UNITED STATES
NUCLEAR REGULATORY COMMISSION
ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
WASHINGTON, D.C. 20555-0001

November 30, 1999

MEMORANDUM TO: ACRS Members

FROM: BP FOR Graham B. Wallis, ACRS

SUBJECT: Comments on EPRI Interim Report, TR-113594 "Resolution of Generic Letter 96-06 Waterhammer Issues"

This report is supposed to resolve concerns about waterhammer in fan coolers in PWR containments. However, no calculations for PWRs are actually made, though conclusions are reached. Several research results are presented and some graphs are presented that are supposed to be usable for predicting plant loads. However, the methods used to derive these graphs are not explained. Some theory is presented and compared with a few data points, but no comprehensive comparison with all data is given. It is unclear if the predictions are for average loads or maximum ones. No analysis of uncertainty is made; it is simply discussed. Sensitivity studies to condensation coefficient and assumed water surface temperature do not show insensitivity to these parameters. The amount of air that is evolved and that cushions the plant waterhammer is an important parameter but methods for computing it are vague and appear to have little justification.

The report needs considerable reworking and a much more complete description and justification should be provided of the recommended calculational methods.

Specific comments.

In the executive summary it is claimed that the tests encompass events in the plant. This needs convincing justification.

The conclusion that the LOOP only waterhammers are the most severe is never justified in the report.

ATTACHMENT 1

3. Memorandum dated November 30, 1999, from Virgil E. Schrock, Consultant, to Dr. Graham Wallis, Chairman, Subcommittee on Thermal Hydraulic Phenomena, Subject: Consultant Report on the November 17, 1999 Subcommittee Meeting: Waterhammer in Plant Service Water Systems.

cc: B. Sheron, NRR
G. Holahan, NRR
T. Collins, NRR
J. Tatum, NRR

G. Wallis Comments
November 30, 1999

The statement that waterhammers are not a credible threat needs a convincing rationale in terms of real plant calculations. Particular attention should be paid to uncertainties, since the data exhibit a large scatter and there appears to be no account of this in the calculations.

p1-3 It is not clear that the experiments really covered all the "complex thermodynamic and hydrodynamic transients" in the plant.

p.3-8 The LOOP event is to be shown to bound waterhammers in Chapter 9. Yet I could not find this discussed at all in Chapter 9.

p.4-3 A subcooling of 36 degrees F is stated as being necessary for waterhammers, yet later the recommendation is that the surface of the water be taken as at 212 degrees F for calculating condensation rates.

p.6-12 It is claimed that there is guidance for defining the "worst case". Yet the analysis does not address uncertainty at all. Is it supposed to be a worst case analysis? This is never demonstrated.

p.8-6 The tested geometries are presented but not justified in terms of their relationship to the plant.

p.8-11 Figure 8-5B is supposed to show that the peak pressure is independent of pipe size. It does not. There are very few 2 inch pipe data and where they do overlap with the 4 inch pipe data, the latter are generally significantly higher, particularly the maximum values.

p.8-16 Why are pulses briefer at higher loads, when L/c is the same?

p.8-17 The loop seal loads are much higher than the horizontal pipe ones. There is clearly a geometry effect. Do the tests bound the plant geometries? Why are there no tests with air for the loop seal, which appears the most critical for large loads? It should be shown that the theory applies to air-cushioned waterhammer in a loop seal, yet there are no data for this condition

<u>Comment about the figures in Chapter 8.</u> In Appendix F there are tables of data. They show several maximum pressures for Configuration 1 above 1000psi, one as high as 2990psi. Why do these not appear on the summary figures? What about the many data for Configuration 2B above 700psi that do not appear in the summary figure?

p.8-20 Figure 8-14 does not show a very convincing decrease of impulse with pressure, particularly for the straight-deaerated data.

p.8-24 The arguments about independence of results on pipe size are imaginative but not really related to the physics. They are not substantiated by data. This conclusion is tenuous at best.

p.9-7 When there are noncondensables there is no "final impact". What is the meaning of impact velocity when there is an air cushion? This is not explained. An explicit description of how the peak pressure is calculated is needed. Is it the peak pressure in the compressed air cushion and not a real waterhammer at all?

