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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: Review of Documentation for the S-RELAP5 Code

Siemens Power Corporation (SPC) has requested NRR approval of their code, S-RELAP5, for best estimate analysis for loss of coolant accidents and plant transients. The following documentation has been submitted for review:

1. **EMF-2101(P), Rev. 1, Dec. 1999, S-RELAP5 Programmers Guide**
2. **EMF-CC-097(P), Rev. 4, Dec. 1999, S-RELAP-5 Data Input Requirements**
3. **EMF-2310(P), Rev. 0, Nov. 1999, SRP Ch. 15 Non LOCA Methodology for PWRs**
4. **EMF-2328(P), Rev. 0, Jan. 2000, PWR LOCA Evaluation Model, S-RELAP5 Based**
5. **EMF-2100(P), Rev. 2, Jan. 2000, S-RELAP5 Models and Correlations Code Manual**

I have done a preliminary review of these documents with emphasis on the issues of critical flow modeling and code assessment. My comments are provided for consideration at the forthcoming Subcommittee Committee Meeting in early August, which I will be unable to attend.

General Observations

S-RELAP5 is a proprietary version created by Siemens Power Corporation from the NRC codes RELAP-5/Mod 2 (1987) and RELAP-5/Mod 3 (1995). The documentation cited above for S-RELAP5 relies excessively on the documentation of previous codes and code versions, including the RELAP-5 series, TRAC, and SPC (and its predecessors') codes, which themselves are often inadequate in explaining the theoretical basis, justifying approximations, explaining the numerical approach and providing suitable assessment results for individual correlations and prediction of integral system performance. The documentation falls far short of meeting the requirements set forth in the regulatory standards for such code documentation, particularly in the current draft versions of the SRP and the REG Guide, now out for public comment. Much of the assessment appears as superficial reference to assessment documents for RELAP-5 (in some cases TRAC). Document 4, Models and Correlations, contains many assertions like "can be shown"

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and "have been shown" which are not supported by analysis or suitable reference to analysis. S-RELAP5 is intended to provide Best Estimate capability for all LOCAs and plant transients. This requires assessing the error in code output. Several references are found to the CSAU methodology, which suggests that SPC intends to utilize that approach, however, the topic is dealt with only superficially. Inference is made that S-RELAP5 is superior to RELAP-5/Mod 3, but the case for this is not clear and convincing.

The RELAP-5/MOD3 documentation was reviewed by the Subcommittee in 1997 in the context of the AP600 Certification review. In my report dated February 25, 1997, I commented at length on the critical flow modeling and the decay power. Excerpts from that report are appended to this report. I will provide additional detailed comments on these matters in relationship to the S-RELAP5 code. The documentation for RELAP-5/Mod3 was issued in 1995, before the development was complete. Thus the documentation could not have been complete. (In my summary , I pointed out that there remains a need for an in-depth review of the Models and Correlations in RELAP-5/Mod3. If one exists, it has not been reviewed by ACRS.) This fact is not mentioned in the S-RELAP5 documentation where the 1995 RELAP-5/Mod3 documentation is cited. . NRC/RES has halted further development of RELAP-5 and mounted a program to consolidate RELAP-5 and TRAC to provide a single code for NRC's interim needs in the regulatory process. This is not mentioned in S-RELAP5 documentation. Until shown otherwise, one must assume that the serious problems inherent in RELAP-5/Mod 3, are carried over into S-RELAP5. In any case, I find the present S-RELAP5 documentation heavily dependent on RELAP-5 documentation, and an inadequate description of the code, its basis and its assessment.

Detailed Comments on S-RELAP5 Documentation

S-RELAP5 MODELS AND CORRELATIONS CODE MANUAL, EMF-2100(P) REV. 2
Introduction

The Introduction (Section 1) characterizes the code as for realistic LBLOCA analysis and suitable for SBLOCA and transients. It is "based" on RELAP-5/Mod2 and RELAP-5/Mod3 with SPC "improvements". It evolved from ANF-RELAP.

Section 1.1 lists the claimed changes as: (Note that in general no attempt was made to key these items to sections of the report where supporting details may be found)

1. Full 2-D treatment added to hydro field equations. (Evidently Rev. 1 had included 3-D modeling.) SPC has applied 2-D equations to downcomer, core and upper plenum. Some improvements in cross flow modeling are claimed but doesn't tell reader where to find them. I will comment further in relation to the discussion of field equations.

