CA30102 PHB

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To: Dr. Graham Wallis
Chairman, Subcommittee on Thermal Hydraulic Phenomena
Advisory Committee on Reactor Safeguards

From: Virgil E. Schrock, Consultant

Subject:: T/H Subcommittee Mtg Jan. 16 & 17, 2001.
a. Resolution of Waterhammer Issue
b. S-RELAP5 For Appendix K SBLOCA
Joint Subcommittees Mtg. Jan. 18
Pressurized Thermal Shock

**1. Water Hammer Issue**

Due to travel problems I missed the discussion on this issue. I reviewed the revised EPRI Report "Resolution of Generic Letter 96-06 Water Hammer Issues", TR- 113594-V1 & V2, Final Report , Dec. 2000. My comments are based on that review only. In November 1999 the Subcommittee reviewed an early version (Interim) of the report. That report was found to be large, poorly written and edited, difficult to follow and technically inadequate. One problem in reviewing the revised report is that there is no indication of the changes that were made. I've looked at my comments in my Consultant Report dated Nov. 30, 1999. Here are some of my comments regarding the so-called Rigid Body Model:

"*........... But the major problem is that some of the analysis is just too crude or worse, in some cases, simply wrong. I tried to put myself in the position of a user of this document and found it very difficult . For example, item 8 in the suggested utility approach (p. 1-4) caught my eye because of questions raised in my mind during the presentations at the meeting. It refers to the amount of noncondensable gas in the initial void and directs the user to Section 9.2. There I find Section 9.2.1 is a description of the so-called Rigid Body Model (RBM). This is an attempt to analyze in 1-D the motion of a slug of liquid driven by condensation from a trapped mixture of gas and steam. There is no clear description of the model assumptions. There is no statement of the initial and boundary conditions. Figure 9-7 shows a gas/steam volume at the closed end of a horizontal pipe. It is said that a more detailed derivation is found in Appendix E. Figure 2.1 in App. E shows a gaseous plug in a horizontal pipe with the liquid filled downstream end closed by*





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context of "Best Estimate" analysis. One could easily interpret the SER to mean that staff finds the analysis to be satisfactory with the exception of specific items that will have to be examined in the Best Estimate Review. I wouldn't agree that the SER is comprehensive in identifying the problems in the code. I hope that SPC will not find it possible use the SER to limit the scope of the Best Estimate review.

At the meeting I brought up the issue of delayed neutron precursor yield dependence on space and time within the reactor and the fact that the code documentation does not explain how this is calculated, particularly since only yields from fissioning of 235-U are given in RELAP5 documentation. The lower yields from 239-Pu have a major impact on the reactor kinetics so "default" 235-U data produce a calculation results that are seriously non conservative for most realistic core states. SPC explained that delayed neutron precursor yield is input to S-RELAP5 from detailed calculations done with their fuel cycle code. There are a number of ways that the calculation might be made and it is not a frivolous question to ask how this is done for point kinetics and multidimensional treatments. This is an example of an item, previously questioned, that escaped mention in the SER. It is relevant to both Appendix K and Best Estimate models. I believe it is a part of what should be covered by the present application approval and the S-RELAP5 code description. .

I have also made comments on the Draft ACRS Letter on S-RELAP5 for App. K, Jan. 26, 2001, and those comments might be considered a part of this report.

**3. Pressurized Thermal Shock**

I found the PTS Re-evaluation Project interesting. The assessment of the thermal hydraulic transients uncertainties is a very difficult problem. I think the approach of viewing the system in a simple way has merit. However, I could not fully understand the presentation by Almenas and how the simplified view fits with use of RELAP5 to assess uncertainty. My study of the Draft document provided has been rather cursory and has not helped very much to clarify issues in my mind. The discussion in Section 7 concerning critical flow seems to me to be overly simplistic and gives little confidence in the approach. The figures (7.1 to 7.7) are strange. The text indicates that these figures show "computed choked (consistently misspelled as *chocked*) flow mass/ energy loss rates as a function of upstream pressure, quality and break size". No specification of flow geometry is stated. In figures 7.1 and 7.2 flow rates are shown as calculated from four different for a 2" diameter break and plotted against upstream pressure. The common parameter for the curves (presumably upstream quality or some other variable defining the thermodynamic state) is not given. The text talks about two of the models included in specific versions of RELAP5 thought to bound the actual processes. In these two figures there are two horizontal lines that are not labeled. At first I thought they were meant to be bounds but the text indicates that they represent ranges of source terms, which are HPI flowrate for Fig. 7.1 and decay heat rate for Fig. 7.2. Figures 7.3 - 7.6 present calculated mass and enthalpy flow rates as a function of break area. Geometry and upstream state (quality or enthalpy are needed in addition to the given pressure) are not given. Again the graphs have two horizontal lines whose relationship to bounds is unclear. In this case, the lines have notations 1 inch and 1.5 inch, but break size is the independent variable for these graphs. I am unable to understand the message here. Some of the discussion in this section also seems naive in understanding of the problems involved in critical flow calculations. It doesn't give me a lot of confidence that the authors have found a suitable way to assess the uncertainty in the calculated break flow.

My overall impression is that the report draft is <u>very</u> rough and needs a good deal of polishing if it is to be convincing. Unfortunately Dr. Almenas was not well prepared to make a clear and well structured presentation. The situation is probably better than it appeared from the report and the presentation, but I would have to hear a more coeherent story before I could say I find it satisfactory for the PTS Program.

Concerning the general approach, I have not seen convincing evidence that one dimensional stress analysis is sufficient to address the thermal shock problem. The boundary conditions imposed by the thermal hydraulics are clearly multidimensional. I don't think the water in the downcomer is well enough mixed to ignore azmuthal variations in temperature. RES is counting on the 1-D approach being good enough. But what will be the basis for deciding whether it is good enough? Approximating the multidimensional problem by a one dimensional analysis is one source of uncertainty in the overall results.