p.9-13 The apparatus for release of noncondensables is unique and not clearly related to the real plant conditions. The physical mechanisms in this device do not duplicate the geometrical, flashing, flow, nucleation, stratification and other phenomena in the plant. Even if the data were good, there is a real question about extrapolation to plant conditions.

p.9-15 There are far too few data in figure 9-9A to support the "curve fit". The data are not consistent and the trends unclear.

p.9-16 The use of 40% air evolution under any and all conditions that is recommended is a stab in the dark. The air evolved must depend on the history of the water. Some of the water will be expelled from the fan cooler before it reaches saturation or has time to boil. The air release by being "exposed to steam" must depend on the time of exposure and many other thermal/hydraulic effects. This is a very shaky part of the theory.

p.9-17 The theory for gas concentration in the void is strange. Equation (9.8) gives it in terms of the mass of the surrounding pipe. Yes, there is condensation on this pipe, but the air has first to be evolved before it is concentrated. The amount present cannot depend on the properties of the pipe as described. There would be air there even if the pipe had no mass.

<u>Comment</u>. The whole report gives the user very little advice on how to compute the amount of air in the collapsing steam bubble, yet this is one of the key variables that has to be input to later calculations.

Section 9-5. The thermal layer test geometry is not typical of void closure in a horizontal pipe. The results show very little dependence on thermal layer in this apparatus, and this seems to be the basis for later assuming that the interface is at 212 degrees F. This may not be justifiable for all the geometries and

conditions in a real plant. Depressurization of the void is one mechanism for reducing the interface temperature (or a symptom of it due to colder water reaching the interface) as seen in the detailed pressure histories.

p. 9-38 There appears to be no significant effect of Jacob Number. Once the pressure is low enough in the void, it doesn't matter what it is?

pp.-43,44 These figures show a weak effect of dissolved oxygen, far less than the order of magnitude effect seen in Chapter 8. There are lines shown through the data but it seems that none of these are computed values. The whole report suffers from a lack of comparison between genuinely predicted values and actual results. How is the dissolved oxygen content in the water to be related to the amount of gas in the collapsing steam bubble? This needs detailed explanation.

pp9-52 to 9-57 The plots are presumably for use by designers. There is no explanation of where they came from. What is K, which is a parameter on the curves? If there is a clear procedure for calculations it should be spelled out and the predictions systematically compared with the evidence in order to validate the methods.

p9-47 There is no gas released by steam condensation. Gas concentration can be increased by removing steam, but gas is released by a different mechanism.

Appendix D seems to describe a separate test that is not related to the previous ones? Explanation is required.

Appendix E describes the long-awaited analytical models (THEY SHOULD BE SPELLED OUT CAREFULLY IN THE MAIN TEXT). Yet there is no derivation to be found for the "expected air mass" that is used to fit a few sample data. Where did the value come from? It was not measured.

p.E-15 The three cases 3,4,5 are fitted with different values of h. What is the justification for recommending a constant value of h? How do the predictions using this assumption compare with all the data points, not just a few selected ones? Since there is considerable scatter in the figures in Chapter 8, where does the prediction lie? Is it around the mean of the data or nearer the peak? The rationale for choosing a certain value of h is obscure. Such a key parameter must be justified in detail. It would be good to compare the data in Chapter 8 with predictions using various values of h.

p.E-42 The figure shows that the peak pressure is significantly influenced by the assumptions about the heat transfer coefficient, h, and the water surface temperature. They cannot just be guessed or stated without justification. Condensation is influenced by air content, but this does not appear in the recipe. Also, the mixing on the water side should be important, perhaps governing, and this is not considered at all. The curves for T=200 and T=210 should not cross.

p.E-60 For the plant simulation a different value of "h" is chosen and the area for condensation is doubled. What is the basis for these new assumptions? No example is given of how to compute the mass of air, which has to be inputted and is one of the biggest uncertainties.

Section F contains tables of data with recorded peak pressures that seem to be much above those shown in the figures in Chapter 8. This is strange and undermines the conclusions.

There should be a corresponding table of predicted pressures so that it can be judged how well the theory represents the data. The report is very weak in this regard.

Please also see the comments of our consultants, Messrs. V. Schrock and N. Zuber.