditions ...', however equation 8.2 has $T_j = T_{w,m}$ as the driving temperature difference, not the melt superheat.

Sorry, this was a typo.

32. The argument (page 8.3) against a small diameter jet seems relatively weak, given that a local failure is expected; as the pour continues one may expect the melt to erode downwards. Again it would be useful to see Sienicki's work. Water in the lower head would play a mitigative role, and, as smaller diameters are considered for the jet, the coherency of its impact is more difficult to maintain. If impingement is on the cylindrical section, is this thicker than 15 cm?

Yes, the cylindrical wall thickness is 20 cm. We agree with the mitigative role of water, and, furthermore, of the mitigative role of melt accumulation, which becomes increasingly more important as one wishes to consider larger pours (see also Mayinger's item # 2).

33. Chapter 9

The absence of lower head penetrations should be added to the key features that lead to the favourable conclusion for the AP600-like design (penetrations may not preclude melt retention in the lower head, but they make the analysis more difficult).

This chapter provides a fair summary of the assessment, and I support the conclusions drawn and the recommendations made. I would also add (i) development of an analytic model for interpretation of the ACOPO tests, and (ii) a limited series of tests with real material to support the conclusions drawn from simulant or simplifying model assumptions. The OECD/Russian RASPLAV project should address (ii).

We agree with addition (i), and can expect that ACOPO will be studied by various analytical approaches, from CFD to simple phenomenological models. We would advise caution about item (ii) however, because prototypic material experimentation in this area along the lines pursued in RASPLAV is expected to introduce significant distortions.

> *33. Having retained the debris as a high temperature melt, a decision would be needed as whether this was an acceptable configuration over a period of many days. I assume this issue will be addressed when late reflood is considered in the subsequent report.*

The report mentioned will address pressurization events due to fuel-coolant interactions, including those due to late addition of coolant. The decay heat source would reduce with time, leading, very gradually, to refreezing from the boundaries inwards. Final quenching would be obtained after recovering injection capability and flooding the vessel internally also.

DRAFT

APPENDIX U

EXPERT COMMENTS AND AUTHORS' RESPONSES CLASSIFIED BY TOPIC

TABLE OF CONTENTS

Each question or comment is identified by the first three letters of the reviewers' last names and a number that corresponds to the index used, *by us*, in Appendix T. That is, May3 is point #3 in our breakdown of Prof. Mayinger's review comments, as done in Appendix T. The three-letter code used for each reviewer is as follows:

Cheung (Che)	Chu (Chu)	Dhir (Dhi)	Epstein (Eps)
Henry (Hen)	Kress (Kre)	Levy (Lev)	Mayinger (May)
Nickell (Nic)	Olander (Ola)	Schmidt (Sch)	Sehgal (Seh)
Seiler (Sei)	Shewmon (She)	Spencer (Spe)	Tuomisto (Tuo)
Turland (Tur)			

General Comments and Highlights

On F.B. Cheung's Review

This reviewer raises four criticisms, only one of which is identified by the reviewer as having conceivably an adverse impact on our conclusions. We disagree with all of them, and wish to rebut them point by point.

On T.Y. Chu's Review

We have some difficulty discerning the reviewer' position relative to the simulating power of the ULPU experiment. Also, it is not clear to us to what extent this reviewer agrees with our conclusions about AP600. On the one hand he states that "because the margin to failure is fairly significant the reviewer feels that despite the inaccuracies involved, the critical heat flux data is sufficient for the present purpose," but on the other hand he opens up the whole issue by requiring a more "detailed justification" of Configuration II.

It is interesting that this reviewer is able to find all sorts of detailed aspects to question the ULPU approach, yet he does not hesitate to use the Cheung and Haddad *quenching* experiment as the standard for judging the appropriateness of the ULPU CHF values at $\theta \sim 0°$, or to compare Vishnev's correlation from a lab-scale experiment in pool boiling, with our Configuration II data. We believe that both are so removed from the phenomena we are interested in, that in the absence of any mechanistic hypotheses such comparisons are not only unjustifiable, they may even be misleading (i.e., create a false sense of security that there is confirmation from multiple sources).

We hope the enlarged data base (new Appendices E.3 and E.4) will help the reviewer better appreciate the simulating power of ULPU and to understand our statement about "other." experiments in the previous paragraph. The nature and potential significance of the "inaccuracies" that he is referring to is addressed in a point-by-point fashion.

On V.K. Dhir's Review

This reviewer hesitates to accept the report conclusions on critical heat flux. Besides seeking a number of clarifications, there seem to be two main reasons for this. First is his assertion (his point #2) that "in the reactor cavity, counter current type of flow simulation will occur rather than that of a natural circulation loop (co-current)". As shown in Figure 3.1 and discussed in Appendix M, this is *not* correct. The flow enters the cavity near the lower head elevation through a tunnel connecting the cavity to the steam generator compartment. Most likely, the reviewer was misled by Figure M.1 (one of the "standard pictures of the AP600 compartment") which represents a cut that is not through this tunnel. The same is true in Figures K.1 and K.2, and we regret that. Also, the confusion may be due to our not showing the insulation in Figure 3.1. The actual geometry can be understood by visualizing the tunnel shown in Figure 3.1, superposed on Figure K.2. To eliminate the chance of such a misunderstanding in the future, we have added a statement to that effect in the caption of Figure K.2.

Second, the reviewer asserts in his closing paragraph, "...at this point, the information is incomplete and it is not possible to conclude that boiling heat flux on the outer surface of the vessel will be below the local critical heat flux under all types of heat fluxes imposed on the inner wall of the vessel." In the intervening few months since the report went out for review, we continued work with ULPU (Configuration II) to better understand the behavior and hopefully zero in on the mechanism. In particular, we now have data under a much wider "spectrum" of flux shapes and natural circulation flow rates that reveal as insensitive behavior. Moreover, we were able to "visualize" the boiling crisis and obtain data on systems effects simulating the actual thermal insulation design, including the exit flow restriction. All this material is provided in new Appendices E.3 and E.4, and we believe fully addresses this concern of the reviewer.

On M. Epstein's Review

This reviewer raised a valid point, about the flux distribution in metal-layer-wall interface (due to boundary layer development). It was an oversight on our part not to be explicit about it. A relevant discussion has now been added at the end of Chapter 5. We also appreciate his point about jet-diameter behavior and the role of metallic jets in the impingement analysis of Chapter 8. These points are addressed in the new Appendix O.

On R.E. Henry's Review

While this reviewer agrees with the technical positions and conclusions of the report, he requests several "enhancements" to be made, in the interest of clarity. These, and their disposition, are discussed point by point.

On T.S. Kress' Review

The main point, and concern, of this review was summarized in the closing statement: "The defense of the case, in my mind, strongly rests on justifying the choice of decay heat value." Having cited the reference, Schnitzler (1981), we did not elaborate on the bases of the calculation. Further, we now realize that the short explanation at the bottom of p.7–1, may mislead one to think that all we are accounting for is the relative decay of volatiles, to obtain an effective non-volatile power fraction. We welcome the opportunity to clarify this point here.

The reference (Schnitzler, 1981) describes models developed for use in the SCDAP-RELAP code in order to properly reflect the time-dependent decay heating in disrupted fuel regions. Our Figure 7.2 curve is one of four given for a typical PWR core evaluated at equilibrium (33,800 MWd/MTU), under various heatup assumptions—we chose the one representing the highest decay heat values in the time range of interest (i.e. after 3 to 4 hours). The calculations were carried out with the ORIGEN2 code, using the time-dependent removal rates shown in Table T.1 (p.T.29), where the various radionuclide groups are identified in Table T.2 (p.T.29). As can be seen in Table T.1, no releases are assumed to occur after 16 minutes, so all subsequently formed volatile products are retained in the melt. Reviewed recently for our present purpose (Osatek, Personal Communication, 1995) the removal rates in Table T.1 were found to be reasonably conservative, except for Groups 6 and 7 (Ba and Sr). However, the *total* contribution of these radionuclides was found to be under 0.4%, which is too small to warrant that an actual correction be made.

On S. Levy's Review

This review is highly critical on *the* whole range of topics covered in the report. Not a single thing seems to have satisfied the reviewer, and not even a few things can be selected as *the* major criticisms. Accordingly, we are reluctant to provide here any highlights. The review and our responses have to be read in their entirety. We found nothing in this review that would alter our conclusions.

On F. Mayinger's Review

We deeply appreciate the favorable remarks and insights offered by this reviewer. We agree completely with his comments and contributions to the issues discussed in the report, based on his own work.

On R.E. Nickell's Review

The main thrust of this review is to express a concern, namely, the possibility of ductile tearing due to longitudinal bending stresses, especially due to "discontinuities" of mechanical loading and thermal conditions along the longitude of the lower head. In fact, we considered this failure mechanism and discarded it as one of secondary importance. We do recognize, however, that its explicit consideration in the report is appropriate, and it has, therefore, been included as an addendum to Chapter 4. In doing so, we also took up some of the more detailed suggestions made by the reviewer, and we trust he will find our response complete and satisfactory.

On R.C. Schmidt's Review

We appreciate the favorable comments regarding the ACOPO experiment concept, and the suggestions made to further justify and improve the validity/usefulness of it. There is, however, an interesting variation (between the reviewer and us), at the conceptual level, on what ACOPO really represents and why it can be expected that it is adequate for our purposes. We believe the reviewer's interpretation is more restrictive, but, as he shows, even that is sufficient for our purposes at the ACOPO 1/2-scale (only marginal at the mini-ACOPO scale). Since the data from ACOPO are imminent, this variation does not give rise to any real issue. On the contrary, this should be very helpful in convincing Prof. Sehgal (of the validity of the ACOPO approach) who actually has questioned it. With this in mind, the two interpretations are discussed further under the point-by-point response.

On B.R. Sehgal's Review

This review is very thoughtful and comprehensive. Even though it is favorable in many respects, it also raises questions about some of the most fundamental aspects of the treatment. Namely, the scenario-independence (bounding nature of the final, steady-state pool), the validity of the ACOPO experiment, and the existence of crusts, as a boundary condition, in the jet-impingement region. The scenario independence was raised by several other reviewers as well. It is addressed in Appendix O. The validity of the ACOPO concept has been questioned by means of several specific technical points, including transient effects on stratification, and on pool response in general, heat flux distribution on the upper surface, and independent Pr number effects. These are all addressed point by point below. Additional light will be shed by the main ACOPO experiment, which is on the way, although we regard it as confirmatory. Finally, about the crust, the basic idea is that the molten oxide solidifies upon contact with molten steel, for any reasonably relevant level of turbulence, and the effective thermal boundary condition is not dependent on crust stability—or growth and remelting. Details are provided in the point-by-point response.

On J.M. Seiler's Review

We appreciate the comprehensiveness and depth of this review. There is general agreement, but a number of issues are raised as well, especially on the intermediate states (transient aspects of scenario), for which several suggestions are made, and the impingement of metallic jets during relocation. These issues are addressed in Appendix O. Several other points of a more detailed nature are discussed in the point-by-point response. We trust these will be found adequate.

On P. Shewmon's Review

The reviewer agrees with our approach and conclusions but is concerned that "someone will have to make the decision to flood the reactor cavity, and do it in a timely manner," and emphasizes that this should not be put off until "the last minute." We certainly agree, and perhaps did not emphasize enough that clear procedures and operator training are tantamount to removing this from the realm of someone's arbitrary decision process. This is now emphasized in the updated analysis of the cavity flooding system and its reliability (see addendum to Appendix M).

On B.W. Spencer's Review

Our perception of this review is that while there appears to be no specific disagreements with the methodology and technical bases of the report, there is no concurrence with the conclusions either. It is implicit, but clear enough, that to obtain concurrence on the conclusions we must address the seven specific recommendations made. We have no problem with four of these recommendations, and believe they have been fulfilled in the manner described in the point-by-point response. We disagree, however, with the remaining three, and provide what we believe to be appropriate and adequate rebuttal.

Briefly (for details see point-by-point below) all three points relate to prototypic material experimentation. Specifically, the reviewer proposes: 1/2-scale ACOPO-like experiments with prototypic materials (his item #5), examination of chemical-related attack using prototypic materials, including fission products (his item #6), and "integral" tests with prototypic materials run over long time periods (his item #7). Our position is as follows:

(a) We agree that the technology for carrying out ACOPO-like prototypic-material experiments is available, and agree that such tests would be a good idea. In fact, we originally proposed these tests to ANL/DOE, once our data from mini-ACOPO confirmed the principle of the technique—we called them PACOPO (for prototypic ACOPO). However, our reasoning for having suggested these tests is totally different from the reviewer's, and we definitely do not believe they are *needed* to reach firm conclusions about the AP600.

(b) We certainly agree about the importance of in-depth understanding of chemical-related attack phenomena, and have continued the work along the lines described in Appendix J. Besides Zr, this work, which included control rod materials (such as silver), is described in an addendum to Appendix J. We did not expect, nor did we find any major surprises. The fundamental point is that because of the very steep temperature gradient in the wall, the remaining thickness and wall integrity are not sensitive at all to any reasonable uncertainties in the wall eutectic-melting temperature. With this additional work we believe we have reached an adequate level of confirmation about IVR in the AP600.

(c) Having addressed chemical attack, as in (b) above, and creep as already done in the report, we see no merit in the long-term testing proposed. These are the only phenomena with potentially long-term effects, and unless a specific shortcoming is pointed to in what has been provided, such tests would be of little or no value.

On H. Tuomisto's Review

We appreciate the favorable comments and the insightful questions raised about several pertinent aspects that we overlooked in preparing the report. Having these questions raised at this time affords us the opportunity to enhance significantly the completeness of the treatment. We believe our response will solidify the reviewer's agreement with our approach and conclusions.

On B.D. Turland's Review

We appreciate this deeply thoughtful and comprehensive review. Broad agreement on the methods and particular aspects of the study and agreement with the conclusions have been noted. Many points of clarification and constructive requests for completeness have also been made. We find ourselves in good agreement throughout, and have attempted to respond in the spirit requested. There are only two areas where there appears to be some difference of opinion. The one is the most severe parametric case in the report, which we call "extreme"; the reviewer considers it to be "not as extreme" as we do. The other is in prototypic material experiments. It is not exactly clear to us how strongly the reviewer feels about this comment, but he has mentioned it a couple of times. We believe that although such experiments would be quite welcome, they are not necessary to support our conclusions for the AP600. The reason is that natural convection is a pretty basic process, and once the correct range of the governing dimensionless group (Ra') has been reached there is nothing really to wonder about. Moreover, we wish to express caution, as such tests will inevitably be attempted, that due care and attention be given, so as not to be lead astray from what we know already, due to distortions forced upon us by experimental difficulties.

General Comments

Che1. *It is a pleasure to participate in the review of the above-referenced report. As you requested, I have concentrated my review in the areas of natural convection and critical heat flux covered in the document.*

The various chapters and appendices that address natural convection and critical heat flux in relation to lower head integrity are generally well written. They provide a detailed description of the major findings of the work performed by the authors and a concise summary of others' past and on-going research efforts. Overall, the information presented in the report appears to be quite convincing and complete. There are, however, several important technical points that are not well substantiated by experimental evidence and/or sound theoretical arguments. These technical points, which need to be further evaluated, are discussed below.

* *

Eps1. *I have read with care the chapters of the above-references report that were assigned to me, namely Chapters 2, 6, 7, 8, and 9. I felt compelled to also read Chapter 5 in order to gain the required background for Chapter 6.*

Overall I find the authors' version of in-vessel retention to be a scrutable and believable one. In particular, I liked the authors accident scenario-independent treatment of the subject. Moreover, I feel that the report will serve as a handy reference source for the pertinent, recent literature on natural convection in volumetrically heated pools, downward boiling, and thermophysical properties of high-temperature materials.

I only have two major comments with regard to the technical content of the report, both of which are aimed at strengthening the authors' already good case for in-vessel retention. These comments are listed below and are followed by several additional, but relatively minor comments that the authors may wish to consider.

* *

Hen1. *As requested, I reviewed the report entitled, "In-Vessel Coolability and Retention of a Core Melt". I agree with the general approach taken in the report, the formulation of the analyses for the molten pool, the relative distribution of heat fluxes from the pool and the conclusions of the report. While I believe some* *additions need to be made to the report, which are discussed below, this report can be used as a document which assembles the major works performed in this area and provides sufficient justification for the conclusion that external cooling of the reactor pressure vessel lower head and cylinder can prevent failure of the structures even when molten core debris exists in the lower head.*

* *

Hen8. *As mentioned above, I believe that this report provides the necessary foundation for documenting the case for in-vessel retention using the numerous attractive features of the AP600 design. However, to provide this foundation, several of the discussions in the report should be enhanced such that the approach and conclusions are clear.*

Done as described above.

Kre1. *Introduction:*

In this review, I considered the key items that would influence the ability of external cooling to prevent vessel failure to be:

1. *Quantity of melt*
2. *Composition of melt*
3. *Decay heat level*
4. *Internal pool heat transfer*
5. *Radiation heat transfer off top surface*
6. *Boiling heat transfer on outside of vessel*
7. *Integration of Items 1–6 (resultant wall temperatures, wall thinning, ability of wall to carry loads, and treatment of uncertainties).*

My review comments that follow are ordered as above and are intended to address the adequacy with which each of these were dealt.

* *

Kre4. *With the ROAAM procedure, I worry about cliff effects. An abrupt and severe change in the probability between ranges could mask a strong sensitivity in the region near the abrupt change. Because of the focussing effect of the* *metallic layer, the content of steel might be such an area to expect such a strong sensitivity.*

One needs always, with or without ROAAM, to worry about cliff effects. This is why we run extensive parametrics, and are open to reviewers' suggestions for even more. It is important to understand, however, that this is (should) not be a random exercise, but rather guided by the physics of the situation, and the results obtained already—as illustrated, for example, in the previous two questions. For example, we explored the focusing effect and its asymptotic implications in general (see Chapter 6), before finalizing the parameter ranges and sensitivities considered, to ensure that no credible near-cliff was overlooked. This sort of approach is essential to the proper application of the ROAAM process. Here it is suggested by the reviewer that because of the focusing effect, the steel content might present a strong sensitivity. In fact, at the end of Chapter 5, we provided a detailed evaluation of the point (i.e., at what metal depth the focusing effect sets in), and even provided general quantitative results in an easy-to-use graphical form. These, and the results in Chapter 7, show that in AP600, we are far from conditions where focusing concerns could arise. Further, focusing cannot be obtained by adding steel, as suggested by the previous question. Moreover, even when a condition leading to focusing was *contrived* (Figure 7.16), failure could not be obtained. See also Appendix O.

Kre20. *Final Comments:*

This was indeed a comprehensive and competent piece of work to address this issue. I checked all of the equations presented and could find no errors.

The report itself suffers, I believe, from including too much peripheral material put there for "perspective". I think the report would have been better if it focused more on what was actually done and on the correlations actually used in the analyses.

We put in the report nothing more than necessary to explain and support our case. In the overall perspective of the 17 reviews obtained this is really a surprising comment.

Lev1. *As per Argonne National Laboratory (ANL) request of November 10, 1994, I have reviewed the subject report and I wish to first command [sic] the authors for their extensive analytical and experimental work in support of the concept of "in-vessel retention" in the AP600 passive nuclear Pressurized Water Reactor (PWR). However, I have several concerns about the studies and I have attempted to group them by specific topic areas to help the authors prepare responses to my comments:*

* *

Lev35. *5. I continue to remain confused by the use of the Risk Oriented Accident Analysis Methodology and the judgments used to formulate probability density functions in Section 7 but I have decided to defer on this topic to other reviewers more familiar with probabilistic and risk assessment techniques.*

* *

Lev36. *6. It is recommended that the report title be limited to the specific case of the AP6OO concept. It requires depressurization of the vessel, its lower-head to be fully submerged and a low power density. The combination of such characteristics is found today in only the AP600.*

The data and techniques can be, and they are being, used for other reactor designs. The AP600 is just the first illustration.

Lev37. *In summary, at the present time I cannot support the conclusion on top of Section 9 that "thermally-induced failure of an externally flooded AP600-like reactor vessel is physically unreasonable". There is no question that the chances of in-vessel retention have been improved but the conclusion that failure is physically unreasonable will require dealing with the comments provided herein and particularly with the need of prototypic CHF tests and natural circulation tests with prototypic corium and metallic pools.*

We found nothing in the above comments that would alter our conclusions. Also, it is not clear to us what is meant by "There is no question that the chances of in-vessel retention have been improved". Improved compared to what?

Lev38. *I hope that these comments are useful to you and I appreciate the opportunity to participate in the review. Before closing, let me reiterate that my negative comments are not meant in any way to detract from the progress made about in-vessel retention by the investigators participating in DOE/ID-1046.*

* *

May1. Introduction

In spite of the experience from the TMI-accident, where several tons of core melt were retained coolable in the lower plenum of the pressure vessel, most of the severe accident studies assume that the melt penetrates the pressure vessel and the only way of retention would be a core catcher, integrated into the concrete of *the containment. In the face of this opinion of many specialists, it is a great service to a realistic assessment of nuclear reactor safety, that the U.S. Department of Energy initiated and sponsored a study on In-Vessel Coolability and Retention of a Core Melt, which was performed by T.G. Theofanous and co-workers and which is subject of review here.*

The study was concentrated on the future concept of the AP600-nuclear power plant, however many general conclusions can be drawn for other types, also for nuclear reactors being in operation. Therefore the report deserves general consideration in the nuclear community.

The capability of the pressure vessel to retain the molten core is a function of the heat transfer coefficient at the inner side of the pressure vessel wall (between corium and wall), of the heat conductivity in the material of the wall and the heat transfer at the outer side of the wall,(between wall and boiling water). In case, that the heat conduction in the wall would be the limiting parameter one has to check, whether in a melting attack, the wall thickness is so much reduced, that it cannot carry the weight of the molten core any more, even being supported by the buoyancy force of the surrounding water-vapour mixture.

* *

May5. <u>Conclusion</u>

The report DOE/ID-10460 is a very fine and reliable document on the coolability of a core melt in the pressure vessel of a medium-sized nuclear reactor and proofes, that a hypothetical core melt situation can be managed and that the debris can be safely retained in the pressure vessel.

Questions rising in connection with that problem are carefully discussed and satisfactory answers are given to all issues, being linked with the thermo- and fluiddynamic phenomena under core melt conditions. The report makes a great and very valuable step forward in the risk assessment of nuclear power plants, especially of nuclear reactors of future design. I would like to congratulate the authors to their work.

A few minor additions to the report – as mentioned in this review – would probably be of interest to the reader, who is not an expert in heat transfer and could improve the value of the report still more.

* *

Nic1. *Thank you for the opportunity to review and comment on the referenced report (Reference 1). Specifically with respect to the experimental and analytical investigations of ex-vessel heat transfer phenomena for a submerged reactor vessel lower head following a severe (core melt) event, this report is very comprehensive. An excellent case is made for the bounding values of heat flux through the vessel wall. (These heat flux limits are referred to erroneously as "thermal loads" in the report, a term that should be reserved for the product of thermal expansion and structural stiffness.) However, my assignment was to review the structural implications of the report, concentrating on Chapter 4 (Structural Failure) and Appendix G (Creep Considerations for the Lower Head). Other portions of the report were examined for context. My comments on the structural sections of the report are provided below.*

We disagree that our term "thermal loads" is erroneous as applied in the report. As explained in the report, heat fluxes above certain limits can cause failure by a mechanism quite distinct from those due to thermally-induced stresses discussed in Chapter 4 and Appendix G. Thus, we have a generalized "load," and a usage quite common in thermohydraulics. Note that to distinguish between the two we use "thermal loads" versus "thermal stresses."

Sch12. *Msc. Editorial type comments, minor corrections and questions*

Pg. 2-1. Next to last sentence: *What is a "Grade B approach"? A reader such as I has no idea what is meant here.*

See Appendix A.

Seh1. *Professor Theofanous and coworkers should be congratulated for writing a beautiful report on the subject, which almost reads like a text book. I honestly enjoyed reading it and appreciate it very much, since the treatment is comprehensive and logical and I learnt a lot. They have performed much original work during the process of resolving this important issue. I do have a number of comments, which will be described in the following text. I will begin by providing general comments on the core melt in-vessel retention (IVR) concept and continue with comments on the document. This will be followed by comments on some specific items in the document and I will state my conclusions in a summary.*

General Comments on the In-Vessel Core Melt Retention Concept

Ever since light water reactor severe accidents stepped into the consciousness of the reactor safety physicists and engineers, they are considered synonymous with molten core on the floor (core melt-down), Certainly, there are other detours, e.g. a steam explosion, or a high pressure melt ejection (HPME), which could distribute the core in particulate form all over the containment. Since the severe accident was considered as "fiction" during the years when the currently installed PWRs and BWRs were designed; the designers just threw up their hands, said "So be it! we will just make the containment strong and let us forget about this melt-down accident".

Since 1979, when the severe accident assumed a little larger reality, the nations of the World, possessing light water nuclear power reactors, have been spending millions of dollars each, every year, on severe accident research to prove (or show with high assurance) that, (1) severe accidents are very rare indeed and (2) even if we get one, and the molten core lands on the floor of the containment, our strong containments (designed with foresight) will hold up long enough to, (a) reduce the radioactive emissions significantly and (b) enable evacuation of the near surroundings. The money spent has produced good results and, except for one or two remaining issues, the case has been made to the satisfaction of most technically-knowledgeable observers, if not to the satisfaction of the public at large.

In the last few years, accident management has come to the fore as a concept to upgrade the safety of the existing and the future plants. Accident management measures for the existing plants have varied from one country to another; e.g. Sweden has installed filtered vents on all of its plants and inerted the BWR containments, while U.S. has only inerted its BWR Mark I and II containments. Accident management has been brought into the design process for the future plants. In particular, the designs of the U.S. passive plants, and of the European pressurized power reactor (EPR), are incorporating accident management features which, hopefully, will provide substantial additional safety margins, so that even the need for public evacuation is virtually eliminated.

The concept of retaining the molten core within the vessel in the event of a severe accident, should be very appealing to both the operators of the plant and the public. Certainly, keeping the radioactivity confined to a smaller volume and not having to cope with an extensive clean-up and decontamination operation, is *a very worthwhile goal. The U.S. passive advanced PWR design, the AP600 has adopted this accident management concept, and establishing its feasibility and reliability is the aim of this document. If it is successful in achieving this aim for the AP600, it will open the door for considering this concept for other plants: present and future. Thus, the effort here is a milestone and should be so treated.*

I believe, nature is quite partial to the in-vessel core melt retention concept. It has been found that the heat removal, with boiling water on the external surface of the reactor pressure vessel, varies the same way, as a function of the polar angle, as the thermal loading imposed on the inside surface of the vessel by a naturally circulating core melt. Additionally, the maximum heat removal rate at the very bottom of the spherical vessel is substantial, due to the particular boiling mechanism that nature prefers there. If these two natural occurrences were not so disposed, the concept of in-vessel core melt retention could not materialize into reality.

* *

Seh27. *Summary*

I believe, Professor Theofanous and his colleagues have written a very beautiful document on the subject of in-vessel retention of core melt in the event of a severe accident. I believe, IVR is a very important issue for future nuclear plants in which accident management should be directly integrated in the system design.

Professor Theofanous and colleagues have considered most every aspect of the in-vessel core melt retention issue and have endeavoured to address the phenomena that are active in the process of retaining the melt in the vessel. Their emphasis is on providing data and models which illuminate and describe the physics of the various processes occurring and then integrating all the various sub processes to emerge with the assessment of the margins. This is the essence of the ROAAM approach, and in this case it actually is much more straight forward than in the case of the issues of the BWR Mark I liner melt-through and the Zion PWR direct containment heating (DCH) loading, which Professor Theofanous helped resolve earlier through USNRC sponsored research efforts. The experimental backing for the correlations and the models employed, in this document, to arrive at the thermal loading, and the maximum heat fluxes allowed, is also much more extensive than it was for the Mark-I liner melt-through issue. Professor Theofanous and colleagues have themselves performed original research and provided key data, *on the CHF at the vessel external surface and on the heat fluxes on the vessel internal surface.*

* *

Seh28. *I have made several comments on the evaluations employed in the document. I believe, some of the questions asked are important in providing greater depth and validity to this document for the resolution of the IVR issue. My major question is about the possibility of getting a smaller thermal margin in some intermediate and transient state before reaching the final stable state, where there is an ample margin to accommodate the thermal loading imposed. I have also asked some questions about the ACOPO experimental technique, which I believe is unique and ingenious, however, should be qualified by, perhaps, measurement of the natural convection flow patterns. The evaluations of the jet impingement thermal loadings, and the vessel ablation-depth estimates, are not as complete as one would wish and, perhaps, the authors could strengthen those analyses. Some other points have also been raised e.g., the Pr number dependence of the downward thermal loadings, the effect of the phase changes at the boundaries on the heat fluxes etc.*

(a) We hope the additional discussion on transient effects provided above, and of intermediate scenarios in Appendix O, has fully addressed this area of the reviewer's concern.

(b) We explained that the questions raised about the ACOPO technique are not valid, and that the stratification patterns measured provide a good indication of the natural convection flow patterns, and their appropriateness to the problem at hand. More confirmation will be obtained from the 1/2-scale ACOPO in the next few months.

(c) The analyses for melt impingement were purposely made such as to clearly bound the behavior and provide an overall perspective on the margins to failure—which are very large. As explained above, we do not agree with the reservations expressed.

(d) All these other points have been addressed above too.

Seh29. *Finally, I believe, that the authors have based their case for the high thermal margins available during the in-vessel core melt retention for the AP600, primarily on the data measured in the ULPU and ACOPO facilities. It would be highly instructive for the reviewers to observe a key experiment, or two, in each of these facilities and examine the instrumentation and the experimental procedures. This will lend much greater confidence to the peer-review process.*

This is a reasonable request and we plan to invite all those reviewers wishing to see the facilities in the near future.

Sei1. *This is clearly a very important work containing very pertinent and new data. Comments will address different parts of the work.*

* *

She1. *This report treats primarily, almost exclusively, the case in which all of the core, core-internals, and lower support structure have melted, and a steady-state has been attained. This molten material fills the lower head with the dense oxide of the core and a layer of molten metal floats on top of it. Convection brings the heat to the top and side surfaces where it is carried away by conduction thru the steel and radiation upward. With water surrounding the vessel, heat can be removed so effectively from the external surface thru nucleate boiling of water that the external surface remains well below the heat flux required for dryout, and vessel failure. I have read the report carefully, and sought other scenarios that might lead to vessel failure. Provided the reactor cavity is flooded in a timely manner, I believe that a molten core could be contained and adequately cooled inside the pressure vessel.*

* *

Spe1. *The report "In-Vessel Coolability and Retention of a Core Melt" by Prof. T. G. Theofanous, et. al., is an excellent synergism of preexisting data plus new data in support of deterministic models, together with a rational methodology for addressing parameter ranges, to address the viability of in-vessel retention in a core melt accident scenario for AP600. This reviewer agrees with the approach and methodology used in the report. Caveats pertaining to key AP600 features and future design decisions are clearly presented and are important in assessing the basis of applicability for the AP600 system.*

* *

Spe4. *It is clear that the upward/downward split of heat transport from the corium pool is crucial to the overall problem, including the presence of a steel top layer wherein radiation heat loss and sideways heat transfer participate in the integral processes. The analyses based on existing database yield the distribution of loads which are shown in the report to be removable from the walls with considerable margin; i.e., in terms of remaining wall thickness in relation to wall* *load bearing requirement (for fully depressurized system) and in terms of the polar variation in heat flux in relation to CHF limitation.*

[3. The additional work the authors list in Chapter 9 to strengthen the report basis should be pursued.]

From the additional work described in Chapter 9, only the item referencing the lower head surface characteristics, and perhaps thermal properties, is germane to concluding the assessment for the AP600. This work has now been completed and is reported in Appendix E.4. The results, as expected, confirm the conclusions reached previously. The other items mentioned in Chapter 9 may be pursued in conjunction with the European Passive Advanced PWR design (a scaled-up version of the AP600).

Spe8. *[7. For a ground-breaking safety approach as important for AP600 as IVR, it is warranted to perform a large-scale, integral test to demonstrate the viability of the integral processes over a lengthy duration. Real reactor materials, real vessel head material, and internal heat generation are required for such a demonstration test. The experiment technology is readily available to utilize a slice geometry analogous to the authors' own COPO experiments. A representative AP600 corium composition should be employed with the wide range of relevant materials as included in (6) above. The test may start from particle bed form.]*

This has been addressed under general comments. The "lengthy time duration," again, seems to drive this question, and again, we cannot identify a technically-based concern.

Tuo1. *General remarks*

In-vessel retention by external flooding is an effective means to reduce thermal and energetic challenges to the containment integrity during core melt accidents. If the concept is applied as a basic severe accident management strategy, it is really an essential task to assess the overall feasibility and reliability. The report makes a remarkable synthesis of the thermal regime of the in-vessel retention by external flooding.

The in-vessel retention concept was introduced to the severe accident management considerations in the end of 1980's. The technical feasibility was initially demonstrated for the Loviisa Nuclear Power Plant by Prof Theofanous (see Ref. 39 of the Report). Since that time, plenty of new research has been performed *to confirm the first demonstration. New information has been generated to a large extent in support of the Loviisa and the current ARSAP program. In these studies, the ROAAM approach has been applied to evaluate the risk of failing in the thermal regime.*

The report and the problem treatment as a whole has been organised in an excellent way. It has been a great pleasure to have an opportunity to read it and to find the beauty of such developments as the idea behind the ACOPO experiments, and the thermal treatment of the metallic layer.

In the following, Approach and Assessment applied in the report are discussed. This is followed by some detailed remarks, which are meant for obtaining further clarification of certain aspects of the thermal regime and for the overall resolution of the in-vessel retention concept.

Approach and Assessment

For evaluation of the approach and the assessment, there are two questions to be answered:

- *Is the approach sufficiently consistent and comprehensive to allow the overall assessment?*

- *Is it justified to say that the issue is principally and practically solved, and that only confirmatory research is necessary any more?*

Fortunate to the reviewer, the applied ROAAM approach itself makes it possible to answer these subjective questions.

The ROAAM approach has been developed to deal with phenomenological uncertainties in complex physical and technical problems. The ROAAM has reached a mature state of development and application. Appendix A to the report is very essential for understanding the principal methodology. The starting point is to create the quantification framework by dividing the problem to such pieces which can be treated in the physically meaningful way. One of the most powerful features is that all new developments of the subject can be easily integrated into the framework and into the quantification.

The quantification framework needed for the in-vessel retention has turned out to be comparatively simple. First of all, the simplicity reflects that large margins are available for the heat transfer from the heat generating oxidic pool itself.

Therefore, the framework concentrates on unfavorable conditions of the metallic layer. On the other hand, the simplicity can be understood to imply that the developmental stage is mature enough.

The available experimental results and theoretical considerations support the conclusion that the modelling uncertainties are very small in comparison to the margins. In terms of the ROAAM, I have no difficulty to agee that the assessment approach is of Grade B type and the maturation status (Phase IV) is reached upon completion of the peer review.

As shown in Fig. 1.1 of the report, the in-vessel retention issue will include the FCI Regime and the Steam Explosion Regime in addition to the Thermal Regime treated here. The final feasibility can be demonstrated after the separate report of melt-coolant interactions is available.

Concerning the practical design and Severe Accident Management measures, i.e. ensuring free water flow on the vessel and assuming low pressure conditions, some comments are included later. Notwithstanding, my answer to the above questions is positive: the treatment is consistent and comprehensive, and it is justified to state that the thermal regime is resolved to the point where only confirmatory research and practical design solutions are necessary.

* *

Turl. *1. INTRODUCTION*

This review concentrates on Natural Convection, covered in Chapter 5 of the report, and the overall approach and assessment covered in Chapters 2, 6, 7, 8 and 9 and therein referenced appendices as requested in the letter from L.W. Deitrich dated 10 November 1994. The review is organised as follows: Overall comments are given in Section 2, while Section 3 contains the detailed comments on Chapter 5 (natural convection), and technical comments on the other nominated chapters are given in Section 4. An appendix contains details of typos found during this review. Below, the word 'authors' refers to the authors of the original study (Theofanous et al).

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Tur2. *2. OVERALL APPROACH*

The authors make clear that the AP-600 design is favourable to in-vessel debris retention by cavity flooding. I support this view, particularly because of the absence of lower head penetrations and the ability to get water into the cavity. However the information given in Table 7.3 (accidents contributing to the core damage frequency), the PRA information given in Appendix M (cavity flooding unsuccessful in 20% of the core damage cases), and the discussion of the thermal insulation (Appendix K) all indicate that even if one had complete confidence in the analysis presented in the report, there are still likely to be circumstances in which debris would not be retained in the lower head. In my view, these PRA and engineering related issues deserve priority, because the report does make a strong technical case for in-vessel retention provided the constraints of prior depressurisation and action to initiate cavity flooding are met. This may require systems enhancements to obtain the necessary degree of assurance.

Agreed. Please see addenda to Appendices M and K and revised Table 7.3 in the addendum to Chapter 7.

Tur3. *Like the authors (apart from more specific comments on Chapter 2, below), the reviewer will assume that the above prior conditions have been met, and concentrate on the heat transfer aspects of demonstrating that the debris will be retained in the vessel. The reviewer's observations are based, in part, on an unpublished study performed in the UK for a large (1.3 GWe) PWR. Apart from penetrations, which are present in this design but of no concern for AP-600, and the somewhat larger inventory of fuel, which reduces margins, the major threat to vessel integrity was identified as being due to the focusing effect of the metal layer, consistent with the sensitivity described in Section 7.3 of the report. In the absence of penetrations, this independent study also concurred with the authors that the greatest challenge to vessel integrity is at high polar angles, not close to the pole of the lower head.*

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Tur4. *Turning to technical matters, the allowable uncertainties from the in-vessel distribution of the heat flux are high because of the high critical heat fluxes found in the ULPU experiments, particularly in Configuration II (Appendix E.2). These were obtained in a full water loop, without significant obstructions* *and opportunities for vapour accumulation. Confirmatory tests with the chosen thermal insulation and more prototypic flow paths are desirable.*

Please see new Appendix E.3, addendum to Appendix K, and implications of this additional information in an addendum to Chapter 3.

Tur5. *Note that most of the above points are covered in Chapter 9 by the authors.*

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Tur33. *Chapter 9*

The absence of lower head penetrations should be added to the key features that lead to the favourable conclusion for the AP600-like design (penetrations may not preclude melt retention in the lower head, but they make the analysis more difficult).

This chapter provides a fair summary of the assessment, and I support the conclusions drawn and the recommendations made. I would also add (i) development of an analytic model for interpretation of the ACOPO tests, and (ii) a limited series of tests with real material to support the conclusions drawn from simulant or simplifying model assumptions. The OECD/Russian RASPLAV project should address (ii).

We agree with addition (i), and can expect that ACOPO will be studied by various analytical approaches, from CFD to simple phenomenological models. We would advise caution about item (ii) however, because prototypic material experimentation in this area along the lines pursued in RASPLAV is expected to introduce significant distortions.

Tur34. *Having retained the debris as a high temperature melt, a decision would be needed as whether this was an acceptable configuration over a period of many days. I assume this issue will be addressed when late reflood is considered in the subsequent report.*

The report mentioned will address pressurization events due to fuel-coolant interactions, including those due to late addition of coolant. The decay heat source would reduce with time, leading, very gradually, to refreezing from the boundaries inwards. Final quenching would be obtained after recovering injection capability and flooding the vessel internally also.

Scenario Aspects

Che2. *1. Configuration Dominated by Natural Convection Phenomena*

The partition of thermal energy flow by natural convection presented in Chapter 5 and the formulation of thermal loads under natural convection presented in Chapter 6 were based on the steady-state configuration shown in Figure 2.2. This specific configuration represents the final state that would actually be realized in any in-vessel retention scenario. However, as explained below, this steady-state configuration may not bound all intermediate states and thus, it can not be solely based upon in assessing the natural convection problem at hand.

Following the initial, major relocation event but before the attainment of a final steady state, a transient situation could arise within the lower head in which a region of the molten pool developed a large local internal heat generation rate due to a concentration of the larger burnup portion of the uranium oxide fuel and fission products. This non-uniform, highly concentrated, volumetric energy source could cause a period of very intense heat transfer from the core melt to the local vessel wall. During this period, the downward heat fluxes in the local region could be considerably higher than those observed under steady-state conditions. Because of this intense, localized heating of the wall, a hot spot could develop in the lower head. This hot spot could lead to wall thinning and jeopardize the lower head integrity. However, the presence of a large localized heat source would induce strong convective currents in the local region, resulting in rapid dispersion and dilution of the fuel rich material. Once the fuel concentration becomes more uniform (i.e., diluted), it no longer would cause a high heat flux in the local vessel wall and the hot spot would diminish. This transient situation, which involves the development of a hot spot, is apparently not bounded *by the enveloping configuration depicted in Figure 2.2.*

It should be noted that a localized hot spot covering an elliptical region of approximately 1 m by 0.8 m was found to exist for about 30 minutes in the reactor lower head during the TMI accident. Results of the TMI-2 Vessel Investigation Project indicated that the hot spot was not caused by impinging molten corium jets. Rather, it was caused by a large localized heat source arising from sustained *heat loading from the debris on the lower head. Conceivably, the transient situation described above could arise under certain circumstances and thus, it can not be excluded in risk analysis.*

The TMI vessel was *not* cooled on the outside, and there is no need for much imagination (or need to invoke a "highly concentrated volumetric energy source" as the reviewer does) to understand that with 20 tons of melt on the lower head wall heating was inevitable. Rather, the mystery continues to be why the vessel did not fail! In fact, as explained already in the report (and further elaborated in a new Appendix O), the radial reflector in the AP600 is rather massive, and would require a prolonged thermal attack before it would fail. During this time the pool grows both radially and axially within the core region, being well mixed by natural convection. To have a realistic perspective on how much more (than the average) concentrated the volumetric energy source could be, one needs to look at the core power shapes, as demonstrated in Appendix O (Figure O.2). One can see a rather uniform distribution, and this, of course, is not an accident—it is obtained through fuel management.

Eps4. *(3) The authors may wish to reference Epstein and Fauske (Nuclear Technology 87, 1989, 1021–1035), as they were the first to suggest the core relocation picture illustrated in Fig. 2.3 (for TMI) and to my knowledge they were the first to examine heat loads from in-vessel molten-core material pools by using a methodology that is very similar to the one used in the subject report.*

Reference added in p. 2-5; but this kind of approach goes back to the LMFBR days.

Lev9. *B. Subdivision into Regimes and Lack of Analysis of Intermediate Transient States*

DOE/ID-1046 is limited to the long term natural convection-dominated thermal regime conditions depicted in Figure 1.2. There are several statements in the report that "this approach is conservative" (see page 2-1) and that "the thermal loads to the pool boundaries throughout the time period of a heat-up transient are bounded by the thermal loads in the final steady state" (see page 2-2) but very little basis and proof are offered for such positions. A few examples are given below to show that it might not be the case:

1. The proposed long term pool configuration depicted in Figure 1.2 shows an oxidic pool surrounded by an oxidic crust with a metallic layer above it. According to the report, most of the metallic layer comes from the melt out of stainless steel *structures in the lower plenum and, during the heat up transient, the steel must rise through the oxidic pool before reaching the top layer.*

Not true. One can see in Table 7.2 that the lower supports (i.e., the lower plenum steel) amount to only 2 tons. This quantity is minuscule compared to the 65 tons from the reflector and the core support plate.

Lev10. *The temperature of the steel because of its high conductivity will approach that of the oxidic pool and during the melt out phase of the lower plenum it would be superheated and could reach temperatures above 2900 K. Such using superheated molten material will have several negative impacts, including:*

(a) As it reaches the vessel, it could lead to CHF conditions on the outside surface of the vessel.

(b) It could lead to failure of the vessel wall because superheated metallic material will attack and erode the vessel at an accelerated rate.

(e) It would not allow the formation of a top oxidic pool crust as depicted in Figure 2.1.

(d) It would radiate to top structural components and cause their melt and failure. Such top components would fall within the pool and disturb the natural circulation patterns as well as possibly produce cracking of the crust layer separating the oxidic pool from the reactor vessel.

In this reviewer's opinion, failure of the reactor vessel during this transient heat-up by the metallic layer or other causes (e.g. falling components) may be a dominant mode of failure and it is not considered in DOE/ID-1046 at the present time.

All these are highly speculative situations given the small quantity of lower plenum steel, as described in the previous passage. The effects of falling masses into the pool would be to mix it up and to cause a temporary reduction in superheat. The impact of a uniform temperature pool has already been accounted for in the mini-ACOPO tests, and ignoring the temporary reduction in superheat is clearly conservative. Note, however, that the two effects would be mutually counteractive.

Lev11. *2. On the top of page 2-2 it is noted that the report is restricted to "scenarios in which failure to supply coolant into the reactor vessel persists indefinitely". On page 1-3 it is stated that "energetic interactions concerning late* *water injection are relatively benign due to the prevailing stratified configuration" and the "integrity in the early potentially energetic, steam explosion regime, can be assessed against the full lower head capability". Addition of water on top of a rising superheated metallic layer will not be benign and may approach the steam explosion regime particularly if it contains between 10 to 65 percent by weight of molten zirconium (see Table 1.2). It will not be benign even with a stratified layer. Furthermore, before such energetic interactions occur the reactor vessel wall would be thinned down by impingement of a molten jet and by erosion by the hot oxides and metals and the full structural capability would not be available.*

The quotes given are out of context. This report addresses retention in the absence of water in the reactor vessel. Fuel coolant interactions potentially arising from late water addition will be assessed in a separate report, as noted already. In the introduction of the present report we only tried to lay out the overall approach, and draw the distinction between the energetics from the relocating event (premixed explosion) and the potential interaction event from late water addition (stratified configuration). There is little value in speculating about the outcome, before the work is even presented.

Lev12. 3. On page 3-3,it is stated that "partially flooded conditions are of limited interest, as discussed in Appendix M". In fact, in Appendix M, it is reported that "the PRA concludes that flooding was unsuccessful in 20% of the core damage cases" and this is high enough to justify dealing with a partially flooded reactor vessel. Under such conditions, the radiation would decrease to the vessel walls but it would increase to the top components and enhance their chance to fail and participate in the scenario. Also, there would be a sharp discontinuity in the vessel wall temperature much closer to the top of the metallic layer. Finally, the degree of water subcooling outside the reactor vessel would be lowered and so will the CHF condition.

Yes, but the report also states that flooding reliability will increase to meet screening levels as needed, if the accident management scheme is to be taken seriously. This was done (see updated Appendix M).

Lev14. *It is therefore recommended to reassess the conclusion on page 2-5 that there are only two specific configurations to be considered because they "bound the thermal loads on the lower head with respect to any other intermediate state* *that can be reasonably be expected". Other configurations, scenarios, and transient intermediate states need to be included and shown to not impact the results.*

This reviewer has not proposed (as discussed above) any scenarios that are not covered already by those in the report. Other reviewers did, and they are discussed in Appendix O. No impact on our conclusions resulted from these additional considerations.

Lev15. *C. Overstylized Pool Configuration*

The pool configurations shown in Figures 1.2 and 3.1 are very stylized and some of the presumed simplifications are expected to impact the predictions in DOE/ID- 1046:

1. The pool configurations and the heat transfer results are predicated on the existence of a crust (or solid interface) separating the oxide pool from the metallic layer. As noted under comment B.1, there can be no crust as the molten material from the bottom stainless steel structure rises through the oxidic pool. Even under long term conditions, it is difficult to visualize how a strong crust could form "naturally" above the oxidic pool and support 67 to 72 tons of metallic material over the large reactor vessel span of the AP600. Without a crust/solid interface the heat transfer at that surface could be higher because there would be a wavy interface produced by two counter flowing fluids. Also, the temperature at that location would be higher and above the specified oxidic components liquidus temperature of 2973 K and so will the bulk metallic layer temperature.

The crust is formed upon contact, and really represents a thermal boundary condition between the two liquids. In particular, it *does not* have to carry *any* loads—it simply transmits them to the liquid below. The report shows that the formation of such a crust is inevitable. Much of the reviewer's difficulty derives (as evidenced in **Lev9** above) from visualized large quantities of steel melting within a UO_2 pool in the lower plenum and producing a counter-current flow situation. As indicated in our response to **Lev9**, this is not true. Moreover, in the new Appendix O, we show that the major metallic components are added on the top of a substantial in size oxidic pool.

Lev34. *4. In Table 7.3 which tabulates the accidents contributing to the AP600 core damage frequency (CDF) from a Level I Probabilistic Risk Assessment (PRA), vessel rupture is shown to account for 23 percent of CDF and it is considered not relevant to in-vessel retention (IVR). This is not fully correct because* *it means that IVR cannot be effective on 23% of the accidents contributing to the CDF.*

This is exactly what we mean—if the vessel fails there cannot be in-vessel retention. The reason vessel failures show as significant contributors is the increased reliability of the passive AP600 design to prevent severe accidents. Moreover, Table 7.3 has been updated according to the revised PRA submitted to the NRC on March 1995. In the update vessel rupture was given more attention based on its earlier relative importance. The contribution of accident sequences lumped into this class is now estimated at 4.1% (of a total core melt frequency of $2.5 \cdot 10^{-7}$). This value should still be conservative, but such improbable events are difficult to quantify in any case.

Ola7. II Amounts and composition of the liquids in the lower head

The report justifies the large amount of steel in the metal pool (~72 tons) with the claim that an oxidic pool height of 1.5 m would touch the core lower support plate. This, in consequence, would melt, and along with it, substantial portions of the core barrel and the reflector. The 1.5 m height is based on the assumption that all of the fuel in the core is relocated to the lower plenum. However, in TMI-2, a larger reactor, only 20 tons of core debris reached the lower head, and consideration must be given to the possibility that the initial oxidic pool height is less than 1.5 m and does not contact the lower support plate. The smaller quantity of oxidic material than the entire fuel loading would reduce the heat fluxes q_{up} *and* q_{dn} *because the surface-to-volume ratio of the pool would increase. However, counteracting this is the probability that the fuel that did melt and reach the lower head would have a higher volumetric heating rate because it came from high-burnup regions of the core, near the center.*

The TMI accident was terminated! The TMI accident retained considerable quantities of water in the vessel that *quenched* the debris. The molten fuel mixes well by natural convection, and the pool has to extend to the radial edges of the core and remain there long enough before the reflector and core barrel are breached. The pool will be of significant size, and the decay heat in this mass *should not* be considered as coming from the core center and having higher decay power levels. See also response to Cheung item #2.

Ola8. *The most profound consequence of melting appreciably less than the entire fuel contents of the core is the reduced quantity of molten steel in the metal* *layer. If large portions of the core barrel, the lower support plate, and the reflector remained in place, the Fe concentration of the metal layer and the height of this phase would be greatly reduced, perhaps by as much as a factor of ten.*

On page 7-16 we explain that a minimum bound on the quantity of steel on top of the oxidic pool is 17 tons. This corresponds to a metal layer depth of 22 cm. This case, under a maximum thermal load (70% of the core), was shown to not yield failure. The "perhaps by as much as a factor of ten" is simply hypothetical and cannot be considered credible without some key considerations of how such a large quantity of core debris can get to the lower plenum without metal in the first place. See Appendix O for an explanation of why this is not consistent with physical behavior. Hence the consequences of it, as given by the reviewer below, are of no interest.

Ola9. *The consequences of this are:*

1. The heat flux to the vessel wall from the metal layer would be more focused, thus increasing $q_{l,w}$ (Fig. 6.3).

2. The composition of the Fe-Zr alloy would be in the Zr-rich region of the phase diagram, which has a lower eutectic temperature than the eutectic in the Fe-rich region which is assumed in the report.

3. Because of the small height-to-diameter of the metal layer, it could no longer be characterized by a single bulk temperature, T_b. There would be some radial bulk temperature gradient in this layer.

4. The remaining core support plate above the metal layer would act as an additional radiation shield and reduce the radiative heat loss from the upper surface of this layer.

These "consequences" are of no interest, as explained above.

Ola10. *5. With a greatly-reduced quantity of steel melted, the metal/oxide system would more closely resemble that of the TMI 2 core debris than the neatly separated liquid phases on which the report is based. Examination of the TMI 2 rock samples was extensively reported in Ref. 5. These studies suggest that the metal and oxide phases were never fully separated. Instead, the metallic phases were interspersed with oxide phases to form a slush that one report characterized as wet sand (Ref 5, p 187); another study (6) suggested that the $(U,Zr)O_2$ is* *transported to the lower plenum as a solid with the spinel phase acting as a lubricant. Relocation of core material to the lower head probably resembles a pour of wet concrete more than a clean flow of fully-liquid phases. It is even possible that distinct oxidic and metallic phases never separate in the core debris but remain in a dispersed state like oil and vinegar salad dressing. The analysis would then have to deal with a single composite medium with heat transfer through the connected liquid metallic phase and the heat source (at least part of it) in the dispersed solid oxide phase.*

Again the reviewer is reminded that the debris in TMI quenched by itself, even though the vessel was not cooled from the outside. The train of thought employed here is fundamentally flawed when applied to accidents that progress sufficiently to produce significant thermal loads in the presence of external cooling. We have to examine here a melt pour that is much more severe than wet sand, because the radial core reflector in the AP600 provides an effective obstacle to relocation (see Appendix O). Significant melt superheating must develop to produce a breach, and a significant length of time is required to do so. At that time the in-core pool has macroscopic dimensions, and we expect it to contain an oxidic melt (see Appendix O).

Ola11. III Mechanical Aspects

(a) Wall loading by internal pressure

The report considers two sources of stress generation in the vessel wall: dead-weight loading and thermal gradients. To these two should be added pressure loading, which may be important in high-pressure accident scenarios. The yield strength of the vessel steel drops sharply above 900 K. On p. 4- 1, the wall thickness that retains full strength (σ_Y = 355 MPa) is given as δ = 1.1 cm. The internal pressure needed to achieve an equivalent stress in a thin-wall spherical shell that is equal to the yield stress is

$$p_{tot}(\text{at yielding}) = 2\delta\sigma_Y/R$$

Using the above figures and $R = 2$ m gives a pressure for yielding of ~4 MPa. This total pressure (or greater) is encountered in some accident scenarios.

The report makes it clear that we are not interested in pressurized scenarios, even though the capability for such may be there. In fact, the capability would be more than 4 MPa, because the 1.1 cm used by the reviewer (taken from Chapter 4) was for the limiting case of fluxes close to

CHF. In the report we show that actual fluxes would be as low as half of that, and the pressure carrying capability correspondingly as high as twice the 4 MPa value.

> Seh2. *Comments on the Document*
>
> *I must start my comments on the document with Table 7.3. I was surprised to find that only 30% of the accidents contributing to core damage frequency (CDF) are relevant to IVR. Again, I was surprised to note that 23% of the CDF is caused by vessel rupture for which no accident management can be provided and 18% of the CDF is related to high pressure melt ejection (HPME). Perhaps, the ARSAP program should also target these two events, i.e. vessel rupture and the ADS failure, and provide reliable prevention strategies, so that their probability of occurrence is substantially reduced. There may be a greater potential of early containment failure with these hazards, than it may be with a few tonnes of oxidic and metallic melt discharged at very low pressure, into the water pool surrounding the vessel.*

Actually, the numbers in Table 7.3 should be understood in the perspective of a core melt frequency of $2.5 \cdot 10^{-7}$ per year. This exceedingly small value is a consequence of the passive emergency core cooling design, and many other attractive safety features of the AP600. That the order of magnitude of the other core melt classes has approached that of the vessel rupture is an indication that prevention of core melt has reached its "natural" limits. In any case the high pressure scenarios appear to have been exaggerated in the original PRA, and have been revised significantly downwards in the most recent version, submitted to the NRC on March 10, 1995. On the basis of this new information, Table 7.3 and the discussion of IVR scenarios are revised in an addemdum to Chapter 7.

