CT-2074

PDR 3/25/94

SUBCOMMITTEE ON THERMAL HYDRAULIC PHENOMENA
MEETING IN IDAHO FALLS
MARCH 4 - 5, 1993

CONSULTANT REPORT by
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The purpose of the meeting was to review the status of the PWR version of RELAP5/Mod3 code and its capability to represent the AP600 reactor. At the start of the meeting Dr. Dhir asked that the presentations give attention to five technical issues. 1. condensation modeling with and without natural convection, 2. capability of drag models at low flow, 3. accuracy of heat transfer with non condensables, 4. stratified flow models, and 5. CHF at low flow and low power. There was some discussion about the RELAP5/mod3 documentation being out of date. What we received was a Draft issued in 1990.

The staff presented a review of issues for AP600 and SBWR (I am in conflict of interest for General Electric), associated code developments and plans for integral testing. NRC plans to use RELAP5 coupled to CONTAIN to review the vendor testing programs and for audit support of the certification process. RAMONA is being improved for use in stability analysis and TRAC-B is in reserve for possible SBWR use. Modeling improvements are being made in the codes but will not be complete for many months. Plans are proceeding for the AP600 simulation in ROSA -V but RES has decided, at least tentatively , not to proceed with the use of the OSU facility for follow-on low pressure tests under NRC sponsorship. They think now that it may not be necessary to go beyond the vendor integral testing.

The staff presented the results of a peer review of the vol. III & IV of RELAP5/MOD3 documentation. The purpose of the review was to determine the adequacy and completeness of the documentation but did not include technical assessment of the models and correlations. Dr. Catton asked the consultants to review the Models and Correlations document for technical content . My comments on vol. IV are appended at the end of this report.

Improvements in RELAP5/MOD3 were described as in two phases. Phase A , the "get well" phase, was aimed at correcting coding errors and other problems encountered in the use of the code by international participants and was already complete. None of the resulting changes were provided as an update to the 1990 draft documentation. The

Phase B effort, which is to improve the code capability to handle the AP600 and SBWR, is underway and should be completed this fiscal year.

In the discussion of stratification in the AP600 IRWST caused by PRHR operation, I agreed with Dr. Catton's observation that this problem is not within the capability of RELAP5 and a solution will not be found by adjustments in the nodalization. I commented on the strong stratification that we have seen in simple laboratory experiments. We suggested that INEL look at the possible use of lumped modeling as a sort of subroutine to RELAP5. I promised to provide papers that we have recently published that may be useful in this regard. Copies of the following papers are enclosed.

"Transient Thermal Stratification in Pools with Shallow Buoyant Jets" by P. F. Peterson, I. J. Rao and V. E. Schrock

"Temperature Distribution in Pools with Shallow Momentum and/or Buoyancy Sources", by R. J. Fox, D. B. McDonald, P. F. Peterson and V. E. Schrock

"Scaling of Integral Simulation of Mixing in Large, Stratified Volumes" by P. F. Peterson, V. E. Schrock and R. Greif

I had previously raised a question about how critical flow is modeled for steam tube rupture, the point being that ROSA V will use a single break path from the cold plenum of the SG. There are in fact two paths and each presents a unique critical flow problem. Based on a cursory reading of the RELAP documentation, I commented that I found no model for critical flow from pipes. Such a model is needed for the steam tube rupture and for breaks in other locations. Dr. Modro insists that RELAP has such capability via the standard flashing model and calculation of the developing flow. This claim is not justified and I will comment further in my review of the documentation.

REVIEW of RELAP5/MOD3 Vol. IV Models and Correlations.

I found the review of this document to be a very frustrating experience. There is a major problem with the shifting nomenclature, mixing the notation of mathematical analysis and of computer programs, and the very inadequate collected nomenclature. The organization is generally rather good and there is an attempt present an assessment at the end of each model presentation. Unfortunately this often reveals that the basis for some of the coding is lost in history or that the model as coded is either partly or

completely ad hoc and has no engineering basis at all. In some cases the comment is made that further review of the literature is needed for a complete assessment or in others that the absence of information from the literature suggests the need for additional experiments. The document is of poor quality in terms of use of the English language and is often unclear on technical points. It contains many errors (spelling, equations, units, etc.) suggesting a lack of critical review. There should be a list of corrections that have been noted in prior reviews and it should be supplied with the draft. I found myself discovering nits that I later learned had already been discovered by some one else in a previous review. It makes one wonder how this document will evolve into a well written and accurate account of what the code contains and why.

