To: Dr. Graham Wallis, Chairman, Subcommittee on Thermal Hydraulic Phenomena

Via: Paul Boehnert

From: Virgil E. Schrock, Consultant

Subject: Consultant Report on the November 17, 1999 Subcommittee Meeting: WATERHAMMER IN PLANT SERVICE WATER SYSTEMS.

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EPRI Waterhammer Report

In response to GL 96-06 and NUREG/CR-5220 a group of utilities and EPRI undertook a program to address the waterhammer issues of GL 96-06 in a "more realistic and cost effective manner". This has resulted in EPRI Interim Report TR - 113594 (Sept, 1999) entitled "Resolution of Generic Letter 96-06 Waterhammer Issues". This large report, which is intended to serve as guidance for plant specific assessments, was provided for our review prior to the meeting. Due to its size, only a cursory review was possible before the meeting. I had a better opinion of the work then than I do after hearing the presentations and delving more deeply into some parts of the report. This (the better opinion) was probably conditioned by the fact that Drs. Griffith, Moody and Wylie served rather extensively in the planning of the experimental program and assessment of the results.

The EPRI report is not well written. The executive summary is superficial. Evidently many different authors contributed different sections and they are not well coordinated. Therefore, it is difficult to follow. It contains three global conclusions which I found difficult to accept based on presentations at the meeting. 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

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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 a valve. In each case it appears that the model assumes that the interfaces remain plane and vertical, although this is not stated. There is no consideration of how the interface on the downstream side can remain vertical. There is no consideration of the stability of the advancing interface. Equation 2.4 of App. E (also a part of equation on p. 9-10) is the thermodynamic relationship for the reversible adiabatic compression of a closed system and is incorrect for the application for several reasons. Its use here implicitly assumes that the gas compresses as though separate from the steam. The two are mixed in the total volume and have common temperature and pressure. Isentropic compression of the gas alone will lead to temperature rise. The value of g is taken as 1.3, correct for low pressure saturated steam but not for the gas. The gas/vapor mixture loses mass by condensation and is therefore an open system. The equation is incompatible with the assumption that the steam in the bubble is always saturated vapor. The bubble is not really adiabatic. App. E indicates that heat transfer to the pipe is negligible but does not comment on sensible heat transfer to the liquid interface. In fact as the void approaches its minimum volume the gas will be highly compressed and hot. Heat transfer will inevitably play some role -- I would expect that its neglect would result in over prediction of the peak pressure. As already stated, the report assumes that the vapor is always saturated. The steam partial pressure is found from steam table data as a function of specific volume. The specific volume is found with the aid of a mass balance on the steam in the mixture employing a constant heat transfer coefficient to get the condensation rate (Eq. 9.2). This equation is flawed both in that the heat transfer coefficient in the problem is far from constant (the thermal resistance is concentrated in the liquid or may involve a gas diffusion boundary layer near the interface and, in any case, is highly transient) and the temperature Ts is not defined. The model contains numerous errors and appears to have compensating errors that would be difficult to assess but may vary on a case by case basis.

Some RBM predictions are presented in Figures 2.4 and 2.5 but there is no direct comparison with data. Section 2.3 is titled Benchmarking the Rigid Body Model but it contains only comparisons with the Method of Characteristics predictions. There appears to be no coherence to the data shown in Figure 2.9-B. Also the numbers on the ordinate are obviously incorrect - they are fractions, not percentages. In any case the rather stochastic nature of the differences between these two deterministic models is difficult to explain.

There is classical literature that is important for giving insight to the waterhammer problem that seems to have been overlooked Rayleigh analyzed collapse of spherical bubbles (both vacuum and with gas) and Cook used the method to calculate the pressure of impact on a small rigid sphere at the center. For cold water his result was 20,000 Atm. This gives a good idea of how extreme the pressure pulse may be in waterhammer and shows the importance of the geometry of the collapsing cavity. Spherical geometry is the most efficient. The plane geometry of models in the report would give the least pressure pulse. In fact the actual water hammer is likely to be very complex and vary widely because the geometry of the collapse is not reproducible. The problem of how to predict the worst real case will not be answered by these simplistic (and erroneous) models. The worst case will result when relatively large bubbles are trapped in liquid that has not been extensively heated near its interface (renewed surface). The report does contain some recognition that there will be mixing in the liquid that has potential to bring the vapor into close proximity to highly subcooled water but it doesn't reach a definitive conclusion.

The role of the subcooling Jakob number was discussed in the report and at the meeting. The interpretation of the physical significance is not correct. They somehow relate it to a thick "thermal layer". In the subject of bubble dynamics it gives a measure of the role of heat/mass transfer compared to that of liquid inertia in controlling the rate of bubble collapse. The form used in the report is the ratio of the volumetric subcooling energy capacity of the liquid to the volumetric heat of vaporization of the steam in the bubble. It gives a measure of the subcooled liquid boundary layer volume needed to condense the steam. In a discussion with Peter Griffith at the break, a not so old classic paper by Florshuetz and Chao (JHT circa 1970 I believe) was mentioned which the authors of this report should study in order to better understand the role of Jakob number.

During the discussion at the meeting I misspoke concerning the effect of steam void fraction on the sound speed in bubbly two phase mixtures. I said the effect is stronger for gas than for vapor whereas the opposite is true, of course (how could my colleagues have let that statement stand). I have no defense for this shocking lapse of rationality. Concerning the "cushioning" effect upon impact the situation is not the same for distributed bubbles as for bubbles confined to the pipe wall. The former is probably well represented by the use of the reduced sound speed of the two phase fluid in the Joukowsky equation. The latter case is , I believe, a two-dimensional problem within a pipe diameter of the surface of impaction. A solid liquid core impacting on the surface should have a pressure pulse given by Joukowsky with the liquid sound speed, but the compressibility near the pipe wall will rapidly reduce the magnitude of the pulse as the reflected wave travels a diameter or two. This is what I was referring to when I asked Dr. Wylie if they had data to show that with bubbles confined to the pipe wall , the pressure pulse is governed by the two phase sound speed. I think that he meant only that he had data to show that bubbles on the wall reduce the sound speed but not to show that peak pressure is correctly predicted by the 1D model with two phase sound speed. Perhaps we may have another opportunity to discuss this point.

The bottom line is that I don't find this report to have adequate technical credibility of the modeling to adequately support resolution of waterhammer issues raised by GL 96-06. I don't think a strong case can be made that the experiments, as valuable as they may be, have identified the worst case waterhammer for this system. The report itself is likely to be found incomprehensible by utility users. I don't think NRR should declare it an acceptable methodology