> Seh5. *I have sorely missed an appendix or a section, on the core melt progression assumed. Clearly, the knowledge-base on the later phases of the melt progression is poor and some assumptions have to be made. If I follow the relatively better known scenario, the first discharge of the melt to the lower plenum (full of water) would be like that in TMI-2 i.e. in the range of 20 to 40 tonnes, (Appendix H actually assumes 47 tonnes, while section 8 assumes 22 tonnes). Next, if it is assumed that a certain fraction of the melt jet fragments and the steam generation leads to lowering of the water level, and to greater melting in the core region, there could be a release of metallic constituents from the core bottom followed by the release of the remaining oxidic material from the melt pool established within the original core boundary. Now, this is not an unlikely scenario, which could*

result in an intermediate state of a three-layered pool (less than 1.4 m depth) with a metallic layer sandwiched between two oxidic layers. This may lead to the condition, investigated by the authors, in which the thin metallic layer has an adiabatic boundary condition at the top surface; which was found to result in much greater thermal loading.

As explained in Appendix O, failure of the lower blockage within the time frame of the core relocation scenario is not possible.

Seh7. *The role of water in the lower plenum in quenching the melt discharges, the timing of its complete evaporation, and the subsequent remelting and layering of the pool, are all undefined. I would also prefer to leave them undefined, if I would be certain that the thermal loading during the intermediate states is always less than that in the end state. The authors have established thermal margins of approximately 100% for the most probable end state; perhaps, some scenario dependence could be considered and thermal margins investigated for some plausible intermediate states.*

See Appendix O.

Seh24. *The densities for the metallic mixtures are not too different from those for the corium. Once the natural circulation starts, it may be difficult to separate out the metallic components from the oxidic components in the corium and have them join up with the metallic layer on top.*

A naturally convecting pool is not the same thing as one participating in a corium concrete interaction. Also, steel cannot remain suspended in a superheated oxidic pool, as it will have to boil away, if not separated.

Sei2. I) Comments concerning Scenario examinations:

In the document, 2 scenarios are considered in the "thermal regime": the stratified pool and the melt jet impingement.

It is considered that the stratified pool is the worst (i.e.: the most conservative) situation, without any discussion In fact other situations may be emphasised and should be, at least, discussed to rule them out.

I-1) A first kind of (different) situation may be linked to the existence of a debris bed with molten metals within it. The assumed scenario is the following:

a) The melt (oxydes+metals) flowing from the core is quenched in the water present in the lower head. The quenched melt forms a debris bed with rather large particles in it (say between a few millimetres to a few centimetres).

b) The residual water is evaporated and debris begin to remelt,

c) The materials which remelt first are metals (mainly Stainless Steel & Zr).

d) These molten metals may migrate within the debris bed and accumulate in the lower part of the debris. Only the porosities are filled with the liquid metal, the oxydic debris staying as solid debris within the molten metal (higher density). Thus rather large heights of such "porous pools" may be emphasised with a rather low metal inventory.

e) The decay heat produced by the metals and the oxydics debris is transported to the boundaries by natural convection of the metal throughout the porous medium. The temperature of the molten metal is expected to stay rather low and depends on the composition of the metal (say less than 1800° C). At such low temperature it may be expected that the dissolution of the oxides by the metals is low (Stainless Steels does, indeed, not dissolve ZrO2 or U02 at these temperature levels).

f) The oxides situated above (but out of the molten metal-solid oxides pool) remelts later (due to their higher melting temperature). The molten oxides will enter into contact with the low temperature "porous metal-oxides pool" and form a crust at the interface which relies on the solid oxydic debris situated below.

g) Under these (inversed) conditions part of the power dissipated in the overlying high temperature oxydic pool may deverse downwards into the low temperature "porous pool". Thus this lower pool will have to evacuate not only the decay heat dissipated within it but also part of the power dissipated in the overlying oxydic pool.

h) Under these conditions, two flux peaks may appear; the first near to the upper surface of the "porous pool", the second later (in time) and above (in height) due to the oxydic pool.

Such a situation has been observed in the in pile SCARABEE experiments. It does not seem to me to be unrealistic for LWR accident situations.

It is not clear to me whether this situation is enveloped by the situation considered in the report.

The metals circulating through a porous oxidic matrix is possible in the presence of a large quantity of metals in the lower plenum, as in BWRs, and, in fact, it has been considered previously in such an application (Theofanous et al., 1991, NUREG/CR- 5423). This is not the case for the AP600, and as explained in Appendix O, the relocation will be basically oxidic. Interestingly enough, we also predict a "layered" situation during the transient evolution, however, under conditions that would prohibit the formation of highly loaded (thermally) thin metal layers. As always, the thermal loads from the oxidic pool are modest.

Also, we should draw attention to the aspect of the reviewer's scenario involving downwards loading from the upper oxidic pool onto the lower "porous pool." Such downwards heat transfer has to be basically conduction-controlled (through the lower crust of the upper pool). As such, while it may be significant at small scales (such as the SCARABEE tests), it is totally unimportant at reactor scales.

Sei3. *I-2) One may also emphasise scenarios leading to debris beds (with water present in the lower head) and with local remelting producing localised hot spots onto the lower head. The heat fluxes to the vessel will be of course much lower than the heat fluxes related to a molten pool situation. It seems from the TMI 2 VIP investigations that the mechanical loads induced by the hot spots on the lower vessel head do not endanger the vessel integrity. This may also be true for AP 600 but should at least be mentioned.*

The TMI reference is especially to be emphasized, because it occurred in the *absence* of external cooling. The consideration offered by the reviewer is welcomed for completeness.

She3. *I feel it is important to emphasize one other thing. It is a given in this problem that there will always be water surrounding the exterior of the pressure vessel. However, the cavity is not normally flooded in an operating plant and someone will have to make the decision to flood the reactor cavity, and do it in a timely manner. It is important to emphasize that this should not be put off until 'the last minute'. If the molten core redistributes before the cavity is flooded, and with minimal water going into the vessel, the vessel will fail long before one gets to the steady-state whose analysis the report dwells on. I realize that assuring timely flooding is more a regulatory matter than a technical one. But, I wish to* *stress that timely flooding is essential if the plant is ever to reach the situation of retention analyzed herein.*

The human factors aspects of cavity flooding reliability are now discussed in an addendum to Appendix M.

Spe3. *A key basis of the second regime, the pool natural convection regime, is that limiting loads are scenario-independent and are bounded by the thermal loads to the wall in the final steady state.*

[2. This fundamental basis of the report should be strengthened via a few selected examples involving particle bed heatup and remelting. It is recommended to investigate the downward heat flux i) during the pool formation process when the convecting pool may be contained by thick crust and upward heat transfer may be small, and ii) for the case that steel melts into a fuel particle bed and permeates to the bottom, facilitating heat transport to the vessel bottom.]

(i) As long as crusts exist (isothermal boundary) the energy flow split depends only on pool geometry, and it favors upwards and sideways heat transfer as compared to downwards. In a transient heatup situation (a thick crust next to the lower head), some of the decay power goes into heating up the crust, and only a portion is conducted away to thermally load the lower head. Maximum thermal load is obtained under steady state conditions. The maximum crust thickness possible is when the crust receives no heat from the melt. This thickness is $\sim$10 cm, and the heat flux delivered to the lower head by it (for the power density of 1.4 MW/m^3), is 120 kW/m^2. If the melt delivers some heat flux to the crust, its thickness has to decrease, so it can accommodate the heat flow with the available (fixed) temperature difference. If there is a gap conductance, the thickness has to be decreased still further. It is very clear from the perspective of the various cases examined in the report that the lower head cannot be endangered by solid crusts.

(ii) The metal relocation process, postulated by the reviewer, is self-limiting as a heat transport mechanism. That is, even if the oxidic debris porosity were largely penetrable by the high surface tension metallic melt (which is, in fact, highly unlikely), it would quickly fill up and, being stably stratified, as it should be, reduce the process back to conduction (for the lowermost nearly flat part of the lower head). More details on the whole issue of the transient meltdown aspects of the lower head thermal-load process can be found in a new Appendix O.

Spe9. *The report should clarify the scenario for the 3BE sequence considered to be of main interest to IVR. This sequence involves a large or medium size pipe break. Figure M1 seems to indicate that if cavity flooding is achieved, much of the RCS piping will be covered with water. Is water reflood of the vessel via the break a part of the 3BE sequence? What effect would water reflood have on the accident scenario?*

Since the issuance of the report, Westinghouse has changed the design of the cavity flooding lines from the 4- and 10-inch lines to two 6-inch lines. The new cavity flooding behavior is discussed in an addendum to Appendix M. The relative timing of cavity flooding to the melt progression is discussed in Appendix O, and shown in Figure O.10.

Tuo5. *In the Grenoble Workshop on "Large Molten Pool Heat Transfer" in March 1994 the question of steel boiling was brought up and was also mentioned in the Workshop Summary. The authors' response on the possibility of this phenomenon to increase to the metallic layer heat transfer would be desirable.*

This is addressed in the new Appendix O.

Tuo6. *Low pressure sequence*

The report assumes that high pressure core melt sequences can be practically excluded. Since the high pressure sequence Case IA turned out to have rather high contribution in the AP600 PRA, some additional aspects are needed.

To show that the contribution of high pressure sequences is negligible, very high reliability requirements are provided for the system, particularly to show that negligible contribution to the in-vessel retention can be excluded. Naturally, this should be done in context with the available time for required operator actions. In case that depressurization by pressurizer surge line failure is argued, it would need quantification.

The data in Table 7.3 have been revised on the basis of additional work on the interplay between PRA and design. It is not planned to argue the case on the basis of pressurizer line failure. A new table, and the implications on the IVR scenarios, including cavity flooding and related human factor aspects, are presented in addenda to Chapter 7 and Appendix M.

Tuo7. *Blocking of the flow paths*

In addition to ensuring availability of the flow paths by proper insulation and cavity exit design, the flow paths must be protected against all debris possibly flowing with water. The sources of such debris are the piping and vessel insulation (mineral wool, glass wool, thin metal sheets), rust paints, concrete dust etc. Particularly, the narrow flow paths out from the cavity might be subject to clogging. The current research for the containment sump clogging can be utilized for the final design.

Because the natural convection flows are rather strong, and the characteristic dimensions macroscopic, we have no mechanism for clogging similar to containment sump grids. This is further addressed in an addendum to Appendix K discussing the insulation design and related flow paths.

Tuo8. *Fouling of the vessel wall*

Another potential problem related to the water chemistry, impurities and all the small size debris flowing with water may be the fouling of the pressure vessel external surface during boiling heat transfer. At the beginning the fouling could have some advantage in increasing the surface wetting properties, but in the long term it might create an insulating layer. The possibility to study the fouling effect e.g. in the next phase ULPU-2000 experiments could be considered.

Because of the very strong convection we do not believe fouling to be a serious problem. However, the suggestion is well taken in the confirmatory sense offered, and it has been added in Chapter 9 of the report (under future work).

Tur6. *3. Chapter 5: NATURAL CONVECTION*

The authors assume (page 5-2) that, in the presence of unoxidised Zr, all the uranium remains as UO_2. *Powers [Chemical Phenomena and Fission Product Behaviour during Core Debris/Concrete Interactions in 'Proc. of the CSNI Specialists' Meeting on Core Debris-Concrete Interactions, EPRI NP-5054-SR; February 1987] has queried this assumption, and notes that only about 5 atom percent uranium in the metal phase is sufficient to make the metal phase of core debris more dense than the oxide phase. As far as I know this question is unresolved. Given the large steel inventory assumed, it may not be possible to achieve inversion of the densities of the phases, and other possible configurations of debris (including* *partitioning of the decay heat source) may prove less of a challenge to the vessel, but I would like to see these points addressed for completeness.*

The points raised here are appropriate to be discussed, for completeness. We can approach such a discussion from a couple of standpoints.

(a) We agree with the reviewer that this question is unresolved, while the author (Powers) writes that "These analyses must be considered speculative until a definitive analysis of the thermochemistry of oxidic core debris can be obtained".

(b) Powers was concerned with corium-concrete interactions, where due to bubbling the metallic and oxidic components of the melt found themselves in good contact (promoting chemical reactions and mass transfer between them). In the present situation most of the zirconium is tied up in the lower blockage, near the core support plate; it would be the last to remelt, together with a large quantity of steel (the plate itself), and thus would be added on top of the oxidic pool (see Appendix O). Such a stratified geometry separated with oxidic crusts is not favorable for extensive chemical reactions between the metallic and oxidic components. So, most of the uranium present in the metallic layer will be the quantity dissolved during the first clad melting and relocation period, and this was estimated at no more than ~5% of the zirconium involved (D. Olander, Nuclear Engineering and Design, 1995 (to appear)). Since the zirconium itself is only a small fraction of the iron present in the metallic layer, we can see that we are far from the 5% uranium composition needed, according to Powers, to obtain inversion.

Tur16. 4. OTHER CHAPTERS: TECHNICAL COMMENTS

Chapter 2

It is right to emphasise that full primary system depressurisation is being assumed (the validity of this can presumably be obtained from the Level 2 PSA referred to in Appendix M) and that pre-flooding of the lower head has taken place prior to debris relocation. Appendix M, while indicating that there is sufficient inventory of water and sufficient flow paths, does suggest that more needs to be done to guarantee that water will be delivered in a timely manner, assuming that cavity flooding is adopted as a primary severe accident management operation. Likewise, my reading of Appendix K suggested that there is no difficulty in principle with designing insulation to allow effective flooding around the vessel, but this also needed further consideration by the plant designers in conjunction with the information obtained from the ULPU tests. As the information is presented here, it still looks that cavity flooding is an 'add-on', as

> it has to be for existing designs, rather than something built into the plant design. I assume there is regular testing of the valves in the drain lines from the IRWST. Overall I agree that the AP600 is an attractive design for the implementation of cavity flooding.

Please see updates to Appendices M and K, with more detailed information on cavity flooding and insulation design. For the AP600 we wish that they be considered as part of this package rather than as "add-ons".

> **Tur17.** *Without a full appreciation of the geometry and the assumptions that have been made on crust behaviour (eg are lower ceramic blockages, formed initially on a metallic blockage sufficient to retain the debris pool) it is not possible to underwrite the statements made on melt relocation; that discharge will occur at the core side and will involve a substantial fraction of the core. With the highest heat fluxes near the top of a molten pool, it seems to me more likely that the initial discharge by this route might be limited to, say, 30% of the core.*

Yes, by substantial we mean something like 30% as an upper limit, as considered in Chapter 8. See also Appendix O.

Che4. *3. Simulation of the Divergent Effect and the Three-Dimensional Aspects of the Two-Phase Boundary Layer*

The local flow structure on the external surface of the pie-segment geometry described in Appendix E.1 can not be fully simulated by using the constant-width test Section of the ULPU facility. Although the local heat flux may be matched by using the power-shaping approach, the detailed hydrodynamic behavior of the two phase boundary layer flow can not be fully simulated. For the Pie-segment geometry, the cross-sectional flow area is not constant but increases downstream in the flow direction. The local power levels in the lower Part (i.e., upstream portion) of the pie-segment geometry are considerably higher than the corresponding values for the constant-width test section. Thus, the bubble activities in the upstream locations are more intensive for the pie-segment geometry than for the constant-width test section. As a result, more vapor per unit surface area will be produced upstream in the pie segment. The population of the vapor phase, however, tends to diverse downstream as they flow upward along the pie segment owing to the increase in the cross-sectional area. This divergent effect, which may strongly influence the boiling process and thus the critical heat flux, is absent altogether in the constant-width test section.

Not true. The whole purpose of the power shaping principle in ULPU is to represent what the reviewer calls here the "divergent [sic] effect." This paragraph, and in particular the second sentence ("Although the local heat flux may be matched by using the power-shaping approach, the detailed hydrodynamic behavior of the two-phase boundary layer can not be fully simulated") lead us to believe that the reviewer did not fully understand the power-shaping principle and its implications. As shown in Figure E.3 of Appendix E.1, there is a sufficiently close approach of the upstream boundary layer for matching angles (θ_m) as small as 10°, and the simulation keeps getting better for larger angles. Only the region $\theta_m < 10°$ is, strictly speaking, deficiently (but conservatively) simulated in this respect, by using a uniform heat flux, but extensive sensitivity-type experiments indicate that the effect of this deficiency is negligible.

Che5. *Besides the divergent effect, the constant-width test section of the ULPU facility can not simulate the three dimensional aspects of the boundary layer boiling process that takes place on the external bottom surface of a AP600-like reactor. The superficial vapor velocity represents only one of the several requirements that need to be satisfied in simulating the boundary layer boiling process.*

Other flow parameters including the local void fraction, characteristic bubble size, bubble growth-and-departure frequency, and the divergence of the vapor bubble population in the flow direction need to be matched in the simulation. These flow parameters may have important effects on the boundary layer boiling process and the local critical heat flux distribution. Note that as a result of the boundary layer flow effects, the dynamics of the two-phase flow may vary significantly along the curved and diverging heating surface. Conceivably, matching the superficial vapor velocity alone is not enough in simulating the actual 3-D process, as the superficial velocity represents only a necessary condition but not a sufficient condition for the simulation.

Our idea is that the independent variable here is local superficial velocity, and that as long as there is a reasonable upstream development length (within which the superficial velocity in ULPU is close to that in the reactor—again, please refer to Figure E.3 of Appendix E.1) all other multiphase aspects are automatically simulated. Again, the statement "Note that as a result of the boundary layer flow effects, the dynamics of the two-phase flow may vary significantly along the curved and diverging heating surface," lead us to believe that the reviewer hasn't fully understood this idea on which ULPU is based. The validity of this idea is further buttressed by the insensitivity of the results to both upstream and downstream power shapes, and in fact even to the natural circulation flow rate (see the new Appendix E.3).

Che6. *With the divergent and the three-dimensional effects, higher vapor velocities can be accommodated without exceeding the CHF limit. Thus more heat can be removed from the heating surface by nucleate boiling. This means that the local CHF values measured in the ULPU tests represent a conservative estimate (rather than the best estimate) of the actual situation. In the actual 3-D case, higher local critical heat fluxes can be anticipated.*

Based on our responses to the above two points we will have to disagree with the reviewer's conclusion that the ULPU results are conservative, except perhaps in a small region around $\theta_m \sim 0°$. But even for this region non-sensitivity to power shapes and mechanistic consideration (see new Appendix E.4) indicate that the effect is small, if not negligible for our purposes.

Che7. *4. Simulalion of the Subcooling Effect due to the Gravity Head*

In a fully flooded cavity, the water in the vicinity of the lower head would have ~14 °C subcooling as a result of the gravity head. Thus it is necessary to properly simulate the phenomenon of subcooled boiling on the external bottom surface of the reactor vessel. However, exactly how this was done using the power-shaping method in the ULPU facility is not immediately clear.

For saturated boiling, the superficial velocity at a given downstream location can be uniquely related to the accumulated power generated in the upstream portion of the test section. Thus matching of the local superficial vapor velocity can be conveniently accomplished by using the power-shaping approach. For subcooled boiling, however, the superficial power vapor velocity at a given downstream location can not be uniquely related to the accumulated power generated upstream. This is due to the fact that condensation of the vapor phase would take place within the boundary layer in the presence of subcooling. The accumulated amount of vapor that is condensed before reaching a given downstream location depends on the size of the vapor bubbles, the local vapor velocity, the vapor population density, the cross-sectional flow area, and the degree of subcooling. None of these parameters except the degree of subcooling can be simulated in the constant-width test section. It does not appear to be feasible to match the superficial vapor velocity in the ULPU test using the power-shaping approach for the case with subcooling.

As noted already in Appendix E.1, "the same results can be obtained ... if one considers the total energy (sensible plus latent) flow per unit width ... reflecting the fact that the convected sensible heat is also important in the local behavior of the two-phase boundary layer." In the lower portion (say $\theta_m < 30°$) stratification is strong enough to make the approach immediately tangible to the power-shaping principle. In the upper portion (say $\theta_m > 45°$) sensible energy diffusion away from the two-phase boundary layer may be substantial, but still relatively small compared to the total energy flow within the boundary layer, and the degree of divergence (due to geometry) is small, thus allowing the power shaping principle to be applicable in this case as well. Further perspectives can be obtained from the new data (see new Appendix E.3) on the effect of flux shapes and throttling of the natural circulation flow at the inlet to the test section.

Che8. *A more detailed description of the power shape used in the experiments for Configuration II should be given in the report.*

All the power shapes used in Configuration II have been supplied in Appendix E.2, so there is no more detail to provide.

Chu1. *I. Comments on Critical Heat Flux*

The review covers the material in Chapter 3 and Appendix E entitled The ULPU Experiments. The experiment appears to be well designed and executed within *the constraints of the assumptions made.*

The review will be presented from two points of view:

A. *Does the ULPU experiment simulate the three-dimensional boiling process on the exterior of the reactor vessel?*

B. *The application of ULPU data to in-vessel core retention.*

* *

Chu2. *A. The two criteria: (1) matching superficial velocity at and beyond the point of interest, and (2) a gradual build-up of superficial velocity up to the point of interest, are reasonable; however, by no means guarantee that the flow fully simulates the actual 3-D flow outside of a reactor vessel. For example, there is no flow divergent effect in the strip and the velocity development is certainly different in the ULPU case due to the difference in the superficial velocity upstream of the point of interest. Furthermore, as pointed out in the report, the dynamic aspect of the flow and condensation effects are not properly taken care of by the criteria. Physically, the shape of a wedge cut from a hemisphere takes a* $\sin\theta$ *profile, since* $\sin\theta$ *varies rather slowly near 90°, the CHF data is likely to be accurate near the equator. However, in the bottom center region, the strip geometry does not adequately simulate the 3-D axisymmetric two-phase boundary layer flow. A comparison of the data in Figure E.12 (Appendix E.2) and the recent data of Cheung and Haddad (Proceedings WRSM 22 October 1994), Figure 1, shows that the CHF values obtained in ULPU might be too low near* $\theta = 0°$. *It is interesting to note that away from the bottom center area, the two sets of data have similar trends.*

We do not agree that the Cheung and Haddad data are, at this time, qualified to be compared with ULPU, or applied to reactors. But since a comparison has been attempted, we must be very clear about it. So, let us look at Table E.2, that contains all the data points. First, runs UF-5-0 and UF-6-0 show a critical heat flux just around the 400 kW/m^2 data point of Cheung and Haddad shown in reviewer's Figure 1. Note, however, that as demonstrated by the other data points, values as low as ~300 kW/m^2 (25% lower) were obtained by various heat flux shapes and by allowing long sampling intervals at a given power level. None of these effects were investigated in the quenching experiment cited by Chu, which, in fact, involves a very rapid "traverse" past the peak heat flux, once surface wetting occurs! We believe that taking the conservative envelope of our data is appropriate, and that nothing much should be made from the *apparent* agreement of the *peak heat flux measured in the quenching experiment* and *some of our critical heat flux values.*

The flow divergence inherent in the geometry can be seen in Figure E.2, which is now supplied also with a scale. We can see that divergence is strong only near $\theta \sim 0°$, and certainly negligible for $\theta > 45°$. As explained in the report, the region $0 < \theta < 22.5°$is rather uninteresting from a failure point of view. This is to put the reviewer's criticism into perspective, independent of whether the ULPU data are realistic or conservative in this small region.

Furthermore, the statement: "...as pointed out in the report, the dynamic aspects of the flow and condensation effects are not properly taken care of by the criteria" has been incorrectly attributed. On condensation (subcooling) effects on p.E.1–6 we noted that "the same results can be obtained under moderate subcoolings, as is the case of interest here, if one considers the total energy (sensible plus latent) flow per unit width, which in turn provides a more generalized similarity criterion, reflecting the fact that the convected sensible heat is also important in the local behavior of the two-phase boundary layer." On the other hand, on p. 3–4 we indicate that "loop flow and dynamic effects" are to be addressed when the thermal insulation design becomes available. This is now the case (see the addendum to Appendix K) and the results can be found in the new Apendices E.3 and E.4.

> **Chu3.** *What criterion is used to determine "For θ_m as small as 10°, the simulation is* <u>*deemed*</u> *to be acceptable, (p. E.1–6)?" The use of passive voice without giving a justification is not informative.*

Statement is based on qualitative judgment comparing the "upstream length" for which $\Delta J/J <$ 20% to the flow regime structures (see also new Appendix E.4).

> **Chu4.** *Unless there are good reasons to discard the UF-6-0 and UF-5-0 data, they should be included in Figure E.18. These values are not far from the Cheung and Haddad data.*

The data were *not* disregarded. They were not shown in Figure E.18 because they are not relevant to the lower envelope correlation presented there. The "similarity" to the quench peak fluxes is irrelevant, as explained above.

> **Chu5.** *The data presented in Figure E.16 suggest that there is considerable lateral gradient in the heating block. If this is not the case, a new plot should be used.*

The few degrees difference is indicative of thermocouple error, without calibration adjustment. See also Appendix E.4.

Chu6. *The large axial conduction correction for Configuration 1 is disconcerting. What would happen, if the experiment is run with the heating zone around the point of interest twice as wide? Or more generally, does the width of the heating zone influence the measured CHF values?*

In p. E.1-16, we show that the conduction correlation factors are 85%, 89%, and 95% at the three different locations respectively. It is not understood why the reviewer finds such a 5 to 15% correction "disconcerting." Moreover, these are *not* errors, but accounting for a well-understood and quantifiable phenomenon.

Chu7. *CYBL can be operated to 400 kW/m^2 as currently designed.*

The statement in the report referred to "demonstrated" capability, as the follow-up sentence indicates.

Chu8. *B. Because the margin to failure is fairly significant (as shown in Chapter 7), the reviewer feels that despite the inaccuracies involved, the critical heat flux data is sufficient for the present purposes, provided the following clarifications are made:*

As discussed under highlights, the meaning of this sentence is not clear, in light of what is brought up immediately below it.

Chu9. *1. There is a substantial increase of CHF in Configuration II, due to the natural circulation loop. The data from Configuration II is used to demonstrate the large thermal margin. Therefore, the authors must provide a more detailed justification that the natural circulation observed is prototypic, in terms of flooding level, dimension of rise and downcomer, and the correspondence between the strip geometry and the axi-symmetric geometry in the* integral *sense. The arguments made in the power shaping principle are largely based on reproducing the* local *condition at the measurement location of interest.*

On the "integral sense" we already explained that we observe the key aspects, including height, vapor flow, and riser dimensions. They provide the correct void fraction, including flashing effects, and hence the correct driving head for natural circulation. In addition, we now have data on the effect of inlet throttling and heat flux shapes (Appendix E.3) that show the robustness of our thermal failure criterion. Further systems effects have been examined in Configuration III that includes the proper exit restriction, as described in Appendix E.4.

Chu10. *2. The experimental methodology stresses "the determination of the critical heat flux ... under the constraint of a specific power shape. (p.E.1-5)" Under this methodology and specifically the power shaping principle, the results presented in Figure E.12 (section E.2) are only valid for the power shape in Figure E.11. (section E.1). Therefore, there is a contradiction in principle, to apply the CHF curve to the assessment of different power shapes in Chapter 7, Figures 7.13 to 7.16. To borrow an expression from thermodynamics, one needs to answer the question of whether CHF is a point function or a path function. It is entirely likely that CHF is only a weak path function. But justifications (which may require sensitivity experiments) must be made to smooth out this apparent contradiction.*

We now have tested all relevant flux shapes (see new Appendix E.3), and have shown that CHF is a very weak function of flux shape. However, for highly peaked situations, as the arbitrary parametric in Chapter 7, with the 20 cm-thick metal layer, the CHF is found to be higher. As expected, our "reference" CHF results are conservative.

Chu11. *3. The authors repeatedly stress the importance of aging the surface; however, there apparently is no attempt to characterize the surface. At lease a simple sessile drop observation or a SEM should be provided. This is especially important in the upcoming tests with the painted steel test section. How does the paint age under the test conditions? Should only data with new paint (never boiled) be used for in-vessel core retention assessment? How does the paint age in service? How can the test data be applied to the "real" accident conditions?*

See Appendix E.4.

Chu12. *4. It is interesting that the Vishnev correlation (Vishnev et al., "Study of heat Transfer in Boiling helium on Surfaces with Various orientations," Heat Transfer-Soviet Research, vol. 8, no. 4, p. 104–108) derived from laboratory scale experiments and using helium as a working fluid, actually predicts the ULPU data trend to within 10% (Figure 1). The Vishnev correlation specialized to the nomenclature of the present report is:*

$$\frac{CHF_\theta}{CHF_{180}} = \left(\frac{10+\theta}{190}\right)^{0.5}$$

Where $\theta = 0°$ corresponds to horizontal downward-facing, and $\theta = 180°$ corresponds to horizontal upward-facing.

Interesting curiosity, but nothing more! See response under General Comment and highlights.

Chu13. *CHF phenomenology is still a mystery.*

See Appendix E.4.

Dhi1. *In Chapter 3, the authors discuss the coolability of the reactor vessel with emphasis on the heat fluxes that can be accommodated under nucleate boiling conditions on the outer surface of the vessel. Local and global aspects of boiling on the vessel outer surface are discussed. Two sets of critical heat flux data have been obtained (Appendices E.1 and E.2) on a one dimensional full length representation of the reactor vessel. In the first set, the data are obtained under pool boiling conditions with heat supplied to only the lower portion; covering angular position from* $+30$ *to* $-30°$. *In the experiments liquid was saturated with angular position of the lower stagnation point being 0°and that of the equator being 90°. A correlation for the critical heat flux obtained from these data is reported. In the second set of experiments, a natural circulation loop was established. Heat flux distribution on the test surface was established to simulate a reference heat flux. The reference heat flux was obtained from an earlier study of Theofanous et al. The heated region spanned from 0 to 90°. Because of the hydrostatic head difference in the natural circulation experiments, a liquid subcooling of about 10 °C existed near the lower edge. The critical heat fluxes obtained in natural circulation experiments are found to be higher then those obtained under pool boiling conditions. Again, the data have been correlated with angular position. The authors have done careful experiments and have obtained nearly full scale simulation of this prototype. They should be complimented for it.*

* *

Dhi2. *1. The authors claim that their full length representation affords an essentially perfect full scale simulation. I cannot agree with this statement. At the stagnation region of a sphere, the behavior of the vapor bubbles at departure will be different than that for a plane surface.*

This point is true, strictly speaking, at the neighborhood of the stagnation point ($\theta \sim 0°$); but in a practical sense, it can be said that the ULPU representation in this area is conservative. Moreover, from the data trends found in angles away from the stagnation point (say $\theta \geq 15°$), for which the ULPU simulation by power shaping is quite adequate, we can say that, in fact, this conservatism is not quantitatively significant. Finally, it should be kept in mind that, as discussed in the report, the stagnation region is the least interesting from the point of view of lower head failure.

Dhi3. *2. In the reactor cavity, counter current type of flow simulation will occur rather than that of a natural circulation loop (co-current). Hence, I believe that the configuration shown in Figure E.4 is more appropriate. Data for this configuration have been obtained when the heated region spanned* $-30° \leq \theta \leq 30°$. *It is important that data be obtained for this configuration when the heated region spans* $0° \leq \theta \leq 90°$. *The critical heat flux in this configuration will be lower than that for the natural circulation case.*

This is not correct, as discussed under highlights. The caption of Figure K.2 was supplemented to prevent such misunderstanding in the future.

Dhi4. *3. Some flashing of the superheated liquid is expected to occur in the upper region* ($\theta \sim 90°$). *The authors do not report any such observation. A discussion of the effect of flashing in the local critical heat flux in the upper region is also needed.*

The important effect of flashing is in flow oscillations, and we hinted on that around the middle of page 9–1, by reference to the "dynamic behavior of the two-phase natural circulation flow." The subject is now explicitly discussed in Appendix E.4.

Dhi5. *4. The heat flux imposed on the inner wall is obtained from the earlier work of Theofanous et al. I do not know if the imposed heat flux distribution represents an upper limit for all types of molten pool scenarios that can be envisioned. This includes partially filled lower vessel heads as well.*

This point is well taken. We now have data for a much wider range of flux shapes, including those found in the parametric and sensitivity studies of Chapter 7 (see also Appendix P). The results demonstrate the cumulative effect of upstream power, especially for the important higher elevations, such that the more peaked the profile is the higher the critical heat flux.

Dhi6. *5. It would have been interesting and informative if the authors had compared their steady state critical heat flux data under pool boiling conditions with the data reported in the literature from small scale (a few centimeters in length) test sections. It should also be noted that most of the data reported in the literature on small scale test sections were obtained under transient conditions.*

We are strongly against such data comparisons. As discussed in the report already, the small scale experiments, besides having been obtained under transient conditions, physically have nothing to do with the problem at hand, and any agreement, or disagreement, with these data is bound to cause confusion. This can only change if a mechanistic connection between ULPU and these small scale experiments is found.

> **Dhi7.** *6. To isolate the effect of global versus local conditions, it would have been valuable if the authors had reported the critical heat flux obtained at a given location when all of the regions upstream of the given location are heated and when heating is provided only locally.*

We now have such data (see Appendix E.3).

> **Dhi8.** *7. The actual heat flux profiles on the heated block surface were obtained by numerically solving the two dimensional conduction equation with appropriate boundary conditions. No information is given as to what those boundary conditions were. Also, we are given little information on the progression of the dryout front from zone to zone after occurrence of critical heat flux conditions at a given location.*

The block surface in contact with the water was assigned the measured temperature (~130°C), while all other surfaces were kept at a zero heat flux. The calculated temperatures on the back face (opposite to the wetted one) was in very good agreement with the measured values. This point is now made in Appendix E.4. This appendix also contains data on the spreading of the dryout region.

> **Dhi9.** *8. It is stated that the annular gap in the prototype is 20 cm. From the information given in the report, I cannot ascertain if the hydraulic diameter in configuration 1 of ULPU is scaled properly with respect to the prototype.*

The inlets of the U-tube in Configuration I have a diameter of 15 cm. This has been added in the description (p. E.1–12). The annular gap in the reactor geometry is also 15 cm.

> **Dhi10.** *Finally, I believe that the authors have obtained very valuable data. However, at this point, the information is incomplete and it is not possible to conclude that boiling heat flux on the outer surface of the vessel will be below the local critical heat flux under all types of heat fluxes imposed on the inner wall of the vessel.*

We are confident that the greatly enlarged data base in Appendix E.3 and interpretations of it will satisfy the reviewer's concerns.

> **Hen4.** *3. There is discussion with respect to the influence of a boil-up level in the gap between the insulation and the reactor vessel cylinder. The inleakage of*

water through the gaps in the insulation must be considered as a two-way street. Water certainly can readily ingress into the insulation, but the boil-up level can also tend to leak out through the gaps in the insulation thereby decreasing the influence of such a boiled-up situation. This should be discussed in terms of both behaviors.

The flow rates are quite high for any such out leakage to be significant.

Kre17. *6. Boiling Heat Transfer on Outside of Vessel:*

The objective here was to determine the distribution of critical heat flux on the bottom head submersed in a water bath. This was accomplished experimentally by the use of the innovative 1-D ULPU test facility that had the following characteristics:

- *full length/correct curvature*
- *a "slice" geometry*
- *power input varied with position to match the distribution of heat transfer from the pool side as measured in Mini-ACOPO*
- *an "aged" copper surface.*

I found the description of the experiment procedure in Appendix E to be somewhat obtuse. With persistence, however, you can figure out what was done.

I believe the experiment procedure to be valid (i.e. determining the local CHF as a function of angular position by matching the steam flow into the local region that would be obtained as produced in upstream areas for the total heat required to produce the local CHF. It is recognized that a 2-D prototype is modelled by 1-D tests. I believe this is conservative because the 2-D streamlines are divergent whereas the 1-D streamlines in the test are parallel. This should result in a slightly lower measured CHF than one would expect in the real case.

I believe when these tests are validated for the surface material, this will be sufficient to determine the distribution of CHF on the external surface of the bottom head.

* *

Lev2. *A. Boiling Crisis or Critical Heat Flux (CHF)*

DOE/ID-1046 relies upon data from Figure 3.3 for CHF as a function of position on the lower head for quantifying the thermal failure criteria. These data were taken under full submergence and natural convection in the ULPU facility. My concerns are as follows:

1. Natural convection enhances the CHF condition. This is clearly visible by comparing the results of Figure 3.3 with those of Figure 3.2 obtained for pool boiling. The increase in CHF is 67% at the zero degree angle position and 36% at the 90 degree angle. This means that the natural circulation in the tests must simulate accurately the flow behavior in the AP600. It should be noted first that in Figure E.1 the cold water is returned at the bottom of the cavity rather than "draining into the reactor cavity through a tunnel at the compartment floor elevation which spills into the cavity at the elevation of the top of the lower head" when the IRWST drain valves are actuated (see page M-4 and Figure M-2). Subsequently during "passive reflux to the cavity" (which is being simulated by the ULPU tests), water "would enter in the outlet nozzle region and drain down through the octagonal portion of the cavity" (see page K-4). During this mode of operation steam water flow will rise in a counter flow mode to the returning water in the cavity annulus. This countercurrent flow will produce less natural circulation flow than in the ULPU tests and also it most likely will impact the subcooling of the water reaching the bottom head.

The counter flow mode described is incorrect. The actual flow path was shown clearly in Figure 3.1, and ULPU simulates it faithfully.

Lev3. *2. The authors have recognized that their tests do not include reactor pressure vessel insulation. The insulation is bound to interfere with the natural circulation flow not only by reducing the size of the annular gap but also by providing increased resistance for the water to reach the vessel outer surface. An allowance should be provided for this reduction until tests with prototypic insulation can be carried out.*

We disagree with this point too. It is highly inappropriate to "make an allowance" for something you know little about. We took the position that to rely on "leaky" insulation is inappropriate. We wish to have "free" access of water towards the pole of the lower head; we explained that clearly, and in Appendix K we even indicated one possible approach to accomplish that.

Meanwhile, Westinghouse has developed an insulation concept along these lines (see addendum to Appendix K), so the point is mute now.

> **Lev4.** *3. The tests were performed with thick highly conducting walls. Past CHF tests have shown that such circumstances will increase the local critical heat flux. While the reactor pressure wall thickness is large to start with, it could thin down significantly during the course of the severe accident and tests with less conduction might be appropriate.*

The test section thickness is 5 cm, and only in the most extreme parametric scenarios considered do we not find sufficient melting to reduce the wall to such an extent. Not only is 5 cm conservative, once you go beyond a few centimeters there is not much effect anyway.

> **Lev5.** *4. The AP600 reactor vessel standoff insulation concept depicted in Figure K.1 shows narrow (about 2.5cm) flow passages between the vessel and the insulation panels. Even in the alternative insulation concept of Figure K.2, the flow passage is about 5 cm. (The concept in Figure K.2 will create strong cavity air recirculation along the reactor vessel wall, which will reduce the effectiveness of the insulation and increase the temperature of the reactor cavity concrete). Such insulation configurations will not only reduce the natural circulation flow rate but they would encourage the steam to flow along the narrow spacing between the reactor vessel and the insulation. Therefore, they would tend to approach conditions found in thin rectangular channels submerged in saturated liquid. A significant amount of CHF data has been obtained in thin vertical channels and they show a drop in pool boiling CHF as the ratio of length to width of the channel increases. At atmospheric pressure and a length to width ratio of about 30 the CHF drops to 32 percent of the accepted pool boiling value (see M. Mode et al, Critical Heat Flux During Natural Convective Boiling in Vertical Rectangular Channels Submerged in Saturated Liquid, ASME Transactions, Journal of Heat Transfer, Vol. 104, pp 300-303, May 1992). Some similar and strong negative impact due to the presence of insulation is expected in the AP600 configuration and its magnitude will depend upon the final design of the insulation. Still, an allowance needs to be provided at this time.*

Much of this is speculative, and again we disagree with an approach based on "allowances." An insulation concept along the lines indicated by the ULPU experiments, has been developed by Westinghouse designers (see addendum to Appendix K). There is no penalty for increased air natural convection during normal operation, there is no significant resistance to inflow, and the minimum clearance is 23 cm, which is larger than in the ULPU Configuration I test section. As promised in the report, confirmatory experiments will be run, in Configuration III ULPU, now that the geometry is known (see Appendix E.4). The reference cited is irrelevant to our situation.

Lev6. *5. The potential impact of the accident upon the insulation is noted in the report. However, if the large LOCA break takes place within the cavity, one can expect significant damage to the insulation and potential flow blockages in the cavity outlet nozzle region.*

The only LOCAs considered possible in the cavity are from failure of the direct vessel injection line. Any failure at this line is unlikely and only failures localized very near the vessel would impact the insulation. Such localized failures are highly unlikely if not below the screening frequency level. The insulation panels are well supported (so failure could not occur far from the break), and the material is reflective steel, to preclude the kinds of blockages that might otherwise occur with fibrous insulation. The only fibrous insulation is used above the steam vents which provide the exit flow path.

Lev7. *6. Because the water refilling the cavity is borated, its boiling will deposit boron on the reactor vessel surfaces and its impact upon CHF was not considered. Also, the water reaching the reactor cavity will contain dirt and dust and it will accumulate in a reactor cavity which cannot be expected to be clean and may contain paint flaking off the vessel. This lack of water purity conditions needs to be recognized.*

Yes, indeed. ULPU was run under highly pure as well as under highly contaminated conditions—rust, pieces of plastic, paint flakes, etc. No effect was found. Any deposits on the surface will increase the wetting property of it and hence the critical heat flux capability. Because of the strongly convective flows we cannot expect to have boron enrichment in the cavity, and as one of the final confirmatory tests we plan to run long term with the proper boron concentration in the water to examine the rate of buildup due to boiling precipitation.

Lev8. *7. In view of the preceding comments, significant degradation in the CHF values of Figure 3.3 are anticipated (possibly by as high a factor as 2 to 3). It is remarkable, therefore, that no sensitivity study of this important parameter was included in Section 7.3 and it is recommended that it be added.*

As described above, any reduction of CHF from those found in ULPU are unfounded, and no such sensitivity studies are warranted.

May4. <u>*Heat transfer from the wall to the flooding water*</u>

Very sophisticated and detailed experiments are reported in the DOE-report, dealing with the subject of critical heat flux at facing-down surfaces and at vertical walls with free convective bubbly flow. This experimental data, together with the nice experiments on free convection heat transfer at the inner side of the vessel wall, proof very reliably, that a safety margin with a factor of 2 exists against critical heat flux, even at positions with very high thermal loads. So one can be sure, that the heat transfer from the wall to the water is negotiated by nucleate boiling, which has very high heat transfer coefficients, as is well known. This means, that the temperature difference between the outer side of the wall and the bulk of the water is very small—in the order of a few Kelvin.

* *

Seh3. *The ULPU experiments conducted by Professor Theofanous and co-workers have provided definitive data on the CHF for the external surface of the vessel; and the CYBL experiments, conducted by Chu et.al., have provided the visual evidence for the substantial heat removal rate at the very bottom of the reactor vessel external surface. I believe, that the heat removal rate aspect of the IVR is quite well assured, and the uncertainties are low, except for the actual AP600 physical design details. In particular, as the authors state, the physical design has to allow sufficient area for the steam produced from the cavity to flow to the containment dome; and the insulation on the vessel has to allow a steady access of the water to the vessel external surface. The flow area and the water access have to be assured throughout the life of the plant and, thus, may be subject to the maintenance and in-service inspection regimens conducted on the plant.*

* *

Sei19. IV) Thermal failure and vessel bottom coolability:

The set of experiments presented (ULPU, CYBL) provides important results.

The most important experiments are the ULPU experiments. The approach which is used supposes that the CHF depends on the local heat flux, on the local two-phase flow conditions, on wall effects and on local pressure. Two-phase flow conditions depend on the overall recirculation path and on 2D local effects.

IV-1) Local Two-Phase flow conditions are expected to be represented if local superficial velocities are represented. This is one of the similarity criteria (the *other is the level of the local heat flux). The theory, valid for saturated conditions, includes also the implicit assumption that the local thickness of the Two-Phase Boundary Layer is identical in the experiment (constant width) and the reactor (pie segment). This assumption is not demonstrated but may perhaps be assumed as realistic since size and inclination effects are represented. This should be discussed.*

We think this was discussed already. First, we emphasized the importance of matching the two-phase boundary layer, upstream, at, and downstream from the point at which boiling crisis is being simulated That led us to the full-length test section as a basic requirement. Next, we discussed the upstream length within which the vapor flow is close (i.e., within some appropriate tolerance) to that in the reactor (pie) case. The point being that there is enough development length so the boundary layer has no memory that it was not generated "exactly" with the same history as in the reactor. Knowing the boundary layer behavior, from ULPU testing (this is now discussed in Appendix E.4), and having a large amount of sensitivity runs, check effects of flux shape and recirculation flow rate (see new Appendix E.3), help further to evaluate these similarity issues. Moreover, we believe that these similarity arguments are applicable in the presence of gravity-induced subcooling.

Sei20. *IV-2) The geometry effect is compensated by a heat flux profile defined on the basis of previous similarity arguments. The upstream (from the investigated location) compensation procedure is quite clear. The interest of the downstream compensation is not very clear to me.*

This is to preserve the overall gravity head due to voids and hence any internal recirculation flow patterns. See also above (item #19) and new Appendix E.4.

Sei21. *For the inner region (angle between 0° and 10°, the heat flux is constant. This should provide conservative CHF conditions in this region.*

Yes, but based on our observations and data, even at the pole we think the ULPU results are realistic (conservative but not significantly so).

Sei22. *IV-3) It is shown that an increase of the subcooling and of the recirculation mass flow rate has a great effect on the CHF (increase from 0,30 MW/m^2 to 0,50 MW/m^2 at the bottom, increase from 1 MW/m^2 to 1,6 MW/m^2 at the side top location). This is clearly very interesting. However the contribution of each effect (subcooling or mass flow rate) is not quantified and nothing is said about the representativity of the flow path in the ULPU experiments.*

In other words the CHF results depend not only on the angle (as suggested by figure E-12) but also on the subcooling and on the recirculation mass flow rate. There is no indication in the text concerning the evolution of the recirculation mass flow rate for the different CHF tests performed at different angles.

→ Thus one must be cautious when using the results presented on Figure E- 12 and correlations El and E2.

See new Appendix E.3.. The correlation in E2, as used in the report, is appropriate.

Sei23. *An optimisation of the flow path (as suggested in appendix K) may lead to an increase of the liquid flow at the bottom of the vessel. Thus, even higher CHF levels may be obtained, locally, than presented on figure E-12.*

On the contrary, bad recirculation conditions (flow restrictions, ...) may lead to lower CHF levels. However it seems that the results obtained for Configuration I hold as a lower bound for CHF (0,3 MW/m^2 at the bottom and 1 MW/m^2 at the side top).

These remarks are important in regard to the large heat fluxes which are computed in the metal layer under some assumptions (1 MW/m^2 in fig. 7.14 (page 7-15) and 1.4 MW/m^2 in fig. 7-16 (page 7-17)) or for applications to other reactors.

Future work may thus be oriented both on:

- *a better knowledge of the contribution of each effect on CHF (pressure, recirculating mass flow rate, subcooling, ...),*
- *an optimisation of the flow path (as proposed under appendix K) for a maximisation of the recirculating mass flow rate, since heat fluxes higher than 1 MW/m^2 cannot be excluded.*

Yes, on both items. Please see new Appendix E.3. Note that pressure in the containment will be low and cannot be conveniently increased. Recirculating mass flow rate is found not to be very important. Subcooling is, but, again, this cannot be conveniently increased beyond that due to gravity head. We think if higher critical heat fluxes are required (i.e., large reactors), optimization of flow paths and the possibility of fin structures could be examined.

Sei24. *IV-4) It is also mentioned that all results have been obtained with a copper wall and that experiments with steel will be performed. It seems essential* *to perform the tests with steel since the elevated thermal conductivity of copper may have an effect on CHF. It may be suspected that the oscillatory behavior of boiling at low inclinations induces periodic dry patches which act as initiators of dry-out. The rewetting of these dry patches may be related to the maximum temperature reached on these surfaces during the dry phase. The maximum temperature in these patches is reduced in the case of a copper wall (when compared to a steel wall) due to heat flux redistribution towards the surrounding wetted zones. This suggests that a better understanding of the mechanisms of initiation of dry-out, if possible under these particular conditions, would be welcome.*

Please see Appendix E.4.

Tur20. *Chapter 3*

This provides a reasonable overview of the data, largely generated at UCSB, on the critical heat flux. It seems reasonable to assume that the vessel is cooled sufficiently during depressurisation from the inside *to guarantee nucleate boiling when cavity flooding is initiated.*

* *

Che3. *2. Dependence of the Surface Heat Fluxes on the Length Scale of the Melt Pool*

For a volumetrically heated pool, the heat removed from the boundaries of the pool must exactly balance the energy generated within the pool under steady-state conditions. This is the case for the oxidic pool illustrated in Figure 5.2. Assuming a uniform volumetric heat generation rate, the energy generated in the pool is a monotonically increasing function of the pool depth. It follows that the surface heat fluxes at the pool boundaries must also increase with the pool depth (although the "up" to "down" energy flow split may either increase or decrease). Otherwise, a steady-state natural convection process can not be maintained in the pool. This is true no matter the natural convection flow regime is laminar or turbulent (see discussion on the turbulent flow regime in the next paragraph). Physically, the steady-state surface heat fluxes from a volumetrically heated pool can not be independent of the pool depth. In view of this, the arguments of length scale independence or small length scale dependency of the surface heat fluxes discussed in Chapter 5 and Appendix B are not physically meaningful. In conducting experimental studies of natural convection in a volumetrically heated pool, the geometry and the size of the pool are always among the key features that need to be correctly simulated.

For highly turbulent natural convection flow (i.e., at sufficiently high internal Rayleigh numbers), the convective heat transfer is expected to be independent of the physical dimensions of the pool. This is because the fine scales of turbulent mixing in the well-mixed region are considerably less than the pool depth. It follows that the Nusselt number - Rayleigh number relationship should be given by a correlation of the form

$$Nu \sim Ra'^{0.25} \quad for \quad Ra' \rightarrow \infty$$

which is consistent with the limiting behavior of the Nusselt number given by equation (5.15). Note that the product, $\dot{Q}H$, in equation (5.1) is proportional to the total heat generated in the pool per unit area of the upper surface. This product term always appears together and should not be separated. For highly turbulent flow, the upper surface heat flux is expected to vary linearly with the *product term, with the remaining terms being independent of the length scale. To be physically meaningful, the index of 0.2 in equation (5.10) should be replaced by 0.25.*

The first paragraph is attributed inappropriately, while the second one issues an inappropriate attribution that is also of no consequence.

First. There is nothing in the report to indicate (as implied by the reviewer) that "surface heat fluxes" are independent of length scale or exhibit small length scale dependency. In the same vein, we do not know where the last sentence of the first paragraph is coming from—especially given the fact that in this report we introduced the *first and only* large-scale experiment, in this field, with the proper geometry.

Second. As explained below Eq. (5.10), the point about little or no length scale dependence of heat transfer coefficient (*not* heat flux) was made to indicate the utility of a much broader data base, at the upper boundary, than that available from hemispherical geometries. This is true whether the exponent is 0.2 (as in our Eq. 5.10) or 0.25 (as in the reviewer's equation for $Ra' \to \infty$). In fact, the experimental data show an intermediate exponent of 0.233 to be valid for Ra' up to $3 \cdot 10^{13}$, which was further extended up to the near prototypic values of $7 \cdot 10^{14}$ by the mini-ACOPO data in the present work. So, in practical terms there is no basis, nor consequence whatsoever, for replacing the 0.2 exponent with 0.25, as the reviewer suggests.

But, even on fundamental grounds the point cannot be taken well, as it was already discussed in conjunction with the reviewer's own result (Cheung, 1980), Eq. (5.15) of the report. Note the following:

(a) Our interest is for intermediate values of the Pr number and finite values (*not* infinite) of the Ra' number.

(b) For such intermediate values, Eq. (5.15) shows an intermediate exponent of 0.227.

(c) Also, it should be noted that the "turning over" in a $\ell n\,(Ra'^{1/4}/Nu)$ vs $\ell n\,Ra'$ plot predicted by Eq. (5.15), towards the asymptotic regime $Nu \sim Ra'^{1/4}$, is not supported by present data (see Figure T.1, p.T-6). Note in particular that while the previously available Kulacki-Emara data stopped just short of the turn-over region, with the mini-ACOPO data we are well into it. As a consequence neither the Pr nor the Ra' number asymptotic dependencies of Cheung (1980) can be considered as verified or appropriate at this time. Further clarification of this point is expected in the near future, through the use of the ACOPO data.

Chu14. *II. Comments on mini-ACOPO Experiments*

The authors should be congratulated for making a conceptual breakthrough in simulating natural convection in pools with internal heat generation. This problem has puzzled experimentalasts for the last twenty years. However, for the experimental results to be applicable in a local sense, more detailed justifications will be needed than presented in the report. The energy equation for the problem of interest is (taken from Kelkar et al., 1993):

$$\frac{\partial(\rho C_p \phi)}{\partial t} + \overline{\nabla} \cdot (\rho C_p \overline{U} \phi) = \overline{\nabla} \cdot \left(\left(k + \frac{\mu_t C_p}{\sigma_\phi} \right) \bar{\nabla}_\phi \right) + S$$

The authors' contention is that by assuming quasi-steady states during a cooldown experiment, the variation of the bulk stored energy (temperature) with time:

$$\frac{\partial(\rho C_p \phi_b)}{\partial t}$$

can be considered to be the internal heat generation rate S. This argument is reasonable in an integral sense. However, if one were interested in local behaviors such as local heat transfer coefficients, it might be necessary to show explicitly that the local (X_f) variation of the stored energy in the fluid

$$\frac{\partial[\rho C_p \phi(X - f)]}{\partial t}$$

is everywhere uniform because the problem of interest is for spatially uniform heat generation. This type of data should be available from the interior thermocouples. While these data are not accessible to the reviewer, the discussions of self-similar profiles, Figures D.4 and D.5 in the report suggest that perhaps the bottom 10% of the volume may follow a different decay history. If this observation were true, local heat transfer coefficients from $\theta = 0°$ and $40°$ could be in error. Another location of interest would be the $\theta = 80°$ or $90°$ region where there is large difference between the mini-ACOPO result and the UCLA result.

We appreciate the favorable remarks, and the cause expressed in the form of several questions is well taken. The reviewer's interpretation of the ACOPO concept is similar but superseded by that of Schmidt's, who went into it at much greater depth. Please refer to our response to Schmidt for a complete treatment of this issue.

Chu15. *The effect of boundary conditions should also be examined. Isothermal boundary conditions will promote mixing (uniform thermal response) but an adiabatic upper boundary may be more problematic. Again, these are observations based on incomplete information, but the reviewer feels that the authors need to examine the data carefully before extracting local information and apply the information to the assessment of in-vessel core retention.*

As can be understood from the report, adiabatic boundary conditions are not of interest to this work. The one run reported in Appendix D, was in an effort to better understand the UCLA experiment.

Chu16. *There are other related issues the reviewer will not cover here. However, all these suspected uncertainties can perhaps be tested in a temperature decay experiment designed to reproduce the Kulacki-Emara data. Although, it must be recognized that a horizontal layer configuration is more likely to promote a uniform interior behavior.*

In effect, we have done that, and the result was successful (see Figure 5.3). As explained in the report, we would not expect to find anything different in a rectangular test section. However, the main issue of similarity hinges on the formation of, and sensitivity to, the stratification observed in the lower part of the hemisphere, and it would remain. Our strategy for addressing this issue is by using a very large scale experiment (as the ACOPO at 1/2-scale) as explained in our response to Schmidt.