2. Restored terms in energy equation omitted in RELAP-5. Doesn't tell the reader where to find the details.

3. Numerical solution of field equations uses analytic reduction of the equations to a pressure equation. RELAP-5 does this by numerical Gaussian elimination system solver. I haven't tried to assess the merits of this change.

4. State of Steam-Noncondensable Mixture. Computation was modified for very low steam mass fraction (inappropriately called steam quality) to allow presence of a pure noncondensable gas below the ice point (0°C). At lower temperatures there is a vapor pressure. I fail to see how the neglect of this vapor pressure will improve the analysis of accumulator depressurization as claimed. I haven't found further discussion yet. Staff may want to ask where details are found.

5. Improvements in constitutive models are claimed in reference to RELAP-5/MOD2 and ANF-RELAP. It is unclear why RELAP-5/MOD3 is excluded, i. e., it implies that no constitutive model improvements were made for RELAP-5/MOD3. Other specific correlation changes mentioned include replacing Colebrook wall friction (single phase) correlation with one by Jain. I don't know that this is a real improvement but am confident that the small uncertainty associated with the Colebrook correlation has much impact compared to the many approximations built into the code. This was subsequently clarified in section 3.5, p. 3-79, where it is indicated that the RELAP-5/MOD2 numerical evaluation of Colebrook produced errors as large as 25%. Since Jain's correlation reproduces Colebrook within 1%, using it rather than fixing the RELAP-5/MOD2 numerical scheme was more expedient. There is no indication of how RELAP-5/MOD3 dealt with this problem.

6. Heat Transfer Models. One of the changes is the replacement of Dittus-Boelter by Sleicher-Rouse for single phase steam. This may be justified ;but the reason given is suspect - namely that it "produces higher steam temperature...". The justification should be based on comparison with appropriate data.

7. Choked Flow. Some improvement in the numerics is claimed. The model is actually quite poor, as I will discuss in some detail.

8. Counter-Current Flow Limiting. It seems to say that S-RELAP makes the same change as implemented in RELAP-5/MOD3. How is this an improvement by SPC? In any case, the dependence on actual reactor geometries is not shown to be well represented.

9. Component Models This item includes pump performance (I have no thoughts on this one) and elimination of the accumulator model (said to have well-known problems) and replaced by pipe modeling. It is unclear how this works and differs from the accumulator model. An explanation is needed. Lastly, the ICECON containment code has been coupled to S-RELAP5. In principle this is desirable since it should provide better boundary conditions for both codes. However, it should be remembered that containment modeling is built on the philosophy of

conservative, not best estimate. approximations. What do you get when you mate the two animals?

10. Fuel Models. SPC fuel performance models replaced the MATPRO data used in RELAP5. I haven't tried to assess the merits of these changes.

Section 2 Field Equations and Numerical Solutions

The code is described as a two phase, two fluid, six equation model plus continuity equations for noncondensable gases and dissolved boron. Thus there are 8 field equations containing the variables p, U_g, U_f, a_g,a_f,x_n,ρ_B, v_g and v_f. Other thermodynamic variables are functions of these via the equation of state. Although not stated, the liquid and vapor are not necessarily in equilibrium in the field equations. Equations 2.1 through 2.6 are the basic model equations without the noncondensable gas or the dissolved boron. The equations are presented without derivation and the comment is made that there are numerous derivations available. Four references are cited. There is also a comment that "some form of averaging is used in all the work". A derivation should be given for clarity. In a two phase flow, a point will be occupied at an instant by one of the two phases. In addition, the motion of each phase may be turbulent in nature. Thus the description is in time averaged variables. The derivation should reveal how the variables are defined and their physical meaning. The requirements for closure are not discussed. The equations are in the form of point partial differential equations. In the momentum equations, the stresses at the point are not represented by Newton's viscosity law, as in the N-S equations for single phase flow, but instead by averaged forces between the phases and the phases and the wall. They are presented in vector form as for a three dimensional flow. The code's ancestor, RELAP-5 is one dimensional , and thus these forces are expressed per unit volume of a differential volume comprised of the channel cross times the differential length in the flow direction. SPC needs to show how the RELAP-5 equations are valid in 3D form. In particular, it should be explained how the wall can exert a force on the individual phases at points in the interior of the field (FWF and FWG).