Section 2 provides a discussion of the field equations the code attempts to solve. The purpose is to identify the coefficients contained in the field equations that require closure relations to facilitate solution. The closure relations represent physical laws usually in the form of empirical correlations. The field equations are an essentially ad hoc version of the one dimensional two fluid model of two phase flow (six equations relating phasic internal energies, volume fractions, velocities and a common pressure as functions of time and one spatial coordinate). Two additional equations are used to give a limited treatment of a dissolved specie. The development of the equations is not given and the reference (2.1-1) is not sufficiently available to serve well. There is allusion to similarity of the RELAP equations to equations developed by Ishii or by Hughes but an aura of mystery remains as to just how the equations really derive from the conservation principles. When a non condensable gas is contained in the vapor it is stated that the thermodynamic properties are from Gibbs-Dalton Law. The vapor is generally not well represented by the ideal gas law as implied by the G-D law. I presume they mean that real vapor (steam table state data) pressure is assumed additive with gas pressure from the ideal gas law to get total pressure. No mention is made of how the transport properties (viscosity and thermal conductivity) of the mixture are evaluated and I didn't find this later in the report. Wilke's method is standard for this. In general the role of the equation of state as a part of the equation set is given short shrift.

The confusion in notation begins with the discussion of the basic equations. Internal energy is sometimes denoted I , sometimes U. There is no attempt to be specific about the dimensions of various quantities and sometimes where units are given they are in error. For example in the Nomenclature G (mass flux) is given units kg / s whereas this should be kg / s m^2 . Γ, which is missing from the nomenclature, has the units kg / m^3 s. The variables h_g^*, h_g^s, h_f^* and h_f^s are never defined. The virtual mass terms FIF

& FIG appear in Equations 2.1-26 and 27 without explanation. The same is true for the interfacial friction factors.

The concept of mass transfer rate per unit volume, Γ, is clear but the physical meaning of Γ_w is not clear. In the physical world, all mass transfer occurs at the interface between phases. There is no "mass transfer at the wall" as used in RELAP5, even near the wall during nucleate boiling there exists a liquid/vapor interface where phase change occurs. There is no source of vapor flowing from the wall. The division of Γ into two contributing parts, Γ_i and Γ_w, is completely artificial.

The momentum equations are used in a nonconservative form. No explanation for doing this is given. I didn't find an assessment of the impact of failing to conserve momentum. After setting down the equations that form the basis of the code, they are extensively manipulated to obtain the equations coded in RELAP5. This entails integrating over finite control volumes and forming difference equations relating volume centered average state variables and fluxes at the junctions. It is only possible to follow the gist of the process. The quality or mass fraction is used in these equations without a clear and consistent definition. One has to distinguish static and flow qualities. There is also the often used "equilibrium" quality which gives a measure of the bulk enthalpy of the flowing fluid (it is zero when the bulk enthapy equals the saturated liquid enthalpy although a nonequilibrium two-phase state may exist at this condition). It is necessary to be precise about which quality is used.

Section 3 presents the flow regimes in MOD 3. It is claimed that this is an improved feature compared with MOD 2. Previously problems were encountered when adjacent volumes were in different regimes. The code logic and numerics were changed so that flow regimes are identified at junctions. Some modifications in the maps was carried out for this purpose. Model based maps of Taitel and Dukler and Ishii are modified for simplicity in coding in terms of void fraction and a flow parameter. In the end the maps are essentially ad hoc. However I agree that they are about as good as can be expected considering the rather primitive state of knowledge in this aspect of two-phase flow.

Section 4 presents the Closure Relations for the Fluid Energy Equations. Article 4.1 gives models for interfacial heat transfer. Interfacial heat transfer is expressed as heat transfer rate per unit volume. It is obtained by multiplying a volumetric heat transfer coefficient by a temperature difference, i. e., the volumetric heat transfer coefficient is the product of a standard heat transfer coefficient and the interfacial area per unit volume.

Simplistic models are used to obtain interfacial area. For example, in the bubbly regime bubbles are treated as spheres and a statistical distribution is assumed in relation to a limiting size imposed by Weber number criteria. The heat transfer coefficient is in most cases just arbitrary and motivated only by what the developer deems to be a desirable magnitude. An example is the coefficient Hig. I quote from the document p. 4.1-15:

"The volumetric heat transfer coefficient, H_{ig}, for Bubbly SHG is not based on a theoretical or empirical correlation. The Nusselt number, $Nu_{ib} = 10^4$, is chosen to be large in order to bring the gas temperature rapidly toward the saturation temperature. Function F6 clearly enhances this tendency, especially as ΔT_{sg} increasesing magnitude. Function F7 apparently improves numerical stability for low void fractions. The determination of volumetric interfacial area, a_{gf}, is discussed in sec. 4.1.1.1.1. Clearly, there is room for improving the determination of H_{ig} for this case, although to the best of our knowledge, this might require further expermental work." This statement is typical of those following discussion of 14 various coefficients presented in the section. It is evident that interfacial heat transfer calculation has little engineering basis. By their nature, these ad hoc equations cannot be compared directly with experimental data to assess their value. The interfacial heat transfer package is weak. The same condition exists in TRAC.