Eps2. *(1) It is not clear to me that the authors have provided a conservative treatment of the melt layer, as stated in Section 5.2. My understanding is that Churchill and Chus' free convection heat-transfer correlation, Eq. 5.39, gives the average heat flux along the vertical segment of the reactor vessel wall in contact with the molten metal layer. I would anticipate a considerable variation of the local heat flux along this segment with a peak heat flux achieved just beneath the surface of the metal layer that may be of the order of a factor of two greater than that predicted with Eq. 5.39. Perhaps the authors feel that they have incorporated or compensated for "heat flux peaking" when they speak of the "focusing effect" and lateral eddy diffusion limitations in the bulk (on page 5-17). Unfortunately I have difficulty in following these arguments or pinpointing where in Appendix N that these arguments are confirmed. Perhaps I am wrong, but my feeling is that the only major limitation to the lateral flow of heat is the* *laminar sublayer adjacent to the vessel wall and that, in order to properly assess the maximum heat flux from the metal layer to the vessel wall analytically, the appropriate coupling (thermal and mechanical) must be made between the upward flowing free stream just outside the side-wall free-convective boundary layer and the downward flow within the boundary layer itself. Alternatively, the heat flux variation along the side wall can be obtained by experiment, perhaps with a modified version of the apparatus described in Appendix N.*

Valid point. See addendum at the end of Chapter 5, where we show that this "entrance effect" is more than compensated for by 2D conduction through the wall.

Eps6. *(5) I was particularly interested and impressed by the experimental work reported in Appendix D. I might mention that we (FAI) proposed the idea of quasi-steady cool down experiment to simulate steady-state turbulent natural convection with volumetric heating some time ago (verbal and written solicitations to ARSAP and EPRI, respectively, June 1992 through February 1993). I was pleased to learn by reading the report that the method works and I hope it will be utilized to once and for all settle the issue of the heat transfer split in hemispherical segment pools at "infinite" Rayleigh number.*

Our program for this project under ARSAP began in January 1993, and we were not planning to conduct original work in this area. We realized the need near the end of 1993, and conceived the ACOPO idea in February 1994. The mini-ACOPO was built in May 1994. We were not aware that FAI has proposed a cool down experiment previously.

Eps7. *(6) The inequality $Ra < 10^{12}$ on the top of page 5.17 bothers me. Given the form of the correlation (Eq. 5.39) 1 am sure that there is a lower Rayleigh number below which this correlation is invalid.*

This is the way the range was specified in the reference. Since it covers the transition regime, the actual lower limit would be much lower than 5×10^9, which is the lower limit used in our analysis.

Hen2. *1. The discussion with respect to the molten pool is focused on a fully molten pool with a rigid boundary at the melting temperature. Certainly this is the case for experiments such as the COPO and UCLA tests. However, as discussed in the report, the core debris in the lower head would be expected to have different temperatures for the solidus and liquidus states. The report* *clearly specifies the temperature that should be used to characterize the heat transfer from the molten pool, i.e. the liquidus temperature. However, there is no discussion on the influence of a "slush layer" between the fully molten pool and the rigid frozen crust on the vessel inner surface when there is a significant difference between the solidus and liquidus. How would this be expected to influence the correlations that have been developed from pools in which the solidus and liquidus temperatures are equal, i.e. a single melting temperature? My intuition is that this would tend to decrease the downward heat transfer and increase the upward heat transfer. If this is the case, the use of the correlations by the authors for fully molten pools tend to be a conservative representation of the reactor system. Some discussion should be included with respect to the importance of this slushy layer between the pool and the crust and the general influence this would have on the calculated results. The details of this behavior are relatively complex, but likely not of first order importance. However, the qualitative influences of this difference should be considered in the report.*

The "slush layer" is a thin region all around the inner crust boundary that allows the transition from the liquidus to the solidus (at the slush-crust interface). From the point of view of natural convection the pool "sees" the inner boundary of this layer, it being isothermal, at the liquidus. This, then, controls convection; only this! The resulting local fluxes (together with the thermal resistance of the remaining path to water, including any "gap" between the crust and the lower head) determine the thickness of this layer and of the crust behind it. But, we are not interested in the details of this split. Rather, we lump the two together in an effective crust. The "approximation," then is only to the extent that the thermal conductivity of the slush layer differs from that of the crust—truly a second order effect. One might think that in regions of low convection, i.e., at $\theta \sim 0°$, the slush layer would tend to buildup. However, this is self-limiting in that conduction alone cannot provide sufficient cooling, and the upper portion is heated to above liquidus to the extent necessary for a stable behavior. This stable behavior is the solution provided from our equations. Since this point was brought up by another reviewer as well, we have added a short explanation in page 6-3, and make reference to the response provided here for more detail.

Kre9. *4. Internal Heat Transfer Coefficients:*

Equation 5.28 was basically used for the pool-to-wall heat transfer coefficients as corrected for local distribution by Eqs. 5.30a and 5.30b. For the upward heat transfer to the overlying metallic layer, the Steinberner-Reineke correlation (Eq.

5.12) was used. Each of these was validated (or derived) via the Mini-ACOPO experiments as discussed in Appendix D. For heat transfer within the metallic layer, an existing literature correlation (Globe- Dropkin) was modified to allow separate application to heat transfer from the pool crust through the bottom boundary layer in the metal and from the metallic layer through the upper boundary layer to the top surface. For the "sideways" heat transfer from the metallic layer to the vessel wall, another existing correlation (Churchill-Chu, Eq. 5.35) was used which, coincidentally, gave a heat transfer coefficient approximately 1/2 that of the modified Globe-Dropkin correlation. The MELAD experiments reported in Appendix N were conducted to demonstrate the validity of the correlations for the metallic layer.

* *

Kre10. *Comments*

The internal heat transfer aspects of this problem are, in general, well done and acceptable. The Mini-ACOPO experiments appear to be well founded and well conducted. The results from the 1/8 scale facility should be applicable to the full scale. I have one major comment and then a number of minor comments on this part of the evaluation.

The major concern I have here is with the use of the Churchill-Chu correlation for the sidewards heat transfer from the metallic layer. I see no good reason why this heat transfer coefficient should be so much less than that for the bottom and top surfaces. The MELAD experiments reported in Appendix N appear to validate the proposed use but these were conducted in a significantly different geometry from that of the disc shape in the reactor case. I would like to see some additional theoretical analyses to justify these results.

Neither of the two correlations can be doubted, because both are supported by extensive data obtained with various fluids and by numerous investigators. To understand the difference between them simply consider the direction of the buoyancy-induced motions, in relation to the orientation of the boundary. On top, the process looks a little like "nucleate boiling," while on the side it is more like "film boiling." Incidentally, we cannot understand the comment about the MELAD experiment geometry. The issue discussed here is one of principle, and surely behavior could not be affected by the cross sectional shape of the pool.

Kre11. *There is a need to better describe in the report the thermocouple locations in the Mini-ACOPO experiments.*

The *exact* position of all thermocouples were clear in Figure D.3 of the report. It is not clear what additional information is requested here.

Kre12. *More justification is needed for the use of transient experiments to model steady-state conditions. This was addressed by Runs A4 and A5 in Appendix D. However, some comparisons of characteristic times would be helpful to completely close this issue.*

For a more complete discussion of this issue see our response to Schmidt.

Kre13. *Figures D4 and D5 should identify the various data points shown at a given value of V_i/V (I assume they are for different times during the transient-but we are not told).*

This can be (and was) done for Figure D.4 which contains data from one run only. The idea of Figure D.5 was to show that even with five runs included the dimensionless stratification trend is very similar to that of a single run. See also the addendum to Appendix D.

Kre14. *There is no figure showing that lateral temperature gradients are negligible as claimed on page D-11. An oversight?*

Actually, the variation is so small that cannot be shown well in a figure. So, we simply added the statement that the maximum deviation between the wall and centerline readings (at any elevation) was less than 5% of the overall $\Delta T = T_{\max} - T_i$.

Lev16. *2. A single molten bulk temperature is used in the oxidic pool and the metallic layer. Physically, one can expect stratification vertically and radially in the oxidic and metallic pools. The temperatures should rise away from the cooled walls in the radial direction. Also, vertical stratification due to gravity will lead to increased temperatures vertically. Such maldistribution of temperatures can be expected to have an impact upon crust formation, natural circulation currents and upwards and downwards split in heat transfer. For example, with a reduced temperature towards the bottom of the vessel, the viscosity will increase (particularly if some solids become present) and the downwards heat transfer will drop. In contrast, the upwards heat transfer will rise which tends to strengthen* *the reviewees concern about reactor vessel failure at the oxidic-metallic interface or above it.*

In this passage, the reviewer tries to predict a very complex heat transfer program with words. And the statements made are contrary to the experiments and analyses presented in the report. There are also factual errors that misrepresent what was shown in the report. Consider the following:

(a) The thermal structure of the oxidic pool is anything but uniform. It was measured experimentally, discussed extensively, and modelled as such (see Appendix D).

(b) The lateral gradients in the metal layer were also discussed and investigated experimentally (see Appendix N). In the model they were ignored, because we had no reliable way to account for them. It is obvious that this is conservative.

(c) There cannot be vertical stratification in the metallic pool. It is heated from below and cooled from above!

(d) We find that the oxidic pool near the top is superheated by ~150 °C. Solids precipitation is not expected until the temperatures drop below the liquidus, and this occurs only within the thermal boundary layer all around the pool boundary. Moreover, in volumetrically heated pools there cannot be any stagnant regions that are significant in size.

Lev18. *D. <u>Natural Convection in Oxidic Pool</u>*

DOE/ID- 1046 relies upon pool natural convection correlations and the mini-ACOPO data to predict the heat transfer in the oxidic pool. There remain several concerns about this approach:

1. Some concerns yet to be resolved are listed in the report:

(a) Timewise variation of the stratification pattern within the pool (see page 5-10) and the relationship of the final, truly steady state to the sequence of transient states leading up to it (see page 5-3).

These are incredibly out of context citations! Pages 5-3 and 5-10 present introductory material, leading up to the genesis of the ACOPO experiment. They provide the rationale for the need for the experiment. Just in case this could be missed, on page 5-11 we begin with: "The mini-ACOPO experiment mentioned above (and described in detail in Appendix D) was built and operated to address this different set of issues as well."

Lev19. *(b) Dependence upon Prandtl number. All the data in the report have been taken at Prandtl number of 7 (Kulacki-Emara, Jahn and Reinecke, and Steinberger-Reinecke), at a Prandtl number of 8 (UCLA) at Prandtl numbers of 2.6 to 10.8 (mini-ACOPO). The Prandtl numbers are higher than those anticipated in the reactor case. In 1955, the reviewer used integral methods to predict natural convection flows (see Attachment 2) and it was clearly shown that for laminar flow the Nusselt number was dependent upon the Grashof number times the square of the Prandtl number for low Prandtl numbers instead of the Grashof number times the Prandtl number. Also, there was an extra dependence found upon the Prandtl number in turbulent flow (this is also true in Eq. (5.39).*

(a) Attachment #2 makes use of velocity/temperature profiles introduced by Eckert and Jackson (E.R.G. Eckert and T.W. Jackson, NACA TN 1015, 1950). The result then is also the same, predicting a 2/5th power law on the Ra number, in contradiction to experimental data that exhibit a 1/3 power law (i.e., Churchill and Chu, 1975). It is now understood that this discrepancy is due to the poor choice of the temperature profile (R. Cheesewright, ASME Journal of Heat Transfer, Vol. 90, 1968, pp. 1-8. Consistent results with experiments can be obtained by recognizing the existence of a two-layer boundary layer structure, as discussed by George (W.K. George, Jr., Proceedings of the 6th International Heat Transfer Conference, Toronto, 1978, pp. 1-6) and George and Capp (W.K. George, Jr. and S.P. Capp, International Journal of Heat and Mass Transfer, Vol. 22, 1979, pp. 813-826).

(b) Laminar flow is of no interest here

(c) The Pr numbers of interest to our problems are 0.13 and 0.6, for the metal and oxide pools respectively, and these are quite close to normal fluids, as compared to those usually referred to as "low" Pr number fluids ($Pr < 10^{-3}$).

(d) The key aspect of the behavior in low Pr number fluids is that the thermal boundary layer extends well beyond the hydrodynamic boundary layer. In reviewer's Attachment 2, we find that this key aspect was ignored, by assuming that the two boundary layers are of equal thickness. This alone would be sufficient to explain the erroneous trend predicted by the equation in Attachment #2.

e Finally, coming to the case with volumetric heating (the oxidic pool), the reviewer is really remiss in not recognizing that ours was the first serious attempt to raise and look into this "extra" Pr number issue. In fact, we believe a value of 0.6 is close enough to unity, and together with the data that show no measurable effect over a four-fold of change (2.6 to 10.8), leaves little doubt about the validity of our formulation.

Lev20. *(c) There is considerable scatter among the available data. This is illustrated in Figures 5.7 and 5.8. The scatter certainly exceeds the "30% discrepancy which could be potentially rather significant to our conclusions due to the importance of the upwards heat flux on the behavior of the steel layer" noted on page 5-6. Similarly, the exponent on the Rayleigh number exhibits considerable variation. This becomes all the more important at the very high Rayleigh numbers anticipated in the oxidic pool.*

Figure 5.7 contains results of only two experiments, and only 2 points from one of them (the UCLA one). The bounds shown around the line for run A16 are $\pm 15\%$. We cannot interpret this as "considerable scatter". Mayinger's line was based on a numerical model, and so are the points shown for Kelkar et al. Same thing on Figure 5.8. In this figure the only non-negligible discrepancy is at the high angles between UCLA and mini-ACOPO. This was discussed in detail in the report. The parametric and sensitivity studies in Chapter 7 more than amply cover any perceived uncertainties from these figures.

Lev21. *Here again, it is worth noting that Attachment 2 shows that the Grashof and Rayleigh number exponent varies for a laminar boundary layer from 0.2 for a horizontal plate facing upwards to 0.25 for a vertical plate which explains the range of exponents shown in Eqs. (5.10) to (5.17),(5.19),(5.20), and (5.22) and (5.23). In the case of turbulent flow along the entire boundary layer, the exponent on the Grashof number according to Attachment 2 is found to vary from 0.36 for a horizontal plate facing upwards to 0.4 for a vertical plate. These turbulent predictions give partial support to the exponents in Eqs. (5.27) and (5.28), particularly if one takes into account the initial buildup of a laminar boundary layer. Also, the change in behavior observed in the mini-ACOPO data at a Rayleigh number of $3(10^{13})$ may be due to a local transition from laminar to turbulent flow.*

Given what went into the formulation in Attachment 2 we cannot see how it can be applied to plate orientations that are near horizontal. Boundary layer separation is not even mentioned in Attachment 2, while boundary layer separation (or "lifting") is the dominant mechanism in horizontal plate configurations. We do not agree with the interpretations offered here.

Lev22. *2. All the tests have been performed with a pool completely liquid and with small temperature differences from the bulk to the heat transfer surface. The use of a film temperature to calculate the heat transfer is questionable,* *particularly in view of the large temperature differences expected in the reactor core, the great number of eutectics, and the presence of solids discussed under comment C.3.*

We raised the issue of the magnitude of $(T_{max} - T_i)$ and of the effective "film" temperature at the bottom of p. 5-3, and addressed it by the mini-ACOPO experiment that attained ΔT's up to ~100 °C, which is quite comparable to the reactor values of 90 to 160 °C. Remarkably, the reviewer presents the first half of it only, here, as a criticism.

Lev23. *It is hoped that the ACOPO experiments being performed presently will help resolve some of the concerns noted above. However, it is important to note that the ACOPO tests are non prototypic of the reactor case because they cannot account for the presence of several eutectics and their solidification at different temperatures or for a metallic layer in direct contact with the oxidic pool.*

* *

Lev24. *E. DOE/ID-1046 relies upon the Globe and Dropkin correlation to predict the heat transfer within the metallic laver. This correlation was supplemented by the use of a Churchill and Chu correlation to predict the heat transfer on the vertical wall of the metallic layer. The combination was justified by a simple simulant experiment (MELAD) described in Appendix N. Several concerns with this approach have already been noted and they are reproduced here for completeness purposes:*

* *

Lev25. *1. There will be no crust between the metallic layer and the oxidic pool. There will be direct contact between these two fluids at a wavy interface and the rates of heat transfer will be different and higher from those obtained from the Globe and Dropkin correlation.*

Wrong. See above.

Lev26. *2. In order to take into account conduction within the fluid the Globe and Dropkin should be modified by adding 1.0 to the right hand side of Eq. (5.34).*

As shown just below Eq. (5.34) the data range for Globe-Dropkin is for $0.02 < \text{Pr} < 8750$.

Lev27. *3. The Churchill and Chu correlation does not agree with the equations proposed in Attachment 2 and this may deserve further examination.*

Equation (5.39) is based on an extensive data base from many sources. Perhaps Attachment 2 needs further examination, but not by us (see for example our response to item #19).

Lev28. *4. The use of film temperature is questionable again particularly close to the metallic layer-oxidic pool interface where the wavy interface could produce a much higher and oscillating temperature.*

This presupposes 1 above, which is incorrect. Incidentally, the presence of waves by no means invalidates the use (or need for) a film temperature.

May2. *Heat transfer between corium and wall of the pressure vessel*

The heat transfer between the corium and the wall, as well as the fluiddynamic conditions in the corium, which consists of an oxidic pool and an overlaying metallic layer, were very carefully studied in the report and the results are clearly presented in chapter 5. The authors compared own measurements with experimental and theoretical data from the literature and found agreement to such an extend, that they were able to predict the Nusselt-number for the heat transfer between the oxidic pool and the wall as average value, as well as in the form of local data versus the circumference of the lower hemisphere of the pressure vessel. Especially at high Rayleigh-numbers (in the order of 10^{15}), which are representative for the situation in a real molten pool, the agreement of the data is good, which means that the heat transfer coefficient can be reliably predicted.

The temperature in the oxidic pool, however, is not only a function of the heat sources and the heat transfer from the melt to the wall, but it is also influenced by the metallic layer, which is superimposed to it. In the metallic layer the density of the heat production by decay heat is much smaller, than that in the oxidic pool. Therefore in a first approximation it was assumed in the report, that pure Benard convection exists, which has a different flow pattern from that of the convection with inner heat sources.

The fluiddynamic behaviour and the heat transfer in a cavity with Benard conditions and the heat transfer to the wall of rectangular cavities are well studied and also understood in the literature. The authors compared data from the literature *and by assuming, that the convection in the metallic layer with its cylindrical surroundings can be treated like that in an rectangular cavity, they could derive reliable data for solving their problem. The simplification in the assumption for the geometry can be certainly justified.*

If the layer is of pure metallic nature, then one can certainly assume, that there are no or at least neglectable heat sources in it. It is a metallurgical question, whether there could be dissolved some $U0_2$ in this metallic liquid. Then the situation would be a little more complicated to handle it.

There is a report in the literature, dealing with the thermal interaction between a lower oxidic pool and an upper metallic layer/1/, which however is a little hidden, because it can be only purchased from the "Gesellschaft für Reaktorsicherheit" (GRS) at Köln. It is not classified and therefore freely available. In this report Steinberner and Mayinger studied the heat transfer in two layers systems by using the holographic interferometry. In Fig. I an example of the interferograms measured in the two layers are presented. This figure is taken from the above mentioned report.

The aim of these experiments was to study the heat transfer at the phase-interface between the two layers and also the heat loss at the upper free surface of the metallic layer. From this one gets the temperature in the metallic layer.

The temperature distribution in both layers is a strong function of the heat transport from the oxidic to the metallic melt and of the heat transfer at the metallic surface. Of course in addition the heat sources in both layers play an important role. Fig. 2 shows three characteristic cases for the temperature distribution in these layers. The dotted lines in this figure represent the temperature distribution, if no heat transfer between the layers would exist.

For the case, that there are no heat Sources in the upper metallic layer, Steinberner and Mayinger/1/ developed simple correlations for predicting the heat flux from the lower boundary of the oxidic pool to the wall of the cavity. When the density of the heat source is given and the Rayleigh-numbers are known. These correlations have the following form.

$$(1-\eta)^{0.87} = 0.172 \cdot Ra'^{0.226}(\eta^2 - 2\theta) \qquad (1)$$

$$\eta = \frac{0.104 \cdot \left[-\frac{1}{0.172\, Ra'^{0.226}} - 2 \cdot \theta\right]^{1.305}}{2.47 \cdot Ra'^{(-0.305)} + 0.104 \cdot \left[-\frac{1}{0.172 \cdot Ra'^{0.226}} - 2 \cdot \theta\right]^{1.305}} \tag{2}$$

The symbols in these correlations are defined as follows:

$$\begin{aligned} \eta &= \frac{q_u}{q_i \cdot L} \\ Ra' &= \frac{g \cdot \beta \cdot q_i \cdot L^5}{\nu \cdot a \cdot \lambda} \\ \theta &= \frac{(T_o - T_u) \cdot \lambda}{q_i L^2} = \frac{Ra_E}{Ra'} \\ Ra_E &= \frac{g \cdot \beta \cdot (T_o - T_u) \cdot L^3}{\nu \cdot a} \end{aligned} \tag{3}$$

Equations (1) and (2) go back to a proposal by Baker et. al./2/ and contain also ideas, which Kulacki et. al. proposed in /3/. The solution of these equation is presented in Fig. 3 in a graphical form.

Please note, that the equations (1) and (2) and the results in Fig. 3 were elaborated for horizontal fluid layers with a flat bottom. They cannot give information about the heat flux at the side wall (90°) of a spherical bottom of a Pressure vessel containing two layers of fluid, the lower one with and the upper one without internal heat sources.

In Fig. 3 and in the equations, being the basis of this Fig. q_u *stands for the heat flux density at the flat bottom of a cavity and* $q_{i\ell}$ *represents the heat source density in the heated fluid layer. The detailed derivation of the equation (1) and (2) can be found in /1/.*

The heat from the upper surface of the metallic layer is transported by radiation mainly. Radiative heat transfer is a strong function of the temperature (T^4) *and one has to also take into account the heat, which is reflected or radiated from the top of the pressure vessel to the metallic layer. This heat exchange strongly influences the temperature in both layers, the metallic and the oxidic one. With very high temperatures of the fluids the wall of the pressure vessel may start to melt (especially at the side-parts) instead of forming an insulating crust as partially assumed in the DOE-report.*

The authors of the DOE-report deliberately do not take into account the very first period of the pool-convection, when the jet of the flowing down melt penetrates *the fluid layer and is impinging onto the bottom of the pressure vessel. They argue, that the period of filling up the lower plenum of the vessel is short compared to the time, when the molten pool is exposed to purely free convection. This statement is certainly correct.*

There is another argument for this assumption of the authors. As Steinberner/4/ proved in his Ph.D. Thesis, the Nusselt-number at the impinging point of the jet is usually similar or smaller, than that one, which exists at the side wall (90°) with free convection, driven by internal heat sources. Only with very low pool heights these Nusselt-numbers are higher than those at the side wall. Fig. 4, taken from Steinberners work, shows the boundary conditions at a different pool height, and also the relative Nusselt-numbers. Low pool heights exist only for a short time, when the melt-down process starts. In most accident- cases water would be still present in the lower plenum of the vessel during this very first period, which changes the situation completely and which produces a preliminary quenching of the melt.

In Fig. 4 also interferograms of the temperature distribution in the pool during jet impingement are presented. The black and white fringes can be read as isotherms.

This difference in the pattern of the isotherms between free convection and under jet conditions can be clearly seen in Fig. 5, where the upper interferogram gives the situation without and the lower one with an impinging jet. Comparing the boundary layer at the impinging point and at the 90° position, one realizes, that the temperature gradient and by this the heat flux are similar, which can be deduced from the densely packed pattern of the isotherms.

So generally speaking one can draw the conclusion, that chapter 5 of the DOE-report precisely and reliably describes the heat transfer from a molten pool — with and without internal heat sources — to a spherical and cylindrical wall. The results presented there are a very good basis for analysing possibilities of retention of a core melt.

* *

Ola13. *(c) <u>Stability of the crust on the pool upper surface</u>*

The report makes a point that the crust separating the oxidic pool and the metal layer is very thin. Yet this crust, which is ceramic, sustains a sizable temperature gradient (leading to thermal stresses in it) and is bounded on both sides by moving *liquids (which probably produce waves much as shown in Fig. 1.2 of the report). It is very difficult to imagine that such a crust would be mechanically stable in this environment. Instead, it would probably be broken into pieces which sink into the oxidic pool because the solid density is greater than the liquid density. The crust would continually reform, but its mechanical disruption would render its thermal resistance much less than if it were a coherent slab as assumed in the report. If this were so, the boundary condition $T = T_m$ at the upper pool surface would no longer be valid, and q_{up} would greatly exceed q_{dn}.*

The crust forms upon contact, and it would be sufficient to establish the thermal boundary condition considered, even if it was unstable.

Sch1. *The review comments presented here are organized into four parts: (1) General Comments on Chapter 5, (2) General Comments on the ACOPO experiments (described in both Chapter 5, and Appendix D), (3) Miscellaneous comments that cover all the sections that I read, and (4) A brief technical note the Validity of the ACOPO Experiments for Natural Convection in Hemispherical Enclosures at High Raleigh Numbers.*

Comments on Chapter 5.

My comments here will be restricted to the discussion of heat transfer in the oxidic pool region.

Section 5.1

The fundamental goal of this section is to obtain the best estimates possible for heat transfer in the oxidic pool to the top and bottom surfaces, and the local beat flux variation on the curved surface. I have carefully reviewed this section and have the following comments.

* *

Sch2. *Upper (Flat) Surface Heat Transfer:*

In the paper cited for Eq. (5.11) the correlation is given as 0.403 $Ra^{0.226}$. Also, the correlation for Eq. (5.13) is given as 0.233 $Pr^{0.239}$ $Ra^{0.233}$. For clarity it would be useful to point out that the constants have been adjusted in this report to account for the different Rayleigh number definition.

Clarification added in the text.

Sch3. *I believe it more accurate to say that there are three (instead of two) correlations that are typically cited when considering the upper surface heat transfer. In addition to the two mentioned, the well known correlation of Jahn and Reineke for semicircular geometries ($Nu_{up} = 0.36\ Ra^{0.23}$) is often used, and in fact has been (in the past) the most commonly used correlation in severe accident codes.*

We stand corrected that Steinberner and Reineke (1978) only verified and extended the data base for the earlier Jahn and Reineke (1974) correlation. It may be semantics, but this extension was crucially into the turbulent region where it has its own merit. On the other hand, we do not believe it would be appropriate to use it for downward heat transfer (semicircular vs hemispherical geometries). See also response to item #6.

Sch4. *In addition to mentioning the Jahn and Reineke correlation, the discussion in 5.1 does not adequately point out the differences in the experiments from which the correlations cited are developed, and could be strengthened by doing so. The Kulacki and Emara study considered a plane fluid layer (rectangular cavity) where only the top surface was cooled. Steinberner and Reineke considered three different thermal boundary conditions. However, the only case for which upper surface data was taken is the one with adiabatic sidewalls, with cooled (isothermal) top and bottom surfaces. The Jahn and Reineke correlation is for semicircular geometries. As can be seen from Fig. 5.2, none of these three situations is exactly the same as the problem of interest, i.e., a hemispherical pool with isothermal surfaces at all boundaries (top, bottom, and side). The remarkable thing to note is that despite major differences in geometry and thermal boundary conditions, the correlations are all relatively close. This provides some confidence that the upper surface heat transfer in hemispherical pools with isothermal surfaces should be similar.*

The text following Eq. (5.10) to the end of the paragraph actually was intended to address this point, but from a slightly different perspective. We do *not* think it is remarkable that all these experiments agree for heat transfer to the upper plate. It would be remarkable if they did not agree! This is why we did not emphasize the geometries and thermal conditions at the upper boundaries. Still, the reviewer's clarification is useful to help sharpen the point, one way or another, and we welcome it.

Sch5. *Overall, I basically concur with authors conclusions about the results for the heat transfer to the curved surface. However, to be "conservative, but not* *overly so", on the upper surface, I think the authors should consider the use of Equation (11) instead of Equation (12) as the reference correlation for the upward heat transfer. I have prepared a figure, (Fig. 1 below) to illustrate why I think this is so. Shown on this figure are the data from the mini-ACOPO experiments (taken from Fig. 5.3 in the report), the Asfia Dhir data (Appendix C), and the three experimentally derived correlations mentioned above. Considering the current uncertainty in the mini-ACOPO data, together with the results of Asfia and Dhir, it seems to me that an appropriately conservative approach (at least for the present) is to use the Kulacki and Emara correlation, not the Steinbemer and Reineke correlation.*

We disagree with the suggestion made on several grounds, and this is important! *First*, lower heat transfer to the upper surface is *not* necessarily conservative. In fact, as discussed already in the report, looking for margins to failure one first finds them where the steel layer is in contact with the wall, which means the higher value is more conservative. *Second*, with the range $2 \cdot 10^4 < \mathrm{Ra}' < 4.4 \cdot 10^{12}$ vs $10^7 < \mathrm{Ra}' < 3 \cdot 10^{13}$ in the Steinberner–Reineke data, the Kulacki–Emara correlation would appear to be too heavily weighted to the laminar/transition regime. We feel the Steinberner–Reineke correlation, essentially confirmed by Jahn and Reineke, besides covering a narrower/higher range it extends it by one order, and hence is preferable. *Third*, with only two data points, one of them significantly lower than all correlations, and both obtained with data only over a portion of the upper wall the Asfia–Dhir data certainly cannot be considered as supporting the Kulacki–Emara correlation. This is not meant as criticism, but only to make sure it is understood that the Asfia–Dhir experiments were focused on the lower boundary. *Fourth*, the mini-ACOPO experiments extend the support of the Steinberner–Reineke correlation by nearly two orders of magnitude, and are in excellent agreement with it in the data overlap region. We take this agreement to be a clear demonstration of the validity of the ACOPO concept and the mini-ACOPO experimental and data reduction techniques. In other words, this *is* the demonstration test that the reviewer wishes to have. And this is the fundamental reason we do not wish to concede this point.

However, it is appropriate to consider using Kulacki–Emara in a parametric/sensitivity calculation, and such was performed. It is reported in Appendix P, together with parametrics suggested by other reviewers.

Sch6. Lower (Curved) Surface Heat Transfer

I basically concur with the conclusions drawn by the author concerning the heat transfer to the lower surface. Figure 5.7 was particularly useful in illustrating the *data which leads them to choose Equations (5.28) and (5.22) as representative of the spread in the current data base. However, it should be noted that the correlation of Jahn and Reineke (Eq. 5.21) was not included in Fig. 5,7. This correlation predicts much lower Nusselt numbers at these high Rayleigh numbers (For example, at Ra =* 10^{15}*, 270 vs about 6OO). It would probably be more complete if the authors directly discuss why they choose not to use this data. My experience leads me however to concur with the apparent judgement of this report and discount these predictions as too low.*

This is a fundamental point, too! The Jahn–Reineke correlation cannot be put on this plot, because it is for semicircular (as opposed to hemispherical) geometry. In fact, Jahn's data are shown in Figure 5.8, renormalized to a hemispherical geometry. We can see in this figure that they are entirely consistent with the mini-ACOPO data. In fact, the average value produced through this area-weighting process is in good agreement with trends in Figure 5.7. This means that the upper portion of the curved wall controls heat transfer, so that the convergence effect that is present in the lower-most portion of the hemispherical geometry is not so important, and the local heat transfer values actually agree. We had neglected to mention all this before, but now a remark is added as a footnote to Figure 5.8.

Sch8. *Heat Flux Distribution on the curved Surface:*

I feel that the review of the data was sufficiently complete and that the base correlation used (Eq. 5.30) is adequate for this study. However, the use of the UCLA data (which shows a more peaked distribution) was definitely needed to bound the uncertainty in the current data.

⋆ ⋆

Sch9. *General Comments on mini-ACOPO experiment (Section 5, and Appendix D)*

My primary comments relative to the ACOPO experiment are contained in the last section, which provides a more technical review of how to validate the ACOPO approach. However, some general comments are appropriate here. First, I feel that the authors should be congratulated for developing and exploring a novel approach to solving a very difficult experimental problem. The approach taken is a variation of the approach used by Chow and Akins for studying convection in Spheres (as well as a number of subsequent numerical studies by others) I am very favorably impressed with the approach, and as a result of this review

I am now a strong supporter of this method as being a good one. I none-the-less have some concerns about the strength of the validation arguments the authors have chosen to present. Furthermore, I might comment that within the context of the report (Chapter 5 in particular), I get a strong sense that the authors have a great deal of confidence in the results of the mini-ACOPO experiments. This is not wrong, and my assessment tends to confirm the validity of the approach, but further work needs to be done before the uncertainty level of the mini-ACOPO data can be clearly determined. Thus, I might recommend a somewhat higher sense of caution (for the present) then is reflected in the tone of the current report.

At the beginning of section D.5, the authors state that "the key point" validating the experimental concept is the establishment of a self similar stratification pattern during the cooldown. They define a local dimensionless temperature in Eq. (D.1), and plot the data for these temperatures in Figures D.4 and D.5. The claim is that because a "well defined, self similar temperature gradient exists in the intermediate 10% to 50% of the pool volume" that the approach is validated. I do not think that this is a correct path to validation. Even if quasi-static behavior is assumed, thermal profiles would be expected to change as the system moves from a high Rayleigh number to a lower Rayleigh number. To my knowledge, there is no basis for expecting the thermal profiles plotted using their dimcnsionless temperature to be exactly the same at say Ra= 10^{16} as they are at say 10^{14}. The argument is better made that because the range of Rayleigh numbers is not very great, and the pool is in the fully turbulent regime, that the normalized thermal profiles would not be expected to change very much. But this is quite different from claiming that "self similar" temperature profiles can be shown to exist at different Rayleigh numbers. Furthermore, I do not see how one can know that the profiles would not show approximately similar stratification patterns if the system was not at quasi-steady states. As mentioned earlier, I think there are better ways to argue the experimental validity, and I have outlined them in the last section.

This is addressed in conjunction with discussing the technical note, at the end.

Sch10. *I think the data in Figures D.4 and D.5 should be plotted as a function of depth, not as a function of normalized fluid volume. It would be easier for the reader to relate to and just as relevant.*

Not really. As seen in Figure 5.8, and as discussed above, the heat flux near the bottom is very low and its distribution very flat—nothing much happens there. A big part of the reason is that there is too little fluid in, and circulating through, it. This is why the way the results were plotted is physically much more meaningful.

> **Sch11.** *On pg. D-11, top of the page, first complete sentence after Eq. (D.1): This sentence somewhat confused me. It states that lateral temperature gradients are always negligible, which is an important piece of information, but no data is actually shown to support this statement. The authors should show the data in some form.*

It is difficult to show in a figure when the data are on top of each other. Rather, we have added a statement on p.D-11 that the agreement is within 5% of the over ΔT. Part of the text from 3rd to 8th lines below Eq. (D.1) was scrambled, and this added to the confusion. This was also corrected.

> **Sch14.** *Pg. 2-2. Last Paragraph, 2nd and third sentences ("What occasionally ...". and "In Particular ...".): These statements seem out of place and confuse the point of the paragraph. Furthermore, they are technically confusing and I don't think they're needed. Steady state is approached slowly with the time constant of the system being a function of both the pool thermal capacitance and the flow strength. Who has suggested otherwise? Appendix D does discuss the presence of strong boundary layers but only makes a conjecture that this impacts the quasi-steady state assumption.*

See discussion on the technical note at the end.

> **Sch19. "On the Validity of the ACOPO Experiments for Natural Convection in Hemispherical Enclosures at High Rayleigh Numbers"** *(see Technical Note in the original — Appendix T)*

As noted under "highlights," this effort by the reviewer is highly appreciated and most welcome. However, in the spirit of continuously deepening the understanding, we offer the following discussion.

The reviewer approaches the problem of quasi-steadiness in a strict/narrow sense. In this sense the cooling rate (he calls it Q) and the bulk-to-wall temperature difference (he calls it ΔT) must remain constant, and if so one has an exact reproduction of the original mathematical problem—i.e., the approach provides an exact analogue. The hope then is that when constancy is approximately observed, the analogue would also be approximately valid. How approximately? We do not know—in a mathematical sense—the problem is non-linear and mathematics abandons us—or rather we must abandon mathematics here—but we are already in a frame of mind that may not put us on the most advantageous path. Both Q and ΔT are varying with time (Figure 1), and we are too insistent on constancy and we find ourselves pushed to operate to a small subregion, near the end of the transient. But here is where data acquisition gets most tricky, errors creep in, and above all how can we be happy applying data obtained with a ΔT of ~ 2°C to a case where ΔT's are 100 to 150 °C? Or, could we operate at 5°C, and what error would we be committing? At the beginning of Section 2 the ΔT ~const. is introduced as a "different" constraint (from that of Q ~const.), but as can be seen in 2.1 and Figure 1, the two are, in fact, quite corresponding. So really, the crux of the argument is *judging*, in some way or another, what constitutes a quasi-steady state. We thought that on a surface-to-volume basis alone by going from 1/8- to 1/2-scale experiment we alter the "transient" by a factor of 1/4. The reviewer reasons that instead of $1/R$ an $\sim 1/R^2$ dependence is appropriate (q/R^2), which means a 1/16 improvement. Whichever is the case, the change is very significant, thus providing a reliable test of the quasi-steady state assumption. Thus we formulated the mini-ACOPO/ACOPO strategy, and we will know the results in the near future.

The reviewer judges quasi-steady state by comparing a pool-internal time constant (t_σ) to the time taken to cover a certain Ra$'$ range (ΔRa$'$) in ACOPO. The t_σ is taken as the time needed to reach a new steady state after a step change in Ra$'$ number in an experiment performed with volumetric heating—an equation from Kulacki-Emara was utilized for estimating t_σ. In this experiment (planar layer) only the top was cooled, and the t_σ is time required for the fluid in the bulk to lose (or gain) the excess temperature. But this is a "bulk" process, and has very little to do with the boundary layer that controls heat transfer. This is what we mean in p.2-2, Last paragraph, second and third sentences, which has been questioned as "confusing" by the reviewer. Namely, that the pool-internal time constant is *misunderstood* as representing some sort of transient effect on the heat transfer *behavior*. Which brings us to our definition of quasi-steady state, as one that the cooldown "should be slow enough to allow the process to pass through a series of quasi-steady states that approximate corresponding steady-states with heating rates equal to the instantaneous cooling rates in the experiments." This means that the *boundary layer* time constant is short compared to the pool cooldown time, and that we can *use* the heat transfer coefficients (determined from a cooldown experiment run under conditions that satisfy quasi-steady state—in our definition of it) evaluated on the instantaneous pool temperature to *predict* the transient response of a steady-state pool subjected to a sudden change in Ra$'$ number. It should be clear, now, that t_σ grossly exaggerates the fundamental characteristic time against which pool cooldown time should be compared to judge quasi-steady state. So we come to the conclusion that the mini-ACOPO is not marginal but amply meets quasi-steady state, and that the ACOPO will do even more so.

Note: The boundary layer time constant can be taken roughly as a few characteristic residence times of the fluid in it. For example, using a mean velocity of ~5 mm/s (see Steinberner and Reineke data) and a length of ~40 cm (as in mini-ACOPO), we find a residence time of ~1 minute, and expect a proper value of t_σ to be only ~2 minutes.

> **Seh8.** *The thermal loadings on the internal wall of the vessel have been determined for the final stable state of the melt pool natural convection. Nature is quite kind in the final stable state, since the stratification in the lower levels of the melt pool reduces the convective heat flux to that transmitted by conduction. In the transient states leading to the final stable state the stratification may not be fully established and the heat fluxes near the pool bottom may be higher. This has been recognised by the authors as an "open issue", on page 5-10, and it primarily affects the thermal margins established for the lower reaches ($\theta \leq \pm 15°$) of the vessel. An evidence of this is also in the Figure D.5, for Vi/V near zero and near 0.06, where the ACOPO quasi static (along the cooling transient) dimensionless pool temperatures show a variation of a factor of $\equiv 2$.*

The four items enumerated on p. 5-10 are establishing the *need for* the ACOPO experiment, and *not the consequence* of having it. Thus, in particular, item 4 (transient effects) is first identified here as an "open issue," and in subsequent pages it is addressed by the mini-ACOPO test. It will be further addressed by the ACOPO test, but as mentioned already, this is only confirmatory. As far as the variation exhibited in mini-ACOPO, it is now addressed in an addendum to Appendix D. A footnote to Figure 5.8 was also added so that this addition is not missed.

> **Seh9.** *Perhaps, further investigation of this "open issue" could be performed through a perusal of the data from the COPO and the UCLA experiments; and also through calculations of the transient natural convection states leading to the steady state.*

The UCLA data are shown (on Figure 5.8) to be well bounded by our data. The COPO data (shown in Figure B.7a,b) show that the local fluxes go to zero as $\theta \to 0°$, so they also are bounded. However, *none of these* data sets provide information on transient states, *while the mini-ACOPO does.*

Seh10. *The technique used in the mini ACOPO experiments, and to be used in the ACOPO experiments, is unique, since the experiments related to IVR, performed by all the other investigators have employed volumetric heating. The ACOPO technique makes the experiment very simple and if it is valid, it really advances the state of the art of the experimentation in this area. I believe, the data obtained has been expressed in the form of correlations developed for the volume-heated experiments by using the cooling rate as equivalent to the heating rate. It seems to follow the same correlations as did the volume-heated experiments, except, perhaps, there may be some differences. The mini ACOPO experiment, having a reasonable volume, seems to reach a stable state within minutes, whereas in the COPO and the other earlier volume-heated experiments, it took much longer time.*

There is a fundamental problem with the reviewer's interpretation here. We have anticipated such difficulty and tried to help it with a note near the bottom of p. 2-2 ("What occasionally has been mentioned as a slow approach to steady state is really attributable to the thermal capacitance of the pool rather than to unsteadiness in the natural convection process. In particular, in Appendix D we demonstrate that boundary layer effects dominate, so that the behavior of such pools is readily predictable even under non-stationary conditions. What this means is that the thermal loads to the pool boundaries throughout the time period of a heat-up transient are bounded by the thermal loads in the final steady state.") In fact, comparing the time needed for steady states in volume heated pools, to the time constant of the natural convection process is like comparing apples and oranges! See also our response to Schmidt—Item #19.

Seh11. *Figure D-9 shows that in the cool-down pool the upward heat fluxes in the center half of the pool are approximately 20% higher than those in the outer half of the pool Such spatial profiles were not measured in the internally-heated pools.*

It is not clear what the reviewer is referring to by "internally-heated pools." Only the average value is available from COPO, and only one value, on the outer portion of the upper boundary is available from the UCLA experiment. On the other hand, the difference between the inner and outer regions in Figure D-9 is *only 15%* or less (not 20%). This cannot be considered very significant for the kind of process/problem considered here.

Seh12. *Instead, unsteady wave-form and dynamically changing upward heat fluxes were measured. Perhaps, the natural convection system with volume-heating is much stiffer than without it, and it may be that the transient nature of* *the cool-down experiments, driven only by the boundary conditions, is different than the unsteadiness of the internally-heated turbulent liquid pool. Periods of unsteadiness in internally-heated pools are in range of 3–10 minutes and it may take many periods before the flow structures shown in Figure 5.2 are established. Do such flow structures get established in the cool-down pool within the few minutes needed to reach the steady state? A demonstration of the cool-down pool natural circulation, as the same as that in the internally-heated pool could be through the measurement of the flow structure in the cool-down pool.*

This is speculative and not consistent with available data. The reviewer refers to the oscillations in Figure C.10 which are *minuscule*, and would be smaller still at the higher Ra' numbers of interest here. Some oscillations are actually expected as plumes form and detach from the boundary layer, and some oscillations are in fact seen in mini-ACOPO. These are all, however, second order effects, and the data themselves show that the first order flow patterns are established in a matter of seconds. Indeed, the flow structure in the mini-ACOPO pool is directly evident from the stratification patterns already shown in Figures D.4 and D.5. It was an omission to not include "time" in these figures. We have now done so in an addendum to Appendix D. Also, it should be noted that the first few minutes marked "unstabilized flow" in the energy balances refers to the transient behavior of the cooling jacket itself, and *not* to the natural convection process within the pool. To avoid a chance of misunderstanding, we have now made that explicit in the caption of these figures as well. Using the bypass flow, this was minimized in run A16, and as shown in Figure D.8, the data are consistent from the first minute on. The time-wise development of stratification is now added as Figure D.21, and should help the reader appreciate how rapidly the internal patterns develop.

Seh13. *On page 5-3 of the report, it is stated that the natural circulation in a pool, with no volumetric internal heating, obeys the correlation* $Nu = F(Ra, Pr^m)$. *Perhaps, the results of the cool-down experiment could be correlated through this correlation; and the upwards and downwards heat fluxes obtained compared with those obtained through Equations (5.12), (5.28) and (5.30). I do not know whether this is a fruitful approach, however, it may provide some insight.*

We do not think this is a fruitful approach, since it would be recasting the same information.

Seh14. *The heat transfer correlations obtained in the document do not have any dependence on Pr number, and the experiments performed for fluids having*

Pr number between 2.6 and 10.8 confirm that. (Cf Figure 5.4) Calculations performed recently by Dinh et.al., to be reported in the NURETH-7 meeting, show that the heat fluxes do not change significantly for Pr numbers between 2.0 and 10.0, but at Pr = 0.6, the downward heat flux increases considerably, while the upwards heat flux decreases slightly. This calculated result is for the laminar natural convection pool (Ra = 10^{11}) and its applicability to highly-turbulent pool is not assured. However, there may be merit in investigating the regime of Pr number below 2.6. The stably stratified flow patterns near the bottom the vessel may be different for the low Pr number fluids, and that may change the heat flux to the very bottom regions ($\theta \leq \pm 15°$) of the reactor vessel.

This is speculative also, and not supported by the data and what we know about convective heat transfer. First of all, as shown in Eq. (5.5), the Ra′ number actually incorporates the Pr number. There is no apparent a priori reason to expect an independent (additional) Pr number dependence, and what we know from limited experiments with liquid metals (Pr$< 10^{-3}$) such dependence is extremely weak ($n < 0.1$). We felt compelled to conduct a special investigation, as a caution due to Eq. (5.13). We showed that a five-fold decrease in Prandtl number has no effect. The reviewer feels that an additional four-fold decrease (from 2.5 to 0.6, which is the value for corium) can have a significant effect. There is no physical reason to expect such a sudden change in trend. This is consistent with the final version of the paper cited by the reviewer (apparently a newer version than the one available at the time of the review), depicting less than a 20% effect on local fluxes, near the bottom, for a Pr number change from 7 to 0.6 (semicircular geometry, Ra′ $= 10^{10}$). The effect is already negligible for our consideration, and it would be even less for the highly turbulent flow of interest in our case (Ra/ $\sim 10^{15}$).

Seh18. *The authors have not considered phase change in their evaluation of heat fluxes, particularly where crust or vessel wall melting may occur. This certainly will complicate the evaluation, however, many times the phase change reduces the heat transfer, due to the needed heat of fusion, and the changes in viscosity that may occur at the melting surface. Perhaps, an estimate of this effect could be made.*

This point is not relevant to the evaluation. At steady state, as analyzed here, there are no phase change effects on the heat fluxes. This may be conservative during the transient, but the time constant of these processes is much shorter than the time scale of the accident scenario, steady state will certainly be reached, and therefore its consideration does not constitute an undue conservatism.

Seh19. *Comments on Specific Items*

<u>Section 5, Pages 5-8 and 5-9</u>. *The Kelkar calculated correlations of* $NU_{up} = 0.18\ Ra'^{0.237}$ *and* $Nu_{dn} = 0.1\ Ra'^{0.25}$, *both under-predict the values of the Nu numbers at* $Ra' = 10^{10}$, *when no turbulence model should be involved. Kelkar correlation gives* $Nu_{up} = 42$ *and* $Nu_{dn} = 32$, *while the Steinbemer-Reineke measured correlation provides* $Nu_{up} = 74$ *and Eq. 5.22 provides* $Nu_{dn} = 55$. *I believe, there is something wrong with the Kelkar calculation. It does not matter that at* $Ra' = 10^{15}$, *the values of* Nu_{dn} *from Kelkar and Mayinger correlations are only 2% different. I believe, the calculated "correlations" should not be put in the same "pot" as the measured data. In fact, I believe, that discovering the correct turbulent eddy-diffusivity model, which will be valid for the experimental and the prototypical conditions (melts, geometrics etc.) would be a great achievement.*

While it is not our job to defend the Kelkar et al. calculation, it is the only one around, and it would be an omission to not include it with all other relevant information on Figure 5.7. We think the comparison is interesting, and we did point out the discrepancy of the same calculation with the flux data in the upper boundary. In the same vein, we should not forget also that the Mayinger correlation was derived from calculations, and it suffers from the same difficulties in the upper boundary. All this may point to the fact that near-vertical boundaries are easier to calculate than horizontal or near-horizontal—see also comparisons of the shapes in Figure 5.8. This makes sense physically also if one thinks about the more intricate nature of turbulence source/sink terms near such boundaries.

Seh20. <u>Section 5. Pages 5-16 and 5-17</u>. *Specialising Eq. (5.35) to two boundaries with equal temperature drop, one would obtain*

$$h = 0.059 \cdot 2^{1/3} \cdot \left(\frac{g\beta}{\alpha v} \cdot \Delta T'\right)^{1/3} = 0.074 \cdot \left(\frac{g\beta}{\alpha v} \cdot \Delta T'\right)^{1/3}$$

which is different from Eq. (5.41). This probably is a typo, or I do not understand the text before Equation (5.41).

No. Equation (5.41) is correct. There is a 2 also that comes from the left-hand-side (for h write $q/\Delta T = q/2\Delta T'$).

Sei5. *II-2) No heat flux profile has been considered for the metallic pool. This should be justified since the margin to critical heat flux is relatively low in some cases (fig 7.16 and 7.15 for adiabatic conditions).*

This is a valid point, and an omission on our part. It is discussed now in an addendum at the end of Chapter 5. No significant impact results.

> **Sei9.** *II-6) The correlations presented in section 5-1 of the report are qualified on experimental results coming from COPO and mini-ACOPO. In the report describing the COPO experiments (appendix B) it is indicated that a thin layer (0.1 mm) of Teflon is used as electrical insulator over the cooled walls. This layer represents a thermal resistance of, about, 4E-4 m^2 K/W. The thermal resistance due to the boundary layer flow in water is estimated to be, about, E-3 m^2 K/W. This means that the Teflon layer represents, about, 30% of the total thermal resistance. (This is an order of magnitude as the local thickness of Teflon may vary). Thus the validation of the correlations against these experimental results is questionable within an uncertainty range of, about, 30% which is quite important and which weakens some other considerations (for instance concerning the way the physical properties must be estimated).*
>
> *Furthermore, the electrical insulation of the top cooling plates is made of alumina which has a high thermal conductivity at low temperatures. Thus, the thermal resistance related to this alumina insulation layer may be lower than the thermal resistance due to the Teflon layer. This may lead to a non prototypical increase of the heat transfer to the top and, consequently, to a decrease of the lateral heat fluxes. May these effects be quantified and included in the uncertainties ? What would then be the consequences on the lateral heat fluxes in the reactor situation ? (a little increase of the lateral heat flux in the region of the oxydic pool may not endanger the vessel and a decrease of the power diverted to the metallic layer may increase the safety margins ?)*

The COPO experiment, with a slice of torospherical shape, was not used in any direct way in the analyses presented in the report. Rather, it provided important background support on the group of correlations by Mayinger and co-workers, which, in turn, provided the *entré* to the correlations employed in conjunction with the mini-ACOPO data for use in our work. It should be noted, however, that the effect of the Teflon resistance was compensated by *local* adjustment of water flow rates in each of the colling units, so as to obtain an isothermal boundary within the tolerances discussed in the paper. So, the 30% "error" quoted above is *not* appropriate.

Sei10. *II-7) Heat transfer in the metallic layer*

The Correlations presented in the report (pages 5-16 and 5-17) are valid for fluids having a Prandtl number higher than 1 and have been validated on water experiments MELAD (Pr about 5 to 10). We also know from the work on LMFBRs that correlations valid for low Prandti numbers (sodium, Pr about 0.005) are based on the adimensional group $GrPr^2$ rather than on GrPr. Steel has a Prandtl number which is intermediate (about 0,1). Thus we ask about the validity of the correlations used for the metallic layer and we are not convinced that experiments performed with water are representative. But the main question concerns the heat flux distribution, and a different choice of correlation may perhaps not affect this distribution. Could a sensitivity study be performed to check this point ?

Actually, the correlations discussed on pages 5–16, 5–17 encompass the Prandtl numbers of interest here. Namely, Eq. (5.34) is reported to be valid for $0.02 < \text{Pr} < 8750$ and the Eq. (5.39), which we use, is valid for *all* Pr numbers. The water experiments were performed to demonstrate the separation of the Globe-Dropkin correlation into 2 boundary layers and the integration with Churchill and Chu for an integral model of the pool. With this background we wouldn't be able to imagine what an appropriate sensitivity study would be, but we are open to suggestions.

Sei11. Mini-ACOPO:

II-8) The definition of the Ra' number based on the transient approach is not given. From the text we understand that this number is based on the thermal inertia of the liquid and on the cooling rate ?

Yes.

Sei12. *II-9) The intemal Rayleigh number (Ra') is much more sensitive to the scale (power 5) than to the temperature difference (power 1). Thus it may be expected that small scale experiments privilege laminar boundary layer flows on the side walls which are not prototypical of reactor conditions.*

Agreed. This is why we are running the 1/2-scale ACOPO, too. But we expect results to be confirmatory, because the Ra' numbers in the mini-ACOPO were large enough already ($Ra' \sim 10^{15}$), to place it well above the transition ($Ra' \sim 10^{12}$).

Sei14. *II-11) I am not sure that the transient approach is representative of all cases with internal heating. For instance in the situation of a homogeneous pool* *with an adiabatic upper boundary we have observed an overshoot in the pool temperature nearby the adiabatic surface in the BAFOND experiments (volume heated)(Ref 1). Overshoot means that the temperature increases much just below the adiabatic surface due to the stagnation condition. This temperature increase may induce heat flux peaking at the top of the cooled sidewalls. Such effect is specific to volume heating conditions and may not be observed in a transient pool experiment.*

→ I would suggest that an analysis of the representativity of transient cooldown experiments and related quantitative scaling should be included in the paper (also in relation with remarks II-7 and II-8).

A top adiabatic boundary leads to a fundamentally different behavior. See, for example, flux shapes in Figure D.19. The key behaviors in volumetrically heated pools, with isothermal boundaries, are quite well known, and mini-ACOPO reflects those key features accurately. More information on this point is given in the comments of Schmidt, and our responses to them (item #19).

Sei15. *II-12) Figures D-12 and D-17 from appendix D suggest that the heat flux distribution is not uniform in the upper isothermal (as suggested by Fig D.15) region. This has not been observed on the COPO experiments (at least no strong effect was observed). Is this related to a scale effect ? (usual heat transfer correlations for turbulent boundary layers suggest that the heat exchange coefficient does not depend on the distance).*

If this observation is extrapolated to the metal layer have we thus to consider a heat flux profile in this layer ?(see also remark II-2) (This would reduce the margins to failure).

From Figure B.6a,b it is hard to discern any trends in COPO due to the rather large data scatter. With values ranging up to almost a factor of 2 it is not immediately clear how to compare with Figure D.15. Moreover, the mini-ACOPO boundary is not vertical even at high angles, and scale could also play a role. We expect to resolve these points with the large ACOPO. The steel layer, as already mentioned, is now discussed in this respect in an addendum to Chapter 5.