Equations 2.3 and 2.4 contain ρ without a subscript. This is not found in the nomenclature but is defined two pages later as the mixture density, Eq. 2.8. Also v_{gl} and v_{fl} are not defined in the nomenclature. Equations 2.5 &2.6 have dot superscripts not described in the nomenclature. This is discussed later in the text but the meaning remains unclear. It has to do with the numerical solution, but then it has no meaning in the p.d.e.s which should be general for every point within the continuum. The term "donor" has no meaning in the p.d.e.s. Also the superscript s on the enthalpies in the last terms in Eq. 2.5 and 2.6 must refer to "saturation", but the nomenclature says they mean "slug flow". These terms appear as vapor enthalpy sources at the wall. If these were true 3-D p.d.e.s, all these wall effects found in RELAP-5 1-D equations (FWF, FWG, Q_{wg}, Q_{wf},

Γ_w)would not be in the conservation equations, but would instead enter as boundary conditions. The concept of mass transfer at the wall is fuzzy. In this context mass transfer means evaporation or condensation. Very active mass transfer may occur near the wall, but at the wall there is either liquid or vapor. No mass transfer takes place through the wall.

The type (font) used in the report makes it very hard to distinguish 1,1 and i in subscripts.

SPC should provide evidence that terms (Eq. 2.13) neglected in the energy equations have little influence for the full range of applications. Actually they assume each term is zero.

The assertion concerning assessment in the first paragraph at the top of page 2-6 is an example of one that needs supporting evidence.

Equation 2.14 implies that the wall is partially wet. Instead I believe that RELAP-5 makes an ad hoc division of wall heat flux between the two phases without regard for the flow regime. This violates the physics since heat can be transferred only to the phase in contact with the wall.

SPC should explain how the last term in Eq. 2-15 represents an "interface transfer term"

SPC should give a better (more physical) explanation of the "wall heat transfer terms" appearing in Eq. 2.16 and 2.17. They contain subscripts denoting "interface", but there is no interface at the wall.

Eq. 2.25 is preceded by the statement "In particular, it can be shown that they should be". This should be shown in the report. (I won't comment on each and every case where such comments appear, however, such comments are a major weakness in this code description.)

Noncondensable Gas and Dissolved Boron

When either of these is present evaporation or condensation transfer only the water species at the interface. This creates changes in concentration of air in the vapor or boron in the liquid near the interface and results is a diffusion process accordingly. Also the dilute species have to be introduced into the system somewhere and mixing occurs in the vicinity of the point of introduction. The continuity equations (2.30 & 2.31) neglect the diffusion terms. SPC should be directed to Bird, Stewart and Lightfoot Art. 18.3 which deals with this matter.

State Relations

1. Single Component

Equations 2.33 - 2.36 contain notation not found in the nomenclature. Eq. 2.37 is the Clapeyron Equation. This exact thermodynamic relationship is put down out of context. It has no relation to the preceding equations and is immediately followed by discussion of properties of the metastable states.

It is not clear how Eq. 2.40 - 2.42 follow from Eq. 2.38 & 2.39 What is meant by a "consistently" extrapolated specific heat and compressibilities? The neglect of a term in Eq. 2.42

is due to the fact that the steam tables do not contain a particular partial derivative. This is not an assessment of the importance of the term. SPC should look at Lienhard's evaluation of the metastable properties of water.

The homogenous equilibrium sound speed is made needlessly complicated. It can be expressed as a function of the saturation thermodynamic properties, their derivatives with respect to pressure and the quality. It may also be calculated as $a = [\gamma pv]^{1/2}$, where $\gamma = v/p(\partial p/\partial v)_s$. Gamma is given in the ASME steam tables (graphical) for the HEM model (but not for low qualities). . .

2. Two Component .

The gas component is treated as an ideal gas. The internal energy is given in terms of absolute temperature. Usually we represent changes in internal energy as $\Delta U = C_v(T - T_o)$, where T_o is an arbitrary datum, when C_v may treated as a constant or by the comparable integral relationship when C_v is a function of temperature. SPC should explain the nonstandard treatment in Eq. 2.49 and give a reference for the table of constants on P. 2-15. Here the value of the internal energy of gaseous nitrogen at absolute zero temperature is given to nine significant figures. Wow!!! SPC should check whether the compressibility factor is significant for the highest gas pressure encountered (600 psia?). The HEM sound speed of the mixture is again unduly complicated. It would be simpler to calculate γ for the mixture and then use the simple relation given above. Overcomplication of the equations actually solved raise concern about accuracy of the results, especially with the complications and variations in the computational scheme described on pp 2-19 & 2-20.

Section 2.5 Other Form and Modifications of Momentum Equations

This is a strange combination of manipulated 3-D vector equations and a specific flow pattern in a 1-D system. The equations in the two bear little relationship and there are many other flow patterns, involving 1-D and multi-D behavior.