Equivalent diameter is a property of the channel. The documentation, p. 4.1-54, refers to Eq. 4.1-53 as the usual definition. These "phasic" equivalent diameters are unusual.

The wall-to-fluid heat transfer is presented in sec. 4.2. The non physical treatment of partitioning wall heat flux into parts (to liquid and to vapor) was discussed at the meeting. In boiling flow the wall heat transfer is entirely to the liquid. All heat and mass transfer to the vapor occurs at the interface. Only in transition boiling, where the wall is alternately wet and dry, is this partitioning correct. The artificial heat transfer directly to the vapor allows some superheating of the vapor that will not occur in the physical system. I recall that vapor superheating was said to have caused a crash of the plant simulator during a demonstration a couple years ago. There may be a relation here.

Correlations are rarely used as published by the original author(s). Arbitrary changes are often made to aid in smooth running but once the practice was adopted the developers seem to make changes for little or no reason. A paragraph on p. 4.2-2 gives an example.

> "The heat transfer package in RELAP5/MOD3 uses heat correlations that are based on fully developed flow, where entrance length effects are not considered except for calculation of CHF. The approach of using these correlations in a

transient code such as RELAP5 is often referred to as the quasi-steady approached. Some of the correlations use a length variable, and the code uses the cell length for this variable. This was felt to be reasonable, since coarse nodalizations are used in system calculations."

It is not reasonable to arbitrarily change the length scale in a correlation to the computational cell length (there are cases where the code arbitrarily uses equivalent diameter instead of cell length, which could be even worse). This produces a dependence upon the nodalization choice which is absolutely ridiculous. The quasi-steady aspect of this statement is a limitation inherent in the code, the importance of which has never been reasonably assessed. What the code really does is to treat correlations, such as Dittus-Boelter for length averaged heat transfer coefficient based upon average heat flux and log mean temperature difference, including the entry region, as giving the local heat transfer coefficient. This comment is more a matter of principle than of practical importance, i.e., in my view it has a relatively small influence on the code results in comparison to other features such as arbitrary switching of length scales. In fact there are places in the system where the coefficients vary significantly within a cell, but the code method averages only by using cell averaged properties in the correlations. The modification of correlations to account for the presence of noncondensable gas is admittedly ad hoc. There is not even an explanation of how it was patched together. It is science fiction, pure and simple. There is a literature on the subject that needs to be consulted.

For the laminar single phase region the code uses only the heat transfer coefficient for fully developed temperature profiles in tubes with constant wall heat flux. The documentation recognizes that friction and heat transfer do not scale with equivalent diameter in the laminar regime. Every channel geometry has a unique constant Nusselt number but the code uses the value for the tube for all. There are others available in the literature. In addition, the fully developed assumption is especially poor in this case. As shown in Kays book, certain variable wall boundary conditions can produce some weird results, far from the fully developed case, in laminar flow. I would be surprised if this weakness of the code caused any major errors in results because laminar heat transfer is probably not a major factor governing the course of a transient. On the other hand, the single value coded does not represent state-of-the-art for a best estimate calculation.

The wall-to-wall radiation should be in the models and correlations document for completeness.

The critical flow model is presented in Section 7. The model uses a local choking criterion in the form of Mach numbers or velocities (Eq. 7.2-25 &23). The

sound speed model assumes that the two phases are in thermal equilibrium but may have different velocities. The virtual mass term plays a major role in the model in terms of momentum coupling of the phases. The sound speed, Eq. 7.2-19, is finally formulated as the product of the homogeneous equilibrium model sound speed times a modifying factor that is a function to the virtual mass coefficient, C, and local slip implicitly represented by local void fraction. The matter of calculating the HEM sound speed is made far more complicated than necessary by introducing specific heats, compressibility and expansion factors for each phase. The coefficients should be defined mathematically to avoid misinterpretation. The basic formulation in terms of derivatives of saturation properties is easily evaluated numerically. I have not checked to see if Equation 7.2-21 produces the correct result. I presume that it does.