Spe6. *[5. The database used for the analysis should be extended to include real reactor materials involving realistic temperature levels, boundary conditions, and crusting effects, and real melt behavior in the superheat range as well as slurry range between T_{sol} and T_{liq} for the $U0_2/ZrO_2/Zr$ system. The authors themselves* *have devised an excellent approach to achieve this data via the ACOPO pool approach wherein high Ra′ data is obtained for Nu_{up} Nu_{dn} and $Nu_{dn}(\theta)$ using large melt heat capacity in a cooling mode in lieu of internal heat generation. A few reactor material tests should be performed analogous to ACOPO at 1/2 scale, including in some cases the integral effect of an overlying steel layer.]*

This has been addressed under "general comments and highlights." As far as we can see, the only "technical" component of this expressed need is concerning the "slurry" range between T_{sol} and T_{liq}. But it is well-known that the inner boundary of the "slurry range" will be at the melt liquidus, and this is all that matters as far as natural convection is concerned. This is the approach taken in the report. Again, the PACOPO experiments would be interesting, but their role should be viewed as strictly confirmatory, and in fact not really necessary in the order of priorities for the AP600.

Tuo2. *Viscous effects*

The corium pool heat transfer experiments have employed water and freon as a working liquid. Corium itself behaves in a different way on the pool boundaries where crust is formed: the increase of viscosity takes place gradually in corium. There is a not a sudden jump from the solid to the liquid phase. On the other hand, the validation calculations for the pool heat transfer take plenty of effort when trying to solve the heat transfer in the turbulent boundary layer.

It would be interested to obtain the authors' opinion on the influence of increasing viscosity to the heat transfer distribution, particularly whether it could increase heat transfer in the upwards direction. What are the authors' recommendations for the future fluid dynamics calculations?

This is also referred to as the "mushy" or "slurry" layer. For the present system this can be thought to exist next to *all* boundaries. The thickness of it depends on the intensity of local convection and hence of local heat flux. The higher the flux the thinner the layer. The existence of this layer is to allow the transition, required by thermodynamics, from the liquidus, on the melt-side face of it, to the solidus, on the inside face in contact with the crust. As a pool boundary temperature we use the liquidus, and as driving force for heat transfer, the pool superheat. Thus, it can be said that in our treatment the crust *and* slurry layer are lumped together in an effective crust. Thus, there is no change on heat flux distribution to be found if the slurry layer were to be treated explicitly. An explicit treatment would allow us to determine how the thermal resistance of our effective crust is split between a real crust and a slurry layer, but this is an complex problem whose solution would require consideration of convection and its effect on the slurry layer thickness and properties. To a first, but adequate in our opinion, approximation in areas of strong convection (which are all *except* the lowermost fluid region exhibiting the strongest stable stratification—see mini-ACOPO data) the mushy layer will be thin and hence of minor interest. In the lowermost region this mushy layer might build up some more, reducing somewhat the local fluxes; however, this is such a small area (compared to the total) and with such already low flux level, that any change in it would not perceptibly change the rest of the heat fluxes, including, in particular, the one in the upwards direction. On this basis, regarding future fluid dynamics calculations we would recommend a similar approach as this, i.e., using a liquidus temperature as the boundary condition and lumping the mushy layer with the crust into one effective crust.

> **Tur8.** *As the primary factor driving convection is the temperature difference, and the system response is determined by the heat flux. I would expect length independence to imply that*
>
> $$Nu \sim (Ra')^{0.25}$$
>
> *not as Ra′ to the 0.2 power as indicated by equation 5.10; I note this is consistent with Cheung's analysis referred to on page 5-6.*

Equation (5.10) is presented as an order-of-magnitude to make the argument of weak, or no scale dependence, so we can use results from horizontal layers for the upper boundary, as discussed. This is certainly true whether one uses Eq. (5.10) or the exponent 0.25 in it. In fact, for finite values of the Ra′, the exponent falls in between 0.2 and 0.25. This point is further discussed in response to Cheung's item #3.

> **Tur9.** *I note that the apparent transition in the COPO experiments (page 5-5; fig B.2, page 5-9) at high Ra corresponds to the sets of experiments with different pool depth.*
>
> *Given the margins that seem to exist, the statement on page 5-6 that 'even a 30% discrepancy could be potentially rather significant to our conclusions' seems rather strong for the AP600 application. However, I am pleased to see this issue has been addressed further in the mini-ACOPO experiments and the planned follow-on larger scale tests.*

We agree. This statement was written before we had the mini-ACOPO and the rest of our analysis together.

Tur10. *The following comments apply to Appendix D, describing the mini-ACOPO experiments in more detail:*

The basic idea behind these experiments - to obtain data in proper geometry using cool-down rather than internal heating - is to be commended. The major question is whether the data should be applied directly, or used to benchmark a model that is then applied to the internal heating case. As the authors note, mathematically a spatially uniform cool-down rate is equivalent to an equilibrium with internal heating. They go on to show that over most of the volume the temperature is close to being uniform, so the cool-down rate is also spatially uniform in this region. However this does not apply in the lower part of the pool, where the 'effective volumetric heating' will be appreciably lower than in the bulk. It is not sufficient to demonstrate self-similarity to claim uniformity of heating. Indeed the curves showing self-similarity are quite constrained - they must asymptote to 1 and average to 0, thus it is not surprising that the largest discrepancies are at small values of V_i/V. A better indicator of the uniformity of the heating would seem to be

$$(T - T_w)/(T_{max} - T_w)$$

where T_w is the wall temperature. It should also be noted that the bottom 10% of the volume corresponds to about one-quarter of the pool depth, so the effective heating close to the pole of the vessel may be significantly reduced. The consequence of this will be to bias the results somewhat to lower downward heat fluxes (particularly near the pole). For completeness, I would prefer these effects to be taken account of in a model of the pool (not a full CFD simulation) although I do not expect them to invalidate the conclusion the authors draw from these experiments.

The points made here are well taken, and we are working on a model. Also, please see the addendum to Appendix D and Schmidt's Technical Note (part of his comments) and our response to it.

Tur11. *The assumption that the pool reaches a quasi-equilibrium configuration is justified by the experimental data, and the observation that the pool suffers no major internal adjustments during the cooldown is also important in justifying the experimental method.*

It is noticeable that there is a stronger dependence of downward Nusselt number on Ra′ than the correlation lines shown in Figs D.10 and D.11. Extrapolations of these data to reactor-size pools will give more equal downward and upward heat transfer correlations, in contrast with the 2-dimensional COPO data referred to in Appendix B. (This is covered in the main text of Chapter 5).

The maximum wall peaking factor of two seems to be well-founded.

The correlation line shown in Figures D.10 and D.11 is Eq. 5.22 (Mayinger) The somewhat stronger dependence of data on the Ra′ number was reflected in Eq. (5.28), which is the one used in the analyses.

Tur12. *The claim on page 5-12 that the UCLA data (for downward heat transfer) indicates an intermediate behaviour is not consistent with the plotting on Fig 5-7. While only a modest extrapolation is necessary, it is not justified to refer to the extrapolations as bounds - can say 'are expected to bound'.*

Strictly speaking, the reviewer is correct. Really, one could not extrapolate from the 2 UCLA data points. Our statement that "trends of the UCLA data . . . indicating an intermediate behavior" derives from the observation that they are somewhat higher than the Mayinger correlation, while the UCLA authors have interpreted them (see Appendix C) to be in essential agreement with it—i.e., same trend.

Tur13. *The treatment of the metal layer appears reasonable and conservative.*

⋆ ⋆

Tur14. *The simplified model (pages 5-17 to 5-24) is interesting. However, the figures 5.9 to 5.11 expect a lot of work from the reader. I suggest only one set of curves per figure, which can then be labelled appropriately. Of the assumptions made for this model, I suspect the energy radiated from the surrounding cavity to the layer (assumption 4, page 5-18) may not always be negligible (equivalently the view-factor is reduced from 1 to allow for sidewalls close to the melting point of the steel).*

Strictly speaking, the point is correct, but as we show in Chapter 6, using the complete model, that includes back radiation, the effect is negligible. We revisited Figures 5.9 through 5.11 regarding clarity, and we agree with the reviewer that they require too much work from the reader. The main reason for this, we think, is the faulty caption in the figures. Rather than expanding the volume with more figures, we decided to remedy this by making the use clearer in the captions.

Tur15. *Overall, this chapter presents a balanced account and the conclusions drawn are consistent with the current experimental database for convection in ideal simulant liquids. Too little account is taken of effects that might come into play with real materials, and no mention is made of the somewhat contradictory results that have been obtained with UO_2 melts in the past (Argonne experiments by L Baker et al and the SCARABEE-N test). However, there is a lot of margin available, provided the ULPU critical heat flux curve is appropriate, and it is difficult to see any circumstance in which non-ideal fluid effects would lead to vessel failure.*

It is very difficult to conduct experiments with prototypic materials, and harder yet to obtain basic information from such. The ANL tests were run with pure UO_2 in a cubic, 10 cm on the side, test section with an essentially adiabatic boundary condition at the upper surface. They found that the downward heat flux was close to that on the side walls, in contradiction with what could be expected from basic natural convection physics. This uniformity in behavior was attributed to the dominance of an internal radiation heat transfer mechanism (i.e., transparency of pure UO_2 to the infrared). This is not expected to be the case (L. Baker, 1995, Personal Communication) for the reactor oxidic melt, due to ZrO_2 and many impurities in it. Also due to the smallness of the test section, the thermal radiation path was rather short, compared to reactor scale.

Tur18. *I support the view expressed on page 2-2 that what has been mentioned as a slow approach to steady state is really attributable to the thermal capacitance of the pool rather than unsteadiness in the natural convection process. However the following statement that the thermal loads are bounded by the steady state is untrue, as the discussion of the metal layer shows: as this layer grows the heat flux to the vessel wall reduces.*

At this stage the focusing effect of the metal layer has not been introduced yet, so our statement refers to the thermal loads from the oxidic pool only. The focusing effect is discussed and treated separately in Chapter 7.

Tur23. *The reality of the thin crust between the oxidic and metallic layer is not questioned in the report. If it is unstable, it may lead to an augmentation of upward heat transfer.*

We do not see significant mechanisms for sustained instability. Moreover, the oxide freezes upon contact with the metal, which is sufficient to establish the thermal boundary conditions.

Integral Model Aspects

Hen3. *2. As discussed in the report, the sequences which are considered are generally those in which the RPV lower plenum is full, or almost full of water, at the time that molten core debris enters the lower head. Experience with such situations indicates that there could be a non-trivial contact resistance develop between the crust and the wall when this occurs. Such a contact resistance is not considered in the analysis presented in the report. Neglecting such a resistance is a conservatism in the analysis for the downward energy transfer to the RPV lower head. Conversely, this increases the upward heat transfer to the remainder of the RPV and therefore the heat flux transferred to these other puts of the reactor vessel. Estimates from the available information suggest that the contact resistance could be the equivalent of conduction through a few centimeters of UO_2. Here again, the details of the analyses do not have to be included; rather, the influence of such behavior should be discussed and perhaps included as part of the sensitivity analyses at the end of the report.*

As discussed above, the effect of any gap would be to decrease the crust thickness and hence the conductive component of the heat going through the vessel wall. The effect would be maximum at $\theta \sim 0°$, and ignoring it there is certainly conservative; by how much can be deduced from the crust thickness distributions shown in Appendix Q. As far as effect on the global behavior, it is negligible, because by the condition of Eq. (6.8), the *total* crust is less than a few percent of the oxidic mass (and hence of the decay heat). The point is clarified by reference to this response in page 6-3. Also see the new Appendix P.

Hen5. *4. The bottom line to the integral evaluation is discussed in Section 6. Since this documents the integral analysis, I recommend that this discussion be expanded to make several of the central elements of the analysis more clear. For example,*

a. Equation 6.6 described the heat flux into the wall. Does $\delta_{cr}(\theta)$ include the power generated in a "slushy layer" dictated by the temperature difference between the liquidus and solidus conditions?

Yes. See response to point #1.

Hen6. *b. The upward radiation calculation described in 6.10 assumes one characteristic temperature for the steel internal structures and therefore does not* *need to consider the respective view factors to individual parts of the rector vessel, i.e. the downcomer and the upper internals. If the discussion is only focused on the integrity of the lower head, this is sufficient. Conversely, if the intent is to describe the potential for in-vessel core debris retention, then it is important to justify that the upward energy flux does not cause the vessel to fail at some other location between the metal layer and the vessel support location, i.e. the hot legs and cold legs. To accomplish this, the analysis should be somewhat more detailed than that which was represented by Equations 6.10 and 6.12.*

The reason there is no concern for the side wall, and hence for a detailed radiation network-type calculation, is because the process is overwhelmed by the large surface available to dissipate the heat radiated off the top of the pool. For example, the heat flux leaving the top of the metal layer is limited by $\epsilon_s \sigma T_{\ell,0}^4$. With $\epsilon_s = 0.45$ and $T_{\ell,0} = 1712$ K, which corresponds to the condition in Figure 7.16, this heat flux is 220 kW/m^2. Actual fluxes through the vessel wall will be much lower than this value due to the much larger surface area available.

Lev29. *5. The energy balance equation (5.43) lacks a radiation term to account for reflected energy from the receiving surfaces. The right hand side of the equation should have a negative term which contains the emissivity of the receiving surface and its absolute temperature raised to the fourth power. This term could have a significant impact on the results presented in DOE/ID-1046.*

See assumption #4 just above the equation. The complete model, including back radiation, is described in Chapter 6. This complete model is used in the calculation of Chapter 7. The simplification in Chapter 5 was made to obtain the universal solution shown in Figures 5.9 through 5.12. Comparisons with the full model, in Chapter 6, shows that the error due to this and the other three assumptions listed is negligible.

May3. *Heat conduction in the wall of the vessel*

To calculate heat conduction in a solid wall is a very simple task, if the transport properties—especially thermal conductivity—are given at the relevant temperatures and if the boundary conditions—heat transfer coefficience and temperatures—are known. There is enough information in the literature and also in the DOE-report about the transport properties. However the boundary conditions at the outer and the inner side of the wall are more complicated to handle.

The heat transfer coefficient at the inner side of the wall is very well described in chapter 5 of the DOE-report, as already mentioned. Also the heat transfer at the *outer side or the guarantee, that DNB will not be exceeded, is well documented in the report, as discussed a little later. An open question seems to be, whether at the inner side of the vessel also at the positions of highly convective flow (90°), a crust is formed or whether the material of the wall is eroded by the hot melt. The report presents data on the thermal conductivity of the steel up to 1500 K (appendix L) and also deals with creep considerations for the lower head (appendix G).*

Including all the other informations in the DOE-report, it is possible to describe the stress in the wall of the lower head during meltdown and during the free convection of the melt. To do this one needs a small computer code, correlating the feedback control between boundary conditions at the inner and at the outer side of the wall, the heat conduction of the wall and the wall thickness. There are some deliberations in the report about this subject, however I missed detailed calculations of this problem.

A very simple estimation may demonstrate this subject. Let us assume, that the temperature at the outside of the wall is 373 K (nucleate boiling) and that the temperature on the inner side must not exceed 1400 K, then the thermal conductivity varies between 40 and 30 W/Km, with a minimum of 25 W/Km at 1100 K, as can be seen in Fig. L-3 (page L-21 of the report). Furthermore we take a heat flux of 500 kW/m^2 from the melt to the side wall. With a very simple application of Fourier's law, we then end up at a maximum wall thickness of 6 cm for these assumptions.

A parametric study of the temperature situation in the wall at various boundary conditions would still give more confidence to the final and certainly correct conclusion of the DOE-report, namely, that the pressure vessel of the AP 600 can retain a pool of molten core, just by flooding the cavity between the pressure vessel and the shielding concrete.

In the calculations discussed in Chapter 7, we accounted for the temperature variation of thermal conductivity, as given in Appendix L, and allowed for wall melting as dictated by the local heat flux (see Eq. (6.6) and Eq. (6.7) and the next few lines of text that follow). The reviewer is correct that we failed to emphasize these, which, to a large degree, is due to the abbreviated reporting of the results in Chapter 7 of the report. To remedy this we have added a new Appendix Q that gives detailed results for all the calculated parameters, including wall thicknesses, for the base case and the most limiting parametric case examined. Also, following Eq. (6.6), we have indicated that the wall thermal conductivity used is the effective value accounting for temperature dependence. Because in the case of inner wall melting the inner and outer temperatures are fixed, it is possible to come up with a single effective value, applicable to all calculations. This is true approximately only when there is no melting, but these are uninteresting regions from the point of view of potential failure.

Tur24. *The model described in this chapter is suitable for the purpose envisaged. If it were to be developed further, the radiation sink(s) should be treated in a more discretised manner as the temperature will vary with distance from the debris.*

* *

Physico-Chemical Aspects

Chu17. *III. Comments on Metal/Oxide Phase Separation*

According to the analyses in the report, the location with the least thermal margin is near the equator of the hemisphere. The main reason for this behavior is due to the steel layer floating on top of the oxide melt. However, according to an analysis by Dana Powers (Dana Powers, "Chemical Phenomena and Fission Product Behavior During Core Debris/Concrete Interactions, Proceedings of the Committee on the Safety of Nuclear Installations (CSNI) Specialists' Meeting on Core Debris-Concrete Interactions, NP-5054-SR, Compiled by R.L. Ritzman, EPRI, September 3–5, 1986), the presence of metallic zirconium can lead to the formation of uranium metal and resulting in a denser metal phase. An experiment by Park et al. is quoted in the paper to illustrate this possibility. Since phase separation is associated with the location of least margin, the authors may want to look into the possible existence of a heavier metal phase.

We have examined this mechanism and are skeptical that we can take credit for it. Even if it was operative there would be transient aspects associated with sufficient uranium getting dissolved, and then the crusts should be dealt with before one can see this metal sinking to the bottom. Also, it should be made clear that the reviewer's second sentence ("The main reason ... on top of the oxide melt") applies only to a couple of limiting parametric evaluations (Figs. 7.15 and 7.16). In the base case, as well as all other parametrics the least thermal margin is at high elevations, but still in regions in contact with the oxidic pool (Figs. 7.10 to 7.14).

Kre6. *My concern stems from concern about the validity of the decay heat value. The overall decay heat curve (that includes all nuclides) looks reasonable for a ∼2000 MW th reactor compared to what I am familiar with for higher power reactors. (The 2000 MW value is my guess for the AP600. The report is remiss in not giving the real value or the source of its decay power curve). The modification to account for the loss of volatiles could be in error. The correct procedure would be to remove the appropriate volatiles at the initial time and redo the ORIGIN-type calculation that includes the decay schemes to determine the evolution of decay heat versus time. I am concerned that the process used may underestimate the decay heat because the decay schemes may build in additional volatiles not correctly accounted for by the procedure and which would remain in the pool to contribute their decay heat.*

Actually, this is what was done (as explained under Highlights). The rated power of the AP600 is 1933 MW_t, and this was obtained from the AP600 SAR.

> **Kre7.** *In addition, core melt accidents do not necessarily release all the volatiles before the melt enters the lower head. Estimates I have seen range as low as 50% released for the Iodine and Cesium and as low as 10% for the Te and Sb. Generally, even some small amounts of the Xe and Kr are assumed to remain with the melt. The conservative approach would have been to retain some portion of the volatiles within the melt.*

Actually these numbers are consistent with what is obtained using the rate constants in Table 4. As noted under highlights only the Ba and Sr numbers would need to be revised downwards in light of present understanding, but their total contribution to the decay power is less than 0.4%. Everything else is state of the art.

> **Kre8.** *The report is remiss in not defining exactly what nuclides it considers to be volatiles and in not defining what fraction of these are assumed to be removed from the melt. This is all wrapped up in Figure 7.2 which, incidentally, looks suspect to me. I do not believe the fractional contribution of the non-volatiles approaches 1 immediately after shutdown.*

As can be seen in Table 4 no significant release is assumed to occur in the first minute or so, and this is why the decay power fraction in Figure 7.2, begins from 1, and starts decreasing only after ~1 to 2 minutes.

> **Lev17.** *3. The report considers only two phase diagrams: an uranium dioxide (UO_2) – Zr oxide (ZrO_2) phase diagram and an iron (Fe) – zirconium (Zr) phase diagram. According to NUREG/CR-5869, several Zr, stainless steel (SS), $U0_2$, and ZrO_2 eutectics were formed in melting experiments at Oak Ridge in 1987 (Nucl. Eng. Des., 121, 324-337, 1990) and they are listed in Table 18.3 in Attachment 1* [[please see original letter]] *taken from NUREG/CR-5869. Furthennore, there can be a large number of other material species involved as illustrated from Table 18.4 in Attachment 1 [please see original letter] for a BWR bottom pool. They come from the species present in stainless and control rod materials which are also present in the AP600. It is also worth noting from Table 18.3 in Attachment 1 that the Zr-SS eutectic has a melting temperature of 1723 K (150 K above the metallic melting point of iron-zirconium used in DOE/ID-1046). There is also a strong possibility for the formation of a Zr-SS-UO_2 eutectic with a melting point*

of 1873 K (300 K above the metallic melting point used in DOE/ID-1046). This eutectic has the added complication of being able to produce some initial heat generation. There is no question that the phase diagrams in the reactor case will be much more complicated than those in the presumed overstylized pool and the presence of additional eutectic mixtures with higher liquidus temperatures and their potential formation of solid particles must be recognized.

As with every real problem one can find complexities forever. The real question is: What is important to the conclusions? First, Table 18.4 is irrelevant, as our interest here is a PWR. Second, the Zr-SS eutectic cited indicates that we may be 150 K conservative in our treatment of the metallic layer. The impact is that for the limiting flux considered in Chapter 4 we would have a ~1 cm thicker wall. The margins to structural failure for the CHF failure criteria are so great that this increment is really of no consequence. The Zr-SS-UO_2 eutectic is irrelevant in the presence of the Zr-SS one. Thus, we find all these "complications" really of no interest to the problem at hand.

Lev32. *Also, as noted under comment C.3, stainless steel zirconium and $U0_2$ can form several eutectics with higher melting temperatures. With the anticipated weight percent of Zirconium (10 to 65 percent), it is not clear why the Zr-SS-UO_2 (0.3/0.6/0.1) eutectic would not play a dominant role and possibly produce a multilayered configuration.*

See above, response to item #17.

Lev33. *3. An important assumption made in DOE/ID-1046 is that the heat generation is uniform and confined to the oxidic pool. With the suggested stratification and temperature maldistribution discussed in comment C.2, it is anticipated that $U0_2$ will tend to favor the upwards portion of the pool and that the heat generation per unit volume could be much higher in that region. Also, note that the SS-Zr-UO_2, eutectic could be present in the metallic layer and provide some limited heat generation.*

This presupposes comment C.2, which is incorrect (see response to item #16). A sensitivity on the fraction of decay heat deposited directly into the metal layer can be found in Appendix P.

Ola1. I Chemical Phenomena

(a) *Reaction of the metallic melt with steam*

The report clearly indicates (pp 1-1 and 1-3) that the cavity above the metal pool is filled with steam. The metallic melt contains ~50% of the core's Zr in elemental form. It is impossible for steam and zirconium to remain unreacted during hours of contact at temperatures of ~1600 K. Reaction of steam and zirconium was responsible for the development of the accident in the first place. In the metallurgical industry, addition of small quantities of Zr to molten steel during the steelmaking process is used as a deoxidizing procedure. Contrary to oxidation of solid Zry, buildup of a coherent ZrO_2 layer on the upper surface of the metal pool is unlikely because the substrate is a liquid in turbulent flow.

The kinetics of steam reaction with Zr in the Fe-Zr liquid alloy is not known. It is probably very rapid because of the absence of a protective oxide scale. A conservatively high estimate of the reaction rate (and the corresponding heat release rate) can be made by assuming complete conversion of steam to hydrogen at the surface with the overall rate controlled by mass transfer in the gas phase adjacent to the pool surface. Mass transfer is by natural convection, driven by both the unstable temperature gradient and by the reduction of the gas density at the surface that accompanies conversion of H_20 to H_2. Using the Sherwood number in place of the Nusselt number for the turbulent natural convection correlation for heated plates facing upward, the mass transfer coefficient is given by:

$$k_g = 0.14D\left[\frac{g(\Delta\rho/\rho_f)}{\nu^2}Sc\right]^{1/3} \tag{1}$$

where, for an ideal gas,

$$\frac{\Delta\rho}{\rho_f} = \frac{\Delta T}{T_f} + \frac{\Delta M}{M_f} \tag{2}$$

$\Delta T = T_{1,0} - T_{b,g}$ *and* $T_{b,g}$ *is the bulk steam temperature, taken as 1000 K.* $\Delta M = M_w - M_H$ *is the difference in the molecular weights of water and* H_2. T_f *and* M_f *are the mean values of these two properties. With these values,* $\Delta\rho/\rho_f \sim 2$.

D *is the diffusion coefficient of the* H_2O/H_2 *system. It is calculated from the correlation given in the appendix of Ref. 2 to be ~11* cm^2/s *at* $T_f = 1300$ *K and a total pressure of 1 atm. The viscosity of a 50 mole % steam-hydrogen mixture* *at T_f is $\sim 4 \times 10^{-4}$ g/cm-s and the mass density of this mixture is $\sim 8 \times 10^{-5}$ g/cm^3. Substituting these values into Eq (1) gives $k_g \sim 5$ cm/s.*

The flux of water vapor to the upper surface of the metal layer is:

$$J = k_g S_{up} (\rho_f / M_f)(y_{b,g} - y_{surf}) \tag{3}$$

For an oxide pool volume of 10 m^3, $S_{up} = 12$ m$^2 \cdot y_{b,g}$ is the mole fraction of steam in the bulk gas and y_{surf} is the value in the steam at the surface. These are taken as 1 and 0, respectively. Equation (3) gives a water vapor flux to the surface of ~5 moles/s. At this rate, all of the Zr in the Fe-Zr alloy pool is consumed in ~12 hours (assuming 50% of available Zr in the metal pool).

The heat released by the steam-metal reaction is calculated from the enthalpies of formation of ZrO_2 and $H_20(g)$ (Ref. 3, Appendix) to be 293 kJ/mole H_20. The chemical heat release at the surface of the metal pool is $5 \times 293 \times 10^{-3} = 1.5$ MW. This is a significant addition to the ~13 MW from decay heat in the oxide pool. The metal layer surface heat source due to chemical reaction is ~120 kW/m^2.

The implication made here can be discounted at three levels.

(i) Inconsistency in steam supply. It is not possible to remove 5 moles/s of hydrogen from the boundary layer (as the reviewer does), if the bulk concentration is zero (as the reviewer assumes). In fact, any continuing supply of steam, as assumed, has to come from the containment atmosphere, together with *a lot of air*! The $\Delta M/M_f$ in Eq. (2) would then be significantly off, and the driving force $(y_{b,g} - y_{surf})$ could be overestimated in Eq. (3) by a factor of 3 or more.

(ii) A good way to have an unlimited supply of steam to the melt is through water addition. But then we have to consider also enhanced heat transfer due to film boiling, and much enhanced radiation loss from the melt due to the increase of emissivity (of the oxide). These by far would outweigh any reasonable chemical energy source and, in fact, even the 120 kW/m^2 number proposed by the reviewer.

(iii) Finally, it is interesting to consider the impact (actually non-impact) of the reviewer's number, even without the two exceptions outlined above. The total decay power considered is 14 MW, and of it ~7 MW goes into the metal layer, so that the 1.5 MW would represent an increase by ~20%. However, this behavior would be accompanied by a substantial increase in emissivity. Using a value of 0.8, as suggested by the review in the next point, we find that the net effect is to increase the sideward heat flux by only 4%. This can also be determined by using Figure 5.11.

Ola3. *(c) Extraction of uranium from the oxidic pool by the metal alloy*

It is well established that molten cladding dissolves UO_2 pellets to produce a melt that contains up to 40 wt% uranium on an oxygen-free basis(4). Therefore, the elemental Zr in the metal pool should also extract uranium from the oxidic pool. The melts from the TMI 2 core contained small quantities of uranium(5). This process will reduce the eutectic temperature of the metal pool from that of the Fe-Zr binary to that of the U-Fe-Zr ternary alloy. A pseudo-binary phase diagram of this alloy can be approximated by averaging the Fe-Zr and Fe-U phase diagrams.

The extraction process considered here would require extensive contact between liquid cladding and UO_2. Here we have a predominantly stainless steel melt, with some Zr dissolved in it, in contact with solid UO_2. The eutectic temperature of such a system (0.3 – 0.6 – 0.1 mole fractions) is 1873 K or ~250 K *higher* than the temperature used in our calculation to attack the vessel wall in contact with the metallic layer. On the other hand, a Zr stainless steel eutectic (0.193 – 0.807 mole fraction) is found at 1723 K, still 100 K higher than the temperature used in the report. Recognizing the huge margins in the structural evaluation, and the conservative choice of the eutectic temperature, further elaboration in this area is not considered to be fruitful.

Ola4. *(d) Vessel wall melting temperature*

The report used the eutectic temperature of the Fe-Zr binary for the melting temperature of the wall ($T_{l,m} = 1335°C$). This is correct only if the melt composition is $x_{Zr} = 0.088$ mole fraction zirconium. For $x_{Zr} \neq 0.088$, the appropriate value for $T_{l,m}$. is the liquidus temperature in the phase diagram shown in Fig. 6.1 of the report. For $x_{Zr} \leq 0.088$, this can be approximated by:

$$T_{l,m}(°C) = 1536 - 2284 x_{Zr} \tag{4}$$

The steady-state heat flux balance at the metal melt-vessel wall interface is:

$$h(T_b - T_{l,m} = k(T_{l,m} - T^{**})/\delta_s \tag{5}$$

where $h = A(T_b - T_{l,m})^{1/3}$ [Eq(5.41)] with A given by Eq(5.47) and δ_s is the thickness of the vessel wall adjacent to the metal layer. It is in general not equal *to the as-fabricated value ($\delta_{s0} = 5$ cm) because iron may precipitate on the wall or the wall may dissolve in the liquid to give a thickness that satisfies Eq(5) for the specified value of T_b. The wall thickness relative to the as-fabricated value calculated from Eq(5) is:*

$$\frac{\delta_s}{\delta_{s0}} = \frac{T_{l,m} - T^{**}}{T_b - T_{l,m}} \frac{1}{Bi} \tag{6}$$

where

$$Bi = h\delta_{s0}/k \tag{7}$$

is the Biot number. Using the value $A = 2764$ given in the example on p. 5-19 of the report, $\delta_{s0} = 0.05$ m, and $k = 25$ W/K-m(Table 7.1):

$$Bi = 5.5(T_b - T_{l,m})^{1/3}$$

and Eq(6) is:

$$\frac{\delta_s}{\delta_{s0}} = 0.18 \frac{T_{l,m} - T^{**}}{(T_b - T_{l,m})^{4/3}} \tag{8}$$

An example of this effect is given in Table 1 using the bulk metal temperature given in the example on p. 5-19 of the report ($T_b = 1405$ °C) and $T^{**} = 100$ °C.

Table 1 Thickness of vessel wall opposite metal layer

x_{Zr}	$T_{l,m}$ (°C)	δ_s (cm)
0.05	1421	*
0.065	1387	25
0.088	1335	4

**bulk temperature is less than the liquidus temperature; Fe-Zr cannot exist as a single-phase liquid*

The table shows that the wall thickness is very sensitive to the mole fraction of Zr in the metal melt. In the model developed in the report, Eq(4) above should be used for $T_{l,m}$ in the last term of Eqs(5.42) and (6.9). Equation(5) above needs to be added to the set of equations to determine the vessel wall thickness.

If $x_{Zr} > 0.088$, the phase that precipitates on the wall is Fe_2Zr and Eq(4) is replaced by the liquidus joining the eutectic point and the melting point of Fe_2Zr in Fig. 6.1 of the report.

First of all, the as-fabricated value of δ_{s0} is *not* 5 cm, but rather 15 cm. This is explained clearly in the report. Second, we used the eutectic because such composition is conservative, and the subject (as described above) is not worthy of extensive elaboration, especially recognizing that the composition cannot be specified with any degree of accuracy.

Ola5. *(e)* *<u>Melting temperature of the oxidic pool</u>*

The melting temperature of the oxidic pool given in Table 7.1 of the report is too high. Because of the addition of transition-metal oxides to the ceramic melt, a melting temperature of ~2700 K is suggested (p. 84 of Ref. 5). Other investigators suggest that the high-melting ceramic may flow as a solid carried like a slurry in the molten spinel (ref. 5, p. 187 and ref. 6). The spinel is $Fe(A\ell,Cr,Ni,Zr)_2O_4$, and may be present at levels as high as 10% in the oxidic material. The oxidic pool may not be a single phase liquid as assumed in the report (see also bottom of p. 5).

The melting temperature of the oxide in Table 7.1 is 2973 K. The reviewer prefers 2700 K. Actually, the absolute temperature level, within a few hundred degrees, does not matter at all. What is important is the melt superheat, and the correct use of properties for the superheated melt (Appendix L). The superheat is obtained from the energy balance, i.e., the melt will superheat sufficiently to allow the boundary fluxes, with the appropriate heat transfer coefficients to just balance the decay power source in the volume.

Ola6. *(f)* *<u>Location of the decay heat source</u>*

The report assumes that the decay heat source is where the uranium is. However, the decay heat is due to the fission products, not the uranium. This fact was partially recognized by the authors of the report when they allowed for loss of volatile fission products (they need to state which fission products are volatile). However, a significant fraction of the fission products may be present in the metal layer. The presence of the noble metals (Ru,Rh, Pd) in the metallic phases of the TMI-2 core debris has been verified (Ref. 5, p. 91). Te is likely to follow elemental Zr in the metal layer. Zr fission product will distribute in the same manner as the structural Zr. Some Cs is found in the debris. The oxides of Mo have higher standard free energies of formation than $U0_2$ or ZrO_2 and Mo probably is more stable in the metal phase(6). Table 2 shows a possible partitioning of all fission products in one of three locations: volatilized and escaped; retained in the oxide; dissolved in the Fe-Zr metal layer.

Table 2 Distribution of fission products in core debris			
Fission product	*Released*	*inoxide pool*	*inmetal layer*
*Zr,Nb**	*0*	*0.15*	*0.15*
Mo	*0*	*0*	*0.24*
noble metals	*0*	*0*	*0.25*
Cs	*0.15*	*0.04*	*0.04*
rare earths	*0*	*0.53*	*0*
Ba,Sr	*0*	*0.15*	*0*
Xe,Kr	*0.25*	*0*	*0*
others+	*0.03*	*0*	*0.01*
Total	*0.43*	*0.87*	*0.70*

* *assuming 50% of Zry from core in each phase*
\+ *Te in metal phase*

The sum of the numbers in the row for each fission product group is the elemental yield of this group from fission. The sum of the Total row is 2.

If the total fission product decay heat source is 13 MW, the above table suggests that it is divided into 7.2 MW in the oxidic pool and 5.8 MW in the metal layer. This heat source in the metal layer should be considered in the report's model.

Using the reviewer's numbers the thermal load in the steel layer would be ~10 MW, as compared to 7.41 in our calculations; that is an increase of ~25%. The impact of this increase can be seen directly from Figure 5.11 (using approximately the extra power to increase q_{up}). The H_ℓ/R of interest here is ~0.5, the corresponding line is "flat" and therefore the wall thermal loading should increase by the same amount, i.e., 25%. Referring then to Figure 7.10, we find that the critical flux ratio in the metal layer increases from 0.4 to 0.5; that is, it reaches the peak value found (previously) in the oxidic pool. Now, of course, the oxidic pool is by comparison only slightly loaded. The margin to failure is still 100%! See also parametric evaluations, carried out with the full model, in Appendix P.

Having said that, we do not agree that in a meltdown scenario there is sufficient opportunity and contact for the "extraction" process contemplated by the reviewer here. Especially, we consider it even more unlikely that this is possible to occur in a transient state with a small quantity of steel in the metal layer. Further perspectives on this topic are provided in Appendix R.

Seh6. *Another point is that a fraction of the Zirconium metal released may be in the form of U-Zr eutectic, which may generate some decay heat in the metallic layer.*

See Appendix P, Olander's item #6 and our response, and Appendix R. Moreover, in Appendix O, we provide what we believe to be a reasonable evaluation.

Sei6. *II-3) Physico-chemical reactions between the metallic pool and the vessel may lead, potentially, to low interfacial temperatures with the vessel wall in the metal pool layer. This may increase the lateral heat flux when there is no metallic crust formed at the surface of the pool or in the presence of a thermal resistance at the surface of this pool. It may perhaps be argued that the interface temperature is not expected to drop below 1500°K (which is considered as boundary condition for the calculations) considering that the mole fraction of Zr in the metallic layer does not exceed 50% (according to phase diagram presented in fig 6.1).*

Yes. This is how we picked the 1335 °C eutectic, as explained on page 6-1.

Spe7. *The report does not address the likely length of time that pool natural convection cooling would be relied upon if this regime were entered in an accident. It could be days or even weeks. The IVR assessment has included structural and thermal loads assessments, but the treatment of chemical processes which may effect head integrity over prolonged time is treated minimally.*

> *[6. A through examination of interfacial chemical processes should be undertaken involving not only the Fe/Zr mixture but also including other potential constituents of the corium including absorber materials, control rod materials, and fission products to address any possible chemical-related attack on the wall integrity at the temperatures and time duration of interest.]*

We do not see the concern about time frames for chemical attack and thus do not concur that tests such as these "should be undertaken". We use equilibrium thermodynamics that presuppose "infinite" contact anyway. However, the demonstrations, along the lines offered originally in Appendix J, have been expanded and are presented in an addendum to it.

Tur7. *The authors assume (page 5-2) that the crusts impose a uniform temperature boundary condition at the melt liquidus. Elsewhere (eg the CORCON code) it is assumed that the melt solidus temperature provides the external boundary* *condition for the melt pool. Evidence from the ACE Phase C experiments and the associated determinations of liquidus and solidus indicate that 'melt' temperatures beneath the liquidus are possible. Further the equations for crust growth are consistent with the solidus assumption, with the deposited crust having the composition associated with the solidus and a concentration gradient in the melt phase close to the crust. In some circumstances compositions in this region may be such as to give nucleation in the boundary region (or encourage the growth of dendrites). Unless the zirconium is fully oxidised, allowing oxides of iron to become part of the oxidic layer, this question is largely academic as far as the crust is concerned — both the solidus and liquidus will be significantly above the steel melting temperature. However, equilibrium phase diagrams indicate that, for the compositions anticipated, UO_2 will preferentially be deposited in the crust and that UO_2 should precipitate near the cool boundaries leaving a liquid richer in the less dense ZrO_2. Should this happen, convection, which depends on local density differences might be modified. This effect was demonstrated in simulant experiments at low Rayleigh numbers [S B Schneider and B D Turland: Experiments on Convection and Solidification in a Binary System in 'Proc. Workshop on Large Molten Pool heat Transfer, Grenoble, March 1994; NEA/CSNI/R(94)11], but was found to disappear for the simulants used at much lower Rayleigh numbers than those expected in a reactor melt pool. Confirmatory experiments with real materials are desirable, and should be performed as part of the OECD/Russian RASPLAV project. A paragraph should be added discussing possible multicomponent effects on natural convection.*

Again, this is a point well taken in the spirit of completeness. Basic molecular diffusion consideration lead to the conclusion that such multicomponent effects could not affect natural convection in any significant way, under the turbulent flow conditions at the high Rayleigh numbers of interest here. We feel this was amply demonstrated by Schneider and Turland (1994) and there is no need for confirmation by prototypic material tests. The reference has been added to the report.

Tur21. *Chapter 6*

In practice a significant amount of the decay heat may be generated in the metallic layer (see page 6-1), if metallic fission products are able to migrate there. However, I do not expect this to affect the conclusions drawn by the authors.

⋆ ⋆

Tur22. *As noted above, the solidus is appropriate for considerations of crust behaviour (an effective solidus, based on the temperature at which more than, say,* *40% of the debris is liquid may be appropriate for melt convection considerations). For the metal layer, the implication of the phase diagram (Fig 6.1) is that attack on the steel wall may be possible at temperatures below the steel melting point, depending on the mole fraction of Zr.*

* *

Input Quantifications and Sensitivities

Kre2. *1. Quantity of Melt and 2. Composition:*

The analysis included all of the oxidic core, all of the Zr available, the lower support plate, the reflector, the lower supports, and some portion of the core barrel. The fraction of Zr oxidized was treated probabilistically in three ranges:

– most likely range...	*.4 to .6*	*(probability of P)*
– unlikely range...	*.6 to .7*	*(P/10)*
– highly unlikely range.	*.7 to .9*	*(P/100).*

Comments:

The greater the quantity of ZrO_2 added to the melt, the more dilution effect you will have (that is, you will reduce the effective volumetric heat generation rate). In addition, putting more of the Zr into the melt as the oxide reduces the thickness of the metallic layer overlying the fuel melt. Thus, I would expect higher values of ZrO_2 fraction to be non-conservative with respect to this problem.

I think the probability density function for the fraction of Zr oxidized should have included some relatively high probability that it would be less than .4.

As explained, we expect that Figure 7.3 is already quite conservative. The sensitivity presented in Figure 7.11 (compared to Figure 7.10) shows that shifting the distribution to the right by 10% has an essentially negligible effect. The nature of the physics is such that the effect of the ZrO_2 quantity is continuous, hence we would expect a similarly low sensitivity if we were to shift the distribution to the left. We confirmed this by carrying out the calculation, and the result is shown in Appendix P. Moreover, we provide additional perspective on this item, by comparing the results for two point values (probability of 1) set at 0.3 and 0.4, also shown in Appendix P. Note that increasing the metal layer thickness reduced the peak heat flux in that region.

Kre3. *Similarly, when one adds the amount of steel in the lower support plate, the reflector, and the lower supports one gets a total of 77 tons without adding in any of the core barrel. I would have expected to see the "likely probability" range for the steel mass in Figure 7.5 to extend upwards to beyond 80 tons instead of the 72 tons shown.*

As explained in the report, Figure 7.5 was specified so as to represent a conservative distribution. By extending the distribution to higher values, as suggested by the reviewer, we actually diminish the thermal loads. But the results show clearly that the metal layer is not limiting (it is thick enough), so increasing the quantity further would produce a slight and inconsequential decrease also.

Kre4. *With the ROAAM procedure, I worry about cliff effects. An abrupt and severe change in the probability between ranges could mask a strong sensitivity in the region near the abrupt change. Because of the focussing effect of the metallic layer, the content of steel might be such an area to expect such a strong sensitivity.*

One needs always, with or without ROAAM, to worry about cliff effects. This is why we run extensive parametrics, and are open to reviewers' suggestions for even more. It is important to understand, however, that this is (should) not be a random exercise, but rather guided by the physics of the situation, and the results obtained already—as illustrated, for example, in the previous two questions. For example, we explored the focusing effect and its asymptotic implications in general (see Chapter 6), before finalizing the parameter ranges and sensitivities considered, to ensure that no credible near-cliff was overlooked. This sort of approach is essential to the proper application of the ROAAM process. Here it is suggested by the reviewer that because of the focusing effect, the steel content might present a strong sensitivity. In fact, at the end of Chapter 5, we provided a detailed evaluation of the point (i.e., at what metal depth the focusing effect sets in), and even provided general quantitative results in an easy-to-use graphical form. These, and the results in Chapter 7, show that in AP600, we are far from conditions where focusing concerns could arise. Further, focusing cannot be obtained by adding steel, as suggested by the previous question. Moreover, even when a condition leading to focusing was *contrived* (Figure 7.16), failure could not be obtained. See also Appendix O.

Kre5. *3. Decay Heat Level (volumetric heat generation rate):*

The report chose to look at a bounding sequence (3BE) as being "of main interest to IVR". According to the MAAP code, this sequence gives the fuel melt in the vessel bottom head at about 4 hours after shutdown. To get the decay heat level at that time, the procedure was to multiply the total decay heat by the fractional contribution due to the non-volatile fission products.

Comments:

I have some concerns about the above procedure. The choice of bounding sequence appears to be well founded. I would not be comfortable, however, in relying on only one codes calculation to determine the timing. I recognize that

Figure 7.12 results from shifting this timing to one hour sooner and that this is an appropriate manner to address the sensitivity to this. Nevertheless, I see some strong sensitivity in the calculated $q_w(\theta)/q_{chf}(\theta)$ to this shift although the decay heat increase was small.

Actually, we do not know where the reviewer sees the "strong sensitivity." Careful examination of Figure 7.1 will show that the increase in decay heat is ~10%, while Figure 7.12 shows an increase in thermal load by 10% also! Again, from the physics we can expect this proportional dependence rather than a "strong sensitivity."

Kre6. *My concern stems from concern about the validity of the decay heat value. The overall decay heat curve (that includes all nuclides) looks reasonable for a ~2000 MW th reactor compared to what I am familiar with for higher power reactors. (The 2000 MW value is my guess for the AP600. The report is remiss in not giving the real value or the source of its decay power curve). The modification to account for the loss of volatiles could be in error. The correct procedure would be to remove the appropriate volatiles at the initial time and redo the ORIGIN-type calculation that includes the decay schemes to determine the evolution of decay heat versus time. I am concerned that the process used may underestimate the decay heat because the decay schemes may build in additional volatiles not correctly accounted for by the procedure and which would remain in the pool to contribute their decay heat.*

Actually, this is what was done (as explained under Highlights). The rated power of the AP600 is 1933 MW_t, and this was obtained from the AP600 SAR.

Kre7. *In addition, core melt accidents do not necessarily release all the volatiles before the melt enters the lower head. Estimates I have seen range as low as 50% released for the Iodine and Cesium and as low as 10% for the Te and Sb. Generally, even some small amounts of the Xe and Kr are assumed to remain with the melt. The conservative approach would have been to retain some portion of the volatiles within the melt.*

As noted under highlights only the Ba and Sr numbers would need to be revised downwards in light of present understanding, but their total contribution to the decay power is less than 0.4%. Everything else is state of the art.

Kre8. *The report is remiss in not defining exactly what nuclides it considers to be volatiles and in not defining what fraction of these are assumed to be removed from the melt. This is all wrapped up in Figure 7.2 which, incidentally, looks suspect to me. I do not believe the fractional contribution of the non-volatiles approaches 1 immediately after shutdown.*

As can be seen in Table 4 no significant release is assumed to occur in the first minute or so, and this is why the decay power fraction in Figure 7.2, begins from 1, and starts decreasing only after $\sim$1 to 2 minutes.

Kre16. *5. Radiation Off Top Surface:*

Radiation off the top surface of the metallic layer was treated in a standard manner that includes back radiation from the sink which was given a single constant temperature (to be solved for from the equations that include the total heat upward through the top surface, radiation, conduction through the heat sink, radiation off the back side of the heat sink to the vessel internal wall, and conduction through the vessel wall essentially to the water temperature). An emissivity of .45 was used and a sensitivity analysis was done for higher emissivity values.

Comments:

The procedure used is appropriate and acceptable. Nevertheless, I would have liked for the sensitivity study to have included lower emissivity values if only as an artificial means to try to enhance the "focussing" effect. I don't know whether or not the metallic layer has a crust on the top surface. A newly formed frozen layer of metal may have a low emissivity value.

The parametrics in Figures 7.13–7.15 were run to provide further perspectives on the effect of additional resistances on top of the metallic layer. The adiabatic case corresponds to an emissivity of zero. All three cases show that the effect is negligible. Hence it does not appear worthwhile to explore in-between values. Moreover, it is well known, and our data demonstrate that a solid surface, even if just-solidified, has much higher emissivity values than the liquid. We do not think that it is appropriate to use emissivities lower than 0.45 in the extreme (and purely hypothetical) case of Figure 7.16. However, to fully respond to this question, we have run this case with an emissivity of 0.35. The results are shown in Appendix P, together with all other additional parametrics. We find that the peak flux increases by only 10%, and with the new critical heat flux correlation specialized for this highly peaked shape we do not obtain failure. It should also be kept in mind that in this highly contrived case, as well as for the case of Figure 7.16 in the report we used a decay power of 1.4 M/m^3, which as we can see in Figure 7.8, is truly the upper bound. Since there was so much interest in this case, we have run, for it, a more "full" calculation, using the decay power as a parameter. The results are shown in Appendix P.

Kre18. *7. Integration to Determine Resultant Wall Temperatures, Wall Thicknesses, Loads, and the Ability to Carry the Loads:*

Mostly, deterministic calculations were used. However, the ROAAM procedure was used with assigned probability distributions for

- *decay power*
- *quantity of Zr oxidized*
- *quantity of steel in metallic layer, and*

some sensitivity studies were also made.

Comments:

I commented earlier on the probability ranges for the above parameters. I also believe the sensitivity studies should have included variations in the opposite directions to those made. For example,

- *a lower value of emissivity*
- *an overprediction of the downward heat flux (rather than Mayinger's correlation which underpredicts the downward heat flux)*
- *a shift of the fraction of Zr oxidized to the left rather than to the right.*

All of these were responded to above. None was found to have a significant impact on the conclusions.

Kre21. *The defense of the case, in my mind, strongly rests on justifying the choice of decay heat value. The comments I made earlier in this review on the content of volatiles, the timing, and the appropriate modification of the curve for loss of volatiles are very important.*

See General Comment and Highlights.

Lev8. *7. In view of the preceding comments, significant degradation in the CHF values of Figure 3.3 are anticipated (possibly by as high a factor as 2 to 3). It is* *remarkable, therefore, that no sensitivity study of this important parameter was included in Section 7.3 and it is recommended that it be added.*

As described above, any reduction of CHF from those found in ULPU are unfounded, and no such sensitivity studies are warranted.

Ola2. *(b) Metal pool emissivity*

The report takes the emissivity of the upper surface of the metal pool to be 0.45, which is reasonable for a clean metal surface. This value was measured by the experiment described in Appendix I of the report. However, if steam had been mixed with the pure argon used in this experiment, the surface of the Fe-Zr liquid would have been oxidized and the emissivity would probably have been ~0.8. In the model, this would have increased the radiant heat loss from the pool upper surface and reduced the heat flux to the vessel wall. Credit should be taken for this reduction.

Yes, to be consistent we must, as described above.

Seh4. *The authors have performed an excellent job on defining the thermal loading on the internal surface of the vessel, however, the situation is not as clean, as it is, for the heat removal on the external surface of the bottom head. The basic misgiving, in my mind, is that the authors have assumed an end state of the melt pool and, thereby, an independence from the core melt scenario, which ignores the intermediate and the transient states, which may impose greater thermal loading on the vessel inner surface. I accept the authors argument that the thermal loading due to a purely oxidic pool would be scenario independent. However, when a metallic layer on top of the oxidic pool provides "focusing"; the authors, themselves, have identified an intermediate state with a 1.18 meter deep oxidic pool and a 0.22 meter metallic layer, which results in larger thermal loading than the assumed end states of the oxidic pool (1. 5 m to 1.6 m depth) and the metallic layer (0.9 to 1.0 m high).*

Actually, this case was identified as "arbitrary parametric" and was used only to provide some perspective on the extreme limit of the focusing effect. Moreover, even at this extreme, the actual margin is more than illustrated in Figure 7.16. In any case, more consideration to transient scenarios can now be found in a new Appendix O.

Sei4. II) Stratified Pool situation:

II-1) A presence fraction of more than 50% in mass of ZrO2 in the oxydic phase would inverse the stratification if the metal is mainly Stainless Steel. This would correspond to 100% oxidation of Zr and less than 30 tons of molten U02 (less than 40% of core inventory). Has this situation to be considered ?

No. 100% oxidation is not credible, and in combination with only 30 tons (out of 75) of UO_2 in the lower plenum, is impossible. See Appendix O.

Sei8. *II-5) Presence of aerosols may decrease the heat transfer by radiation from the pool surface. But this is bounded by the adiabatic conditions.*

Yes, and moreover, natural convection and precipitation mechanisms would help keep the atmosphere clear.

Tuo3. *The influence of the metallic layer*

The problem definition in Chapter 2 defines that the thermal load to the lower head is maximized when the debris pool has reached a steady state, the heat generating debris volume has been maximized and the thermal resistance along the upward thermal radiation path has been maximized. Maximizing the debris volume creates some confusion with the "focusing effect" of the metallic layer. The most significant parameter by far is the height of the metallic layer on the top of the oxidic pool. As an extreme parametric study of Chapter 7 demonstrates, the limiting case presupposes only partial relocation of the oxidic part. Could the partial relocation cases make a nonnegligible increase in the failure risk?

The extreme parametric case considered in Chapter 7 is bounding because it uses the minimum amount of steel possible and the maximum amount of oxide that can physically exist with it. More oxide will produce contact with the lower support plate and a melt-in process that would drastically increase the metal layer thickness, while less oxide would produce, clearly, lower thermal loads. Even this extreme case cannot quite produce failure, and the margin is somewhat greater than shown in Figure 7.16. This is because the very sharp local peaking is combined with much lower upstream fluxes (or vapor flow), a situation that according to new ULPU data yields higher critical heat fluxes. These data, which were obtained with flux shapes appropriate to the parametric and sensitivity studies are summarized in Appendix E.3. In any case, some further consideration of partial relocation is given in Appendix O.

Tur25. *Chapter 7*

The use of the whole UO_2 inventory may not be bounding. If only 80% of the core relocates (eg leaves remnants of low rated assemblies) then the oxidic pool will be beneath the lower support plate, the metal layer may be thinner and the focusing effect more pronounced (particularly as the surroundings will be close to the melting point of steel). Such a configuration could occur before a final 'equilibrium' state is reached. In this configuration smaller amounts of metal are possible.

This is the case actually examined as a parametric. The likelihood for it is really a matter of judgment, especially since the final stages of core relocation cannot be defined with any degree of confidence. The dominant factor against focusing, as explained already, is the large steel inventory in the lower internals and the reflector. We do not believe it is possible to have more than, say, 60% of the core relocated without having melted most, if not all, of the reflector. It is on this basis that we call the particular parametric "extreme." Actually, the margins for this case are greater than shown in Figure 7.16. The reason is that with such local peaking the CHF is greater than that used in Figure 7.16. These new results can be found in Appendix E.3. More on the "focusing" problem can be found in the new Appendix O.

Tur26. *The ranges for the amount of metal involved seem rather narrow, and the text does not seem that consistent; the first part of the second paragraph (on page 7-5) implies that 105 tons of metal are expected, whilst this is way out of range of the probability distribution, presumably as all the core barrel is not expected (is the reflector attached to the core barrel?). Molten metal from the lower head should also be included. I would make this distribution broader in both directions. This would broaden the distribution for the height of the metallic layer (Fig 7.6).*

No, the reflector is not attached to the core barrel; it rests on the core support plate which is hung from the core barrel (with secondary support from the lower head). Perhaps our expression about the core barrel mass was not clear enough. We refer to the "lowest portions" only, while the *total* mass is 40 tons. Thus the quantity of melt expected would be somewhat more than 67 tons, the "more" depending on what fraction of the 40 tons is to be considered. By comparison to these quantities the amount supplied by lower head melting would be indeed negligible. So we do not find any real reason to broaden the distribution of the melt mass.

Tur27. *It would be useful to have Sienicki's material in an Appendix. However, the timings seem reasonable.*

* *

Tur28. *The statement (page 7-11) that 'the zirconium oxidation is clearly quite independent of* τ' *is rhetoric. I would only say there is no obvious correlation.*

We disagree on this point, and our reasoning has been stated. If the independence is questioned, it puts in question the calculation procedure. If we accept that a correlation exists, but we do not know what it is, the problem should be considered as a splinter, and the treatment should afford the most adverse type of dependence.

Tur29. *I do not regard "the limits to failure" case (page 7-16) to be as 'extreme' as the authors do. However, the results are encouraging, when allowance is made for likely lateral temperature gradients in the (relatively) thin metal layer.*

This was discussed above, item #25.