Section 2.6 Semi-Implicit Numerical Solution

There is a need to assemble the final form of equations to be solved and then to systematically show how they are reduced to a system of difference equations and how they are solved. I find this section confusing and unconvincing. If I have time, I may try again later to make sense of it.

Section 3 Hydrodynamic Constitutive Models

Flow Regimes

The flow regime characterization is very ad hoc and lacking strong experimental support. The regimes included are simplistic for horizontal and vertical pipes and assume steady state. The complications of bends, internal fixed structures and many other features of actual plants are not represented by these simplistic descriptions.

Virtual Mass

Discussion here and in Section 2.1 do not adequately explain the formulation of the terms in the p.d.e.s or the evaluation of the coefficient. There is some vague discussion about inaccuracies resulting in the spatial derivatives in Eq. 3.94 caused by course noding. Course noding in RELAP-5 and other similar codes is of a matter of concern, which needs to be dealt with in a more general way. But this should not be presented a basis for deleting terms from the analysis. It is not clear that this is an appropriate fix for the perceived problem. SPC should discuss the development of the terms in the momentum equations and give the basis of the virtual mass coefficient used (Eq. 3.95).

The virtual mass effect arises the fact that an object submerged in a fluid is more difficult to accelerate than the same object in vacuum. The reason is that some surrounding fluid must also be accelerated. Potential flow theory (e.g., Streeter's book) has been used to show that a spherical particle behaves as though its mass is is equal to it's own mass plus half the mass of the displaced fluid. Thus the terms added or virtual mass and virtual mass coefficient ($C = 0.5$). Note $m = \rho_s V(1 + C\rho_f/\rho_s)$ per Wallis , p. 319. Potential flow theory has been used for some other simple shapes and for multiple particles and the coefficients found to be both higher and lower depending on shape and orientation to the flow. If there is a wake present, the coefficient is increased. For the complex topology of two phase flow in a confined channel the problem is not so simple and would appear on the face of it to be dependent upon flow regime. I know of no good theoretical develop for the two phase flow virtual mass coefficients. SPC should give the basis of Equation 3.95. I note that it gives values $0.5 \leq C \leq 2.0$. These numbers may seem reasonable. Could that be the basis of Eq. 3.95?

Wall Friction

This section gives a development for the 1-D case based upon the Martinelli type of "two-phase friction Multiplier" It gives no information for FWG and FWL in the multidimensional momentum equations.

Section 5 Hydrodynamic Process Models

Choked Flow

In the introduction the geometry involved is referred to as "break" and "nozzle" . The two are not synonymous. Use of the term nozzle is generally incorrect for reactor geometries.

RELAP-5 and S-RELAP5 use a special model for critical flow. Developed by Ransom and Trapp, the model assumes 1-D equilibrium slip flow, i.e. the phases are assumed to remain in thermal equilibrium but to have different velocities. As such the model is inconsistent with the basic model of S-RELAP5. The Ransom - Trapp (R-T) model is said to be used to calculate break flow and to check for internal choking (possible choking at restrictions within the system. The R-T model further assumes that the approach to the point of choking follows an isentropic process. Thus friction and heat addition, which can each contribute to an increase in Mach number, are absent for the model. This assumption limits the application to convergent nozzle geometry, the only geometry that can produce isentropic flow. The governing equations are given as the mixture continuity equation, two phasic momentum equations and a mixture entropy equation. The momentum equations, Eq. 5.5 &5.6, contain a term multiplied by C, the virtual mass coefficient, but the form of the terms are different from those in Eq 2.3 & 2.4. The spatial derivative terms, omitted in Eq. 2.3 & 2.4, are retained in Eq. 5.5 & 5.6. In addition, the term $\Gamma_g(v_{fi}-v_f)$ in Eq. 2.3 is missing in Eq.5.5. The similar term of Eq 2.4 is not present in Eq. 5.6. SPC should be asked to comment on these differences.

The statement (p. 5-3) "For dispersed flow, the constant has a theoretical value of 0.5; whereas for a separated flow , the value may approach zero" needs supporting evidence.

The dots appearing in Eq. 5.8 and 5.9 are referred to as asterisks in the text.

In the R-T model, the method of characteristics is used to formulate the choking criterion and sound speed at choking in terms of the virtual mass coefficient. The additional statement (p. 5-7) is made that for homogeneous equilibrium, the virtual mass coefficient is $C = \infty$. Figure 5.1 is given as evidence of the significant effect the virtual mass coefficient has on the choked flow. As noted previously, the range of C for all two phase flow regimes, given by Eq. 3.94, is 0.5 to 2.0. SPC should plot the lines for these values in Fig. 5.1 to see what the sensitivity actually is by this model.