The dependence of sound speed upon the factor C is shown in Fig. 7.2-3 for a specified pressure and an unspecified local slip. The virtual mass coefficient covers an unrealistic range (0-infinity). In the classical literature this coefficient is derived from potential flow over submerged bodies or groups of bodies. The single sphere gives a value of 0.5 while slender bodies with the long axis in the flow direction give lower values and flatter objects normal to the flow give higher values. The association of HEM with an infinite value of C is a misinterpretation. There is no slip in the HEM idealization. An infinite C is not required to have zero slip. RELAP documentation recognizes that C for two-phase flows is generally unknown. To eliminate the problem for application of the proposed criterion, the determinant of coefficients, D, is approximated by setting C to infinity and obtaining the simplified choking criterion given by Eq. 7.2-26. (Look at the consequences of setting C to infinity in the momentum equations. They do not reduce to the homogeneous form. Note also that an infinite virtual mass coefficient implies that a particle could not be accelerated by a finite force, no matter how large.) When a function of phasic volume fractions, densities and velocities is equal to the HEM sound speed, the flow is choked. This is an essentially ad hoc model. In the literature on two-phase choking it is well developed that local sound speed deviates from the HEM prediction due to two effects, the presence of phasic slip and thermal nonequilibrium. In some circumstances slip is the more important cause of the deviation while in others the thermal equilibrium is the dominate effect. RELAP5 accounts only for slip, and this is done on an ad hoc basis. It is noted that at the limit $\alpha = 1$, $v_g = a_{HE}$. This ignores the theoretical discontinuity in HEM sound speed at the phase boundary, which is small. At the other extreme, the liquid boundary, the discontinuity is very large. It is the region near this limit where thermal nonequilibrium is likely to play a major role.

On the issue of a model for flashing pipe flow, RELAP5/MOD3 uses the Alamgir-Leinhard correlation to predict the location of incipient flashing. The correlation is semi theoretical and is built upon the theory of homogeneous nucleation. It was developed to represent transient decompression induced flashing and predicts the pressure undershoot at which flashing occurs. Jones used the correlation without modification to show that flashing critical flow in converging nozzles occurs with the location of flashing very near the throat. Amos and Schrock and Lee and Schrock showed that the flashing in straight channels occurs with significantly less undershoot than predicted by A-L. With the location of flashing properly located, the basic equations of RELAP5 could be solved in a finely noded calculation to obtain a prediction of the flow evolution as choking is approached. The noding has to be exceptionally fine near the critical location to retain satisfactory accuracy in the numerical solution. At one time this kind of approach was taken with TRAC but it was deemed too expensive and abandoned. INEL claims that they can do this with RELAP5 but they have shown no details of how they do it. I do not believe they have done the calculation with sufficiently fine nodalization. To summarize on this point, RELAP5 does not have a satisfactory model for incipient flashing in quasi steady flow. The use of the code to obtain critical flashing flow in pipes is not documented. (There is a description of nodalization used to compare the RELAP5 prediction with the Henry-Fauske correlation (Fig. 7.2-7). The scheme uses a pipe with three nodes. This is ridiculous.) RELAP5 documentation fails to recognize the importance of the difference between the area change and pipe friction as the mechanisms driving state change. For single phase gas dynamics, this is the difference between isentropic flow in a nozzle and the Fanno flow in a pipe.

On the correlations for quality entering a branch line under critical flow conditions, I have previously commented that the package supplied by K. Ardron does not represent the data taken at Berkeley for steam and water. The problem lies in the fact that for bottom breaks, the incipient level for vapor pull through does not scale with Froude number alone. On that basis we found a difference between air-water and steam-water. Ardron preferred the KfK air-water correlation. The quality correlation uses the ratio of present stratified liquid height to that at incipient pull through as the independent variable. The KfK correlation for incipient height used in the variable for the correlation of entering quality will predict values that are not in agreement with our steam water measurements. The RELAP5 documentation implies that it does agree.

The RELAP5 critical flow model is very old and has not been seriously reviewed with the view to answering - does it represent the current best estimate for the phenomenon? I think that it does not. There has been extensive use of arbitrary

discharge coefficients in conjunction with the model as indicated in casual comments in the documentation. I think that Dr. Hassan did this sort of thing in his use of MOD3 in calculations to support the Small Break CSAU study. There may be some confusion here with Appendix K requirements. If comparisons of the model with the available experimental data justify development of discharge coefficients, they should be related to the geometry and thermodynamic conditions and incorporated into the code. I have sometimes been lead to believe that this was done, however I found no reference to such factors in the documentation.

The decay heat model is very superficially described in the section on reactor kinetics. This was described in greater detail in earlier versions of the code. The code allows user input of decay power curves for individual applications. It is also supposed to have the option of calculating the decay power using the ANS/ANSI Standard. The documentation should explain in detail how this is done in the code. This is missing from the current Draft.