Tur30. *I note that no uncertainty has been allowed in the application of the ULPU critical heat flux data.*

Actually this was discussed near the bottom of the text in Figure 3.2 (page 3-5): "From an evaluation of uncertainties, as discussed in Appendix E, we expect this result to be good within a few percent, so that CHF can be excluded outside a rather narrow range around it." We did not elaborate on this point in the calculations (Ch. 7) themselves, and the presentation of results because the wide margins shown dwarf completely such effects. With the much larger data base now available from ULPU (new Appendix E.3), the accuracy and realistic-lower-bound nature of it is made much more clear.

Jet Impingement Problem

Eps3. *(2) I think the authors can provide a more convincing jet impingement analysis (argument) than the one presented In Section 8. In particular, I believe more information is needed to justify the lower bound jet diameter of 10 cm. It seems to me that a breach on the core-side boundary may first appear as a small opening (pin-hole or crack). Thus the early stages of the core draining process may occur via a narrow, high-impingement heat-transfer jet. Of course, the jet heat flux will decrease with time owing to the enlargement of the breach. An analysis of this process should appear in Section 8, and apparently such an analysis is available (Sienicki, 1995). More detail regarding Turland's (1994) work should also be included. In other words, all the available arguments that put the jet impingement issue to rest should be spelled out in Section 8. also, something should be said about the unlikelihood of molten metal jet impingement during core relocation.*

See new Appendix O.

Eps5. *(4) Is there any experimental data that supports the last sentence of the paragraph that follows Eq. (H.8) in Appendix H (page H-6)? I believe that this sentence should read "when the stream diameter becomes sufficiently small compared to the boundary layer thickness ahead of the ablation front ...". It would seem to me that the head thickness is not an important parameter with respect to the erosion rate, as long as melt is removed from the cavity formed by the jet as the jet erosion process proceeds.*

But this is the point. The melt is *not*completely removed from the cavity (see Saito et al., (1990).

Kre17. *For the "thermal jets" issue, the use of only 1/3 of the fuel volume and a jet diameter of 10 cm need better justification. Figure 8.1 shows that even with $V_r = 1/3$ of the fuel volume and $D = 10$ cm, you get a total ablation depth of 12.5 cm – perilously close to the wall thickness of 15.24 cm. It doesn't take much more fuel or a much smaller jet diameter to ablate through.*

The calculations in Chapter 8 and in Appendix H were done at such extreme conditions that it is not appropriate to take, as the reviewer does, the results as something that might actually happen. To leave 3 cm of metal under such a calculation we interpret as comfortable margins to failure, rather the "perilously close" of the reviewer. In reality it is extremely unlikely that (a) 1/3 of the core can come out as a coherent pour, and (b) remain as a single release point with a diameter as small as 10 cm. Moreover, the calculation assumed a normal impingement and a continuous removal of both melts. A more detailed analysis is now provided as an addendum to Chapter 8.

> **Lev13.** *4. There is no reason not to expect the partial melting material configuration depicted in Figure 2.1 to progress to that of the complete meltdown of Figure 2.2. If this is the case, the melt impingement produced by partial meltdown could erode the reactor vessel steel by as much as 12 to 14 centimeters (see page H-7). The corresponding weakening of the reactor vessel is not considered in the Structural Section 4.*

Again, materials from different parts of the report are taken, and combined, out of context. The assessments in Chapter 8 and Appendix H are for massive releases of already accumulated melt quantities. This applies to the initial relocation event following meltthrough of the core reflector and core barrel. The configuration in Figure 2.1 will evolve to that of Figure 2.2, of course (this is why we put it there!), if the accident is allowed to proceed without water addition (as assumed here); however, as explained already, any subsequent relocations will be gradual and subject to decay power limitations. It is completely erroneous to apply the results of Chapter 8 (and Appendix H) to this situation.

> **Seh15.** *The attack of the vessel by the impingement of a melt jet has been discussed in section 8 and in the Appendix H, with different approaches. The section 8 approach employs Saito's correlation and derives a curve for the vessel ablation depth vs. jet diameter. It uses a melt volume of 2.5 $m^3 \equiv 20$ tonnes and for a jet diameter of 10 cms obtains the ablation depth of 12.6 cms. The Appendix H, on the other hand, uses a melt mass of 47 tonnes and melt jet diameter of 4.8 cms to arrive at the ablation depth of 12.4 to 13.6 cms. If the section 8 analysis is redone with 47 tonnes melt mass and melt jet diameter of 4.8 cms, the ablation depth will be larger than the vessel wall thickness and no pool will form in the lower head.*

Appendix H was provided for some additional perspectives *on the margins to failure*, and not to have its arbitrary use of 47 tons be combined with the much more limiting analysis of Chapter 8.

Seh16. *Both the section 8 and the Appendix H evaluations assume the formation of an oxidic crust on the vessel wall. Thus the* ΔT*, for the heat transfer, is respectively 200 and 165 K. This is correct if the crust formed is stable and not swept out by the jet action. The jets are highly-turbulent with Reynolds numbers in the range of 3 to* 5×10^5*, and the survival of the crust in this regime may not be easy. The crust existence could be estimated by comparing the characteristic times for the convection-controlled crust growth, the remelting of the crust and the convection-controlled residence. The remelt time at the heat flux of 6* MW/m^2 *may be much longer than the crust growth time, however, the convection-controlled residence time may be less than 0.01 sec. Perhaps, the crust may exist at the peripheral parts of the jet impingement zone, but not at its center.*

This is speculative also, and counter to existing data. The reviewer is referred to Saito et al. (1990), already referenced in the report, and to Epstein et al. (1980), referenced by Saito. In the report we discussed also the applicability of these data (i.e., Re up to $3 \cdot 10^5$, Pr$\sim$ 1). The key point is that by crust we do not require the macroscopic existence of the crust, nor any thermal resistance associated with such macroscopic crusts. Rather, the rule is to impose a thermal boundary condition, at the melt liquidus, and for this a dynamic creation and washout (by the ablating melt beneath) of microscopic crust pieces is sufficient. Basically, what happens is that the melt freezes on contact, and it is totally unimportant that the resulting crust is washed out moments later, as a new one forms immediately. The data show that *very clearly*! And, as noted in the report already, with turbulent convection (with 10 cm and 5 m/s jet diameter and velocity respectively) even a hot steel substrate ($\sim$1200 K) requires an oxidic melt temperature of over 4400 K before the regime changes to one without crusts.

Seh17. *I believe that both the Section 8 and the Appendix H evaluations of the ablation depth, due to melt jet impingement, are overly simplistic and, perhaps overly conservative by not considering the presence of water. It is true that large scale data on this type of configuration is non-existent and the estimates made can not be validated. Nevertheless, the estimated made in Section 8 and the Appendix H are so close to the vessel wall thickness that one is left wondering about the seriousness of the jet impingement hazard, inspite of the fact that in the TMI-2 accident 20 tonnes of oxidic melt having a substantial superheat did not damage the vessel.*

First of all, Appendix H does consider the effect of water. Second, the whole idea of both Chapter 8 and Appendix H was to provide two complementary perspectives of how hard it is to see real physics doing the job of melting through the wall, even under some extreme conditions of impingement. This, of course, agrees with TMI and makes it hard to accept the "simplistic" characterization applied to these analyses by the reviewer. We accept the "perhaps overly conservative" characterization, but then, why is the reviewer concerned that the results obtained "are so close to the vessel wall thickness"?

> **Seh21.** *Appendix H. In this appendix, Table 2 provides Reynolds numbers for the melt jet as 260,000 to 480,000, which signify that the jets are turbulent. However, the correlation of Swedish used for determining the Nu number is for laminar jets. If Martin's correlation $Nu = 0.606\ Re^{0.547}\ Pr^{0.42}$, appropriate for turbulent jets, is employed, the value of Nu number for the second case in the Table 2 would be 776 instead of 560, which would lead to an even greater vessel ablation rate.*

Actually, for turbulent jets the correct correlation (supported by relevant data) is Saito's, as used in Chapter 8. We do not know where the reviewer found Martin's equation, but it is not appropriate for this problem. Also note that at a Re number of $5 \cdot 10^5$ Martin would produce a non-conservative result by more than a factor of 2. The assumption in Appendix H is that the melt jet exits the pool under insufficient shear to create any appreciable level of turbulence. Again, Appendix H is to provide a complementary perspective to Chapter 8, and the 47 tons of discharge utilized in it are way too much material for any realistic accident scenario—See also Appendix O. Moreover, as the next question and response indicate, there is a huge conservatism on the time duration of the pour.

> **Seh22.** *I believe the impingement time in Table 2 is too long. The analysis does not consider ablation of the hole of 4.8 cm through which 47 tonnes of melt is being poured into the vessel. The hole size will increase by factor of 5 or more, increasing the jet size, reducing the impingement time and the vessel ablation.*

This is indeed correct, and all the more reason to appreciate that penetrating the wall with an oxidic jet is physically unreasonable. See also Appendix O.

> **Sei16. III) Thermal loads under jet impingement:**
>
> *III-1) Only oxydic jets are considered. Why have metallic jets coming from the core been outruled ? Are such jets not credible ? Metallic jet would much more*

endanger the vessel integrity The EROS tests at KfK have shown very fast ablation for Iron jets impacting on a Steel plate.

This is a valid point, and an omission on our part not to discuss it. It is now discussed in Appendix O.

Sei17. *III-2) The calculations presented in the report for oxydic jets make the implicit hypothesis that the crust which forms on contact with the vessel is stable. The stability of the crust has been observed in the tests performed by Saito with Salt and Tin plates. However there is no general agreement, to my knowledge, on this point (the durations of the tests performed with real materials have not been sufficient to come to a clear conclusion). The stability of the crust may depend on several parameters such as:*

- *the temperature of the oxydic material (we estimate that the crust may survive several seconds for a 100° C overheat but less than 0.15 second for a 500 °C overheat)*
- *the inclination of the wall (the FARO BLOKKER test n°1 (molten U02 jet on an inclined plate 5° from vertical) has shown ablation of the plate).*

Even if the crust were unstable, the instantaneous freezing would establish the thermal boundary condition, and this is what matters. The 100 °C superheat rather than the 500 °C one is pertinent to this problem. The FARO BLOKKER tests were all rather benign. The 5° one showed slight ablation only. No analysis was presented that contradicts our, or Saito's, approach.

Sei18. *III-3) The inclination of the wall would also impede the occurrence of the "pool effect" (accumulation of molten material in the eroded cavity inducing a reduction of the heat transfer).*

→ Thus, I am not convinced that the analysis presented in the document is complete.

The pool effect was not included in the analysis. See Saito's paper.

Spe2. *The report considers the melt-relocation-related jet impingement heat flux as one of the regimes producing limiting thermal loads, in the two sections of the report that address this thermal loading mechanism, Chapter 8 and Appendix H, the relocating melt mass amounted to ~0.3 and ~0.6 of the core fuel mass.*

[1. The report should state the basis for selecting an amount of melt used in the jet ablation calculations.]

The basis reason was already expressed in the report; namely, relocation through a side-failure. The 30% shown in the main report was chosen as a reasonable upper value for a coherent relocation event. The 60% value shown in Appendix H was arbitrary and chosen only to show the huge margins to failure. The actual details of this relocation are much more interesting for in-vessel steam explosion considerations and therefore are being addressed in Theofanous et al. (1995). For a prelude, see Appendix O.

Tur32. *The argument (page 8.3) against a small diameter jet seems relatively weak, given that a local failure is expected; as the pour continues one may expect the melt to erode downwards. Again it would be useful to see Sienicki's work. Water in the lower head would play a mitigative role, and, as smaller diameters are considered for the jet, the coherency of its impact is more difficult to maintain. If impingement is on the cylindrical section, is this thicker than 15 cm?*

Yes, the cylindrical wall thickness is 20 cm. We agree with the mitigative role of water, and, furthermore, of the mitigative role of melt accumulation, which becomes increasingly more important as one wishes to consider larger pours (see also Mayinger's item # 2).

Structural Aspects

Lev30. *F. Other Comments*

1. The reviewer spent little time on the structural aspects of the report except to note that:

(a) An impulse methodology is utilized in Figure 1.1 to determine the potential for the structural failures. As mentioned on the top of page 1-4, "this is illustrative of global considerations; the actual assessment is likely to require additional details, such as the space-time distribution of the loads" as well as the space distribution of vessel wall thickness and temperature.

(b) There will be discontinuities in vessel wall thickness and temperatures due to the initial melt impingement on the bottom reactor vessel head (see B.4) or due to different erosion rates at the oxidic pool-metallic layer interface, or due to partially flooding the reactor vessel. Stress concentration factors need to be applied to take such discontinuities into account.

The study will consider whatever conditions are appropriate and important.

Nic2. *Chapter 4 contains an argument that the vessel lower head, in the submerged condition, will not fail absent a boiling crisis on or near its external surface. The structural failure criterion is not given explicitly but, from a close examination of the argument, appears to be based on a tensile membrane stress limit equal to the yield strength of the vessel material at an appropriate metal temperature. At the bottom of page 4-1, the required membrane wall thickness of 0.15 mm, when multiplied by a tensile yield strength of 355 Mpa and a vessel circumference of about 12 meters, gives a membrane resultant force of 71 tons. This required wall thickness is then compared to a minimum wall thickness of 1.1 cm that is kept sufficiently cool by the convective heat transfer in the external pool to maintain its strength.*

It is crucial to remember that the condition of heat flux examined here is a very high value that corresponds to the thermal failure criteria, as explained in Chapter 3. In Chapter 7 we find that there is a thermal margin of $\sim$100%, which means that the 1.1 cm value considered here has an *additional* 100% margin to structural failure in the membrane stress mode.

Nic3. *This argument is intended to address the stresses due to dead weight less buoyancy forces from displaced water in the pool, with the dead weight inclusive of the weight of the core melt that accumulates at the bottom of the head. The thermal expansion stresses due to temperature gradients across the vessel wall are treated in a similar, simplified manner by recognizing the longitudinal bending stress caused by the gradient (and the differential thermal expansion), but then limiting the discussion of the compressive (inside) and tensile (outside) bending stresses to regions away from any geometric or loading discontinuities.*

The simple analysis was provided to make the membrane stress argument quite transparent. Actually, due to axial conduction in the vessel wall, regions of discontinuity do not exist as such. A complete analysis is given in an addendum to Chapter 4.

Nic4. *These stresses were not identified in the report as longitudinal bending stresses, and this omission is unfortunate. The report also does not discuss longitudinal bending that might be caused by either a non-uniform distribution of the core melt weight, nor is the effect of non-uniform buoyancy force considered. A stress analyst would expect the deformation of the bottom head and cylindrical side wall to be non-uniform in the radial direction, reflecting the non-uniform distribution of weight, temperature, and buoyancy force, let alone the geometric discontinuity represented by the changes in curvature at the junction between the spherical lower head and the cylindrical side wall. The vessel would be expected to "pinch in" at some points around the longitude, relative to the outward radial motion elsewhere. This does not mean that the net radial displacement would be inward; it means that some portions of the vessel would have greater radial displacement than the inner surface of the vessel. One might suspect that one location of reversed curvature would be at the very bottom of the head, as the result of slightly greater buoyancy forces that cause the head to "dent." Another possibility is at the junction between the head and the cylindrical shell where the meridional curvature changes.*

The finite element model shown in Figure 4.5 could be used to study these longitudinal bending effects, provided that the mesh layout in the radial direction (across the shell thickness) is sufficient bending stiffness, in addition to membrane stiffness.

Following the suggestions made here, we carried out additional finite element calculations (with sufficient bending stiffness and distributed loads due to the weight of the melt inside and the buoyancy outside). The results are presented in an addendum to Chapter 4. These demonstrate the existence of significant margins to failure.

> Nic5. *In an effort to determine whether the longitudinal bending effects would be significant, this reviewer searched the other chapters and appendices of the report for: (1) any discussion on the distribution of dead weight (or distribution of equivalent internal pressure), as a function of the meridional coordinate, θ; (2) distribution of the buoyancy forces, as a function of θ; and (3) distribution of temperature, even for approximately the same gradient, as a function of θ. Some estimates of the variation in temperature are available (see Figure C.6), showing that the temperature at $\theta = 0$ will be lower than that at $\theta = 90$ degrees, with perhaps a 20 to 25 % variation, irrespective of heat flux.*

Actually, Figure C.6 is for an experiment, and the temperatures in it have nothing to do with the reactor. The inside wall temperatures in the reactor can be obtained from the local fluxes presented in Chapter 7, and an outside wall temperature of 130 °C (nucleate boiling). Better yet, the fluxes should be imposed in a 2D conduction calculation to obtain the smoothing due to conduction along the wall. This was done in the new calculation that includes the effects of distributed dead weight and buoyancy as already mentioned above. We found these effects to be negligible impact on the results.

> Nic6. *In order to complete this study with respect to the potential for structural failure of the vessel lower head or cylindrical side wall, the following steps should be taken.* First, *real structural failure modes and structural failure criteria must be considered. Real structural failure modes include such phenomena as ductile rupture, ductile tearing, brittle fracture, low-cycle fatigue, corrosion fatigue, buckling, creep rupture, and creep fatigue. The report currently addresses ductile rupture, on a partial basis, and uses the value of membrane tensile stress (and its comparison to tensile yield strength) as the failure criterion. Ductile tearing at the inside surface of the vessel, caused by reversed longitudinal bending, with either a strain limit or a peak stress limit, would also seem plausible. Creep rupture has been addressed in Appendix G, again for a simplified state of membrane tensile stress. The other failure modes do not apply to this loading and environmental situation.*

We agree with the suggestion that ductile tearing needed further consideration. This was done as described below.

Nic7. **Second,** *in order to determine the probable state of stress and deformation in the vessel as the result of the core melt event, the ABAQUS analysis reported in Chapter 4 should be revisited. The effects of longitudinal bending and potential reversed curvature caused by changes or discontinuities in the geometry or loading should be considered. Of particular importance is the effect of distributing the melt content weight, the temperatures, and the buoyancy resistance in the longitudinal direction. The buoyancy resistance will have an effect similar to a change in vessel stiffness; changes in wall thickness and in radii of curvature will also affect vessel stiffness. The existing ABAQUS model may be too crude, or the applied loadings may have been inappropriate, to detect these longitudinal bending effects.*

Third, *the calculated stresses and strains from any revised ABAQUS model should be subjected to a sensitivity study over a range of temperature distributions, wall thickness changes, etc., in order to scope out the worst case situations Then,* **fourth,** *the stresses and strains for these worst cases can be compared to real failure criteria. A basis for the latter was prepared by Teledyne Engineering Services for the Electric Power Research Institute (EPRI) some years ago, following the TMI-2 event. The relevant pages from Reference 2 are provided as an attachment.*

Done as described in an addendum to Chapter 4. These results show that the thermal failure criterion, as used in the report, is appropriate.

Nic8. *I hope that these comments are constructive, and will enable the excellent work done to date to be placed in a proper context. Once again, thank you for the opportunity to review and comment on this report.*

* *

Ola12. *(b)* *Wall failure by fracture*

In the report, high thermal stresses are accommodated by yielding and by creep. However, the outer surface of the vessel wall is held at 100 °C by boiling water, so the possibility of brittle behavior should be considered. The problem is not unlike pressurized thermal shock, in which cold water contacts a hot wall resulting in temperature gradients and thermal stresses. In the present case, a hot liquid contacts a cold wall.

i) In Fig. 4.4 of the report, a significant fraction of the wall thickness on the outside surface is at stresses larger than the yield stress. This is also the region that is coldest. It is possible that the stress intensity factor (K_I) exceeds the fracture toughness (K_{Ic}) and cracks develop on the outer surface, propagating inward until the crack arrest fracture toughness (K_{Ia}) is reached. Although through-wall cracking is not possible, the outer surface of the vessel could develop a population of cracks that render this region unable to sustain thermal stresses.

ii) The vessel wall above the metal layer is relatively cold throughout its thickness. Because the bottom of the vessel is hot, its thermal expansion places the upper vessel walls in tension. Again, K_I could exceed K_{Ic} and fast crack growth may occur.

iii) The temperature gradients developed during the initial thermal transient when the oxide liquid first pours into the lower head are steeper than those that prevail at steady state. As in the case of pressurized thermal shock, the transient behavior of the temperature distribution leads to crack propagation early in the event. Thermal stress distributions early in the core relocation to the lower head should be computed as well as the steady-state distributions treated in the report.

The minimum water temperature in the IRWST is 55 °C, and the end-of-life RTNDT of the AP600 vessel wall material is specified as 20 °C at the vessel belt-line welds. The only welds of interest here (i.e., subject to severe thermal stresses) are two circumferential welds on the lower head, one at the junction with the cylindrical section, and the other at some lower angle. These welds are expected to suffer minimal irradiation damage as compared to those in the belt-line regions, so the 20 °C temperature is conservative. Given that the vessel is depressurized (no primary loads) we do not see a concern with brittle fracture at all. These considerations are now briefly summarized in an addendum to Chapter 7.

Sei7. *II-4) Nothing is said concerning the evacuation of the heat flux released at the top of the pool.*

This heat flux is expected to melt a variable part of the in-core structures; but what happens afterwards ? Is this power diverted to the upper part of the vessel ? What would be the related heat flux distribution ? May the heat flux discontinuity at the metal layer surface induce unexpected **buckling** *of the vessel ?*

The heat flux released from the top of the pool is treated by the conduction/radiation path described in the original report at the bottom of p.6-2 and top of p.6-4. The resulting temperatures in that upper region are now displayed as part of "detailed results" summarized in Appendix Q. Rather than "buckling," the proper term here is "ductile tearing"; it is now considered in an addendum to Chapter 4.

She2. *You asked me to pay particular attention to Chapt. 4, Structural Failure Criteria. The authors basically consider net section collapse as the most probably failure mode. The net load acting on the wall in the situation considered in this report is extremely low, due to the combination of buoyancy forces and the weight of the internal melt. Only a fraction of a millimeter of steel would be sufficient to support this load. The other significant stress acting in the wall is thermal stress. It is greater than the yield strength, but such stresses are self-limiting and thus relieved with a minor amount of strain. The only way the vessel could fail is by the eating away of essentially all of the wall thickness. The authors show that when the wall is thinner than 2.5 cm (one inch) the water on the outside is sufficient to keep the wall from thinning, i.e. melting, any more.*

* *

Tuo9. *Thermal shock of the vessel*

External flooding brings two potential problems to the vessel integrity due to thermal shock.

The first concern is an inadvertent flooding of the cavity that may bring a problem of the pressurized thermal shock to the vessel material (and to the weld if existing on the core area) exposed to the fast neutron fluence. This is not directly concern of the in-vessel retention concept but any adverse effects for the safety of the vessel under design basis conditions should not be caused. The potential for inadvertent flooding should be checked under normal operating and overcooling transient conditions. The cracks located on the outside surface of the vessel may start propagating, since the outside cooling temperature is very low. Low initial and end-of-life brittle transition temperature of the vessel and weld material can minimize the risk.

See the addendum to Chapter 7. The vessel is designed to withstand one inadvertent flooding during normal operation.

Tuo10. *Secondly, the relocation of core material onto the lower head causes a severe thermal shock to the vessel bottom. Before relocation, the inner surface* *temperature of the vessel may be about 100 °C and the external surface temperature equals to that of the flooding water. The contact with hot corium creates very steep temperature gradient in the wall. Now the cracks cannot propagate through the vessel wall, because they will stop in the heated part. However, it should be checked that the cracks are not so long and deep that they could cause the failure of the vessel (global rupture of the bottom) after partial melting of the wall thickness.*

No cracks would be expected to propagate because of the extremely good quality of steel (RTNDT of 20° at end-of-life at belt-line, even lower in the lower head region) and the relatively high temperature of the flooding water (IRWST at least of 55 °C).

Arithmetic Checks

Hen7. *d. The solution scheme for Section 6.12 discusses using T_s as an iteration parameter By deduction it appears that this is the average temperature between T_{si} and T_{so}. However, I could not find this stated in the discussion. Since the upward heat flux from the pool and the dissipation to the respective parts of the reactor vessel and its internals are equally as important for in-vessel retention as the behavior of the lower head, the specific details of how this solution is determined and the respective split between upward and downward energy transfer should be displayed in this section. This needs to be done to justify the conclusion that "thermally-induced failure of an externally flooded AP600-like reactor vessel Is physically unreasonable."*

As defined in the nomenclature, T_s is the radiative sink temperature. It is *not* the average between the T_{si} and T_{so}. The solution to Eqs. (6.9) to (6.12) is *unique* and we find it in the manner described just below Eq. (6.12). This solution does not affect the upward to downward energy split. It determines the upward to sideways (the metal layer to vessel wall) energy split. Detailed results such as T_{so}, T_s, etc., are collected in Appendix Q.

Ola15. *(b) <u>Verification of numerical examples in the text</u>*

i) Starting with Eq(5.33) of the report with $S_{up} = \pi H(2R-H)$ and $S_{dn} = 2\pi RH$ (instead of hemisphere values) and V given by Eq(6.1), Eq(5.34) is:

$$q_{dn} = \dot{Q}FR \quad \text{where} \quad F = \frac{\gamma(1-\gamma/3)}{(2-\gamma)R'+2} \quad \text{and} \quad \gamma = \frac{H}{R}$$

For the example given on p 5-15, $\dot{Q} = 1.3\ MW/m^3$, $V = 10\ m^3$ and $R = 2$ m. These values give $H = 1.45$ m, and from the above equation for $R' = 1.31$, $q_{dn} = 391\ kW/m^2$ instead of the value of 313 given in the text.

The problem is that the 10 m^3 volume is not enough to fill a whole hemisphere. We used this value because it is typical for our interest, and Eq. (5.34) (or 5.33$'$ in the present version) is only approximately applicable. This was a bad choice for a numerical example. In fact, Eq. (5.33), which in general should be used, and the reviewer's number is correct. As additional perspective using the complete numerical calculation, we obtain 357 and 296 kW/m^2 for the two values of R' respectively. This point is clarified by means of a small addendum at the end of Section 5.1.

Ola16. *ii) The value of $\tilde{q}$ given in the example on p 5-19 should be* 9.1×10^5

Using $q_{up} = 600\ kW/m^2$ and $A = 2764$ in Eq(5.44) results in a difference between $T_{l,i}$ and T_b of ~0.4°C. This does not seem to be physically reasonable. However, using $T_{l,i} = T_b + 0.4 = 1678.4\ K$ and $\dot{Q} = 1.5\ MW/m^3$, $q_{up} = 600\ kW/m^2$ in Eq(6.14) gives $\delta_{cr} = 7$ cm, and the group $\delta_{cr}\dot{Q}/q_{up} = 0.17$, which violates the condition given by Eq(6.15).

These three points made here, and our responses, are as follows:

(a) Our 9.1×10^8 value is correct.

(b) Using the q_{up} and A values of the reviewer, $T_{\ell,i} - T_b = 54$ °C, and *not* 0.4.

(c) This then makes $T_{\ell,i} = 1736$ K, which with $\dot{Q} = 1.5$ MW/m^2 and $q_{up} = 600$ kW/m^2, gives $\delta_{cr} = 1.1$ cm. Then the criterion $\delta_{cr}\dot{Q}/q_{up} \sim 0.027$ and *not* 0.17. Actually, there was a typo error in Eq. (6.15), which is corrected (0.03 instead of 0.01).

We are not sure what the reviewer's errors are due to, but it appears that he is forgetting the 10^3 factor between kW and W.

Material Properties

Lev31. *2. The thermophysical properties derived in Appendix L utilize iron (Fe) rather than stainless steel. Stainless steel has about half the thermal conductivity of iron and similar variations are expected for other properties. This needs to be corrected.*

The vessel wall is not stainless, and the proper thermal conductivity for it was utilized. Internal components are all stainless, but for these the melt properties are of interest, and they are dominated by the properties of iron. The sensitivity of thermal conductivity of various iron alloys to composition is due to solid state microstructural effects.

Seh23. *Appendix L. This is a very valuable compilation of the relevant thermophysical properties. The viscosities shown for $U0_2$ and ZrO_2, and the rules for the mixtures, are apparently valid only for the liquidus state i.e., above the melting temperature. Is there any data or equation to evaluate the viscosities for temperatures between the solidus and liquidus. The boundary conditions at all the inside surfaces of the vessel are in that uncertain temperature range between the solidus and liquidus, where the properties will affect the heat transfer rates.*

This is not of real interest to the natural convection process. The slurry layer allows for the temperature to go from liquidus to solidus, and the slurry layer exists all around the boundary. The natural convection process "sees" the liquidus as an isothermal boundary condition. The slurry layer can be seen as a largely immobile thin region, and together with the crust makes up an effective crust. See also response to Henry Item #12.

Ola14. IV Miscellaneous

(a) Information

The report should contain summary tabular or graphical information on the reactor vessel which is the subject of its analysis. Even as basic a piece of information as the vessel wall thickness is only casually mentioned in the text and on the abscissa of some figures. Useful vessel information should include:

- *geometry, including instrument penetrations (if any) of lower head*
- *composition of wall steel*
- *plot of yield strength Vs temperature*
- *thermal expansion coefficient*
- *elastic and creep properties*
- *fracture toughness properties as functions of temperature*

This information as an appendix would be much more useful than the series of appendices describing the various heat transfer experiments. These contribute little to the tenor of the report and could simply be referred to in their original documentation. Appendix D describes an experiment that is not even built.

(a) There are no penetrations on the lower head. The geometry was described in Figure 2.1 and Table 7.2.

(b) Composition of wall steel was described and discussed in Appendix L.

(c) The yield strength vs temperature can be found in Chapter 4 and Appendix G.

(d) The thermal expansion coefficient along with all other properties were given in detail in Appendix L and in summary form in Table 7.2.

(e) Elastic and creep properties were presented in detail in Appendix G.

(f) The fracture toughness is provided in the final paragraph of Chapter 7.

(g) The heat transfer experiments constitute the heart of the case, and the experiment in Appendix D was not only built, it provided unique and essential data!

Sch15. *Fig. 7.3, Pg. 7-4: Why are there irregular wiggles on the flat portions of the probability density function plotted?*

They are due to the finite sample interval and total sample size.

Seh26. *On page N-5, it is not clear which two equations were solved for T_b and T_{ij}.*

The first two, Equations (N.3) and (N.4). Clarification made in text.

Sei13. *II-10) For high temperature differences, how are estimated the physical properties which are involved in the Adimensional numbers ? Are these properties also estimated at "film" temperature ?*

Yes. This was discussed on the bottom of p. 5-3.

Spe5. *[4. The extent of the key AP600 assessment results cited in Chapter 7 should be broadened. Key results are presented in terms of the ratio $q(\theta)/q(\theta)_{CHF}$. Other key representative results should also be given such as pool and metal layer bulk temperatures, crust thicknesses, wall thicknesses, and pool and metal layer energy splits.]*

This is a very good suggestion, and for completeness, we provide a complete set of results for the base case, and for the most limiting parametric case (of Chapter 7), in Appendix Q.

Tuo4. *The amount of steel in the metallic layer has been explained from the inner structures and their melting during the accident. Only schematic structural drawings of the reactor vessel and its internals have been given, For the readers' own judgment, a detailed drawing of the reactor core, internals and vessel would be useful.*

A figure with key components and dimensions is provided in the new Appendix O.

Tur19. *The term 'sizeable fraction of the core' on page 2-5 is undefined, but no evidence is produced to indicate that it is anything close to 100%. This is significant given the later arguments over the depth of the steel layer.*

By "sizeable" we mean "not small," i.e., ~30%. We have added this to the text.

Typographical Errors

Eps8. *(7) Typos: (i) Page C-17, change ragid to rigid in figure caption for Fig. C.6 and (ii) Page N-5, 4 lines from bottom: "furtitious"?*

Corrections made.

Kre15. *The report should do a better job of defining "Φ". It does not appear on the Figures or in the Nomenclature.*

Actually, this was a typo. Φ should be θ_p, and it was corrected.

Sch7. *The constant shown in Fig. 5.7 for the Mayinger et al. correlation should be changed from 0.54 to 0.55.*

Typo was corrected.

Sch18. *Pg. 10-2. Ref. 19: The Kelkar et al. reference title should be changed to "Computational Modeling ...", instead of "Computer Modeling ...".*

Typo was corrected.

Seh25. *Appendix N. In the run no A-2 in Table N-2, T_{li} should be 75.6 instead of 25.6. This is a typo, I am sure.*

Yes. Typo corrected.

Spe11. *Pg. 5–7, 2nd line, believe Ra′ exponent should be 14 rather than 16.*

Typo has been corrected.

Tur31. *Chapter 8*

It is stated in the second paragraph that 'The fundamental consideration is that molten oxide cannot exist next to a steel boundary even under strongly convective conditions ...', however equation 8.2 has $T_j = T_{w,m}$ as the driving temperature difference, not the melt superheat.

Sorry, this was a typo.

Editorial

Sch13. *Pg. 2-2. Second Paragraph, last sentence: I suggest replacing the terms "production and dissipation" with "heat input and heat loss." The terms production and dissipation are more commonly used in terms of turbulence production and dissipation as compared to energy or heat transfer.*

Clarification added, although content leaves no room for misunderstanding.

Sch16. *Page 7-11 3rd to last sentence: I might suggest changing "contrary to popular opinion" to "contrary to what might have been expected."*

Disagree. The "might have been expected" would include the author in the expectation crowd, while our expression does not. We mean the latter.

Sch17. *Fig. 7.9,. pg 7-12: I cannot distinguish which curves correspond to which values of θ in this plot. Could something be done to correct this problem?*

As the caption in the figures says, "the fluxes increase monotonically with angle (all solid lines)." So all one needs to do is count the lines.

Spe10. *In Appendix M, it would be better to refer to the cavity flooding valves as "remote actuated, motor operated valves" rather than "manual valves".*

This editorial change has been incorporated.

APPENDIX V

CLOSURE

This appendix contains the second round of comments received from the experts. It demonstrates that we have reached a common understanding and convergence on the study and its conclusions, so this second is also the final round, completing this effort. The purpose of this appendix is to provide some final touches, as requested by some of the experts and/or as offered by them. We take this opportunity also to provide the status of certain on-going confirmatory activities; namely, on the ACOPO and ULPU experiments. So, this appendix is organized in three parts.

APPENDIX V-1

FINAL ROUND OF COMMENTS AND RESPONSES

Of the 17 participating experts, two (Cheung and Olander) have significant concerns remaining, and one (Levy) chose to withdraw. For these, we provide a point-by-point response. Of the remaining ones, a few require some further clarifications, and these are provided immediately following each letter. The rest indicate complete agreement, and some of those make additional valuable and supportive contributions. For these, no response is required, and none is provided. The index below indicates which is the case, and the page numbers allow for efficient location.

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<u>Note:</u> ⋆ ⋆ ⋆ ⋆ ⋆ ⋆ ⋆ ⋆ indicates closing statement, not in need of further comment.

PENNSTATE

(814) 865-2519
FAX: (814) 863-4848

Department of Mechanical Engineering
College of Engineering

The Pennsylvania State University
137 Reber Building
University Park, PA 16802-1412

RECEIVED
REACTOR ENGINEERING DIVISION
—DIRECTOR'S OFFICE—
OCT - 2 1995
ACTION: LB
INFORMATION: LWD

September 27, 1995

Dr. L. W. Deitrich, Director
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439

Re: Review of DOE/ID-10460
"In-Vessel Coolability and Retention of a Core Melt," by T. G. Theofanous, C. Liu, S. Addition, S. Angelini, O. Kymalainen, and T. Salmassi

Dear Dr. Deitrich:

I have reviewed Volume 2 of the above-referenced report. In particular, I have read with care the authors' responses to my comments as well as to other expert comments on critical heat flux and natural convection. As explained point-by-point below, I am not satisfied with the authors' responses to the major comments I made, i.e., **Che2** to **Che7**. I also strongly disagree with the authors responses to (i) **Chu2** on the applicability of the CHF data to reactors, and (ii) **Chu9**, **Dhi3**, **Lev2**, and **Sei22** on the effect of natural recirculation.

1. **Che2** -- *Configuration Dominated by Natural Convection Phenomena*

It is not conservative to assume that the early transient heat-up events observed in the TMI vessel could never occur in AP600. Although the vessel external cooling conditions have a significant effect on the long-term steady-state behavior of the debris system, it has very little influence on the early transient heat-up events.

For most reactors including AP600, there is a considerable amount of thermal mass associated with the reactor vessel wall. Because of this thermal mass, it will take a period of time for the heat-generating debris in the reactor lower head to heat up the bottom side of the vessel. During this transient heat-up period, there are no significant thermal gradients in the outer portion of the vessel wall. As a result, the effect of external cooling of the reactor vessel will not be felt by the core debris inside the lower head. Evidently, the early transient heat-up events are not sensitive to the external cooling conditions on the vessel outer surface. This means that the early events in the transient heat-up period observed in the TMI lower head could also happen to reactors that are cooled on the outside. In particular, the transient situation involving the development of a localized hot spot observed in the TMI vessel could arise under certain circumstances during a severe accident in reactors such as AP600. This transient situation, which apparently is not bounded by the steady-state enveloping configuration depicted in Figure 2.2, should not be completely excluded in risk analysis.

Since the enveloping configuration employed by the authors may *not* really bound *all* intermediate states in the transient heat-up period, it can not be solely based upon in assessing the natural convection problem at hand.

2. **Che3 (see also Tur8)** -- *Dependence of the Surface Heat Fluxes on the Length Scale of the Melt Pool*

The authors fail to see the fundamental and practical importance of the Nusselt number - Rayleigh number relation given by equation (5.15). All the existing data including those by the authors were obtained at Rayleigh numbers below the typical values for reactor core melt which are on the order of 10^{15} - 10^{16} or higher. Extrapolation of the experimental data is, therefore, inevitable when applied to reactors. As explained in the work of Cheung (1980), the empirical power-law correlation of the form, $Nu = CRa^m$, is valid only over a finite range of Rayleigh numbers. In other words, the index m is not a real constant but a function of the Rayleigh number. At a Rayleigh number of 10^{15} -10^{16}, a difference of 0.05 in the index m would result in a factor of about six (i.e., 600%) in the Nusselt number. Even a difference of only 0.01 in the index m would result in a factor of more than 1.4 (i.e., 40%) in the calculated value of Nu. Thus, it is very important to know how the index m would vary with the Rayleigh number in extrapolating the experimental data to higher Rayleigh numbers for reactor applications.

According to equation (5.15), the asymptotic behavior of the Nusselt number corresponding to the case of length scale independence is given by $Nu \sim Ra^{0.25}$. This implies that the index m would *increase* from 0.233 toward the asymptotic value of 0.25 as the Rayleigh number is increased by several orders of magnitudes from 10^{13} or 10^{14}. On the contrary, the Nusselt number - Rayleigh number relation proposed by the authors, i.e., equation (5.10), gives $Nu \sim Ra^{0.20}$. This implies that the index m would *decrease* from 0.233 toward the asymptotic value of 0.20 as the Rayleigh number is increased by several orders of magnitude. Whereas equation (5.15) was derived from theoretical considerations of turbulent natural convection in a heat-generating fluid layer (Cheung 1980), equation (5.10) is merely a postulation by the authors. Based on what *theoretical ground* equation (5.10) is valid?

3. **Che4, Che5,** and **Che6 (see also Dhi2)** --- *Simulation of the Divergence Effect and the Three-Dimensional Aspects of the Two-Phase Boundary Layer*

There is no theoretical basis for the authors to claim that "the independent variable here is local superficial velocity, and as long as there is a reasonable upstream development length, *all* other multiphase aspects are *automatically* simulated." As discussed in **Che5**, the local superficial velocity represents only one of the key flow parameters that need to be adequately simulated. Other key parameters include the local void fraction, the size and shape of the vapor masses, the vapor dynamics adjacent to the heating surface (i.e., growth-and-departure of the vapor masses, local mixing and agitation, flow and agglomeration, etc.), the divergence of the vapor population in the flow direction, and the local two-phase boundary layer thickness. This reviewer could not imagine how *all* these local flow parameters can be simulated *automatically* by matching only the local superficial velocity. It should be noted that the problem under consideration involves natural convection boundary layer boiling on a downward facing curved surface, which is a rather unconventional problem. On one hand, the two-phase boundary layer flow is induced under the influence of gravity by the boiling process. On the other hand, the boiling process itself is strongly dependent upon the two-phase boundary layer flow behavior. The power-shaping method employed by the authors does *not* allow the *natural* development of the two-phase boundary layer, which is buoyancy-driven and *three-dimensional*. Specifically, the growth of the two-phase boundary layer in the flow direction and the resulting boundary layer flow characteristics can *not* be correctly simulated in their experiments. It is widely recognized that for boundary layer flows, the downstream behavior depends strongly upon the upstream flow conditions. If the upstream behavior can not be simulated adequately, how could the authors claim that the downstream flow behavior can be matched?

In response to **Che4**, the authors stated (see page U-41) that "Only the region $\theta_m < 10°$ is, strictly speaking, deficiently (but conservatively) simulated in this respect, by using a uniform heat flux, but extensive sensitivity type experiments indicate that the effect of this deficiency is negligible." Similarly, in response to **Dhi2**, the authors stated (see page U-48) that "at the neighborhood of the stagnation point, it can be said that the ULPU representation in this area is conservative. Moreover, from the data trends found in angles away from the stagnation point (say $\theta > 15°$), for which the ULPU simulation by power shaping is quite adequate, we can say that, in fact, this conservatism is not quantitatively significant." Evidently, the authors recognized the deficiency or inadequacy of their power-shaping approach in the bottom center region. Unfortunately, they did not recognize the effect of this deficiency on the downstream flow behavior. The flow characteristics in the bottom center region represents the upstream flow conditions of the two-phase boundary layer. If the experimental conditions in the upstream locations are not adequate, how could the *downstream* behavior of the *boundary layer* flow be correctly simulated?

4. **Che7** -- *Simulation of the Subcooling Effect due to the Gravity Head*

The authors are wrong in stating that "the same results can be obtained ... if one considers the total energy (sensible plus latent) flow per unit width." It appears that the authors did not get the correct physical picture. The total energy is useful only in estimating the rate of vapor mass generation on the heating surface. However, this vapor mass generation rate is *not* the same as the mass flow rate of the vapor phase in the two-phase boundary layer. This is because a portion of the vapor mass generated by boiling will eventually *condense* as the vapor mass departs from the heating surface and travels downstream in the *subcooled* boundary layer. It is the difference between the vapor mass generation rate and the accumulated condensation rate that dictates the magnitude of the local superficial velocity. The effect of subcooling *can not* be simulated by simply adding the sensible heat to the latent heat, as proposed by the authors. One has to estimate the local rate of condensation of the vapor mass in the two-phase boundary layer and then integrate this local rate over the heating length from the bottom center to the downstream location under consideration to obtain the *accumulated* condensation rate.

It should be stressed that with subcooling, condensation of the vapor phase will take place continuously in the downstream direction along the two-phase boundary layer. This will reduce the size and the mass of the vapor phase as well as the local superficial velocity. The latter quantity, which represents a *major unknown* of the problem, can *not* be determined by adding the sensible heat to the latent heat. Rather, it has to be determined from the difference between the vapor mass generation rate and the accumulated condensation rate. Note that the rate of condensation depends on the interfacial energy transport between the vapor and the subcooled liquid in the two-phase boundary layer.. Unless one solve for the two-phase boundary layer flow behavior, the interfacial energy transport and thus the rate of condensation can not be determined. Evidently, in the presence of liquid subcooling, there is no simple way to correctly predict the local superficial velocity without simultaneously solving for the two-phase boundary layer flow behavior. Since the local superficial velocity for the case with subcooling is not known a prior, how could the authors know *what local value to match* in their experiments?!

5. **Chu2** -- *Applicability of the CHF Data to Reactors*

In response to **Chu2**, the authors made a remarkable statement concerning the work of Cheung and Haddad. The statement was made, however, *without* being substantiated by sound theoretical arguments and/or experimental evidence. In fact, the authors did not provide *any* physical explanation for their statement. The authors' statement was: "We do not agree that the Cheung and Haddad data are, at this time, qualified to be compared with ULPU, or applied to reactors." Making such a statement *without* providing any physical explanation is not only technically unacceptable but also irresponsible. As explained below, the authors' statement is *technically wrong*. This reviewer encourages the authors to ponder the following points:

(i) The authors are incorrect to assume that the Cheung and Haddad data were similar to other small-scale boiling data, i.e., being restricted to transient quenching conditions. The work performed by Cheung and Haddad involved not only transient quenching experiments but also *steady-state boiling experiments*. The latter included direct observations of the steady-state two-phase boundary layer as well as quantitative measurements of the steady-state nucleate boiling heat transfer and local critical heat fluxes. Some steady-state boiling results have already been presented at the 22nd WRSM (October 1994) and the recent CSARP meeting (May 1995). Additional steady-state data will be presented in future meetings including the 23rd WRSM (October 1995). Apparently, the authors did not have enough information on the work of Cheung and Haddad. This reviewer wonders how the authors could comment on (or, more precisely, criticize) the work of Cheung and Haddad if they know very little about it.

(ii) When applying experimental results (i.e., CHF data) to reactors, it is important to assure that the experiments from which the data were obtained, correctly simulated the actual flow situation by accounting for the effects of the *geometry* and the *size* of the reactor vessel. A full-scale experiment (referring to the work of the authors) that fails to correctly simulate the effect of geometry (i.e., the shape of the reactor vessel particularly the lower head) on the two-phase boundary layer flow characteristics, *will not* be sufficient to generate realistic CHF data for reactor applications. On the other hand, a sub-scale experiment (referring to the work of Cheung and Haddad) that correctly simulates the flow behavior by (i) having the same geometry as the reactor vessel and (ii) adequately accounting for the size effect based on a valid scaling methodology, *will* generate useful CHF data for reactor applications (see items iii and iv below). The heating surface employed in the ULPU experiments, which is a *two-dimensional* representation of a full-scale reactor vessel, does not correctly simulate the effect of geometry. As discussed in items (3) and (4) above, the *three-dimensional* aspects of the two-phase boundary layer, the *divergence effect* in the lower head region, and the *effect of subcooling* on the local superficial velocity were not properly simulated in the ULPU experiments. In addition to these three important effects (i.e., the 3-D effect, divergence effect, and subcooling effect), the *effect of natural recirculation* in the reactor cavity was not correctly simulated in the ULPU experiments either, as discussed in detail under item (6) below. Conceivably, the local CHF data measured in the ULPU experiments could be unrealistic.

(iii) The experiments performed by Cheung and Haddad were conducted in a subscale boundary layer boiling (SBLB) test facility at Penn State. The SBLB facility was developed specifically for simulating the phenomena of downward facing boiling on the external bottom surface of a reactor vessel in a flooded cavity. It consists of three major components, i.e., a large pressurized water tank with a condenser unit, a segment-by-segment heated test vessel, and a data acquisition/photographic system. The facility can be operated at a pressure up to 20 psig and can be used to perform photographic studies and heat transfer measurements on test vessels of varying sizes (1/24 to 1/10 scale of the AP600 vessel) and shapes (hemispherical and toriodal). Transient quenching and steady-state boiling experiments can be conducted in the facility under well-controlled saturated and subcooled boiling conditions. The facility allows direct observations of the dynamic behavior of the two-phase boundary layer, the downward facing boiling process, and the local critical heat flux phenomenon. It also allows accurate measurements of the spatially variation of the local critical heat flux from the bottom center to the upper edge of the test vessels. The CHF data obtained to date clearly indicate that the critical heat flux varies significantly along the curved heating surface and is a strong function of subcooling. On the other hand, the effect of vessel size is of secondary importance. The differences in the CHF data obtained for test vessels of varying sizes were within the experimental uncertainties.

It should be mentioned that the sizes of the heating surface employed in the conventional small-scale experiments by previous investigators were on the order of several millimeters to centimeters, which were 1/1000 to 1/100 scale of the AP600 vessel size. On the other hand, the test vessel used in the SBLB experiments were 1/24 to 1/10 scale of the AP600 vessel size. Thus, the SBLB results represent the intermediate-scale data which are needed to bridge the conventional small-scale results and the full-scale data.

(iv) In addition to experimental studies, the work of Cheung and Haddad also involves the theoretical development of a mechanistic hydrodynamic CHF model for boundary layer boiling on a downward facing hemispherical vessel. The model predicts the spatial variation of the local critical heat flux on the vessel outer surface. Once validated with the CHF data obtained in the experimental part of the work, the model can be used to establish a suitable scaling law for reactor applications. Mathematical formulation of the model is almost completed and will be presented at the 23rd WRSM (October 1995). Preliminary results obtained to date indicate that the local critical heat flux on the vessel outer surface is a weak function of the vessel size, as long as the vessel diameter is much larger than the characteristic size of the vapor mass in the two-phase boundary layer. The dimensionless local heating length (i.e., the local heating length normalized by the vessel diameter), which is equivalent to the angular position of the curved heating surface, and the degree of subcooling of water have dominant effects on the local critical heat flux. They totally dwarf the effect of the physical dimensions of the test vessel.

6. **Chu9**, **Dhi3**, **Lev2**, and **Sei22** -- *Effect of Natural Recirculation*

The arguments made by the authors are not valid. It is recognized that the local CHF is highly dependent on the local mass velocity induced by natural recirculation. The natural recirculation itself, in turn, is induced by the process of boundary layer boiling on the heating surface and is strongly dependent on the geometrical characteristics of the overall system. The latter includes the geometry of the reactor vessel, the shape of the reactor cavity, the spacing between the vessel outer surface and the cavity wall, and the flow inlet/outlet drain configurations. Thus, to adequately account for the effect of natural recirculation in a flooded cavity, it is necessary to simulate not only (i) the *3-D* two-phase boundary layer flow along the heating surface, i.e., along the entire vessel outer surface in the three-dimensional domain, but also (ii) the geometrical characteristics of the entire system. A full-scale experiment that simulates only item (i) but not item (ii) is *not sufficient* to quantitatively account for the natural recirculation effect. Moreover, a full-scale experiment that does not correctly simulate either item (i) or (ii) could lead to *unrealistic* data involving significant errors and uncertainties. As discussed in **Che4** and **Che6**, the divergence effect, the 3-D aspects of the two-phase boundary layer, and the subcooling effect were *not* adequately simulated in the ULPU experiments. This means that item (i) was *not* properly satisfied. In view of the significant difference between Configuration II and the geometrical characteristics of a typical reactor cavity system, it is evident that item (ii) was also *not* satisfied in the experiments. Whether or not the natural recirculation effect observed in Configuration II is prototypic is still open to question. Much remains to be done to (a) systematically simulate the natural recirculation, (b) determine the recirculation mass velocity induced under various heat loads, i.e., boiling conditions on the heating surface, and (c) quantify the effect of natural recirculation on the local CHF.

As you can see, there are many important technical points that need to be further discussed and evaluated in order to realistically assess the efficacy of external flooding of a reactor vessel as a severe accident management strategy.

Thank you for the opportunity to participate in the second-round review.

Sincerely,

Fan-Bill Cheung

Fan-Bill Cheung
Professor of Mechanical Engineering
and Graduate Faculty Member of Nuclear Engineering

cc: Dr. L. Baker, Jr.

POINT-BY-POINT RESPONSE TO CHEUNG

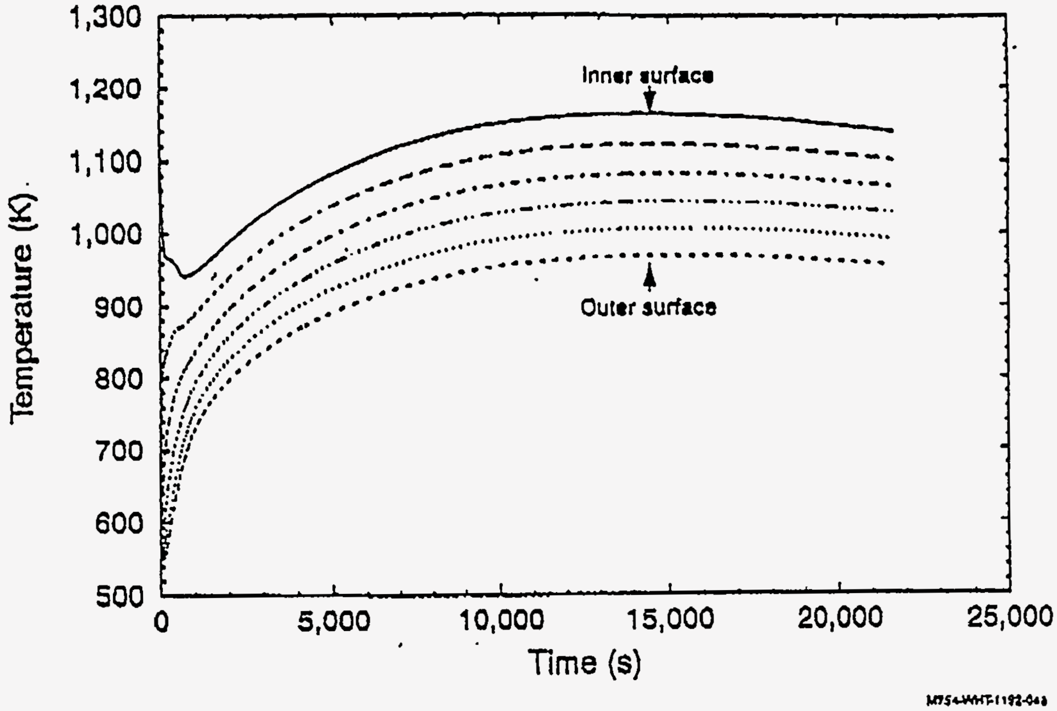
Notwithstanding an appearance of strong disagreement, the issues here, and their resolution, are rather straightforward. Let us be brief and to the point.

(a) **On the TMI Hot Spot.** The referee's initial point (Che2) about a "highly concentrate, volumetric energy source ..." was responded to. Now, it is not clear where the reviewer is trying to take this point, but there is absolutely no meaningful relevance. The heat fluxes that produced the hot spot have been deduced through a careful examination of integral heat transfer calculations in relation to boat sample metalographic analyses (Stickler et al., 1994*) Summary results are indicated in Figures V.1 and V.2 (see next two pages). It is clear that these fluxes are well below the fluxes delivered in our steady-state enveloping configuration, and directly contradicts the reviewer's statements that, "This transient-situation, which apparently is not bounded ... should not be completely excluded in risk analysis." Moreover, it is trivially simply to recognize that if TMI-2 was externally flooded the "hot spot" could not develop—as noted already in our previous response to this point. Further, insights on potential loads from a wide variety of intermediate states can be found in Appendix P.

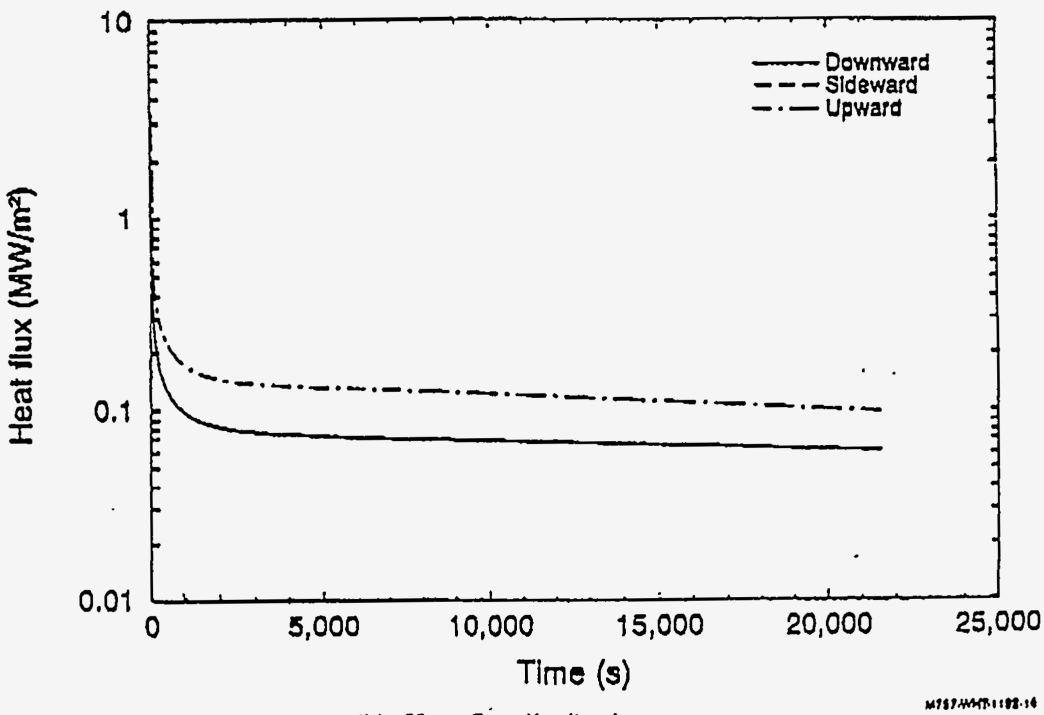
(b) **On the Nu-Ra′ Relationship.** It looks like the reviewer did not read beyond the **introduction** in Chapter 5 and Section 5.1. We did NOT use Eq. (5.10)! And, our data extend up to Ra′ $\sim 10^{15}$, so that the extrapolation needed (see Figure 7.18) is less than one order of magnitude. So, the "scenarios" for uncertainty indicated ("i.e. 600% in the Nusselt number") are strictly imaginary. In any case, the ACOPO (see Appendix V-2) will take us well above 10^{16}, so there will not be even a trace of a doubt allowed for him.

(c) **On the Critical Heat Flux.** It looks like the reviewer likes his experiment better, which should not be very surprising. Our comment to Chu2 about comparability (with ours) was based on it being at a very small scale, and it was not meant to be critical—but rather that the comparison should be made only when the basis for such comparison is clear. This means scaleup by more than one order of magnitude in size, which is still not available. Also note that the 1/24 to 1/10 scales provide a variation by only a factor of two, at a level that is by more than one order undersized. But again, it is rather straightforward to resolve this dispute. When the reviewer has an **adequate set of data and scaling analysis to predict the reactor** condition he should make them available, so we can compare. This is identified as an open item, at the closure of this review, in the summary section.

* L.A. Stickler, J.L. Rempe, S.A. Chavez, G.L. Thinnes, S.D. Snow, R.J. Witt, M.L. Corradini and J.A. Kos, Calculations to Estimate the Margin to Failure in the TMI-2 Vessel, NUREG/CR-6196, TMI V(93)EG01, EGG-2733, EG&G Idaho, Inc., March 1994.

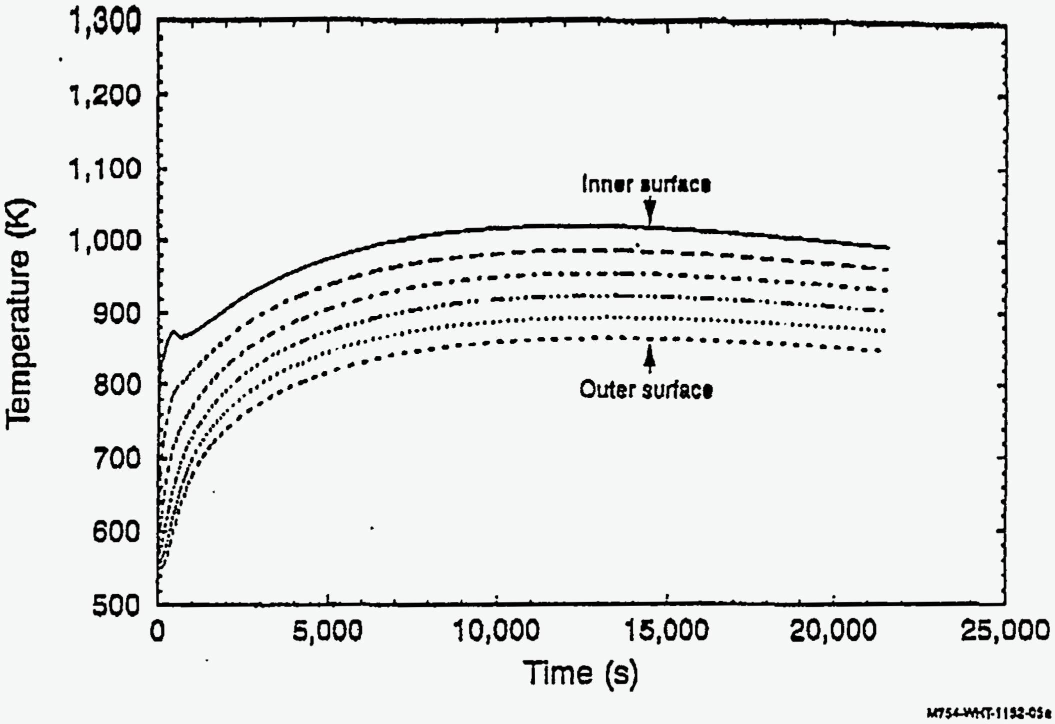


(a) Temperature distribution

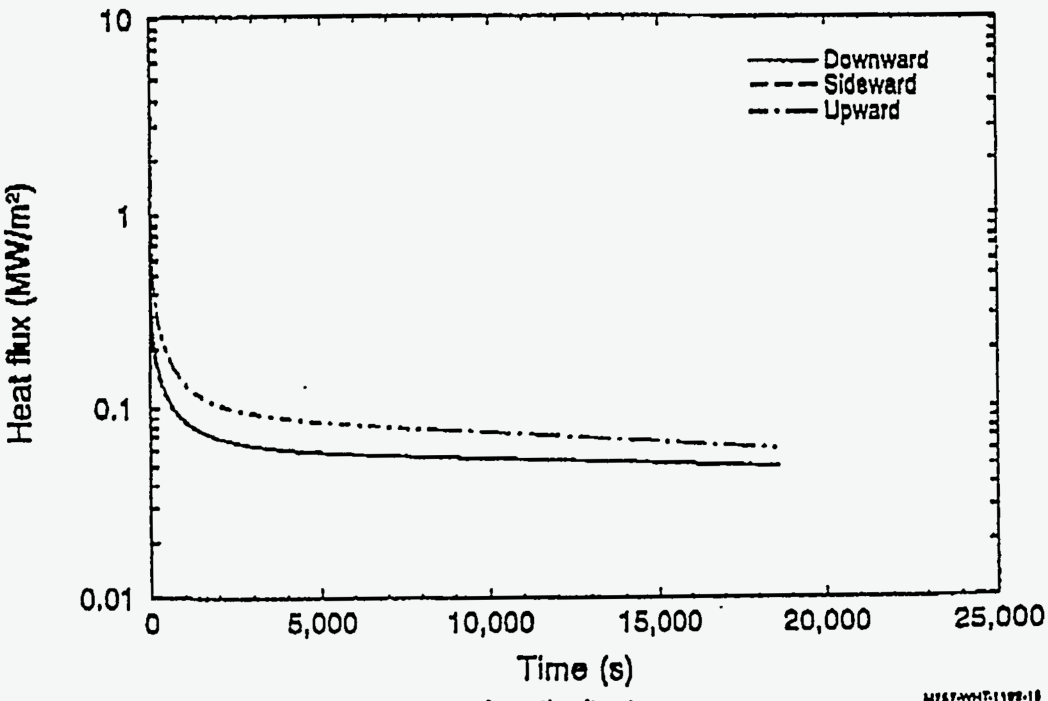


(b) Heat flux distribution

Figure V.1. Nominal case results of matching the TMI wall temperatures (from Stickler et al., 1994).



(a) Temperature distribution



(b) Heat flux distribution

Figure V.2. Lower bound case results of matching the TMI wall temperatures (from Stickler et al., 1994).

Sandia National Laboratories

P.O. Box 5800
Albuquerque, New Mexico 87185-1137

September 25, 1995

Dr. L. W. Deitrich
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439
Fax 708-252-4780

Dear Dr. Deitrich:

Enclosed is a summary of my comments on the authors' response to my initial review of the report: In-vessel Coolability and Retention of a Core Melt, and some new comments on the revision of the report.

Sincerely,

T.Y. Chu

RECEIVED
REACTOR ENGINEERING DIVISION
—DIRECTOR'S OFFICE—
SEP 29 1995
ACTION: LB
INFORMATION: LWD

Comments concerning the responses given by the Authors to T.Y. Chu Review comments

The authors have done a very good job in addressing some of the comments brought up in the reviews concerning the boiling process outside of the reactor vessel bottom head. I believe that the additional data provided in section E.3. addresses the CHF sensitivity issue adequately, and the data provided in E.4. addresses the thermal aspect of the insulation problem adequately. However, as observed in CYBL experiments, as well as in ULPU experiments, there are significant pressure pulses due to the condensation of vapor masses in the subcooled water surrounding the reactor vessel bottom head. Such pressure pulses are of sufficient strength to induce significant vibrations in the boiling apparatus. This reviewer believes that as a result of these pressure pulses, the structural integrity of the insulation must be assessed. In assessing the pressure pulses, it might be necessary to take into account the differences in pressure pulse propagation in a channel geometry (ULPU) and in an axi-symmetric geometry (AP600).

In the authors' response (P. T-9), the question of the "difficulty discerning the reviewer's position relative to the simulating power of the ULPU experiment" arose. The reviewer will provide the following clarification:

- The reviewer feels that ULPU is **inadequate** in simulating the three-dimensional boiling process near the bottom of the reactor vessel head.
- However, it is the reviewer's technical judgment, that the data developed from the ULPU experiments are **adequate** in AP-600 applications. The main reason is due to the large margin in the heat flux dissipation requirements for the vessel bottom.

With respect to item 5 (P. T7) of the response, the temperature spread between T1 and T4 at CHF is about 8°C and the average surface super heat is slightly over 20°C. "Calibration adjustment" of 8°C, for temperature measurements near 100°C is too large. If this is not an oversight, then the authors need to re-assess all the other measurements such as heat fluxes and flow rates.

With respect to "(b) Surface Condition (P. E-4 of Appendix E.4)," what is the basis to expect wettability of the reactor vessel steel to behave as the fully-aged copper?

In conclusion, the reviewer would like to observe that ULPU experiments, despite inadequacies, represent a major contribution to the assessment of in-vessel coolability and retention. The data confirms the AP600 coolability design. There remains a need to assess the structural integrity of the reactor vessel insulation. The data also represents an important fundamental contribution to boiling heat transfer.

AUTHOR'S CLARIFYING COMMENTS ON T.Y. CHU'S REPORT

- With respect to item 5: point well taken. Figure E.16 was composed from a rapid succession of two boiling crises separated by a brief period of quenching (only ~3 minutes), and it was shown to illustrate the excursions, primarily. It was an inappropriate choice for illustrating the performance of the heating block under the conditions of an actual run (gradually approaching CHF). But since the point was raised, this sort of detail is presented in conjunction with the mechanisms in Section V-3 of this appendix.
- With respect to item (b): They are both covered by oxides and are highly wetable, while our aging data show that the effect is significant mainly in the extreme of very poorly wetable surfaces—such as freshly polished copper (see Figure E.11). This is further supported by the close agreement in CHFs found between the aged copper and prototypic steel test section (see Appendix E-4).

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MECHANICAL AEROSPACE AND
NUCLEAR ENGINEERING DEPARTMENT
SCHOOL OF ENGINEERING AND APPLIED SCIENCE
405 HILGARD AVENUE
BOX 951597
LOS ANGELES, CALIFORNIA 90095-1597
FAX: (310) 206-4830

October 10, 1995

Dr. L. Walter Deitrich
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439

Dear Dr. Deitrich,

Enclosed please find my review of the responses of the authors to my earlier comments. I am sorry for being late in sending you my comments. Thank you.

With regards,

Vijay

V.K. Dhir
Professor and Chair

VKD:caf

c:\wp51\dhir\reviews\deitrich.ltr

Review of Responses to Comments
on Chapter 3 and Associated Appendices of
<u>In Vessel Coolability and Retention of a Core Melt</u>
DOE/ID-1046

Comment 1. Response generally satisfactory.

Comment 2. Response satisfactory.

Comment 3. I did not find, in Section E-4 faxed to me, a discussion of the effect of flashing as claimed in the response to my comment.

Comment 4. I find the authors response satisfactory with respect to the effect of heat flux shape or critical heat flux. However, reading Chapter 7, I am not convinced that ratios q/q_{CHF}, plotted in Fig. 7.13-1.15, are not optimistic. For partially filled cavities ($40° \angle \Theta \angle 60°$), it is quite possible that q/q_{CHF} will exceed the maximum values shown in these Figures. I did not receive either Appendix P or O. Hence, I cannot determine if this evaluation has indeed been carried out.

Comment 5. It is the authors prerogative.

Comment 6. Response is satisfactory.

Comment 7. The response is generally satisfactory, but I did not find any discussion in Appendix E-4 on progression of dryout region.

Comment 8. Response is satisfactory.

Overall, I am satisfied with the authors responses. However, it will be helpful if the authors will further consider my comments 3, 4, and 7.

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MECHANICAL, AEROSPACE AND
NUCLEAR ENGINEERING DEPARTMENT
SCHOOL OF ENGINEERING AND APPLIED SCIENCE
405 HILGARD AVENUE
BOX 951597
LOS ANGELES, CALIFORNIA 90095-1597

FAX: (310) 206-4830

February 5, 1996

Dr. L. Walter Deitrich
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439

Dear Dr. Deitrich,

This is a follow-up to my previous letter of October 10, 1995, regarding the "Review of Responses to Comments on Chapter 3 and Associated Appendices of In-Vessel Coolability and Retention of a Core Melt - DOE/ID 1046". At that time my copy of the report did not have Appendices P and Q. Since then I have received the two appendices and a response to my comments appears to have been given. Thus, response to this comment is not an outstanding item.

With regards,

V.K. Dhir
Professor and Chair

RECEIVED
REACTOR ENGINEERING DIVISION
—DIRECTOR'S OFFICE—

FEB 12 1996

ACTION: LB

INFORMATION: Theo, Sorrell

c:\wp51\dhir\techpapr\deitrio.let

AUTHOR'S CLARIFYING COMMENTS ON V.K. DHIR'S REPORT

(a) **Comment 3.** The explicit discussion was inadvertently omitted from Appendix E-4, so it is provided here. Flashing occurs due to the large gravity head, and it is most pronounced (i.e., visible) under near stagnant conditions—i.e., level near to, but below the top of the riser and/or during startup. One can then observe a periodic behavior with rapid emptying and filling of the riser, separated by comparatively longer periods, for establishing the thermal (and void) gradients. The emptying is a rather impressive phenomenon, and it occurs by means of a flashing/void wave that initiates at the top and propagates all the way to the bottom of the riser. The phenomenon is accompanied by a strong sound effect as the resulting two-phase mixture escapes into the condenser tank. The singular conditions required to produce this special geyser-like flow instability are of no interest to the thermal limits in cooling the reactor lower head. Flashing is also present under typical conditions of interest here. It participates in the flow oscillations of the natural circulation loop, but the relative amplitudes are small and the effect on CHF is negligible. This can be readily surmised by the data in Appendix E-3 that demonstrate relative insensitivity to flow rates, and to the presence of a the exit flow restriction.

(b) **Comment 7.** Again, the information on the progression of the dryout region was inadvertently omitted from Appendix E-4. It is included now, in section V-3 of this appendix, together with some new data and analysis on the mechanism of boiling crisis.

Fauske & Associates, Inc.

August 15, 1995

Dr. L. W. Deitrich, Director
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, IL 60439

RE: "In-Vessel Coolability and Retention of a Core Melt," by Theofanous et al.

Dear Dr. Deitrich:

I have examined the additions to the above-referenced report in response to my earlier comments and I feel that the authors have properly addressed my concerns. Given the limited time available for this second review, I did not consider in detail the comments submitted by the other reviewers.

It was a pleasure to be of service in the review of the in-vessel coolability report.

Sincerely,

Michael Epstein

Michael Epstein, Vice President
Consulting Services

ME:lak

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REACTOR ENGINEERING DIVISION
—DIRECTOR'S OFFICE—
AUG 17 1995
ACTION:
INFORMATION:

V-1.19

Fauske & Associates, Inc.

February 26, 1996

Dr. L. W. Deitrich
Argonne National Laboratory
9700 S. Cass Avenue
Building 208
Argonne, Illinois 60439

Dear Walt:

I have received the author's comments in response to the review group comments on the DOE report "In-Vessel Coolability in Retention of a Core Melt". I apologize in my delay in responding to your request for evaluating the comments. I have scanned the comments provided by the authors and I believe that the responses are sufficient to close this issue for those reactor designs in which water is available to submerge the RPV lower head and some of the vessel cylinder. This of course would be the case for the AP600 design discussed in the subject report.

In summary, I believe that the work that has been performed by Professor Theofanous and his colleagues at UCSB, in conjunction with the large scale tests at Sandia National Laboratory and the work performed here at FAI for the Commonwealth Edison PWR IPEs provide a sufficient technical case to consider the issue of in-vessel retention as a resolved issue for all designs for which the RPV lower head is submerged at the time that the core debris might drain into the lower plenum.

Thank you for including me as part of this review effort. Please call should you have any questions regarding my comments.

Sincerely yours,

Robert E. Henry /lab

Robert E. Henry
Senior Vice President

REH:lab

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—DIRECTOR'S OFFICE—
FEB 29 1996
ACTION: LB
INFORMATION: SORRELL
THEO

16W070 West 83rd Street • Burr Ridge, Illinois 60521 • (708) 323-8750
Telefax (708) 986-5481

August 16, 1995

Response to Authors Replies to my Review Comments
T. S. Kress

I have reviewed the responses to my review comments and find them to be quite adequate with the possible exception of the response to my major concern about the validity of the choice of decay heat for use in the ROAAM procedure.

The response to this concern does clearly state the source and derivation of the decay heat curves of Figures 7.1 and 7.2. These curves were derived from a "rapid heat-up and quench (after 16 minutes) scenario" for a "regular" PWR using SCDAP/RELAP with an out-of-date model for fission product release.

Given the differences between a "regular" PWR and the AP600, the differences among various sequences, and the effects of the particular release model, it is difficult for any reader to decide the appropriateness (conservatism) of Figures 7.1 and 7.2. To put this to bed, I would suggest the use of what surely must be existing calculations for this from the Westinghouse AP600 PRA using the MAAP code for the particular sequence of interest to IVR. Then, the question becomes simply "what is the uncertainty in the MAAP code results?"

If this is not done, additional justification is needed for the appropriateness of Figures 7.1 and 7.2. As a minor comment, the title of Figure 7.2 is extremely misleading and should be changed. In fact, this figure adds virtually nothing to the information already in Figure 7.1 and could very well be omitted. If so, I would then change the second sentence in the title of Fig. 7.1 to read..."The 'in-pool' curve accounts for selective loss of some of the volatiles for a particular PWR heat-up and quench sequence."

AUTHOR'S CLARIFYING COMMENTS ON T.S. KRESS'S REPORT

This comment was prepared with the help of J. Scobel (Westinghouse), who ran the MAAP 4.0 calculations, and D. Osatek (LATA), who helped with the interpretation of uncertainties in it. The sequence considered was a fully depressurized DVI line break.

The MAAP 4.0 calculation resulted in an estimated decay power in the core debris at 4 hours of 1.17 MW/m^3, as compared to 1.3 MW/m^3 obtained from Figure 7.1 of the report. Figure V.3 (next page) shows the transient evolution of the decay power density of the debris found in the lower head. The fission product release fractions calculated, at 4 hours, are listed in Table V.1 (next page). There is no release of tellurium, and only small amounts of barium, strantium, lunthanum, and cerium are calculated to be released.

The potential impact of uncertainties in the MAAP fission product release calculation can be evaluated by estimating the effect of postulated smaller (conservative) release of the volatile species. If all the strontium and barium were to remain in the debris, the decay heat at 4 hours would increase by less than 1%. If the molybdenum and antimony were to remain in the debris, the decay heat would increase by less than 2%. Even if half of the iodine were to remain in the debris, the decay heat would not exceed the 1.3 MW/m^3 value used in the report.

This verifies that the decay power density basis used in the report is reasonably and appropriately conservative.

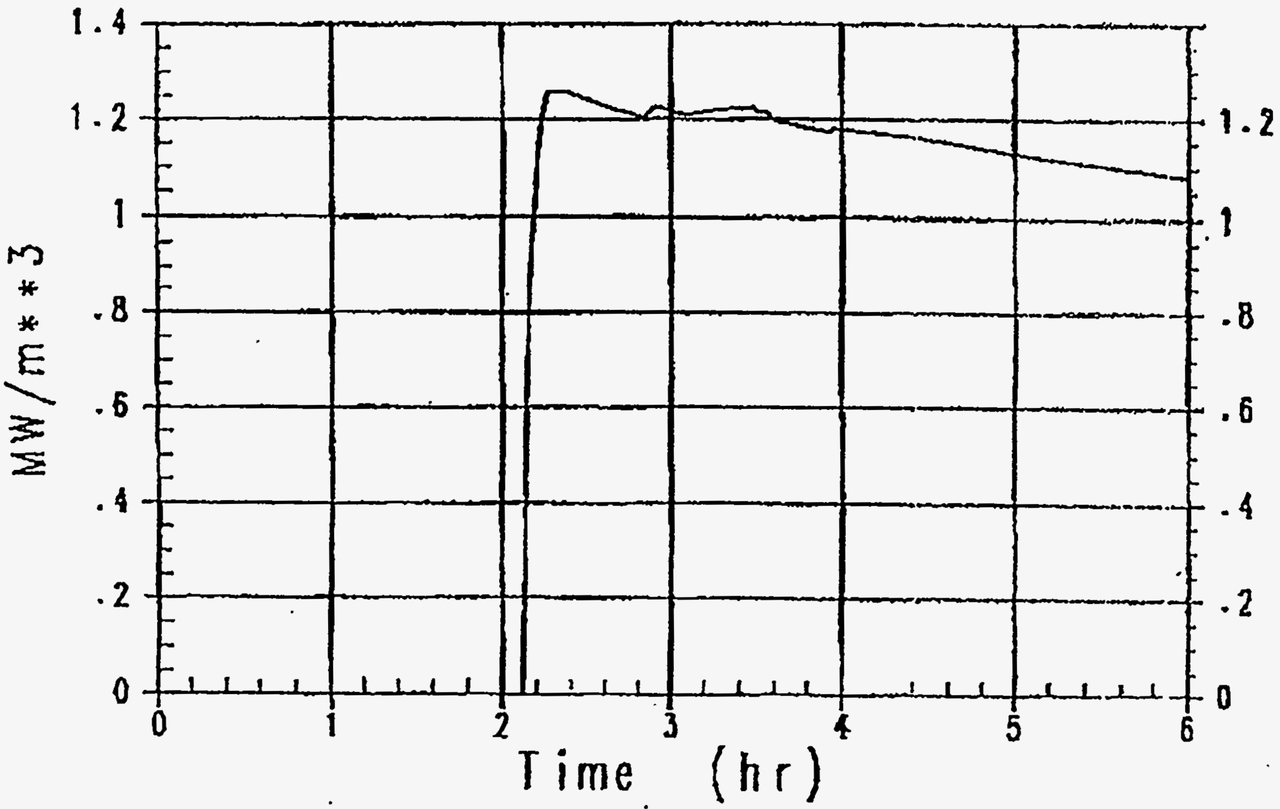


Figure V.3. The decay power density of the debris found in the lower head as calculated by MAAP for the DVI line break severe accident sequence in the AP600.

Table V.1. Fission Product Release Fractions (at 4 hours) Calculated in the MAAP Analysis

Specie	RF
Nobles	1.000
CsI + RbI	0.9997
CsOH	0.9997
TeO_2	0.0000
SrO	0.0181
MoO_2	0.6260
BaO	0.1897
La_2O_3	0.0193
CeO_2	0.0446
Sb	0.7097
Te_2	0.0000

LEVY & ASSOCIATES

3880 S. Bascom Ave., Suite 112
San Jose, CA 95124
408/369/6500
FAX 408/369-8720

August 14, 1995

Dr. L. W. Deitrich, Director
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439

Dear Walt:

Upon receiving the Peer Re- Review of DOE/ID-10460 (July 1995), I started my reading by first looking at answers to my comments. I was quite surprised by the authors' responses and their adamant position not to alter their conclusions under any circumstance. I had hoped for a more receptive attitude to criticism and for much more professional and specific responses particularly as a result of the involvement of the Argonne National Laboratory (ANL).

It is hoped that the nuclear industry will never have to deal with the degree of core melt proposed in DOE/ID-10460. There is little basis, knowledge, or prototypic data to support the behavior predicted in DOE/ID-10460. Because, at the present time, this behavior may be just a figment of somebody's imagination, it is essential to recognize the non-prototypicality of the available data or models in terms of geometry, employed fluids, and configuration changes with time. The authors have decided to disregard such issues because they might weaken their conclusions. Let me just say again that my original comments stand and will stand until a much better case can be made for many of the authors' comments and calculations.

I have decided that further comments on the new version of DOE/ID-10460 will be just as useless as the first set and I shall appreciate it if you attach this letter to the final issue of the report so that there is no doubt left about my decision to withdraw from the final review. The good news is that there will be no charge against the 8 hours allocated to me for this final review step.

Sincerely Yours,

Salomon Levy

Salomon Levy

AUTHOR'S CLARIFYING COMMENTS ON S. LEVY'S REPORT

(a) "... of core melt proposed in DOE/ID-10460." We did not invent severely degraded core melt accidents here, but rather an effective means for managing them by preventing vessel failure.

(b) "... just as useless as the first set ...." A critical comment does not have to be accepted (and agreed upon) in order to be useful. The issues are indeed complex, and everything counts towards resolution. Contrary to the reviewer's expressed opinion, his first set of comments are very useful. It is regrettable that he chose to withdraw.

TECHNISCHE UNIVERSITÄT MÜNCHEN
LEHRSTUHL A FÜR THERMODYNAMIK
Prof. Dr.-Ing. Dr.-Ing. E.h. F. Mayinger

Thermodynamik A - Technische Universität München - 80290 München

80333 München
Arcisstraße 21
TEL: (089) 21 05 34 35
21 05 34 36
TELEX: 522854 tumue d
FAX: (089) 21 05 34 51
may@thermo-a.mw.tu-muenchen.de

Dr. L. Walter Deitrich, Director
Reactor Engineering Division
Building 208 - Room C 213
Argonne National Laboratory
University of Chicago

Argonne, Illinois 60439
USA

Ihr Zeichen	Ihr Datum	Unser Zeichen	Unser Datum
		M/ba	22.08.1995

In-Vessel Coolability and Retention of a Core Melt
Review Comments on the Additions of the above mentioned Report

Dear Dr. Deitrich,

With your letter, dated 20.07.1995, you asked me to look over the comments, given by the authors on my short review.

Please find enclosed my brief answer.

Sincerely yours,

F. Mayinger

Prof. Dr.-Ing. Dr.-Ing.E.h. F. Mayinger

Enclosure

RECEIVED
REACTOR ENGINEERING DIVISION
—DIRECTOR'S OFFICE—
AUG 28 1995
ACTION: LB
INFORMATION: LWD

TECHNISCHE UNIVERSITÄT MÜNCHEN
LEHRSTUHL A FÜR THERMODYNAMIK
Prof. Dr.-Ing. Dr.-Ing. E.h. F. Mayinger

In-Vessel Coolability and Retention of a Core Melt Review Comments on the Additions made by the Authors in DOE/ID-10460, Volume 2

In their response to my review remarks, the authors agree with my statements. So there is absolutely no need for further discussion in this respect.

Also, I propose to discuss a little more possible parameters, when studying the temperature situation in the wall at various boundary conditions. The authors followed this recommendation and added a new Appendix Q, that gives detailed results for the calculated parameters and which also takes into account various wall thicknesses. So I completely agree with the information, given in the report DOE/ID-10460.

I would like to make a general remark, which results from reading comments of some other reviewers.

In spite of the fact, that the accident in TMI occurred, it is very unlikely, that a similar scenario could also happen in an advanced reactor of the type AP 600600, due to the fact, that advanced reactors will be enforced with respect to passive cooling systems. So for advanced reactors, a core melt scenario will have a much lower probability than for TMI. TMI had no or almost no passive cooling capability. So for advanced reactors, a core melt is a highly hypothetical accident.

Dealing with highly hypothetical accidents, we should not start from again highly conservative assumptions, when calculating thermodynamic and fluiddynamic phenomena. Furthermore, we should do such an analysis by using at best estimate assumptions with respect to the physical conditions and the transport phenomena - mainly heat transport - to be expected. Combining conservative assumptions with analysing hypothetical accidents would end up in long discussions, finally leading in the not very effective question: "How conservative is conservative?"

I think, the authors of the report succeeded to find a good balance between conservatism and optimism and described a physically well based scenario for the case, that such an accident would have happened.

F. Mayinger

München, den 22.08.1995

Prof. Dr.-Ing. Dr.-Ing.E.h. F. Mayinger

August 22, 1995

Professor T. G. Theofanous
Department of Chemical and Nuclear Engineering
Center for Risk Studies and Safety
University of California
Santa Barbara, California 93106-1070

Reference 1: T. G. Theofanous, C. Liu, S. Additon, S. Angelini, O. Kymalainen, and T. Salmassi, "*In-Vessel Coolability and Retention of a Core Melt*," Report No. DOE/ID-10460, Volume 1, University of California, Santa Barbara, California, July 1995.

Reference 2: T. G. Theofanous, C. Liu, S. Additon, S. Angelini, O. Kymalainen, and T. Salmassi, "*In-Vessel Coolability and Retention of a Core Melt*," Report No. DOE/ID-10460, Volume 2, University of California, Santa Barbara, California, July 1995.

Dear Professor Theofanous:

In accordance with your instructions, I have reviewed References 1 and 2 with particular emphasis on structural considerations and changes made to respond to my comments on the previous draft. Also, I reviewed all of the comments and the responses to the comments in order to examine the disposition of other structural issues.

I was especially pleased with the evaluation of bending stresses in the vessel shell caused by a combination of longitudinal variations in vessel stiffness (e.g., localized thinning) and loading, and the potential for ductile tearing failure, in response to my comments. The geometric model (Figure 4.8), the undeformed ABAQUS finite element mesh (Figure 4.9(a)), and the thermal boundary conditions (Figure 4.7) are understandable. The deformed ABAQUS mesh seems a little strange, since I would have predicted a double reversal of curvature, with the second reversal near or below the bottom end of the mesh shown. The principal plastic strains are somewhat useful in explaining the structural behavior, but the reader has to infer the sign of the surface longitudinal and circumferential strains that are the key to the evaluation of ductile tearing. Primarily, the reader is looking for states of biaxial or triaxial tension at or near the inner or outer surfaces of the shell, along with the largest associated value of equivalent plastic strain.

The discussion on page 4-10 is very close to adequate, but let me suggest that relatively minor modifications by an experienced structural engineer would make the arguments more pursuasive. The worst-case plastic strains in the biaxial or triaxial tensile zone should be explicitly compared to some failure criterion (e.g., uniform tensile strain at

ROBERT E. NICKELL, PH.D. • 1663C

E • POWAY, CALIFORNIA 92064
TEL: 619.485.9024 • FAX: 619.485.9024 • eMAIL: 76500.155@compuserve.com

failure in a uniaxial tensile test of ASTM SA-533, Grade B, Class 1 material, with a knockdown factor to compensate for the multiaxial tensile state of stress). These modest changes will improve the report considerably.

Let me congratulate you and your colleagues on a fine piece of work. These recommendations are intended only to help you finish the story completely. Thank you for letting me comment on these efforts.

Sincerely,

Robert E. Nickell

Robert E. Nickell
Applied Science & Technology

P.S. Would you please correct your address, telephone, and telefacsimile for me to read:

Dr. Robert E. Nickell
Applied Science & Technology
16630 Sagewood Lane
Poway, California 92064-1408
(619) 485-9024
(619) 485-9024 (FAX)

FINAL COMMENTS ON DOE/ID10460

by D. R. Olander August 1995

General Summary: All but points (a), (d), (e), and 13.(c) are satisfactorily answered. Detailed comments on these issues are given below.

Point (a) (concerning steam reaction with Zr in the liquid metal layer)

Three reasons are given for dismissing this criticism(p. T-69). Each will be discussed in turn.

(i) The authors' answer relating to the inconsistency in steam supply appears to suggest that there is not enough water in the vessel to sustain the reaction rate of 5 moles/s. However, consider the following:

According to an inquiry to ANL, the volume of water in the lower head up to the bottom of the fuel rods is 11 m^3, or $6x10^5$ moles. This is sufficient to oxidize $3x10^5$ moles of Zr. According to Table 7.2, there are 19.2 MT, or $2.1x10^5$ total moles of Zr in the core, of which only a fraction is in the melt. Thus the available water is more than enough to oxidize all of the Zr in the melt.

The authors' response also implies that the Zr oxidation reaction would have to be sustained by oxygen drawn in from the containment vessel, and that this would decrease the rate by reducing the value of $\Delta M/M_f$ used in my analysis of mass transfer from the gas to the liquid surface. However, the mean molecular weight of air is larger, not smaller, than that of steam, so natural convection would be enhanced, not reduced, if O_2 from air were the reactant. Moreover, if O_2 rather than H_2O were to react with Zr, the heat release would be 1088 kJ/mole O_2, which is over four times larger than the heat of reaction of a mole of H_2O with Zr. Thus O_2 provides a considerably larger reaction heat source than does steam.

ii) The response cited additional heat removal mechanisms that come into play if the Zr oxidation is considered. The emissivity effect is discussed in iii) below. The other supposition is that liquid water on top of the melt layer would greatly enhance heat transfer. This is stated without supporting analysis. I do not believe that liquid water could sit on top of a melt layer and remove heat by film boiling. If liquid water addition is a credible scenario, it should be analyzed in detail, not in one sentence.

iii) (and point(b)) This reply concerns the effect of the emissivity of the metal pool surface. Response iii suggests that the 20% increase in heat flux to the metal layer is essentially compensated for by the increase in the emissivity. This may be so, but simply pointing to Fig. 5.11(which does not have ε as a parameter) does not prove this assertion. If radiant heat emission from the metal layer surface is such a crucial process in controlling the sidewall heat flux, this heat transfer process should have been treated more thoroughly than the simple $\varepsilon\sigma T^4$ form used in

Eq(5.43). First, radiation from a heat source to enclosure walls involves more than just the emissivity of the former; radiant heat exchange involves the emissivities of all surfaces, their relative areas, and the geometry of the enclosure. Moreover, steam is an effective absorber/emitter of thermal radiation and affects radiant heat loss from the metal pool. There is an extensive literature on radiant heat transfer in furnaces that demonstrates the importance of these two features. Simply increasing ε in Eq(5.43) from 0.45 to 0.80 does not convince me that an extra 1.5 MW of heat produced by the reaction can be radiated away.

In general, in dismissing the consequences of the steam-metal reaction on the bases given in the authors' response is not sufficient. This reaction, when it occurs with the Zr structures in the core, is responsible for the core meltdown in the first place. Imagine that the early analyses of core degradation in severe accidents had concluded that the heat source due to the oxidation of cladding could be ignored because the ZrO_2 reaction product would have a higher emissivity than the metal and so the extra chemical heat would simply be radiated away. That would have been a major misjudgment

(b) Metal Pool Emissivitiy

No further comment on changing ε from 0.45 to 0.8 - it should be done. However, if this change is made, the radiation heat transfer aspect of the analysis should be treated more rigorously(see above).

(c) Ternary eutectic

I don't know where the authors obtained the eutectic temperatures cited in their reply. For low Zr contents, the eutectic in the Fe-Zr system is at 1608K, and occurs at a Zr mole fraction of 0.088(Fig. 6.1). In the ternary phase diagram(Pelton et al, J. Nucl. Mater. 210,324 (1994)) a peritectic is found with a liquid atom % composition Fe=88.6, U = 4.9, Zr = 6.5 at T=1540 K. It is probably true that inclusion of this effect in the analysis would not change the report's overall conclusions.

(d) Vessel Wall Melting Temperature

The authors' reply to this point claims that the vessel wall thickness is 5 cm, not the 15 cm I assumed in my analysis of this effect. The figure I used was taken from the statement of p. 3-4 of the draft report, "...the lower head thickness could vary from its initial value of 15 cm(for most of the region)...." Doesn't this mean what I think it does, or is this a typo?

The effect of the Zr/Fe ratio of the melt is more significant than the effect of adding a small amount of U. The authors dismiss the point I made with the comments: (i) the eutectic composition(or temperature) is conservative and (ii) the composition of the melt cannot be specified accurately, so presumably there is no need to worry about it.

With respect to (ii), many other parameters in the calculation cannot be specified accurately. The report(correctly) treats these uncertainties by analyzing a range of values and assessing the effect on the final determination of vessel wall integrity. Why should not the same approach be applied to the Fe/Zr ratio of the melt? Had they attempted this, the wall melting temperature would have varied as shown in Table 1 of my analysis of this point. These changes are significant, and could only be dismissed by incorporating the analysis I gave into the overall computation. Note that the analysis of the composition dependence of the wall melting temperature is independent of the choice of the initial wall thickness.

(e) Melting Temperature of the Oxidic Pool

The authors' reply seems to imply that the results they calculated are independent of the oxidic phase melting temperature, T_l. I do not find this to be so. Heat transfer in the oxidic pool is given by Eq(5.7). Although Ra'(and hence Nu for this phase) does not depend on T_l, the heat flux calculated from Eq(5.2) does. If their claim that the value of T_l chosen is not important, why not run the code with $T_l = 2700$ K instead of 2973 K and prove it?

(f) Location of the decay heat source

I generally agree with the authors' conclusion that this is not going to affect the results appreciably. However, two items concerning the analysis of Appendix R are in order. First, the tellurium group was assigned to the oxidic pool. It is well known that Zircaloy cladding effectively sequesters Te and only releases it when the cladding is oxidized. From this, one concludes that Te is stable in the presence of metal phase but not in the oxide. Therefore, the metal layer heat source should also include the Te group. Second, all of the Zr fission product was assigned to the oxidic pool. However, the metal contains elemental Zr, and fission product Zr should partition between oxide and metal phases in the same proportion as structural Zr. These two modifications would probably not change the conclusions, though.

It is puzzling that from Table R.3, the noble metals and Mo provide only ~ 10% of the decay heat source while they constitute ~ 25% of the fission products(Table 2 in my analysis). Is the energy of the β/γ radiation from the noble metals that much lower than that of the other fission products or the decay rate so much smaller?

Concerning the last comment of the authors, extraction of the noble metals by the metal pool need not be restricted to the configuration achieved during the analysis treated in the report; most of the redistribution of fission products to their thermodynamically-favored phase probably occurred during core degradation, prior to slumping to the lower head.

P. T-74 - T-76 No additional comments

13. (c) Stability of the Pool upper crust. The authors simply restate their assumption of an intact crust. My original comment still stands, unanswered as far as I am concerned.

IV Miscellaneous No comments

POINT-BY-POINT RESPONSE TO OLANDER

We will comment on points (a), d), (e), and 13(c), for which are original response seems not to have satisfied the reviewer. We would like to discuss point (a), the most potentially confusing one, last, so we will approach them in reverse order.

- 13(c). See response to Turland's item (d).
- (e). Please note that in Eq. (5.2), T_i does not appear by itself, but in difference from T_{max}, i.e., the superheat calculation is not necessary!
- (d). The reviewer acknowledges and does not disagree with our assertion that "the eutectic composition (or temperature) is conservative." The composition is an intangible parameter, and as such in ROAAM it has to be quantified conservatively.
- (a). Before we lose the reader to the details, the key points here are as follows:
  - (i) For superheated steam at 1000 °C and 1 bar the radiation absorption length is ~2 m. Moreover, aerosols would condense on the relatively cold walls and/or be carried out by the hydrogen (in the reviewer's scenario), so it could not provide significant obstruction to the radiation emitted from the top of the melt pool.
  - (ii) Radiation heat transfer was treated with sufficient accuracy, and the emissivity effect is very significant, as can be seen from Figures 5.10 and 5.11. We are concerned that these points are not clear to him, because he has not understood well what we are doing. This is evidenced from his comment that "This may be so, but simply pointing to Fig. 5.11 (**which does not have ϵ as a parameter**) [emphasis added] does not prove this assertion." Actually, these figures are in dimensionless terms, and emissivity (ϵ) **does** appear in them.
  - (iii) Water on top of the melt would lead to film boiling heat transfer, augmenting radiation heat transfer. This is not a matter of opinion.
  - (iv) Finally, the reviewer misunderstood our comments on steam limitations, and the use of water in his scenario is out of context.

In conclusion, although we disagree on the appropriateness of this additional heat source, the fact remains (as we stated in our previous response) the increased emissive from 0.45 to 0.8 (see his point (b)) is more than enough to compensate for it. This conclusion is based on basic principles, as noted in our point (ii) above, and no amount of detail included in the analysis can cause significant deviations from it.

FAX: Theo Sorrell 9/1 pw

Sandia National Laboratories

P.O. Box 5800
Albuquerque, New Mexico 87185-1139

RECEIVED
REACTOR ENGINEERING DIVISION
—DIRECTOR'S OFFICE—
AUG 30 1995
ACTION: LB
INFORMATION: LWD

August 23, 1995

Dr. L. W. Deitrich
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439

Dear Dr. Deitrich:

I have reviewed the authors responses to my review comments on the report "In-Vessel Coolability and Retention of a Core Melt," prepared by T. G. Theofanous, S. Additon, C. Liu, O. Kymalainen, S. Angelini, and T. Salmassi. My comments are as follows:

Comments concerning the responses given by the Authors to R. Schmidts Review comments

In Appendix T, the authors have itemized my review comments into 19 pieces. I will use this same identification system here. I have no additional comments to make except on items 9, 12-15, and 19.

9. The authors have not responded to my concern discussed in the second paragraph under item 9 (starting with "At the beginning . .", and ending with " . . .the last section." The report (see section D.5) still uses the argument that a "self similar stratification pattern" validates the ACOPO approach. As expressed before, I believe this to be an error, and that a different rationale must be used to validate the approach. The discussion given (by the authors) relative to the technical note does not address this specific issue.

12. A reader should not be required to read Appendix A first to understand what is being written here. Either remove the reference to a "Grade B" approach, or explain it.

13. I am surprized at the authors resistance to such a simple suggestion. My recommendation remains that in the context of this section of the report, they should replace the word "production" with "heat input", and the word "dissipation" with "heat loss" wherever they occur (which is more than once).

14. This item concerns statements made in the last paragraph on pg. 2-2. The discussion on the technical note at the end only touches on the boundary layer effects, and does not demonstrate to my satisfaction what the authors claim it does. However, my point was that I felt that this paragraph was confusing as written and could be reworked to be short and clear. Since the authors have left it as is, so be it. At the very least, they should provide a specific reference for who they are thinking of when they say "what occasionally has been mentioned as ...."

15. I reaffirm my objection to using the unreferenced phrase "contrary to popular opinion" in the context of a technical report such as this. Specific references should be provided supporting the statement, or the phrase dropped.

19. Typo: The original technical note is in Appendix S, not Appendix T (see the heading)

In my first review, I have shown that by a simple mathematical transformation the equations governing the fluid flow and heat transfer in the experimental case can be directly compared with the desired physical problem. By so doing, the conditions under which the problems are equivalent were clearly identified. I also made a specific suggestion: control the isothermal temperatures of the experimental boundaries in such a way so as to keep the overall ΔT constant, thereby creating an exact experimental analog. The authors have not commented on the feasibility of this suggestion.

As pointed out in my review, the ACOPO experimental approach is directly related to previous work done by Chow and Akins, Lin and Akins, and Hutchins and Marschall. An appropriate reference to this earlier work should be made by the authors.

In my first review I also suggested an approach for comparing the ***relative*** value of going to a larger scale experiment. At first, the authors appear to agree with this analysis. However, they continue by arguing that the time scale being looked at, t_σ, is much larger that the real time scale of interest because it relates to the bulk temperature instead of the boundary layer. They make a heuristic argument that the time scale of interest should be related to the boundary layer. I like the idea, and certainly the boundary layer is a controlling factor in the heat transfer. But it must be remembered that the system is a finite, enclosed system, and therefore the boundary layer cannot be entirely isolated from the bulk behavior. Thus to me it is **not** yet clear that "t_σ grossly exaggerates the fundamental characteristic time against which pool cooldown should be compared to judge quasi-steady state." Unless a more rigorous mathematical or experimental rationale can be provided, I believe the data from the mini-ACOPO experiment should be treated with optimistic caution until the data from the ACOPO experiment is available to test the quasi-steady state assumption.

Thank you again for the opportunity to participate in this review.

Sincerely,

Rodney C. Schmidt

Rodney C. Schmidt
Reactor Safety Experiments Dept. 6423
Mail Stop 1139

RCS:6423

Copy to:

MS0742	J. Kelly (6414)
MS0739	K. Bergeron (6421)
MS1137	T. Y. Chu (6422)
MS1137	T. Heames (6422)
MS1139	K. O. Reil (6423)

AUTHOR'S CLARIFYING COMMENTS ON R.C. SCHMIDT'S REPORT

(a) Points 12 through 15 are minor editorial, once they have been made and responded to; it is the author's prerogative to make changes or not. The typo in point 19 was corrected.

(b) The ACOPO (1/2-scale) experimental facility has just been completed (Appendix V-2), and soon we will have the data needed to settle the last statement of the reviewer regarding his "...optimistic caution ...." We certainly can, and will, run the cooling such as to keep a constant overall ΔT. Through this and other variations in experimental conditions we can then address details, such as the importance of stratification patterns (point 9), and the characteristic time for quasi-steady state. As noted previously, between the mini-ACOPO, COPO, and the UCLA experiments, we have enough experience to expect that ACOPO will be confirmatory.

9/-7 Theo Sarrell

VETENSKAP OCH KONST

ROYAL INSTITUTE OF TECHNOLOGY

1995-09-08

Division of Nuclear Power Safety
Professor Bal Raj Sehgal
Tel: 46-8-790 65 41
Fax: 46-8-790 76 78

Dr. L.W. Dietrich
Director, Reactor Engineering Division
Argonne National Laboratory
Argonne, Illinois 60439

U.S.A

Subject: Second review of the Document, "In-vessel Coolability and Retention of a Core Melt" prepared by T.G. Theofanous, C. Liu, S. Addition, S. Angelini, O. Kymäläinen and T. Salmassi, Volumes 1 and 2, DOE/ID-10460, July 1995.

Dear Walt,

I have enclosed the second review of the subject document. I have read the comments made by all of the reviewers and the responses made by the authors of the subject document. In addition, I have reviewed the new information that the authors have provided in the report. I believe the process of synergism that the authors of the subject document wanted to achieve was accomplished quite well.

My general comment is that the authors have provided excellent responses to the comments made by the reviewers. The responses and the new information provided further solidifies the authors claim that the accident management provision of cavity flooding, adopted by the AP-600 design, will be able to assure the integrity of the vessel, inspite of a melt pool inside, to a very high probability.

I enjoyed reading the document. If you have any questions please telephone or write.

With best regards,

Sincerely yours,

Raj

Bal Raj Sehgal
Professor

Postal address
Kungl Tekniska Högskolan
S-100 44 STOCKHOLM, Sweden

Visitors address

Telephone
+46 8 790 60 00

Telegram
Technology

Second Review of the Document
"In-Vessel Coolability and Retention of a Core Melt"
prepared by T.G. Theofanous, C. Liu, S. Addition, S. Angelini, O. Kymäläinen and T. Salmassi, Vols. 1 and 2, DOE/ID-10460, July 1995

by
B.R. Sehgal
Professor, Nuclear Power Safety
Royal Institute of Technology
100 44 Stockholm, Sweden

The major addition to the subject document is the volume no. 2, which contains the comments made by the 17 reviewers and the point by point responses made by the authors to the comments. Volume 2 also contains the very important Appendix 0 and the Appendices P, Q and R. Other information provided is in the form of the addenda in volume 1, which supports the responses made by the authors in volume 2.

The comments made by the reviewers, and point by point responses of the authors, make interesting reading. A certain synergism in the review process has been achieved by having each reviewer review the comments made by the other reviewers. I believe this process is very beneficial. For example, my questions about validity of the experimental technique, employed in the ACOPO tests, were answered convincingly by the technical note, submitted by Dr. Schmidt, with his review comments.

In the following, I have revisited my earlier comments after reading the responses and the additional information provided by the authors.

(1) High CDF for Vessel Rupture and HPME

The correction of these values is welcome. The previous numbers seemed to be very high. I hope some PRA person checks these.

(2) End State vs. Intermediate State of the Melt Pool

I have read the Appendix 0 with interest. I believe that the information presented there is crucial in resolving the comments made not only by myself, but also by other reviewers. The authors delineate the large difference between the AP600 core-reflector-core barrel configuration versus that in TMI-2, to which all the reviewers are accustomed. The scenario offered by the authors makes good sense and I think that the configuration of the end state of the melt pool derived is reasonable. The fear that some intermediate states may give higher thermal loading on the vessel is not completely alleviated. However, it is clear that there is a safety margin of ≈ 100%, which would be sufficient to take care of any possibly higher thermal loadings on the vessel, in the vicinity of the metal layer.

3. Failure of the Lower Blockage

Appendix 0 makes a convincing case for ignoring this mode of failure during the time frame of the pool formation in the vessel.

4. U-Zr Eutectic in the Metal Layer

I believe the authors have responded satisfactory to the concerns about some heat generation in the metal layer, through parametric variations in the distribution of heat generation between the oxidic and metallic layers. The concern about higher density due to Uranium addition has not been addressed.

5. Transient Thermal Loadings Near Vessel Bottom

The addenda to Appendix D provided in Vol. 1, responds to this concern adequately.

6. ACOPO Experimental Technique

I believe concerns about the ACOPO experimental technique have been adequately responded to.

7. Prandtl Number Dependence

The dependence of the heat flux on the Prandtl number was investigated by authors from Pr=2.6 to 7, and no dependence was observed. Calculations, to be reported by Dinh et.al. in the forthcoming NURETH meeting, also show no dependence on Pr number down to a value of ≈ 2. However, at Pr = 0.6, the value for corium, the heat flux increases by 20 to 25% for $0 \leq 0 \leq 30°$. I believe this is a real effect, and this has been a subject of comment by some other reviewers as well. I think, the uncertainty will be covered by the margins available for the region near the vessel bottom. Also, I believe the crust formed on the inside of the vessel will ameliorate the differences in the imposed heat flux, as a function of the polar angle.

8. Melt Jet Impingment

The melt jet impingment treatment discussed in Section 8 and the Appendix H created the confusion about this topic. The authors should have, at least, used the same melt initial conditions in the two places. Be that may, the clarifications offered are plausible. Melt jets going through water would not be a threat. Also, if a large mass is involved, the jet diameter will increase and the time of impingement will decrease; both of which will reduce the vessel ablation depth. Thus, there should be ample margin in the very conservative estimates derived in the report.

I believe, I have revisited the major comments that I had made in the earlier review. I believe, those concerns have been adequately addressed and I believe, that there is ample margin to cover any residual uncertainties, which I believe may be of the order of 10 to 30%. Thus, the claims made by the authors regarding in-vessel melt retention, with external vessel cooling, for the AP-600 severe accidents, are justified.

Commissariat à l'Energie Atomique

CEA - GRENOBLE
DIRECTION DES REACTEURS NUCLEAIRES
Département de Thermohydraulique et de Physique
Service de Thermohydraulique des Réacteurs
Laboratoire d'Etudes Thermohydrauliques des Coeurs

ADRESSER LA CORRESPONDANCE A :
M. Seiler
CEA-Grenoble
DTP/STR/LETC
17 rue des Martyrs
38054 GRENOBLE CEDEX 9
Tél. : 76.88.30.23
Fax : 76.88.52.51

Dr. L.W. Deitrich,
Director Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue, Argonne, Illinois 60439
USA

REF. STR/LETC/95-123/JMS/mo

October 9, 1995

Objet : **Review of the document *"In Vessel Coolability and retention of a Core Melt"* DOE/ID-10460 prepared by T.G. Theofanous, C. Liu, S. Additon, S. Angelini, O. Kymäläinen, T. Salmassi (july 1995)**

Dear Dr. Deitrich

Please find enclosed a review of the second version of the paper in reference.

I tried to analyse the different items but I had some difficulties due to the scattering of the different arguments throughout the new documents.

However I think that the authors have built a "strong line of defense" for the corium recovery in the AP600 lower head. Subsisting problems are mainly related to FCI.

RECEIVED
REACTOR ENGINEERING DIVISION
—DIRECTOR'S OFFICE—
OCT 17 1995
ACTION: LB
INFORMATION: LWD
10/23

Best Regards,

J.M. Seiler

Copy (letter + P.J.) : Prof Theofanous
Copies (P.J.) : D. Grand, S. Rougé, J.M. Bonnet
DTP/EA

Review of the document "In-Vessel Coolability and retention of a Core-Melt" DOE/ID-10460 by T.G. Theofanous, C. Liu, S. Additon, S. Angelini, O. Kymäläinen, T. Salmassi

DOE/ID-1046

The new report offers valuable complementary informations; however I had some difficulties to get an overview because many arguments concerning one same item are distributed throughout the documents.

Most responses made to my different previous comments seem to be adequate and seem generally convincing. However, there subsist a few points of discussion which are outlined below.

Scenario analysis

Point 2 page T-104

I am not yet convinced that the porous pool situation can be outruled for the AP 600 situation. In the new appendix O it is outlined that about 42% of the oxydic core inventory main be partially quenched in the 1meter high lower part of the vessel in the LIS. The steel plate just situated above may remelt before the oxydic debris begin to remelt. The draining liquid steel may then fill the porosities of the debris bed situated below (steel may wet oxydic debris above, say, 2200°C). It will then no more move upwards since it will stay trapped by the solid oxydic debris (cooled by natural convection in the steel). This may reduce the molten steel layer height at the top of the pool and potentially increase the "focusing" effect at the top of the molten oxydic pool (which would then rely on the lower solid debris). I have no idea about the size of oxydic debris and thus no idea about the potential for convective flow of steel through these debris.

Note that if all the steel could be trapped in such a manner there might be no "focusing" steel layer problem at all.

Mini ACOPO

Point 11 page T 109

I read the paper by R.C. Schmidt with some attention.

The new approach is very interesting. I have however some problems with this approach.

The general gain related to the transient approach is evident because it allows to perform experiments without sustained heating. But I do not agree that the related Rayleigh numbers may be, for the same geometry, much higher than the Rayleigh numbers which may be obtained with volumetric heating. The Rayleigh number, calculated on page 8 of R.C. Schmidt's review is calculated on the basis of the Radius as it is usually. A high-end Rayleigh number of $8.\ 10^{14}$ is found where I only find $6\ 10^{12}$.

The approach supposes that quasi steady state conditions (temperatures and velocity fields) are established within the pool during the cooldown process. The velocity distributions in the boundary layers, and thus the heat exchanges, are related to the temperature distribution within the pool and to the size of the pool. As the temperature distribution are supposed to be similar in the slow transient state and in the steady state there is no reason that the characteristic Rayleigh number is very different.

Another way to come to this conclusion is based on energy conservation:

Starting from equ. (14) in the paper from R.C. Schmidt:

$$\rho Cp \frac{\partial T_b}{\partial t} V = \bar{h}\, A \Delta T$$

and noting that quasi steady state conditions are established:

$$\bar{h}\, A\Delta T = \text{Power} = qV$$

leads simply to:

$$q = \rho Cp \frac{\partial T_b}{\partial t}$$

where q is the volumetric heat dissipation necessary to obtain a steady state mean temperature difference ΔT_b in the pool.

Thus the maximum Rayleigh number based on the transient decay rate Q is simply interpretated as the Rayleigh number calculated on the basis of the volumetric heat dissipation which would be necessary to obtain the initial maximum mean temperature difference within the pool.

This leads to the conclusion that for a given geometry, the range of Rayleigh numbers for transient experiments is similar to the range of Rayleigh numbers obtained for steady state experiments. I do not see any physical possibility to change the flow regime from laminar to turbulent in a same geometry with the same mean temperature difference only by doing transient experiments instead of steady state experiments.

Superheated metallic jets

Superheated metallic jets are excluded on the basis of their probable cooldown related to the large thermal inertia of the reactor structures and the small flow pathes. This seems reasonable but I think that this should be supported quantitatively.

The characteristic time for temperature decrease of a metallic flow (heat transfert is supposed to be controled by conduction) is approached by:

$$\tau \approx \frac{H^2 \rho Cp}{4\lambda}$$

where H is the thickness of the flow.

For H equal to 1 cm (characteristic width of flow path), this time is equal to, about, 1 second. This means that the melting temperature of the flowing metal should be approached within, about, 5 seconds. How do these 5 seconds compare with the retention time within the structures before the metallic jet may interact with the vessel ?

Oxydic pool interface temperature

I support the statement that the interface temperature is near to the liquidus temperature. There is no contradiction with the fact that in the ACE experiments the melt temperature were below the liquidus temperature as suggested by Brian Turland (page T-131 in the report). The work we have presented at the Nationnal Heat Transfer Conference in Portland shows that the solid/liquid interface temperature is related to the solid fraction (mass of solid oxydic crust divided by the total mass of oxydes). In ACE the solid fraction is rather elevated (from 10% to 70%, depending on tests) and thus the solid-liquid interface temperature may be below the liquidus temperature of the mixture. When the solid fraction is low we have shown that this interface temperature increases and reaches the liquidus temperature.

This also supports the hypothesis that the mushy zone is very thin (even inexistant) and that the pool is entirely liquid. Thus the molten material would be properly represented by simulant fluids.

Furthermore, if the oxydic corium (U,Zr,O) separates at high temperatures (which I believe to occur) this would lead to the formation of a lower UO_2 + ZrO_2 pool. The temperature interval between solidus and liquidus would be much restricted in comparison with the (U,O,Zr) initial mixture. This would obsolete the problem of the determination of the interface conditions.

Steel fragilisation due to low temperature melting metals

The metallic melt contains also low temperature melting metals such as Tin (Sn), Silver (Ag), and others. I spoke to some specialists from CEA, and he rised some stricking problem: there might be a strong decrease of the mechanical properties (yield stress ...) of the steel if these metallic phases diffuse through the steel wall (long term behaviour). This decrease of the mechanical properties concerns steel layers whose temperature is higher than, approximately, 0.8 times the melting temperature (in K) of the considered metallic item. For tin this means: temperature higher than, about, 130°C (i.e. the whole thickness of the vessel wall is concerned). As shown on figure 1, this might affect the mechanical behaviour of the vessel.

At present time I have no precise idea on the time scale needed for this metallic diffusion.

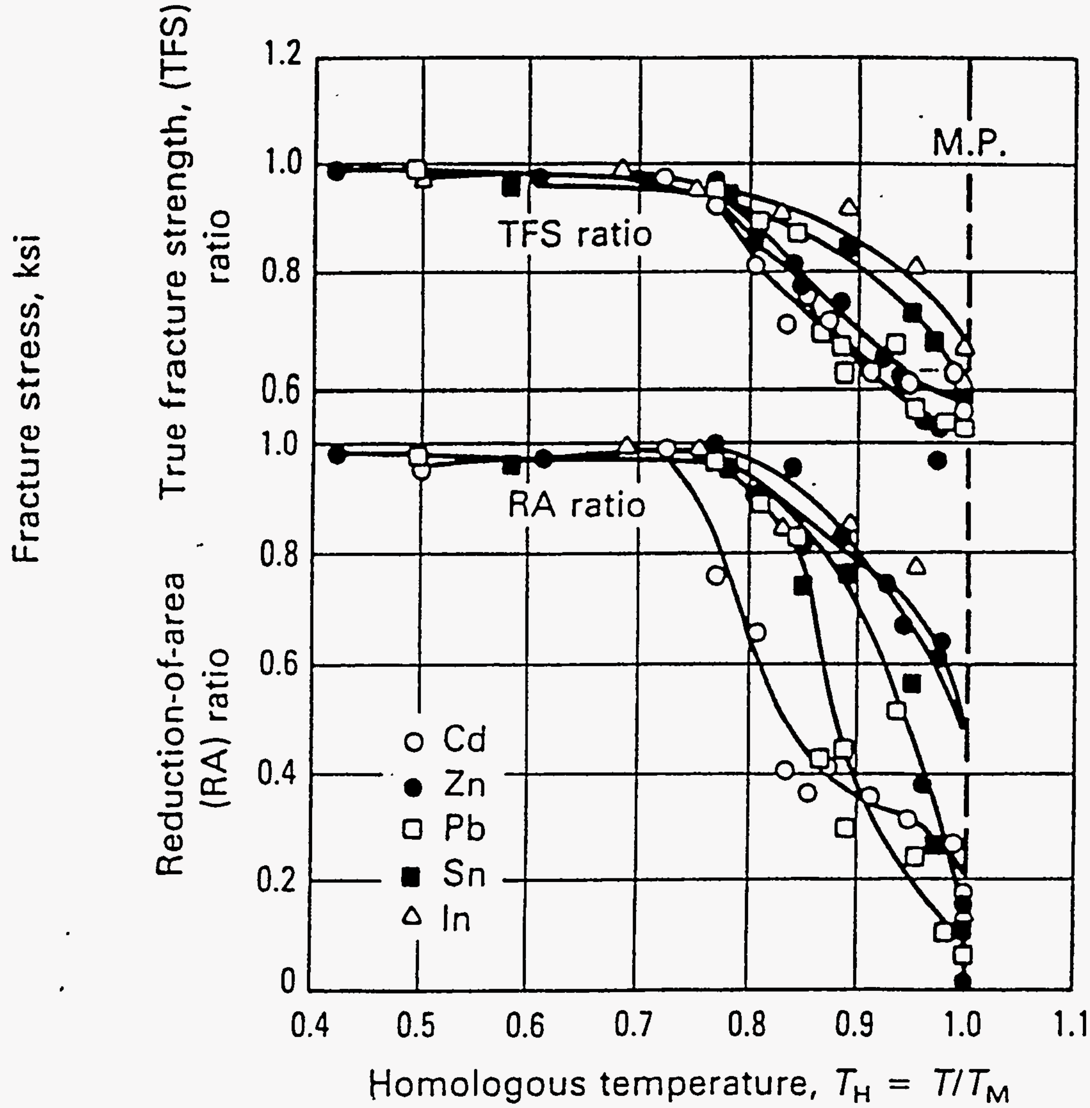
Pool Crust instabilities

We have performed a specific analysis which shows that crust instabilities on the upper side of the oxydic pool should not have consequences. The analysis supposes that a supplementary wall thickness may be ablated due to the transient heat flux and temperature fluctuations induced by the crust renewals. It is concluded that the wall may be maximum ablated to the " mean " isothermal 1200°C. But as the wall layer whose temperature is higher than 1200°C plays no role in the overall mechanical behaviour of the vessel wall, this has no incidence on the retention.

Vessel ablation by impinging jets

Ablation thicknesses of about the vessel thickness have been calculated for cases which are considered as " maximum ". However the margins are very tiny (about 20%) taking into account all the unknowns which may affect the phenomenon: initial melt temperature (superheat may be higher that 165 K), initial melt volume (may be higher than 2,5 m3 since more melt may accumulate in the more tight AP 600 structures in comparison to TMI 2).

It might be expected that it is possible to gain appreciable supplementary margins if the effects related to the presence of water in the lower head is taken into account. However this should mean that aditionnal experimental results in this field should be necessary.



T_M : melting temperature (K°) of Cd, Zn, Pb, Sn, In

FIGURE 1

AUTHOR'S CLARIFYING COMMENTS ON J.M. SEILER'S REPORT

(a) **Scenario Analysis.** Even if there was a significant internal porosity (as noted on p.O-13, item 4, the water in the lower plenum is sufficient to quench less than 40% of the debris), we can have no focusing effect because the core support plate maintains contact from above, thus preventing any significant superheating of the molten metal layer, no matter how thin it is. This is a key point, as explained on p.O-11.

(b) **Superheated Metallic Jets.** Rather than jets, metal relocation within the core region occurs as rivulets. Detailed modelling specific to the AP600 was carried out by Sienicki (ANL). The results indicate blockage formation, and they will be reported in the follow-up work on the FCI aspects of the issue.

(c) **Steel Fragilisation.** Solid state diffusion is typically characterized by diffusivities of $\sim 10^{-10}$ cm^2/s or less. For example, carbon diffusion in iron at 1000 °C takes place with a diffusivity of 10^{-7} cm^2/s. For the much larger tin atom, and the much lower temperatures in the important outer part of the vessel wall, we can easily expect diffusivities at least several orders lower. With a value of 10^{-10} cm^2/s, it would require $\sim$3 years to penetrate by $\sim$1 mm! The phenomenon is real, but irrelevant to in-vessel retention.

(d) Further perspectives on relocation phenomena leading to potential jet impingement loads are to be provided in the follow-up work on the FCI aspects of the problem. This is the natural place to address the behavior explicitly accounting for the effect of water.

FAx: Theo Sonell

PAUL SHEWMON

2477 Lytham Rd, Columbus, OH 43220
Tel. 614-457-4378 e-mail: SHEWMON.1@OSU.EDU

August 27, 1995

To: L.W. Deitrich, Director Reactor Engr. Div., ANL

REVIEW, FINAL REPORT 'IN-VESSEL COOLABILITY AND RETENTION OF CORE MELT'
by Theofanous, et al., DOE/ID 10460

I have gone over the revised version of the report as well as the reviews of several of the other reviewers in the area of Chapt. 4, Structural Failure Criteria. My comments follow:

1. I thought the initial report adequately treated the credible failure mechanisms, and the revised version has not changed this opinion.

2. I think the authors have replied appropriately to the structural failure comments from the other two reviewers with comments in this area, namely those found in App. N and O. In particular, both of these reviewers suggest the consideration of brittle fracture. I believe brittle fracture is an incredible failure mode. Such fracture would require that the temperature of the steel be below the Nihl Ductility Temp. (NDT) of the vessel and that a significant flaw be present to provide a stress riser. The material in the lower head will have an NDT well below 100C (well below 20C) when the reactor is built and any rise in NDT during subsequent operation due to fast neutron damage will be negligible. Also, the original, and subsequent, inspections of the vessel will assure that there will be no significant flaw in the lower head to aid brittle fracture.

Finally, the various fatigue mechanisms listed in App. N can be safely neglected in this analysis.

Paul Shewmon

Paul Shewmon

FAX: Sorrell
Theofanous

ARGONNE NATIONAL LABORATORY

9700 South Cass Avenue, Argonne, Illinois 60439

January 10, 1996

Dr. L. W. Deitrich, Director
Reactor Engineering Division
Argonne National Laboratory
9700 S. Cass Avenue
Argonne, IL 60439

Dear Dr. Deitrich:

Reference: T. G. Theofanous et al., "In-Vessel Coolability and Retention of a Core Melt," DOE/ID-10460, July 1995.

Attached are my comments on the peer re-review version of the ROAAM IVR report for AP600.

Sincerely,

Bruce W. Spencer

Bruce W. Spencer, Manager
Engineering Development Laboratories
Reactor Engineering Division

BWS/jaw

Attachment

cc: (w/attachment)

Dr. L. Baker, Jr. ANL

Comments on Peer Re-Review Version of DOE/ID-10460 (7/95)
Bruce W. Spencer
Argonne National Laboratory

I have reviewed sections of the revised report "In-vessel Coolability and Retention of a Core Melt" (July 1995) by Professor Theofanous et al., and I have additionally reviewed the author's responses to my comments on the preceding version of the report (Nov. 1994). I have no new questions to raise and I concur with the author's responses to my earlier comments. There are three points that have concerned me however, and I would like to elaborate on these briefly. Specifically, this includes the following points made in the report:

1) There are no intermediate debris states which can challenge the vessel integrity more than the final bounding state;

2) A reactor-material database is not needed to support the report conclusions; and

3) Vessel reflood will not affect the sequence of events.

I will address each of these three points briefly in the following.

1. Intermediate Debris States - In my earlier review I had suggested that the report could be strengthened by analyzing a few selected intermediate states and illustrating that they are bounded by the final bounding state. I specifically mentioned the downward heat transport facilitated by metal in a heat-generating particle bed with a high temperature (adiabatic) upper surface. (This is actually similar to SEI 2). To test the robustness of the report assertion, I did a simple, bounding analysis of this downward heat transport. Consistent with the melt relocation scenario introduced in Appendix O, I assumed that early relocation of molten oxide and metallic material would result in quenching and formation of a particle bed in the lower head. The available water is boiled off, and the debris bed begins to heat up. In the simple model, the metallic layer melts first and agglomerates in the available porosity forming a metallic continuum which fills the bottom of the bed up to a level which depends on the available mass of metal during the early relocation stage. Based on available data, the particulate size would be in the range 2-10 mm and the solid fraction ~ 0.65.[1] In the analysis, it was assumed for simplicity that the decay heat originated only in the oxide (1.3 MW per cubic meter of oxide material), that owing to small void dimensions, conduction rather than convection in the metal was the principal heat transport mechanism, that decay heat from the metal-filled zone only was conducted downward to the vessel bottom (higher temperature oxide region above the metal depth), and that the downward heat transport varied simply as the depth of the layer in the bottom head. A reasonable upper bound depth for this region is 0.5 R (i.e., 1m) which involves 0.5m^3 metal. The resultant peak downward heat flux, q(o) was found to be ~ 265 kW/m^2. For comparison, the Configuration II data by Prof. Theofanous and co-workers has shown a critical heat flux, $q_{cr}(o)$, of 500 kW/m^2. The conclusion is that even for an extreme case of metal serving to conduct decay heat to the vessel bottom in an intermediate, nonconvecting debris state, the margin to CHF is still ~ 100%.

2. Reactor-material Database - In my earlier review I suggested that the natural convection heat transfer database upon which the IVR work is founded be extended to include real reactor materials under realistic reactor conditions. I agree with the authors that such work probably is not required for AP600 in view of the large coolability margins that exist. I also agree with them that such tests are a good idea for their generic applicability, albeit "strictly confirmatory" for the AP600.

3. Vessel Reflood - In my earlier review I had inquired whether vessel reflood (via cavity flood depth surpassing break elevation) were a part of the 3BE sequence and, if so, what effect would reflood have on the accident sequence. The added Table 7.4 in the revised report, which is based on the AP600 Level 2 PRA, indicates that for the 3BE sequence the vessel "may reflood through break as cavity floods." The report authors addressed this in Appendix O and concluded that vessel reflooding could not affect the sequence of events. This is a strange conclusion since the indicated time of reflooding spans the time of early melt relocation to the lead-in intermediate stage, based on the cavity flooding rate with 2 - 6" pipes. It would appear that, as a minimum, this could stabilize the configuration of some of the fuel in the original core region. Of course this would be helpful, not harmful to the IVR case, and I agree with the authors in the sense that the report conclusions would not be affected.

My main concern and reason for raising the question originally is whether there is a possibility of a stratified layer steam explosion, occurring from water accumulation atop a melt layer, at a time when the vessel wall may be becoming significantly weakened by heatup and erosion. This appears to me to be a possibility in view of the potential timing of reflood and also in view of the relatively rapid vessel refill rate of ~ 2 ft/min (based on a break size equivalent to one 6" dia. pipe). If cavity flooding occurred by one rather than two 6" pipes, the vessel reflood would take place after FIBS is established, resulting in rapid water addition atop the molten metal layer at a time when the vessel walls have been appreciably thinned according to the analysis. Of course, this could be avoided by assuring the cavity floods at the rate afforded by use of the two 6" pipes.

In summary, I concur with the author's responses to my earlier comments and I concur with the report and its conclusions for AP600. I harbor a remaining concern about a steam explosion resulting from vessel reflood and would like to see that addressed in the AP600 in-vessel steam explosion assessment.

Ref. 1 B. W. Spencer, et al., "Fragmentation and Quench Behavior of Corium Melt Streams in Water," NUREG/CR-6133, ANL-93/32 (2/94)

September 20, 1995

Dr. L.W. Deitrich, Director
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439

Reference: ARSAP report on "In-Vessel Coolability and Retention of a Core Melt" prepared by T.G. Theofanous et al., DOE/ID-10460 Peer Re-Review Version, July 1995, Vol. 1 and 2

Dear Dr. Deitrich

Enclosed is my written response to Peer Review answers.

I found it very interesting to go through all the presented comments and answers to them. This has been a significant process, and I am more than glad to have been able to participate in it.

Yours Sincerely

Harri Tuomisto

Harri Tuomisto

IVO International Ltd
FIN-01019 IVO, Finland

RECEIVED
REACTOR ENGINEERING DIVISION
—DIRECTOR'S OFFICE—

OCT 9 1995

ACTION: LR

INFORMATION:

September 20, 1995

Review of "In-vessel Coolability and Retention of Core Melt"
by T.G. Theofanous, C. Liu, S. Additon, S. Angelini, O. Kymäläinen, T. Salmassi
ARSAP Report DOE/ID-10460, November 1994

Response to the Authors' Answers and to the Addenda and new Appendices as in ARSAP Report DOE/ID-10640, Vol. 1, 2, Peer Re-Review Version, July 1995

General

It was a great pleasure to read through all the comments and answers of this Peer Review. A significant effort has been carried out and a great number of good insights were brought in to the subject.

In the meantime, in Finland we have been approaching to the final steps of getting approval for in-vessel retention concept as a viable approach to Severe Accident Management of the Loviisa NPP. Critical points of an existing plant are somewhat different concerning the survivability of the reactor vessel built to the earlier standards. Ensuring the flow path availability for existing structures makes a more complex situation. As well there are plant-specific features involved in the amounts of metal components, for instance, but rather many aspects bear a similarity in nature.

Any significant criticism was not raised concerning the overall approach and application of the ROAAM method, which in my opinion is really the way to go on in the applications of severe accident assessments of plant vulnerabilities. For this peer review process the ROAAM approach demonstrates very well its strength. The questions arisen that have importance for the quantification environment, and their significance can be evaluated in the real technical terms.

I didn't find in the multitude of presented comments and answers any serious faults that would change my previous assessment for AP-600: *The treatment of the thermal regime is consistent and comprehensive, and only confirmatory research and demonstrating the effectiveness of practical design solutions is necessary.*

When the IVR concept is transferred to other plant designs, there are several points which will need a separate and additional treatment, such as:

- lower head penetrations
- significance of intermediate states (e.g. such considerations as presented by Seiler in **Sei2)**

- thickness of the metal layer
- flow path availability
- significance of thermal shocks

In the following I will shortly express my position to the obtained answers reflecting also some of the comments by other reviewers.

Viscous effects

Discussions were provided on influence of "mushy" layers to convective heat transfer near the crust (also **Hen2**). Using the liquidus temperature as a boundary condition for the oxidic pool will result in realistic assessment.

Metallic layer

The reasoning for the expected steel layer thickness is now presented in clear terms. The scenario descriptions in Appendix Q together with the enclosed drawings help to conclude that the base case can be supported. Thin metal layers are to be understood only as parametric cases to demonstrate sensitivity to the layer thickness.

Low-pressure sequences

Newly presented results from PRA and system considerations give a sufficient support for restricting the considerations only to low-pressure sequences.

Blocking of the flow paths

Newly prepared Appendix K gives further support on the flow path availability. Avoiding the flow blockages is not, however, fully demonstrated before relevant experiments will be done for the proposed flow paths. The clogging of the sump and suppression pool strainers by the insulation material and other debris is now under reinvestigation for the existing LWR plants. When these studies will be completed, the experience should be transferred to check any possible consequences on the IVR concept of AP-600. Additionally, the forces due to the flow oscillations — and possible forces due to condensing phenomena in subcooled conditions — and the flow path strength, should be experimentally checked. From the thermal loading point of view, it is clearly required that the flow paths must remain available during accidents.

Fouling of the vessel wall

Confirmatory results from future ULPU experiments should clarify any concern of the excessive fouling of the surface, which might result in overheating of the wall.

Thermal shock of the vessel

Good ductile properties of the vessel steel remove concerns of the crack propagation during thermal shock. The end-of-life RT_{NDT} is specified as 20°C at the vessel belt-line welds according to answer to **Ola 12**. On the other hand, the addendum to Chapter 7 states that the vessel manufacturer can meet the end-of-life RT_{NDT} of 20°F for the vessel steel. It should be stated clearly in Addendum to Chapter 7, what are the specified end-of-life RT_{NDT} values for belt-line welds, base material and any possible weld in the lower head region.

During the review process of the Loviisa IVR concept, another question was raised concerning any possible hardening of the vessel steel that could result in the additional stresses and the brittle behaviour of the vessel. Hardening process could take place in the vessel steel heated up to over 800°C and cooled later rapidly. Two possibilities were identified: hardening of the lower head steel after relocation and quenching, and hardening of the vessel wall around the metal layer area after recovery of water injection to the vessel. Hardening of the lower head was deemed possible only in a thin inner layer of the wall. The latter case, I suppose, will be taken into account in the forthcoming report that includes the FCI phenomena relevant to the IVR concept.

Harri Tuomisto

Harri Tuomisto, Dr.Tech.

AEA

AEA Technology

Dr L W Deitrich
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439
USA

Winfrith
Dorchester
Dorset DT2 8DH
United Kingdom
Tel: (44) 1305 251888

Extension 3029

Direct Facsimile
01305 202508

18 August 1995

Dear Dr Deitrich,

Review of authors' responses to Review Comments:

In-Vessel Coolability and Retention of a Core Melt,
T G Theofanous, C Liu, S Additon, S Angelini, O Kymäläinen and T Salmassi
DOE/ID-10460

I have considered the authors' responses to the comments I provided, looked briefly at their response to other reviewers, and scanned the revised report as a whole. With the time limitation, this is not as thorough a review as I would have liked to perform given the amount of work that is presented. However, you may recall that in my initial review I expressed support for the conclusions that the authors drew and the recommendations they made, and I see no reason to change this judgement. It is clear though that this report cannot be considered to be complete until the proposed ULPU Configuration III tests have been reported.

I will now deal with the authors' responses to my comments, using their numbering system:

Tur1 No comment needed.

Tur2 (Insulation, engineering and PSA aspects). It is encouraging that the insulation design is now taking account of the proposed cavity flooding, and that the revised PSA results indicate a higher fraction of sequences for which in-vessel retention is an option. As indicated above, results of ULPU experiments with the proposed thermal insulation placements are awaited.

Tur3 No comment needed.

Tur4 (Dependence of assessment on high critical heat fluxes found in ULPU). While the new data for ULPU in Configuration II are welcome, and show relative insensitivity to the natural circulation rate, confirmatory experiments in Configuration III are highly desirable.

Tur5 No comment needed.

Tur6 (Does the metal layer segregate - is it less dense?) This question has been addressed adequately for this application.

Tur7 (Multi-component effects). The conditions at the pool/crust interface are still unclear for multi-component systems (eg the form of dendrite growth into the principally liquid region). As natural convection heat transfer, particularly in turbulent conditions, is governed by density differences across relatively thin boundary layers, some effects may be expected. I don't anticipate they will be sufficiently large to affect this application significantly, but I do still think it is an area where confirmatory experiments with real materials are desirable.

Tur8 (Asymptotic relation between *Nu* and *Ra*). An academic argument, but in terms of anticipated asymptotic behaviour I agree with Cheung. It is of no consequence for the assessment. I support the general point of the authors that a wider database for convection can be used than that obtained from volumetrically heated experiments.

Tur9 No comment needed.

Tur10 (Justification of ACOPO). I welcome the authors' comments.

Tur11 (Correlation of experimental data). I note the authors' clarification.

Tur12 (Data extrapolation). The authors acknowledged that strictly speaking these are extrapolations not absolute bounds.

Tur13 No comment needed.

Tur14 (Simplified model - figure captions). I accept that more detailed calculations may justify the omission of back radiation. I have been able to use the revised figures (though not to the accuracy given on page 5-19). However, the last sentence of the caption of Figure 5.9 seems to be wrong: replace q^*_{up} by T^*_b ?

Tur15 (Convection in prototypic materials). I did not find the radiation explanation for the ANL experiments convincing - remembering that if it is transparent it is also a poor emitter at these wavelengths. The SCARABEE-N data, as noted, points in the opposite direction with enhanced upward heat fluxes, attributed by Kayser (Grenoble Workshop, 1994) to not forming an oxidic crust. This ought to be commented on. I agree that all experiments with prototypic materials should be interpreted with caution because of the difficulties of conducting such tests.

Tur16 (Is IVR an add-on for AP-600). The new material does now indicate the in-vessel retention is being integrated into the AP-600 design.

Tur17 (Melt progression). The new Appendix O is useful - particularly the information on the lower zirconium plugs and the structure of the reflector. While I consider the relocation scenario presented highly plausible, I do not think that one is currently in a position to 'guarantee' (page 2-5) a particular scenario. My concern would be interactions between the predominantly oxidic melt and the predominantly metallic blockage, leading to downward progression of the oxide and the sources of decay heat. However, although possible thermodynamically, there is no evidence for such interactions in the two Sandia ACRR MP tests, which could be quoted in support of the authors' scenario.

Tur18 (Is the steady state bounding?) The clarification given by the authors should be reflected in the text.

Tur19 No comment needed.

Tur20 (Cooling of the vessel). A comment on the initial cooling of the lower head during depressurisation should be incorporated, as data presented in Appendix F1 suggests that with the vessel at normal operating temperature, film boiling would be expected if the cavity were flooded.

Tur21 (Metallic fission products). This is dealt with now in Appendix P.

Tur22 (Crusting temperature). Following Seiler's recent work I am revising my views somewhat on what is the correct temperature for the crust-melt interface (at least for in-vessel considerations). However this issue was only of academic interest and the Zr-Fe interactions have been dealt with in Appendix J.

Tur23 (Crust instability). The large horizontal dimension and lack of supports are noted, as is the fact that crust material (UO_2 - rich) is likely to be significantly denser than the pool itself. See previous reference to SCARABEE N interpretation.

Tur24 (Adequacy of model). I accepted the model as suitable for the purpose envisaged. The question remains - what is the complete end-state inside the vessel as this may need to be maintained for many days, or be considered if in-vessel quenching is attempted at a later stage.

Tur25 (Use of the whole UO_2 inventory). As the authors concede this is a matter of judgement. The new material indicates that the margins in the original draft were conservative.

Tur26 (Metal inventory). I accept the clarification on the design. What concerns me about over-restrictive ranges (when they are not necessary) - eg the range 77 to 87 tons is 'outside the spectrum of reason' [see Appendix A] - is that it is an unnecessary hostage to fortune.

Tur27 (Sienicki's material). Now included.

Tur28 (Correlation of Zr oxidation). I still believe the authors overstate the position. I would say it is reasonable to assume that zirconium oxidation is effectively independent of τ.

Tur29 (How extreme is the extreme case). I note the larger margin expected.

Tur30 (Uncertainty in ULPU data). Confirmation from Configuration III tests is still required.

Tur31 No comment needed.

Tur32 (Jet coherence). No comment needed.

Tur33 (Overall conclusions). I accept the authors' comments.

Tur34 (Long term retention). As noted above it would be interesting to see the prediction of the long term evolution of the melt with time and the considerations for late addition of water.

I do not feel that my comments above merit a further round of response, but would be happy to have then on the record.

Yours sincerely,

Brian D Turland

Brian D Turland

AEA

AEA Technology

Dr L W Deitrich
Reactor Engineering Division
Argonne National Laboratory
9700 South Cass Avenue
Argonne, Illinois 60439
USA

Winfrith
Dorchester
Dorset DT2 8DH
United Kingdom
Tel: (44) 1305 251888

Extension 3029

Direct Facsimile
01305 202508

19 October 1995

Dear Dr Deitrich,

Additional Comments:

Review of authors' responses to Review Comments:

In-Vessel Coolability and Retention of a Core Melt,
T G Theofanous, C Liu, S Additon, S Angelini, O Kymäläinen and T Salmassi
DOE/ID-10460

Following the increase in my allocation for review of the authors' responses, I have looked through the report as a whole and taken account of the new information supplied on the results from the ULPU Configuration III tests. This has not caused me to change my original assessment to any great extent. As you may recall, in my initial review I expressed support for the conclusions that the authors drew and the recommendations they made. I see no reason to change this judgement.

On this read through a few additional comments occurred:

1. In the ROAMM definitions (Appendix A and their use in Section 7) it should be made clearer that a process likelihood of 'x' encompasses the assigned probability of all parameters that represents behaviour at the specified level. In other words:

   90% of the overall probability represents the expected range of the parameter.

   99% of the overall probability represents the expected range of the parameters **plus** behaviour that is within known trends but obtainable only at the edge of spectrum.

   etc.

Looked at this way a probability distribution is being fitted through a number of points, constrained by the need to make the cumulative probabilities satisfy the above criteria. In some cases, the simple approach of piece-wise uniform distributions may be adequate, in others a smoother representation would be more desirable and might impact the assessment (but not for APR-600 ex-vessel cooling).

2. I would expect the vessel wall temperature T_v to be an important parameter, but I missed where its value was discussed. The sensitivity calculation with an adiabatic top largely addresses the concern here that T_v may be sufficiently high to significantly suppress radiation from the top surface. (This issue is certainly important for the assessment of higher power reactors.)

3. The discussion of reflector and core barrel failure during core melting seems to imply a global failure. In TMI-2 the upper melt pool does not appear to have formed symmetrically within the core barrel, and thus led to a preferential pour down one side; this may apply to other designs.

4. The jet impingement correlations cited in Chapter 8 and Appendix H are different. For the scenarios calculated in Appendix H, use of the Chapter 8 heat transfer correlation would give Nusselt numbers more than twice those given in Table 2 of Appendix H and leads to predictions of complete vessel erosion. Calculations I have performed in this area have been referred to. We used essentially the same correlation as Appendix H and have found it to fit the experimental data reasonably well. Bearing this uncertainty in heat transfer in mind, the reality of the impingement scenarios merits greater scrutiny (even if complete erosion is not anticipated).

I have also reviewed the responses I gave to the authors' replies in my letter to you of 18 August 1995. Additional responses are given below, using the authors' numbering:

Tur4 (Dependence of assessment on high critical heat fluxes found in ULPU). The results obtained in the ULPU Configuration III support the use of the data based on Configuration II. However, along with some other reviewers I would have liked to see uncertainty in the CHF acknowledged in the ROAMM process.

Tur6 (Does the metal layer segregate - is it less dense?) I note that it is assumed that Zr will inevitably go to the metal layer, while as other reviewers pointed out more complex distributions of species are possible. However, the additional sensitivity calculations illustrate that similar phenomena have only a small effect on the overall margins.

Tur7 (Multi-component effects). The authors contention that 'basic molecular diffusion considerations lead to the conclusion that such multi-component effects could not affect natural convection in any significant way' should be supported by references. Molecular diffusion acts to overcome compositional gradients, but effects at cooled boundaries can act to establish them. In my view, based on our experiments, it is bulk turbulence that breaks up stratification due to compositional effects.

Tur26 (Metal inventory). To push this further, we are told on page 7-16 that a modified scenario developed to give 'an extreme specification' provides a reasonable lower bound of 17 tons of metal. However, the quantification in Fig 7.5 shows no probability of getting less than 67 tons of metal. Now one may argue that this is indeed the case for the end-sate with full relocation of the oxide, but if this is not the worst case, then other possibilities should be considered in the main quantification.

Tur30 (Uncertainty in ULPU data). Confirmation from Configuration III tests has now been achieved.

As previously, I do not feel that my comments above merit a further round of response, but would be happy to have then on the record.

Yours sincerely,

Brian D Turland

Brian D Turland

AUTHOR'S CLARIFYING COMMENTS ON B.D. TURLAND'S REPORT

(a) **Tur14.** The last sentence in the caption of Figure 5.9 is correct.

(b) **Tur18.** Clarification made by a new footnote on page 2-2.

(c) **Tur20.** Footnote added on page 3-1.

(d) **Tur23.** Molten ceramic simply cannot exist in contact with a metallic melt. So, even if we could imagine that the crust was broken up locally, freezing would be essentially immediate, and the resulting large-in-lateral-dimension crust could not sink. As far as the relevance of the SCARABEE experiments (the reviewer probably refers to test BF1 discussed by Kayser — p.207 the Proceedings OECD/CSNI/NEA Workshop on Large Molten Pool Heat Transfer, Nuclear Research Center, Grenoble, France, March 9–11, 1994) it should be emphasized that the test for which the **possibility** of a molten oxide on the top was proposed was powered at 140 MW/m^3 — that is **one hundred** times the power densities of interest here!! Even so, the authors could not exclude other explanations, "for instance the effect of lateral heat transfer ... and, to some degree, uncertainties in physical properties."

(e) **Tur34.** This is considered in the more germane context of the FCI aspects of the problem.

(f) **#1 of Addl. Comments.** Clarification made as a footnote on page A-6.

(g) **#2 of Addl. Comments.** Inner vessel wall and core barrel temperatures can be found in Figures Q.10. Because of the very large surface area available heat can be readily dissipated without causing a significant back-radiation effect.

(h) **#3 of Addl. Comments.** We can expect a significantly more symmetrical behavior here because of the large retaining capacity of the reflector (compared, for example, to TMI-2).

(i) **#4 of Addl. Comments.** It is acknowledged that the correlation used in Chapter 8 is for turbulent flow, and may be conservative. On the other hand, the parameters chosen in Appendix H were such as to test the margins to failure (based on the correlation used therein). So, it is not appropriate to combine these with the correlation in Chapter 8. It is stressed that the purpose of both of these analyses was to provide, independently, some rough bounding perspectives on how difficult it is to produce failure by melt impingement. Further elaboration of this issue, with more details and consistent physical models, will be provided in follow-up work, on the FCI aspects of the problem, where these details are of far greater significance.

(j) **Tur7.** We miscommunicated, while trying to express the same idea. Molecular diffusion was mentioned for its "slowness" (in the liquid state) rather than as a way to overcome compositional gradients. We agree, and as noted already, we cited the Schneider-Turland experiment.

(k) **Tur26.** The distribution in Figure 7.5 refers to the end-state, while the discussion of page 7-16 refers to an arbitrary parametric, aimed to specifically address the margins to failure due to focusing. We argue that the final state bounds the thermal loads. This can, in principle, be violated due to extreme focusing, so we examined this question separately by means of basic relocation behavior (Appendix O) and parametric evaluations (Appendix P). We do not think it is appropriate to approach this problem by trying to model the whole evolution of the metal layer thickness, on top of an evolving oxidic debris, nor do we think it is necessary.

APPENDIX V-2

[[THE FIRST RESULTS FROM THE ACOPO EXPERIMENT]]

Proceedings, PSA'96–International Topical Meeting on
Probabilistic Safety Assessment
Park City, Utah, September 29–October 3, 1996

[[APPENDIX V-2]]

THE FIRST RESULTS FROM THE ACOPO EXPERIMENT

T.G. Theofanous, M. Maguire, S. Angelini and T. Salmassi

Departments of Chemical and Mechanical Engineering
Center for Risk Studies and Safety
University of California, Santa Barbara, CA 93106

ABSTRACT

The ACOPO experiment simulates natural convection heat transfer from volumetrically heated pools, at a half-scale reactor lower head geometry (hemispherical). Data for Rayleigh numbers of up to $2 \cdot 10^{16}$, from the first round of experiments, are presented in this paper. The results are in substantial agreement with those of the mini-ACOPO proof-of-concept experiment. Moreover, it is shown that these ACOPO results confirm a key component of the in-vessel retention capability for an AP600-like design, as recently established in DOE/ID-10460.

V.1. INTRODUCTION

The purpose of this paper is to present the first experimental data of natural convection heat transfer for the range of Rayleigh numbers, $10^{15} - 10^{16}$, directly relevant to a severe accident management concept known as "in-vessel retention" (IVR). The geometry is illustrated in Figure V.1, and involves a volumetrically heated oxidic pool, and a lower head that is externally submerged in water. For an AP600-like design, the diameter is $\sim$4 m, the decay power $\sim$1.3 MW/m^3, and the distribution of Rayleigh numbers, accounting conservatively, for uncertainties, is as illustrated in Figure V.2 (Theofanous et al., 1996). By comparison, previous data were limited to $\sim 10^{14}$ in the UCLA experiments (Asfia and Dhir, 1996), to $\sim 7 \cdot 10^{14}$ in the mini-ACOPO experiment (Theofanous and Liu, 1995), and to $\sim 10^{15}$ for the COPO experiments (Kymäläinen et al., 1997); that is, lacking by about one order of magnitude. For larger reactors there may be a need for almost another order of magnitude, to $\sim 10^{17}$. Besides this practical need, there are also some interesting fundamental questions on the behavior as $\mathrm{Ra}' \to \infty$.

As explained in detail before (Theofanous et al., 1996), the present practical need is to determine (a) the energy flow split between the upper (flat) and lower (hemispherical) boundaries, and (b) the shape factor along the hemispherical boundary, so that local heat flux conditions can be determined from the area-average value. Also, it was explained that the problem is completely determined from the shape, and the isothermal boundary conditions (due to the presence of crusts), and that it

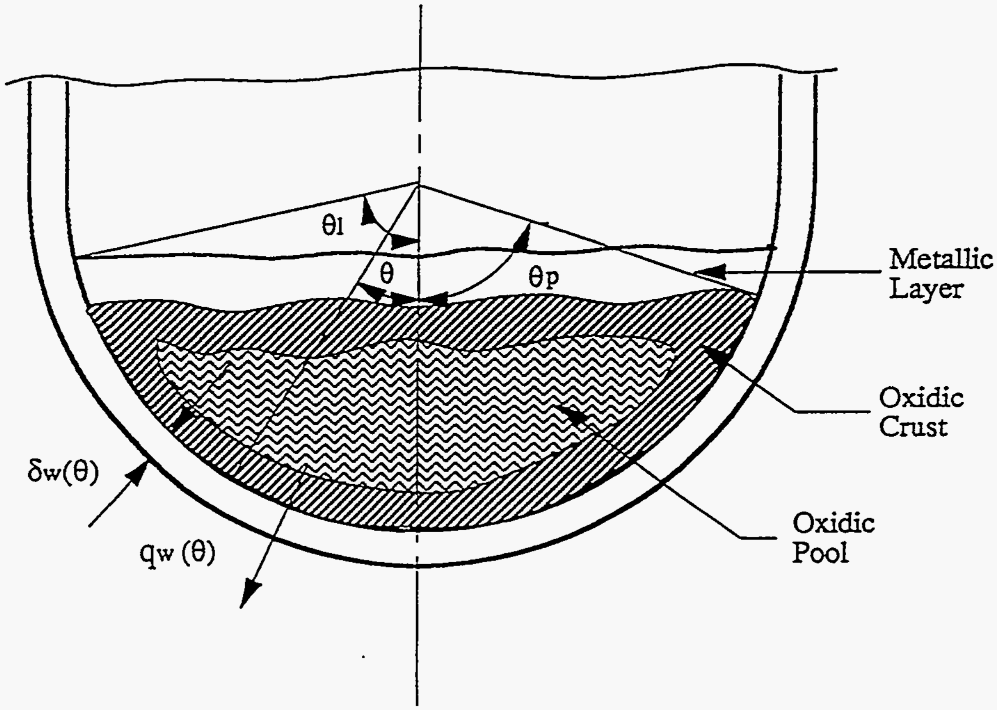


Figure V.1. Schematic of the in-vessel retention geometry (Theofanous et al., 1995). The lower head is externally cooled by boiling water.

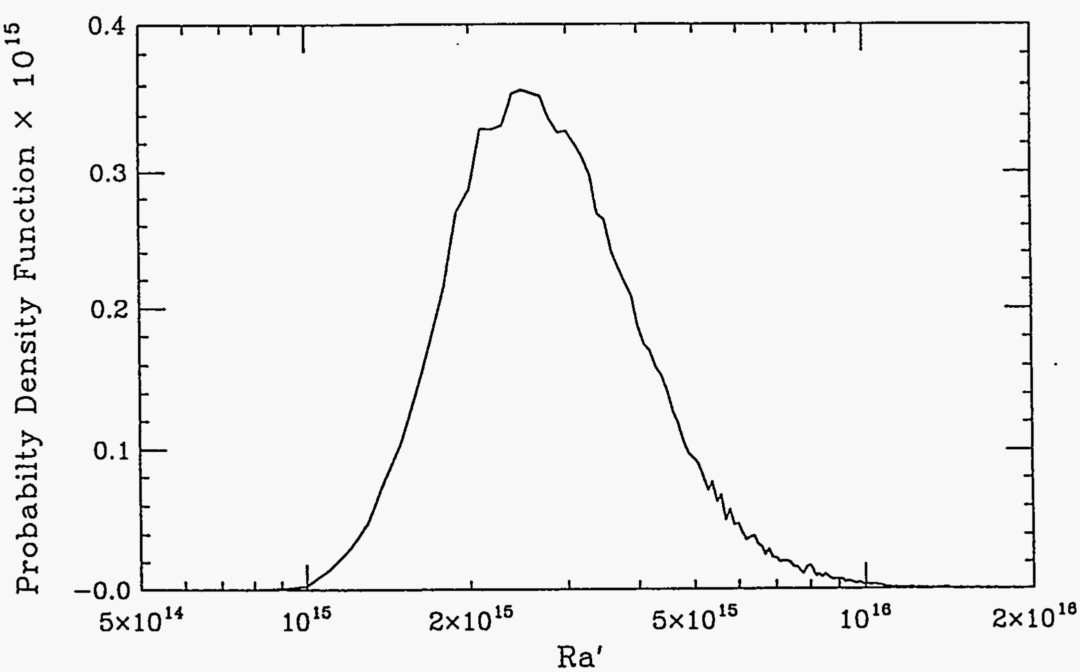


Figure V.2. The Ra′ number distribution found in calculations assessing the in-vessel retention concept for an AP600-like design (Theofanous et al., 1995).

is properly and completely scaled by the Rayleigh number (Ra$'$), with the Pr number having only a minor independent effect

$$\mathrm{Nu} = f(\mathrm{Ra}') \qquad \mathrm{Ra}' = \frac{g\beta\dot{Q}H^5}{\nu\alpha k} \tag{V.1}$$

The long-standing difficulty in reaching the range of Ra$'$ numbers of interest, experimentally is due to the strong dependence on the length scale, and the difficulty of producing uniform volumetric heating at large enough scales and hemispherical geometries. For example, with a radiation method (such as used in the UCLA experiment) uniformity of power deposition requires a low-coupling system ("transparent fluid"), which really limits the magnitude of power depositions and the pool superheating possible. On the other hand, for direct electrical heating, power uniformity requires a parallel electrode configuration (as in the slice geometry of COPO), which rules out the hemispherical geometry of interest.

The ACOPO idea bypasses this difficulty, by using the internal energy of the fluid, preheated to some high initial temperature, to simulate volumetric heating, by suddenly cooling the boundaries and interpreting the transient system cool down as a sequence of quasi-stationary natural convection states. That is, from the local instantaneous fluxes at the boundaries, a total heat loss rate can be obtained to define the instantaneous Rayleigh numbers, which then are correlated to the instantaneous Nusselt numbers. The idea is that the cool down would be arrested, and nothing would really change, if at any instant in time during the cool down, a volumetric heating rate could be supplied that was equal to the then heat loss rate. The mini-ACOPO experiments confirmed that this idea actually works. The present experiments provide additional, definitive evidence that this is so.

The mini-ACOPO test section has a diameter of 0.4 m (1/8 scale) and reached Ra$'$ numbers of $7 \cdot 10^{14}$ and $3 \cdot 10^{13}$, using Freon 113 and water as working fluids, respectively. The ACOPO test section has a diameter of 2 m (1/2 scale) and with water it reached a Ra$'$ number of up to $2 \cdot 10^{16}$.

V.2. THE EXPERIMENTAL FACILITY

The ACOPO experiment is a large version of the mini-ACOPO, the basic design of which is illustrated in Figure V.3. The figure shows the individual cooling units, the insulation between them, internal fluid temperature measurement locations, and the expansion volume needed to accommodate the fluid during the transient, while keeping the vessel completely full. In the ACOPO, construction details were much more involved, and actually building the facility proved to be a major challenge. Some perspectives of the sheer size of the project are provided in Figures V.4—V.8, which will also be used to explain its key components.

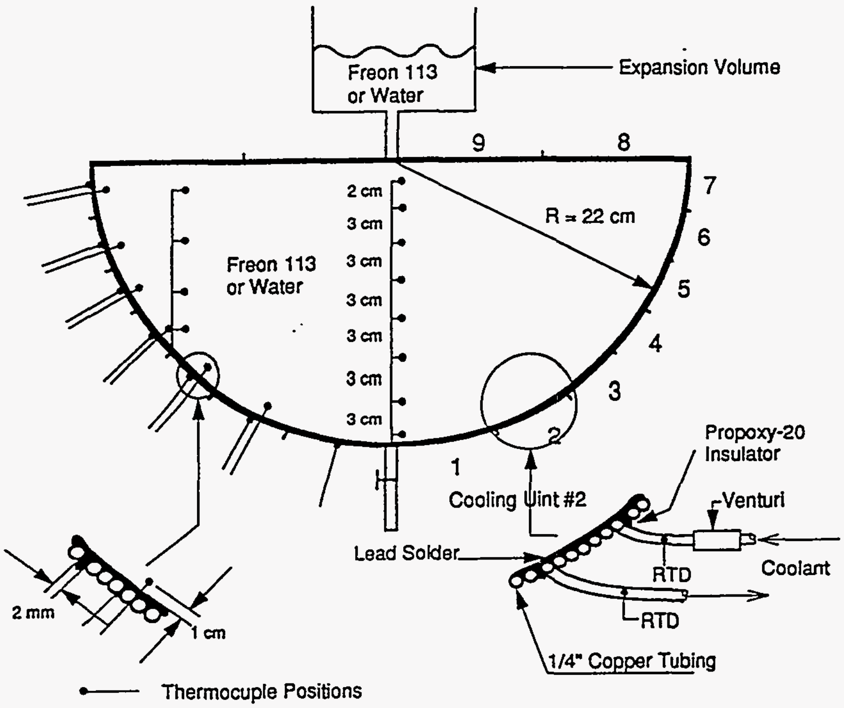


Figure V.3. Schematic of the mini-ACOPO experiment, including the key construction details and instrumentation.

Starting with Figure V.4, we can see the test vessel (shown, in the photo, prior to insulation), the pump and venturi racks, the heat sink tank, the temperature instrumentation locations, and the data acquisition and experiment control system. The heat sink is a large stainless steel cylindrical tank (2 m x 3.5 m), loaded with ice (see Fig. V.8), so as to maintain a constant water temperature at ~0 °C. There are 15 cooling units, 10 on the lower and 5 on the upper boundaries, that constitute the vessel wall, as shown in Figure V.7. Unlike the mini-ACOPO, here each cooling unit is independently fed by a respective pump (see Fig. V.4), whose speed is controlled such as to maintain the cooling unit operating at a near-optimum for the instrumentation. The object, as discussed in the next section, is to keep the walls as nearly isothermal as possible, and yet obtain a ΔT in each cooling unit that is large enough to minimize measurement error.

The ACOPO test vessel is shown in Figures V.5, V.6, and V.7. Each cooling unit was manufactured separately by welding together properly bent rings of square copper tubing (1/2-inch on the side), so as to make an effectively seamless internal surface. Within each cooling unit the rings could communicate, so that with a single inlet and outlet, the flow would traverse through all the rings. The whole vessel lower and upper parts were then built by putting together the cooling units, with special silicon rings between them for thermal insulation, on wooden supports, as shown in Figures V.6 and V.7. The test vessel was well insulated on the outside, and special care was taken

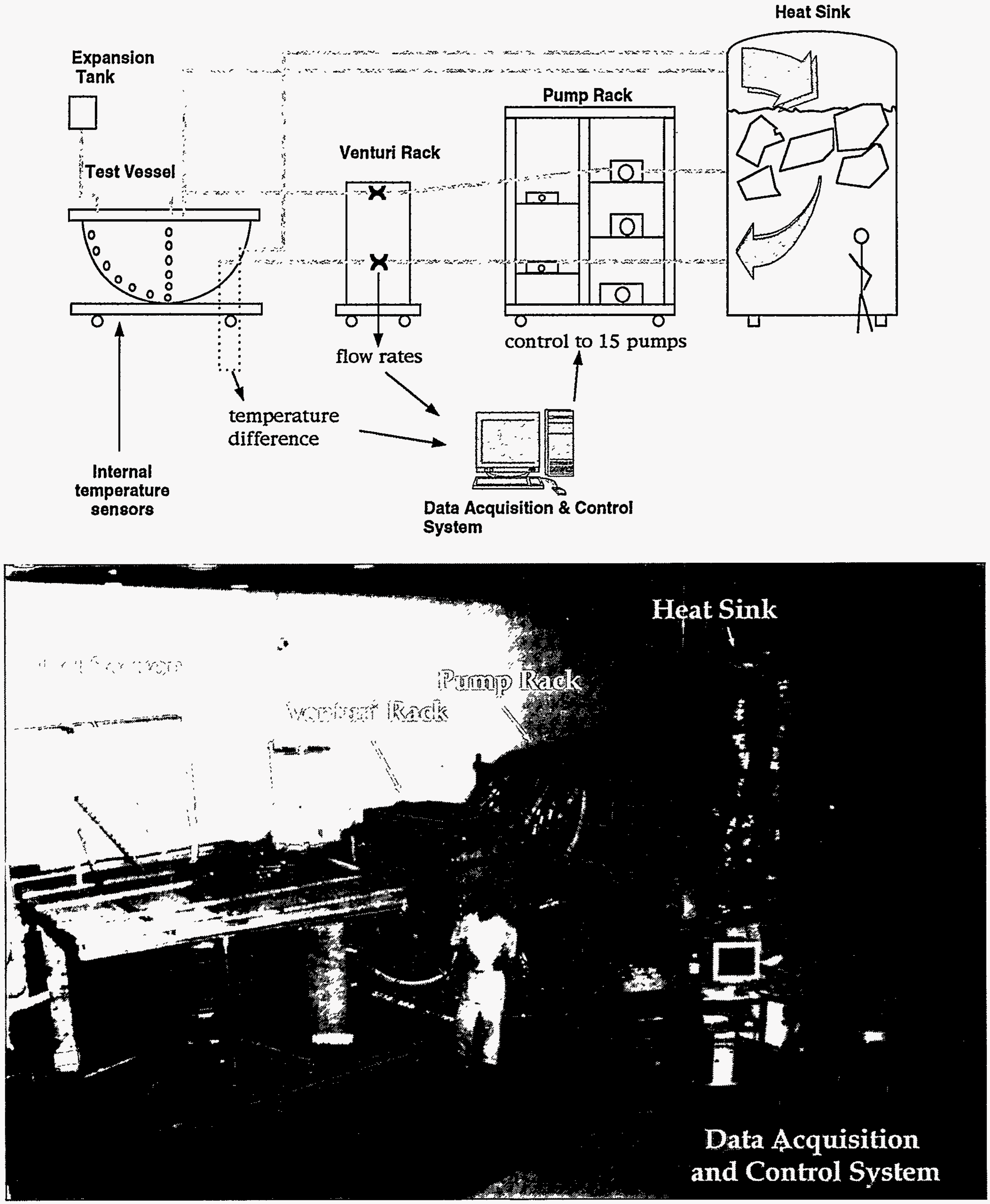


Figure V.4. The ACOPO half-scale facility.

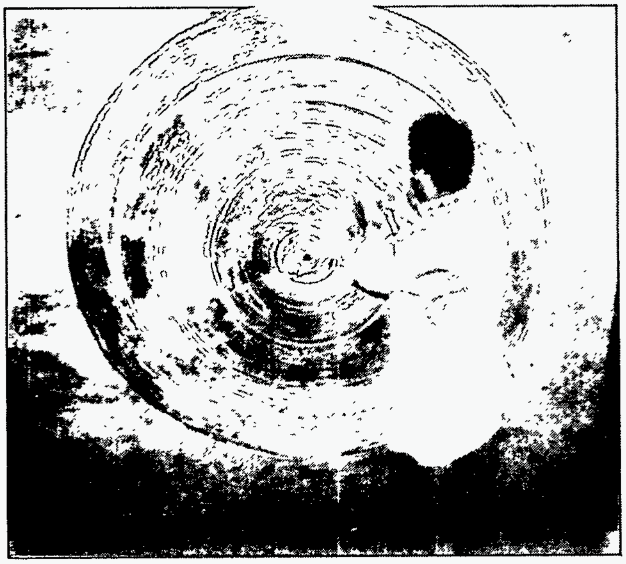


Figure V.5. The ACOPO test vessel lid in the final stages of polishing.

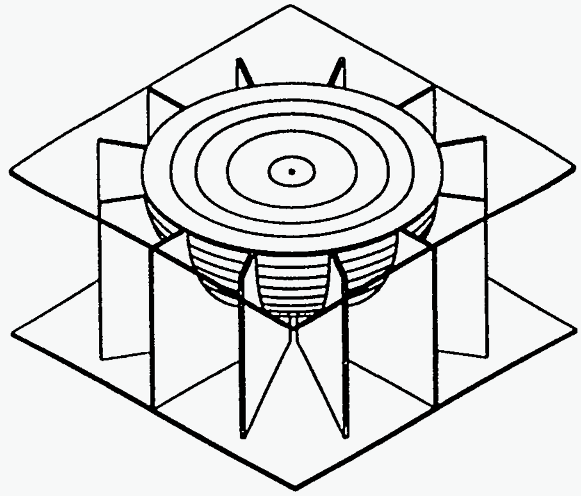


Figure V.7. Schematic of the ACOPO test vessel, showing the individual cooling units and the vessel support.

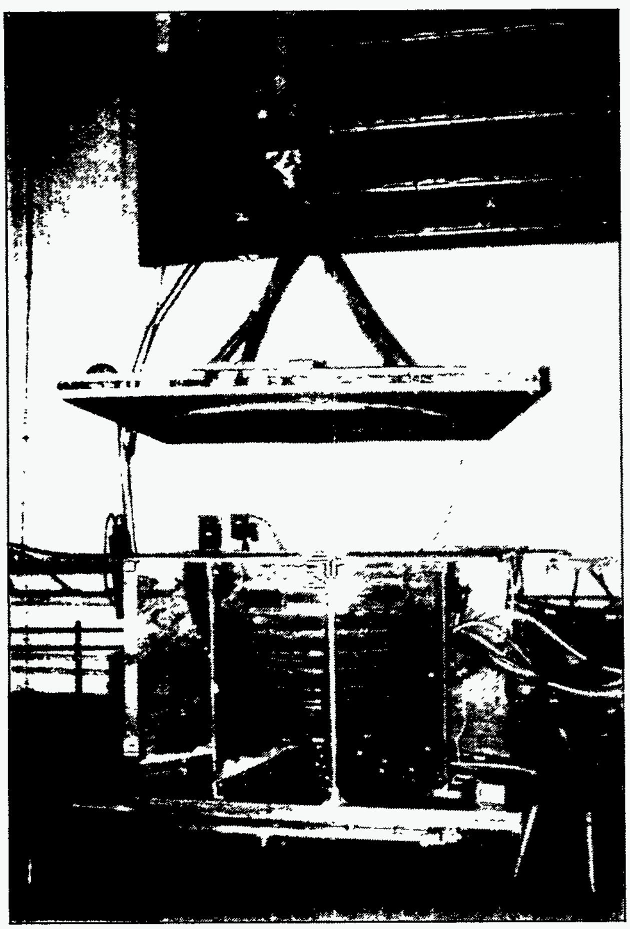


Figure V.6. The ACOPO test vessel lid being lowered upon the ACOPO test vessel.

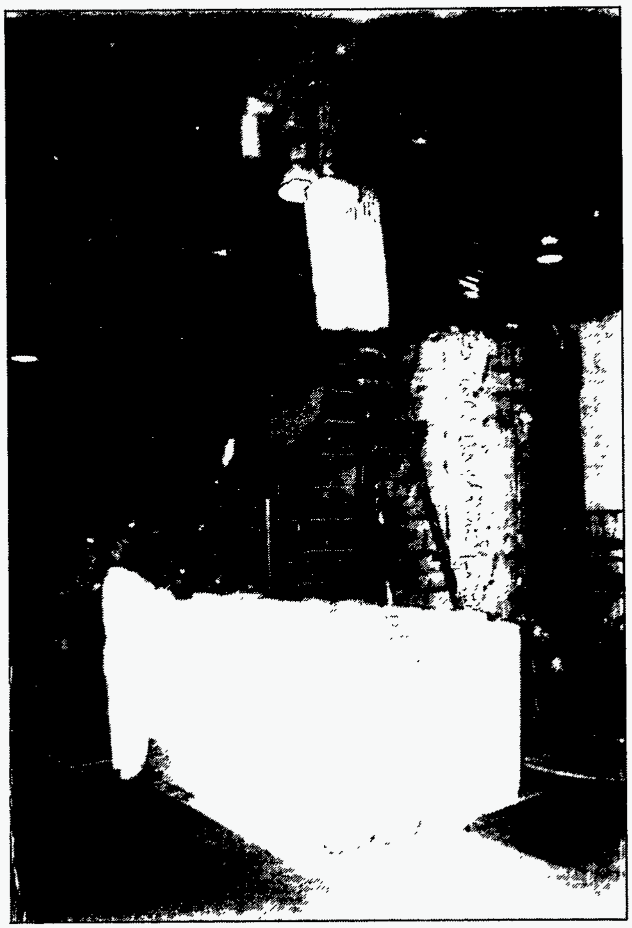


Figure V.8. Load of ice being transferred to the ACOPO heat sink vessel prior to a run.

that it is not connected to any thermal masses that could introduce external heat flow to the cooling units during operation.

V.3. MEASUREMENTS AND OPERATION

As noted above, the key aspect of the operation is in regards to balancing measurement accuracy against the required condition for isothermal boundaries of the test vessel. This was resolved as follows. With a maximum fluid-to-wall temperature difference of the order of ~100 K, it was decided that the isothermal condition would be satisfied well enough if the cooling units operated, inlet-to-outlet, within a few degrees K. This then led to a requirement for measuring this temperature difference with an accuracy of better than 0.1 K. For this purpose, we chose thermistors, with a quoted accuracy of ±0.1 K. The bulk fluid temperatures were measured with chromel-alumel thermocouples to an accuracy of ±1 K. Thermocouples, thermistors, as well as the venturis used for flow rate measurement, were calibrated in situ, using the complete data acquisition system, and were found to perform very stably throughout this first experimental campaign. As shown from a typical energy balance in Figure V.9, the overall accuracy is much better than 10%, which for an experiment of this size is deemed quite satisfactory.

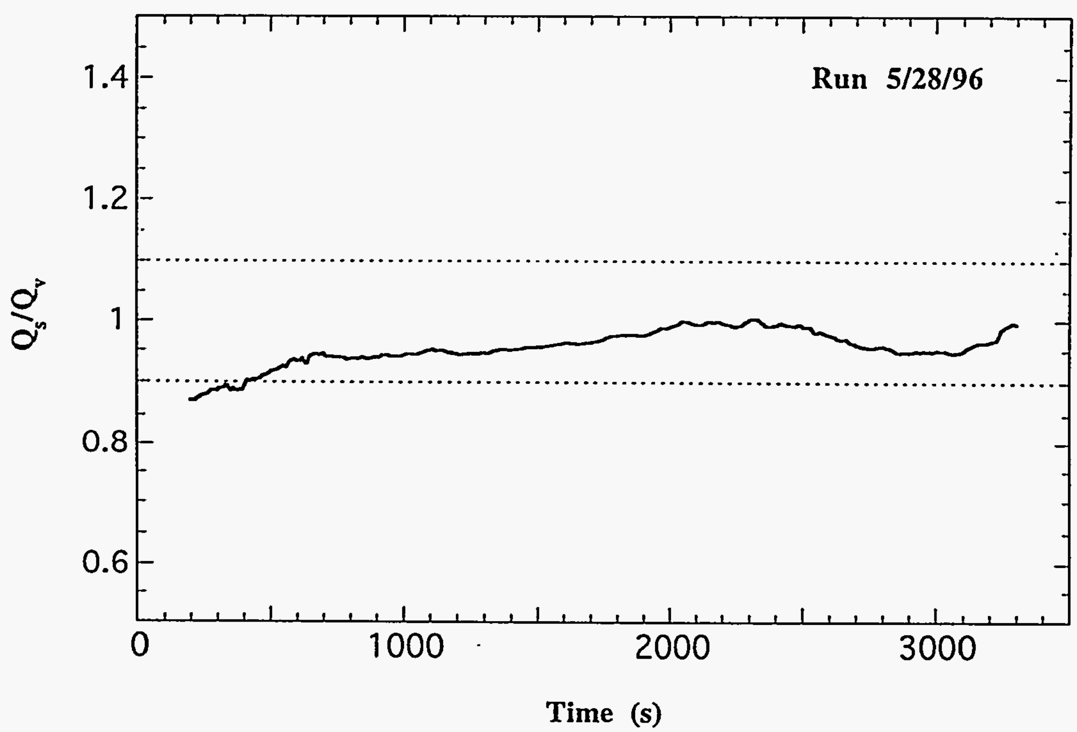


Figure V.9. The overall energy balance for Run 5/28/96. Q_s is the total heating rate of the cooling circuits, and Q_v is the total cooling rate of the vessel contents.

A run was begun by heating the vessel contents, to some high temperature near 95 °C, very slowly, by recirculating the contents through an external heater. The water level was then adjusted to a few centimeters below the top lid, and steam was injected into the freeboard volume while also allowing for an exhaust, until the temperatures in this upper region reached 100 °C. This freeboard volume was then isolated, and immediately connected to the expansion tank, thus allowing this volume to fill, by the draining, of degassed, 100 °C water. This procedure ensured that there would be no air trapped, as bubbles, in the underside of the vessel lid. The cooling circuits were then switched on, to initiate the cool down, which was continued until measurement accuracy was lost, typically about 1 hour later.

Data were recorded by a PC at a rate of 0.5 Hz, and were reduced with an interfacing computer program using a local smoothing routine before taking the time derivatives needed. All thermophysical properties were evaluated at the "film" temperature, i.e., the average value between the bulk and the wall. The energy balance was well within the 10% error bounds, as shown in Figure V.9, and all data in fact were highly reproducible, as shown in Section V.4.

V.4. EXPERIMENTAL RESULTS

A total of five experiments have been run so far, in the manner described above. A typical transient of the Rayleigh number is shown in Figure V.10, and a typical comparison of the heat flux shapes with the correlation obtained from mini-ACOPO is provided in Figure V.11. The data variation around the correlation in this figure is also typical of what was found in mini-ACOPO; i.e., the correlation represents a fair representation through the middle of the data.

The upward heat transfer from Run 5/28/96 is compared to the Steinberner and Reineke (1978) correlation in Figure V.12. The trend of the data veering off the correlation for $Ra' > 10^{13}$ was already slightly evident in the mini-ACOPO data, but it is quite clear now with the range extension by more than one order of magnitude. In this upper range of $10^{15} < Ra' < 10^{16}$, the data seem to indicate a Rayleigh number exponent near 0.2. This is the highly turbulent regime, and there has been some question of whether it should tend asymptotically to 0.2 or 0.25 (see Chapter 5, and the section on Natural Convection in Appendix U, of DOE/ID-10460). By a Ra' number of 10^{16} the deviation from the Steinberner-Reineke correlation is already significant. As shown in Figure V.12, the data from Run 5/28/96 can be well correlated by

$$Nu = 1.95\, Ra'^{0.18} \qquad (V.2)$$

which is shown in relation to all ACOPO data in Figure V.13. An essentially tight bound of $\pm 10\%$ is observed.

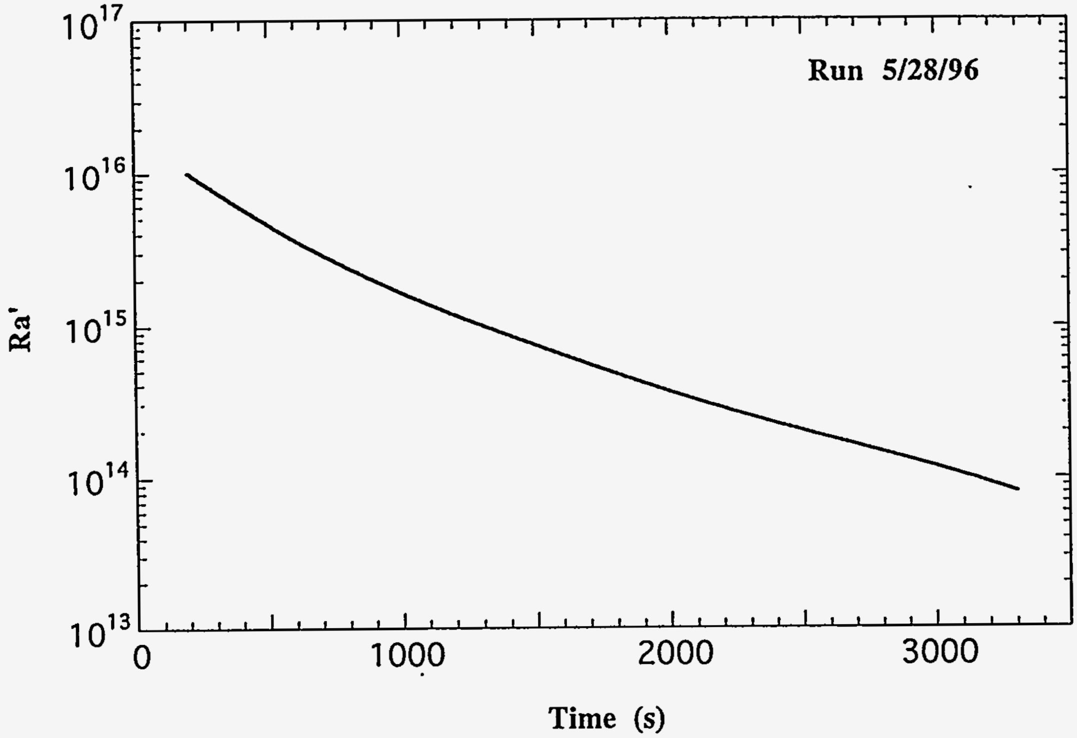


Figure V.10. The Rayleigh number transient in ACOPO Run 5/28/96.

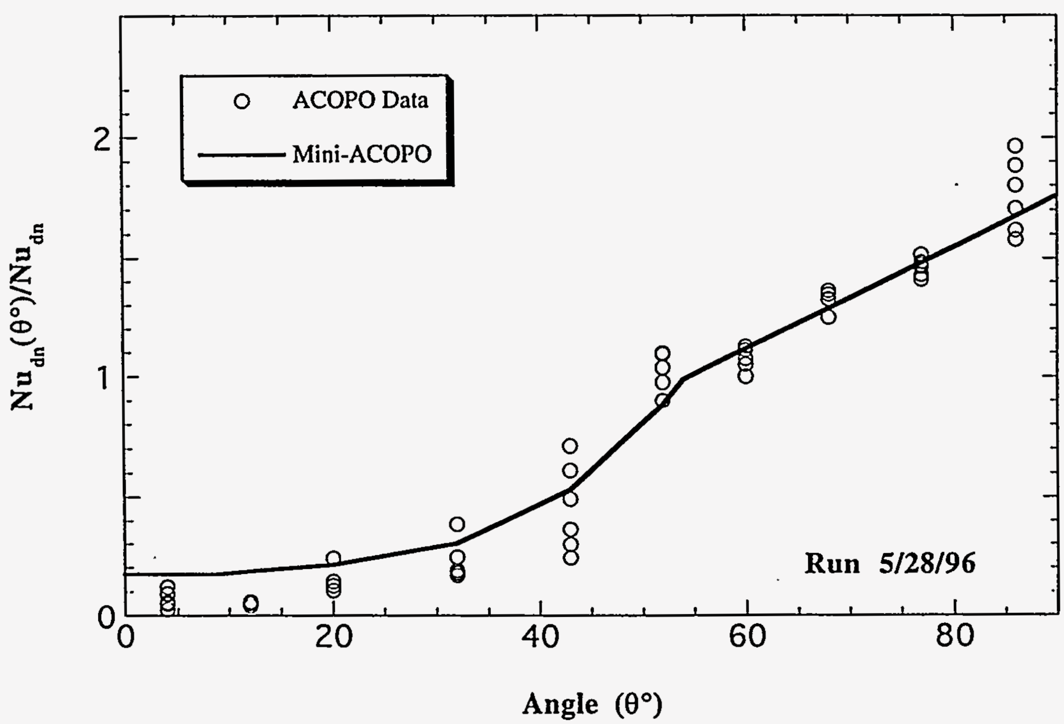


Figure V.11. The heat flux distribution along the lower boundary in ACOPO Run 5/28/96 compared with the correlation obtained from mini-ACOPO. Data shown only every 600 s (for clarity), for the duration of the run.

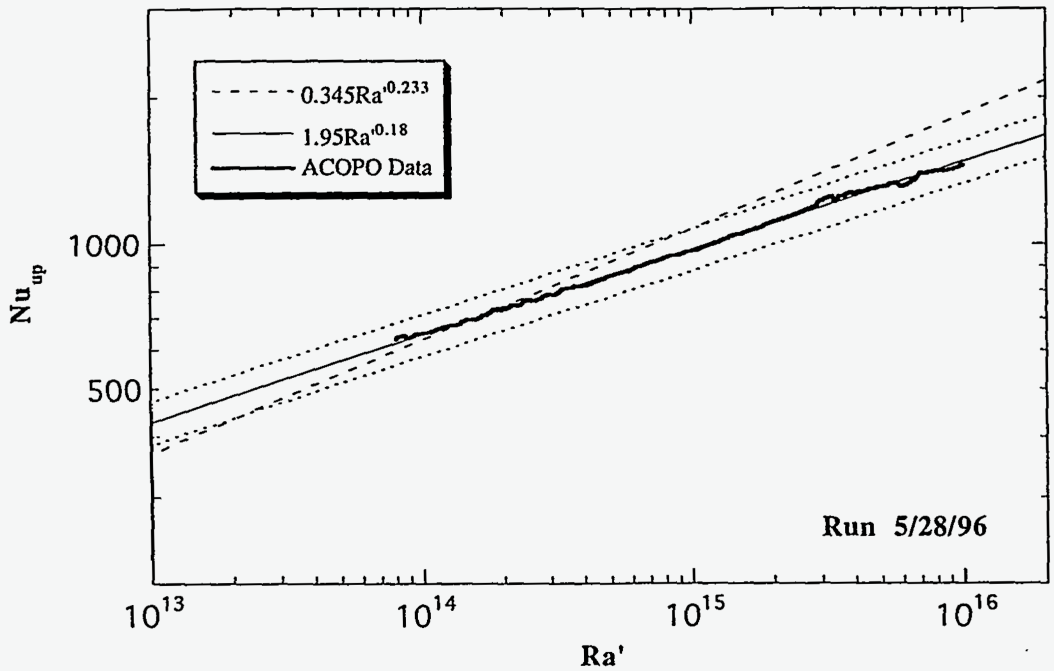


Figure V.12 Upward heat transfer from ACOPO Run 5/28/96 compared to the Steinberner-Reineke correlation. The dotted line shows the ±10% margins on the correlation. The solid line shows the present data fit.

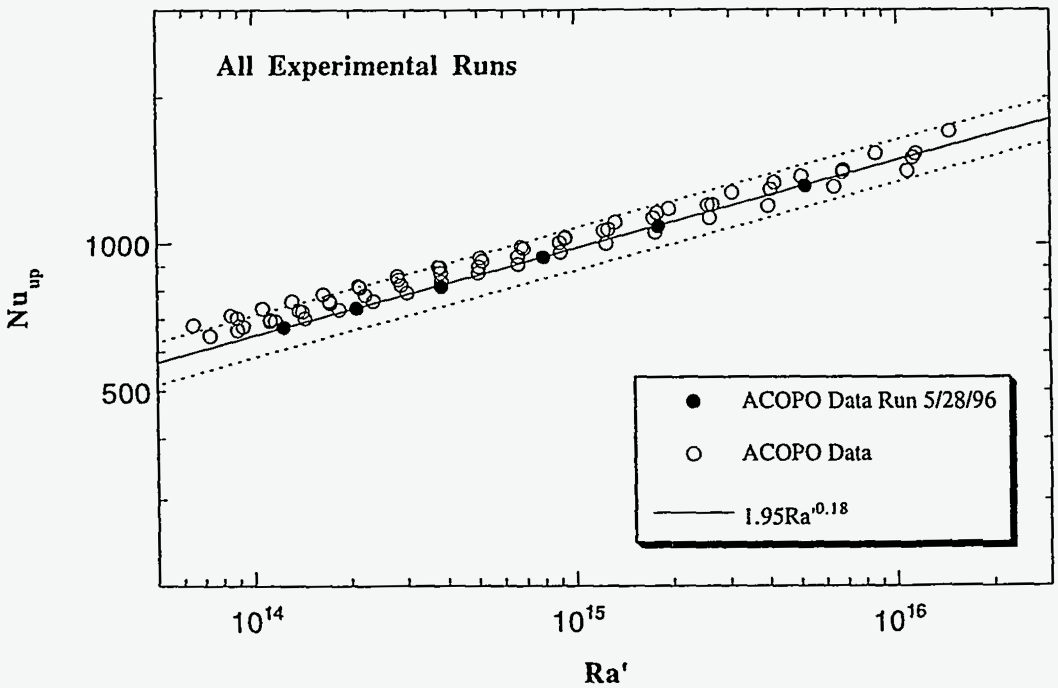


Figure V.13 Upward heat transfer from all five ACOPO runs. Data shown every 200 s, for clarity. The full points are from Run 5/28/96, and the solid line represents the fit to these data. The dotted lines show the ±10% margins on the correlation.

The downward heat transfer data from ACOPO Run 5/28/96 is shown in comparison to the Mayinger et al. (1975) and mini-ACOPO (Theofanous and Liu, 1995) correlations in Figure V.14. In the latter case, the extension of the lower branch of the correlation, representing the water data obtained in the range $10^{12} < \mathrm{Ra}' < 4 \cdot 10^{13}$, is used. It is seen that in the upper range both correlations and the data come together to a close agreement. The upper branch of the mini-ACOPO correlation, obtained with Freon 113, and extending from $\sim 3 \cdot 10^{13}$ to $7 \cdot 10^{14}$, exhibits a somewhat steeper slope. This matter is under investigation. A fit to the data from Run 5/28/96 yields

$$\mathrm{Nu}_{dn} = 0.3\,\mathrm{Ra}'^{0.22} \tag{V.3}$$

and it is shown in relation to all ACOPO data in Figure V.15. An essentially 10% tight fit over the whole range of Ra′ is observed.

V.5. DISCUSSION

The in-vessel retention analysis for the AP600 noted above (Theofanous et al., 1996) was based on the Steinberner–Reineke and the mini-ACOPO (upper branch) correlations for the upward and downward heat transfer, respectively. They are given by

$$\mathrm{Nu}_{up} = 0.345\,\mathrm{Ra}'^{0.233} \tag{V.4}$$

and

$$\mathrm{Nu}_{dn} = 0.0038\,\mathrm{Ra}'^{0.35} \tag{V.5}$$

although the Mayinger correlation

$$\mathrm{Nu}_{dn} = 0.55\,\mathrm{Ra}'^{0.2} \tag{V.6}$$

was also utilized in sensitivity analysis. It is interesting, therefore, to consider how the new results, obtained directly on the prototypic range of Rayleigh numbers (see Fig. V.2), might affect the conclusions.

Given the agreement on the flux shape, it is sufficient for this purpose to consider the average heat fluxes in the upward and downward directions. Let us denote their ratio by R′, and with subscripts "o" and "n" the "old" and "new" results respectively. That is, from Eqs. (V.4) and (V.5), we have

$$\mathrm{R}_0' = \frac{\mathrm{Nu}_{up,o}}{\mathrm{Nu}_{dn,o}} = 90.7\,\mathrm{Ra}'^{-0.117} \tag{V.7}$$

while based on Eqs. (V.2) and (V.3), we have

$$\mathrm{R}_n' = \frac{\mathrm{Nu}_{up,n}}{\mathrm{Nu}_{dn,n}} = 6.5\,\mathrm{Ra}'^{-0.04} \tag{V.8}$$

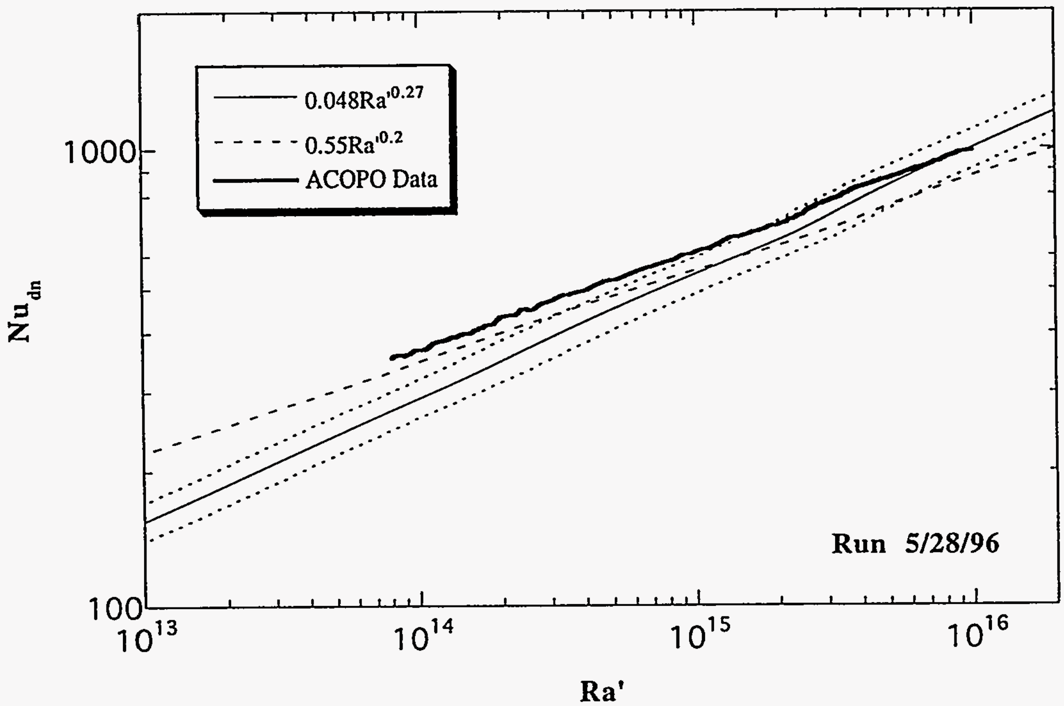


Figure V.14. Downward heat transfer from ACOPO Run 5/28/96 compared to the Mayinger correlation (- - -), and to the extension of the lower branch of the mini-ACOPO correlation (—). The dotted lines show the $\pm$10% margins on the correlation.

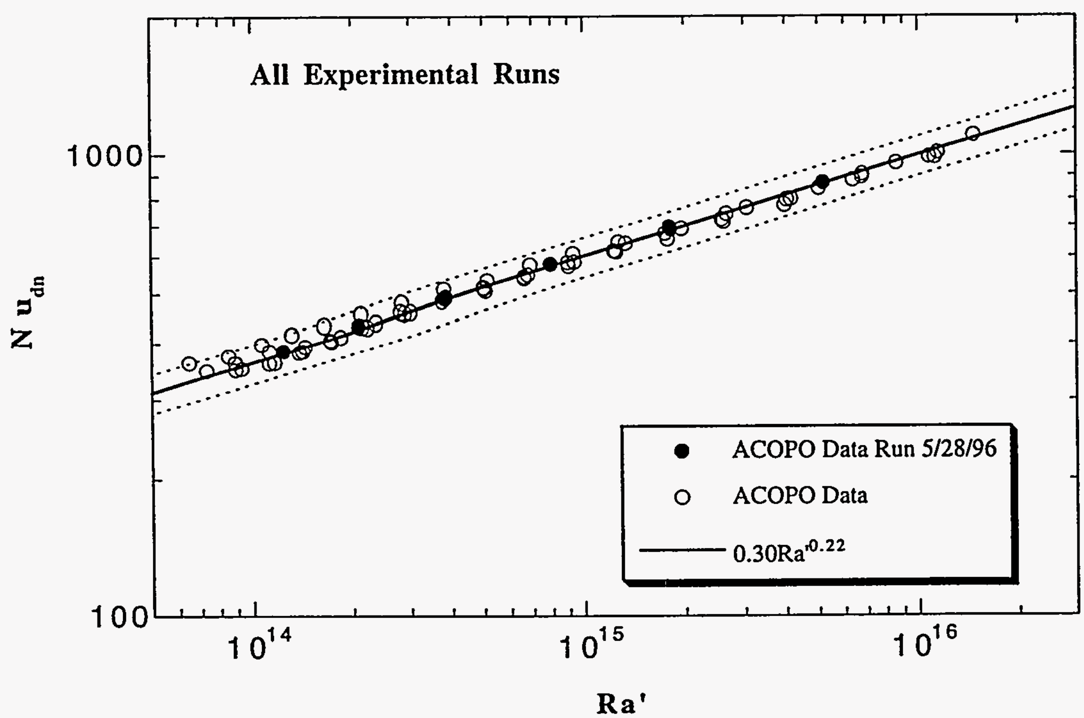


Figure V.15.Downward heat transfer from all five ACOPO runs compared to correlation (3). Data shown every 200 s, for clarity. The dotted lines show the $\pm$10% margins on the correlation.

Now the heat flux ratios of interest can be obtained (see Section 5.1 of DOE/ID-10460) from.

$$\frac{q_{up,n}}{q_{up,o}} = \frac{1 + 2/\mathrm{R}'_o}{1 + 2/\mathrm{R}'_n} \tag{V.9}$$

and

$$\frac{q_{dn,n}}{q_{dn,o}} = \frac{1 + 0.5\,\mathrm{R}'_0}{1 + 0.5\,\mathrm{R}'_n} \tag{V.10}$$

The results, for Rayleigh numbers bounding the region of interest, are summarized in Tables V.1 and V.2. It can be seen that in the previous results the upward flux was previously underestimated by ~10%, while the downward flux was overestimated by less than ~8%. These variations are negligible in the context of the analysis, and the margins to failure found in DOE/ID-10460.

Table V.1. Illustration of the Variation in the Heat Flux Ratio, R', as a Result of the New Correlation Basis

Ra'	R'_o	R'_n
10^{15}	1.59	1.63
$5 \cdot 10^{15}$	1.32	1.53
10^{16}	1.22	1.49

Table V.2. Bounding Values of the Effect of the New Correlation Basis on In-Vessel Retention in an AP600-Like Design

Ra'	$q_{up,n}/q_{up,o}$	$q_{dn,n}/q_{dn,o}$
10^{15}	1.01	0.99
$5 \cdot 10^{15}$	1.09	0.94
10^{16}	1.12	0.92

V.6 CONCLUSIONS

- The first round of experiments from the ACOPO facility confirm the experimental concept, and extend the mini-ACOPO results, to fully cover the prototypic range of Rayleigh numbers of current interest to in-vessel retention
- Some variations from the extensions of previous correlations are found, but they are mainly of a detailed fundamental interest. The net impact on the assessment of in-vessel retention is less than 10%.

NOMENCLATURE

g	acceleration of gravity
H	depth of pool
Nu	Nusselt number$\equiv (qH)/k(T_{max} - T_w)$
q	average heat flux at pool boundaries
$\dot{Q}$	volumetric heat generate rate
Ra$'$	Rayleigh number, internal $\equiv (g\beta\dot{Q}H^5/(k\nu\alpha)$
T	temperature

Greek

α	thermal diffusivity
β	thermal expansion coefficient
ν	kinematic viscosity

Subscripts

dn	downward (over the hemispherical boundary)
n	new
o	old
up	upward (over the flat boundary)
w	wall value

ACKNOWLEDGEMENTS

Support from DOE's ARSAP program, and of the program's Project Manager, Mr. S. Sorrell (DOE, Idaho Operations Office), are gratefully acknowledged. The authors also wish to express their appreciation to Dr. C. Liu for his participation in the design of ACOPO, and to Messrs. Al Khamseh, Richard Becker and Godfrey Nairn, for their essential contribution in the construction of the ACOPO test vessel.

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APPENDIX V-3

[[THE BOILING CRISIS MECHANISM]]

[[A New Boiling Transition Regime]]

Proceedings, The Japan-US Seminar on Two-Phase Flow Dynamics
Fukuoka, Japan, July 15-20, 1996

[[APPENDIX V-3]]

A NEW BOILING TRANSITION REGIME

S. Angelini and T.G. Theofanous

Departments of Chemical and Mechanical Engineering
Center for Risk Studies and Safety
University of California, Santa Barbara, CA 93106

ABSTRACT

Recent interest in a severe accident management scheme known as "In-Vessel Retention" has created the need to establish the coolability limits of large, inverted geometries. In this paper, full-scale simulations conducted at UCSB's ULPU facility are examined at the microscopic level. Because of the peculiar geometry, it has become possible to directly visualize the boiling transition phenomenon, and with the help of microthermocouples to quantitatively identify the mechanism. Altogether, a new boiling transition regime was identified, with a significant coupling between overall systems dynamics and the microphenomena. This leads the way to the a priori prediction of critical heat flux and factors that may influence it.

V.1. INTRODUCTION

The In-Vessel Retention severe accident management concept (Theofanous et al., 1995) involves the external submergence of a nuclear reactor lower head, holding the core debris, in a pool of water. The lower head is a 4 m in diameter, 15 cm thick (steel), hemisphere, and the thermal loading on the inside may vary from a few hundred kilowatts per square meter at the bottom, to over one megawatt at the upper portion. Lower head integrity is assured, as long as the film boiling condition is avoided.

The whole problem of In-Vessel Retention, including boiling transition, has been assessed from a practical standpoint, and failure for a reactor like the Advanced Passive PWR designed by Westinghouse (the AP600) has been found to be physically unreasonable (Theofanous et al., 1995). This document contains, in particular, the approach taken to quantify the coolability limits, the experimental facility (ULPU-2000) used to in effect obtain a full scale simulation, the experimental programs, and the results obtained. Our purpose here is to present some of the more detailed features of the phenomena leading to boiling transition, as a way of getting to the scaling and predictive aspects in more basic terms.

The experimental design involves a full length, slice geometry, as illustrated in Figure V.1. The radius of curvature of the surface defined by the heater blocks is ~2 m, as is a nuclear reactor vessel lower head. The height of the facility is ~6 m, again very close to the height of a reactor vessel. The heater blocks are made of 7.6 cm thick copper, they have a width of 15 cm, and they are heated by imbedded cartridges, individually controlled. Power shaping is used to simulate the axisymmetric geometry in the reactor. The near horizontal portion ($\theta \sim 0°$) was studied in a more focused manner with Configuration I, illustrated in Figure V.2. An overall view of the facility, identifying the key components, is shown in Figure V.3.

There have been several experimental campaigns, with all three configurations, and one including a steel block (same grade as used for the reactor), which was also painted with the special paint used to protect reactor vessels. The quantitative results were very reproducible and consistent. Under the pressure of the AP600 certification, to begin with, we focused on quantifying critical heat flux. The results are summarized in Figure V.4, reproduced from Theofanous and Syri (1995).

More recently, we equipped the region near $\theta \sim 0°$ with microthermocouples, and intensified our visualization efforts. This work led to the present paper. We are currently installing microthermocouples in selected regions covering all other angles, for a more comprehensive examination of regimes, which apparently (from current visualizations) vary significantly with orientation.

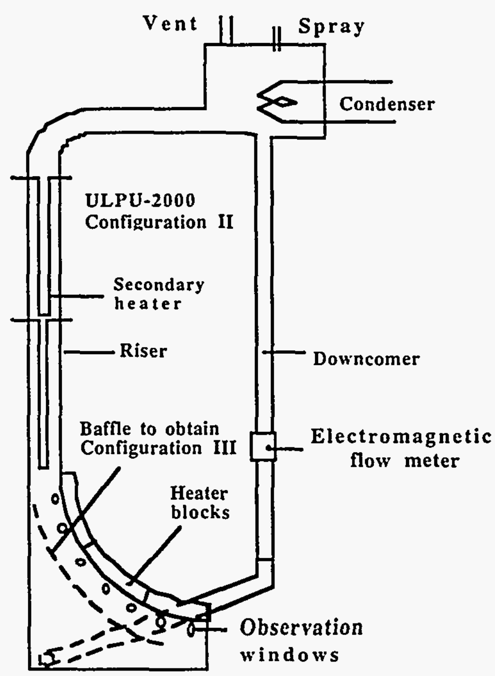

Figure V.1. Schematic of the ULPU facility. Configuration III is obtained by installing the baffle shown, to simulate the reactor vessel thermal insulation.

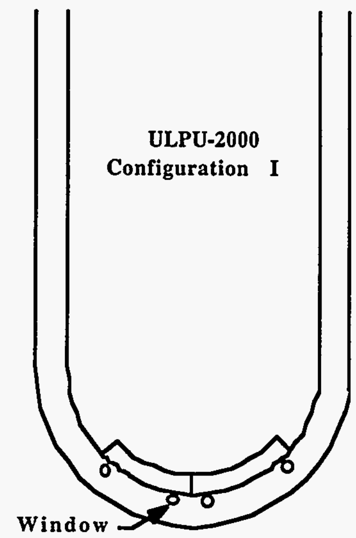

Figure V.2. ULPU Configuration I.

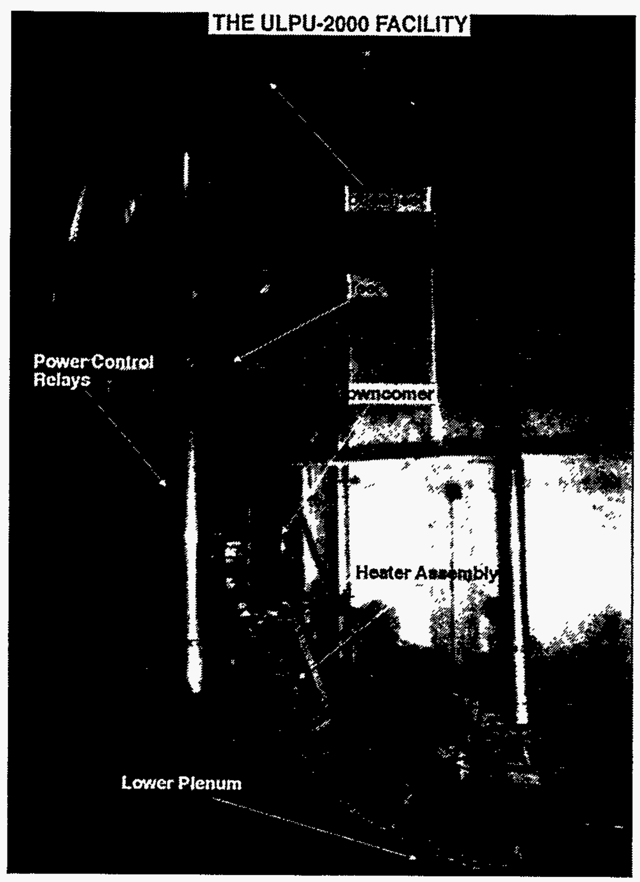

Figure V.3. An overall view of the ULPU facility.

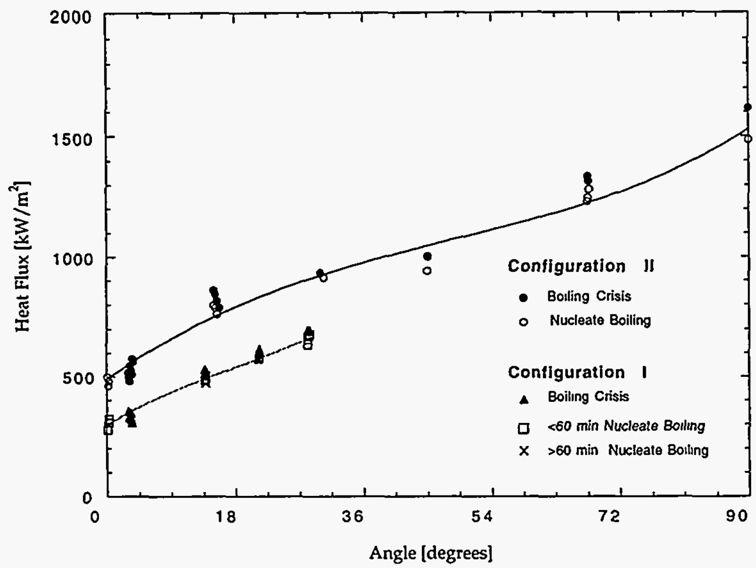

Figure V.4. The coolability limits of a reactor pressure vessel lower head, from ULPU simulations.

V.2. QUALITATIVE DEFINITION OF KEY PHYSICS

Whether in natural circulation (Configurations II, III) or not (Configuration I), the system was found to always operate in an oscillatory mode. The oscillations appear to be driven by the accumulation and release of vapor in the $\theta \sim 0°$ position, but they certainly couple with the dynamics of vapor flow over the remainder of the heater, and of the two phase flow in the riser, too. These oscillations define the external boundary conditions of what is going on in the immediate vicinity of the heating surface, and they are an integral part of the system performance. Here we will accept these oscillations as they are, and will focus on the boiling dynamics under this condition, for the time being taken to be externally imposed. In parallel efforts we address the oscillations, too, and envision that the two will be integrated in due time.

Moreover, we concentrate on the region around the pole of the hemisphere ($\theta \sim 0°$), which is of particular interest. This is because this region favors vapor accumulation, which as noted above makes a key contribution to the oscillations. Also, this is the region that allows, in the ULPU design, a unique "internal" visualization, and hence direct access to the phenomena controlling the boiling crisis. Specifically, at power levels approaching critical, the vapor coalesces into a macroscopic bubble, or really a vapor film, with the liquid-vapor interface passing, momentarily, past the windows (see Fig. V.1), thus allowing a direct view of the heater surface. This is a unique advantage of the slice geometry and, it turns out, a very fortunate one.

In addition to the windows shown in Figure V.1, a large optical-quality window was installed at the narrow end of the test section, at position $\theta = 0°$, as illustrated in Figure V.5. Visual records were obtained with normal as well as high speed videos.

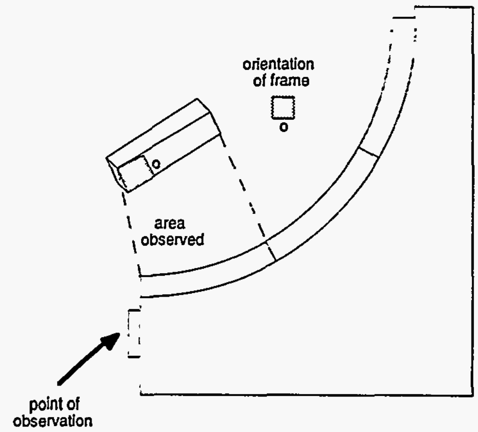

Figure V.5. The view of the heating surface at $\theta \sim 0°$, from the window at the narrow end of the test section. The letter "o" defines the orientation of the area observed in the frames of Figure V.6.

Three main events in the cycle were identified, and they will be explained with the help of Figure V.6. This has been extracted from a high speed video taken at 1000 frames/second in a run with 400 kW/m^2 heating power.

1. Starting with the middle frame, obtained immediately after the "escape" of the vapor film, the liquid makes contact with the heating surface, and a densely-packed bubble nucleation appears.

2. The bubbles grow quickly, and coalesce into a coherent vapor film, as shown in the top frame of Figure V.6. The film grows downward, pushing the liquid away, as well as up along the curved boundary. This frame succeeds the middle one by 34 ms.

3. At some point the vapor is "released," and the liquid approaches the heater, in a wave-like fashion, as shown at the bottom of Figure V.6. The heater is totally covered by liquid, and only the tail end of the escaping vapor film (dark area at bottom) can be seen. Nucleation has not begun yet. The frame precedes by 52 ms the middle frame in the sequence, in which nucleation is evident.

Correspondingly, with these three events we can define the "nucleation" time, the "vapor residence" time, and the "liquid sweep" time. A key observation is that, while the sum of these times remains relatively unchanged with power level, the individual components change remarkably, as illustrated in Figure V.7. The cyclic behavior is evident also by the surface temperature oscillations, as shown in Figure V.8.

Clearly, this is a "nucleate boiling" regime in that cooling remains high and stable, but also it isn't, in that the surface spends a very small fraction of the time in actual nucleate boiling. At high heat fluxes, cooling actually occurs for the most part by liquid microlayer evaporation. This very

Figure V.6. The main physical events in a cycle, at high power levels approaching the coolability limit (see text for explanation). Visualization and orientation from the window as shown in Figure V.5.

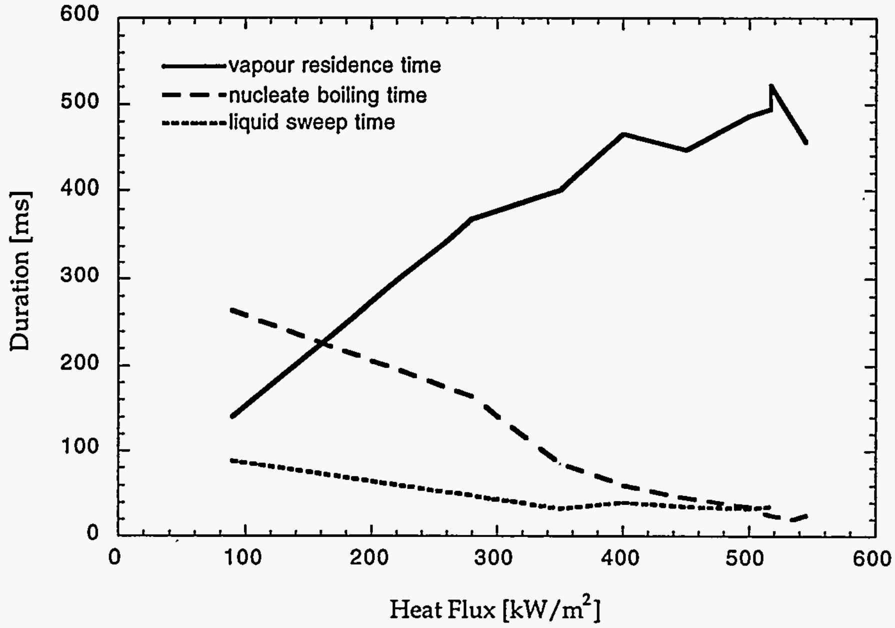

Figure V.7. The timing of the three main events in a cycle, as functions of the input heat flux.

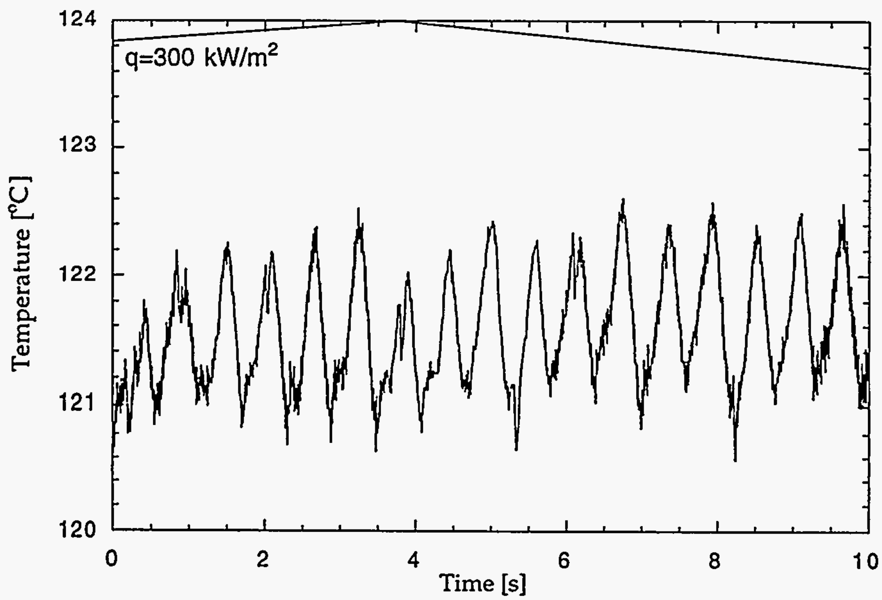

Figure V.8. Surface temperature oscillations under stable cooling conditions.

peculiar situation carries over to the occurrence of excursive instability, which defines the limits of coolability. Its onset is evident by a temperature excursion, but one that continues to be characterized by periodic wettings and dryouts, with respective surface temperature fluctuations. This leads to gradual heatup, and quite long times before the true film boiling (Leidenfrost) condition is reached.

This mechanism was actually observed, as shown in Figure V.9. Specifically, the top frame shows the very early inception of dryout, and liquid drops "falling" off the surface (removing any liquid in excess of the microlayer), the middle frame shows progressive dryout, and the bottom frame shows the microlayer existing only as small islands (in the movie, they are rapidly shrinking in area). This figure also shows the large dimensions of the vapor film, as deduced by comparison to the size of the window (5 cm).

Thus we arrive at the key ingredients of the phenomenon that defines the coolability limits (the boiling crisis) in this geometry. They are the thickness and stability of extended microlayers, and the frequency of oscillation. As long as the liquid returns prior to microlayer dryout, cooling is stable. Conversely, any delay in return will lead to partial dryout, gradual heat accumulation, and eventually film boiling. This is a long transition regime, partly externally controlled (by the oscillations), and strongly dependent, in addition to the heat flux, on the rupture properties of the microlayer. For highly wetting surfaces, we can expect that dryout will be delayed until complete extinction of the film by evaporation. In the next section, we provide quantitative evidence that this is the case, and in the process of doing so, we obtain a first estimate of the microlayer thickness.

V.3. QUANTITATIVE INTERPRETATIONS

The transient behavior can be divided into three categories, depending on the temperature transient measured at the copper surface:

1. Regular fluctuations in sync with global pulsations imposed by the system.
2. Recovered temperature excursions, and
3. Runaway excursions, leading to film boiling.

Category 1 is illustrated in Figure V.8, and will not be pursued further here. Categories 2 and 3 allow a quantitative interpretation of the boiling transition phenomenon, as summarized below.

Recovered Excursions

As power increases more time is available to thinning out the film (see Fig. V.7). Also, more power is available to this process, which eventually leads to complete film dryout. As a consequence a marked increase of the surface temperature is observed, as illustrated in Figure V.10. On this

Figure V.9. Direct visualization (from side window) of the phenomenon leading to temperature excursions. Dark areas correspond to the presence of microlayer, and light areas to dry heater surface.

figure we also show the results of a simple, one-dimensional transient conduction model, in terms of heat transfer coefficient at the heater surface and the corresponding temperature variation at the surface. There are two aspects to be considered in this figure. The one is with regards to all regions using an insulated boundary. The agreement with the experimental heatup excursions indicates that they are indeed characterized by complete microlayer dryout. These periodic heatups are seen to be interrupted by periodic rewets. Matching the cooldown during these periods shows the magnitude of the heat flux, and that generally this remains relatively consistent throughout. Also, it is evident that, although the heatup following the first excursion is strong enough to cause a sequence of dryouts, the rewets in between are sufficient to contain the excursion and lower the surface temperature to its previous, oscillatory values.

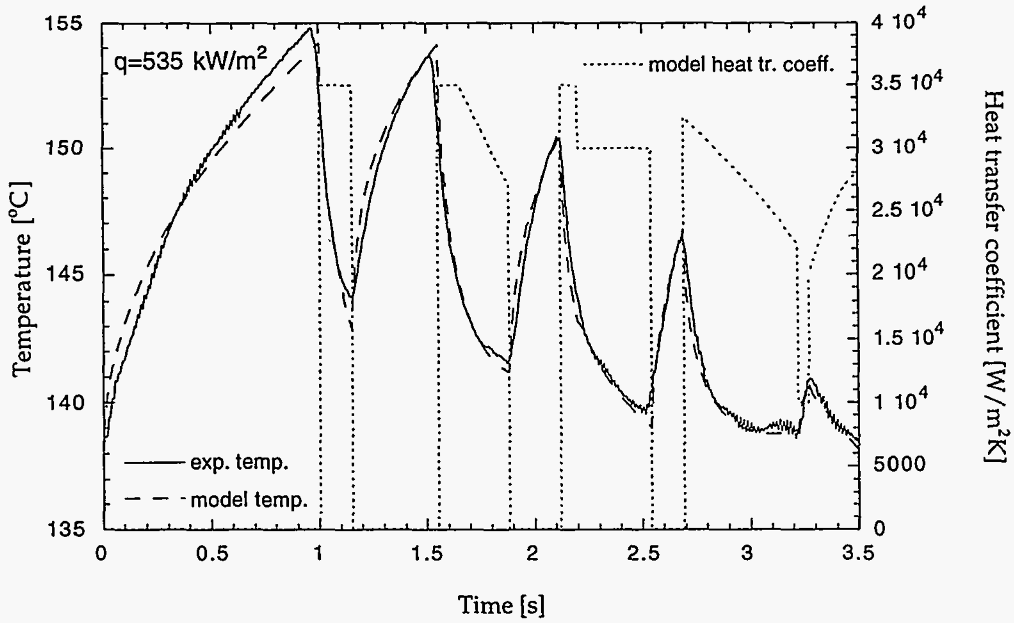

Figure V.10. Illustration of a recovered excursion, and the heat transfer coefficient needed at the heater surface to match the measured temperature.

Runaway Excursions

As we increase the power further, higher temperature excursions during dryout and lower heat removal capability from the film can cause the situation illustrated in Figure V.11. In here, periodic rewets are still present, though they can only mitigate the heatup, rather than contain it. Two important observations can be made: first, we can estimate the thickness of the film as follows. If $\dot{q}$ is the heat flux at the heater surface, and δt is the duration of the cooldowns between excursions in Figure V.11, the energy balance between heat input and evaporation/dryout of the film yields:

$$b = \frac{\dot{q}\delta t}{\rho i} \qquad (V.1)$$

where b is the film thickness, ρ is the density of water and i its latent heat. From the data of Figure V.11, we see that the typical film thickness is between 10 and 30 μm. Second, we can compare Figure 11 with the transient behavior obtained from the simple conduction model with an uninterrupted insulated boundary condition. The latter is shown in Figure V.12. We find heat up rates after 30-35 s of 40 °C/min when the insulated boundary is interrupted by rewets, 120 °C/min when it isn't. It is evident that, in spite of its short existence during the cycle, film deposition is very effective in mitigating the surface heatup.

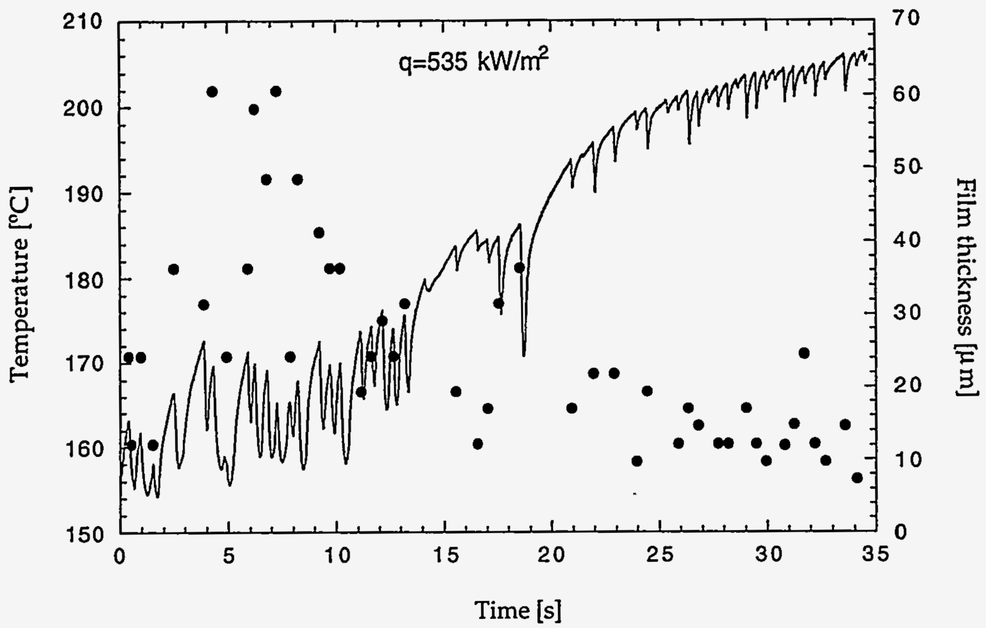

Figure V.11. Illustration of a runaway excursion and the film thicknesses obtained from the time required to vaporize the microlayer.

V.4. CONCLUSIONS

- The coolability of extended, curved, inverted surfaces is controlled by microlayer evaporation, and the time available for this process through the system self-oscillations.
- The period of oscillations is affected by the vapor accumulation and release phenomena, but is integrated in the overall dynamics of the system.
- Near boiling transition the vapor film forms essentially instantaneously upon liquid-heater contact, and the initial vapor film thickness is of the order of 10 to 30 μm.
- The other two dimensions of the microlayer are macroscopic, as it covers coherently large surface areas of the heater. It appears that the microlayer is formed by the liquid trapped

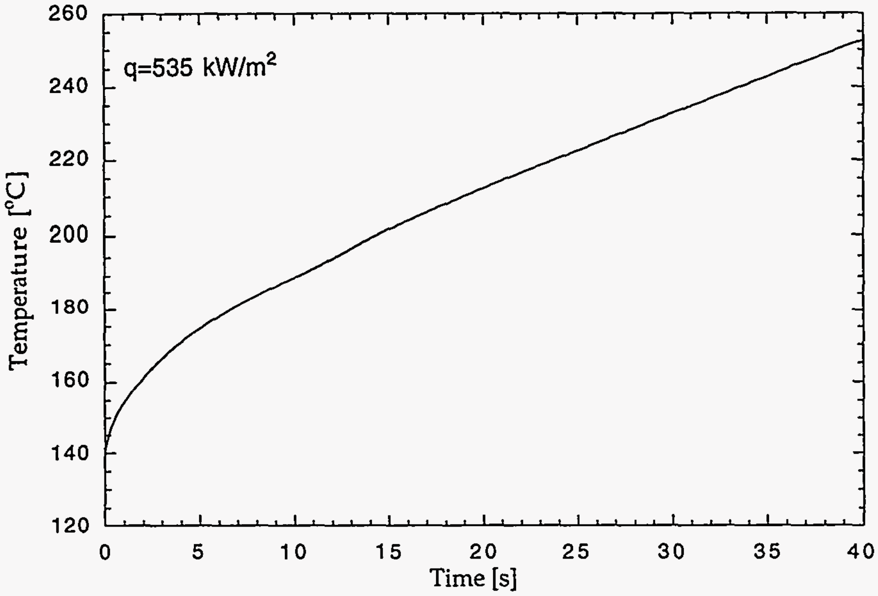

Figure V.12. Illustration of a temperature excursion from the simple conduction model for uninterrupted insulated boundary condition.

beneath densely spaced, rapidly growing vapor bubbles, and it may even be subject to draining of any excess.

- Microlayer dryout leads to a gradual transition behavior, with persisting dryout and rewetting events, that moderate the rate of heatup significantly, until eventually a true Leiderfrost condition is reached.

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