The last sentence on page 5-7 involves circular reasoning. The variation from the homogeneous (equilibrium) model result is entirely due to the assumption of velocity equilibrium.

On page 5-11, after Eq. 5.20, it says ..."Eq. (5.19) can be obtained by setting $C = \infty$ in Eq (5.13) and C = 0 in Eq. (5.14) ". What is the rational for this? The first assumes HEM and the second "separated flow". How does one get Eq. 5.19 from this?

The discussion of "Subcooled Choking", Art. 5.1.1.2, is based concepts from the isentropic homogeneous equilibrium model. Again the focus is on a convergent nozzle geometry. The terminology "subcooled choking " refers to choking where the upstream stagnation state is in the subcooled liquid region. The idea that there exists a stepwise change in Mach number at the the onset of flashing at throat follows from the strict adherence to the HEM model, which has discontinuities in the sound speeds at the phase boundaries.. It should be noted that the

assumption of flashing at $p = p_S(T)$, the HEM assumption, is inconsistent with data (e.g., Schrock, et. al, JHT 1976) which show a flashing delay to some level of supersaturation. The fluid is superheated (metastable) at the onset of flashing and this must be followed by a region of non-equilibrium two-phase flow(which is inconsistent with the R-T model). Also the nonequilibrium sound speed in the real flow is quite different from the HEM. The "frozen composition" model like the two component fluid case has no discontinuity, although the sound speed decreases very rapidly with small increase in void fraction near the limit of $\alpha = 0$.. A better explanation of the physics of the choked flow, based on real fluid behavior, is that the fluid reaches the point of flashing at a pressure below saturation and a position slightly upstream of the throat. (Note that it must be upstream of the throat because the flow is not choked on liquid compressiblity. It is therefore necessary for compressibility to increase by addition of void.) As the void begins to develop, the nonequilibrium sound speed starts at the liquid value but rapidly drops as void becomes significant. The combination of acceleration and reducing sound speed quickly lead to the point of choking (at the throat) where the velocity equals the nonequilibrium sound speed. Since both the fluid density and the sound speed are higher than would be the case for HEM, the mass flux is considerably higher than predicted by the HEM model. The value of the critical pressure at this point is lower than the HEM prediction.

The use of the Abuaf, Jones, et. al., method for choking in nozzles with subcooled stagnation states is reasonable although it could be improved by including the liquid compressibility, which is significant for high liquid temperatures. The model neglects the short zone of two phase expansion upstream of the throat and therefore might be expected to over predict the critical mass flux. The method is described in section 5.1.2.1. The flashing delay is predicted by the Alamgir - Lienhard semi-empirical correlation for rapid decompression with an additional term intended to account for the role of turbulence (suggested by Abuaf and Jones). It is applied in S-RELAP5 for break flow in a modified way at the level of the numerical analysis. First, the noding is fixed and the assumption is made that the channel connecting the volume center immediately upstream to the break is a nozzle. The source of this geometric information is not discussed. Presumably all conditions at that upstream point are from the solution of the six equation S-RELAP5 model reduced to the single phase liquid case. Then the incompressible relation (Bernoulli), with throat pressure from the pressure undershot prediction, is used to calculate the throat velocity . HEM sound speed is calculated, presumably for zero void and liquid temperature, and the larger of this sound speed and the calculated liquid velocity is "selected for limiting the mass flowrate". The rate of decompression appearing in Eq. 5.22 is said to be approximated by Eq. 5.25 or 5.26 for "smooth area change" or "abrupt area change". What does this mean? This becomes even more incomprehensible when we read the following paragraph that says "To accurately compute the subcooled choked mass flow rate, the

nodalization needs to be adjusted so that the actual area change rate is close to that given by Eq. 5.25 or 5.26. This places an additional restriction on the geometry of the volume upstream to the break. For LBLOCA conditions, the area change rate is practically zero and is not an important factor. The derivative of v_c with respect to the volume pressure P_k is required to improve the implicitness of the choked calculations." None of this quotation makes any sense to me. My conclusion is that something very strange has been done in applying the Abuaf- Jones method for subcooled stagnation states. The method is not applicable to straight pipes. How can one adjust noding of straight pipes to make them look like nozzles?

The situation for two phase choking is not much better. The Ransom-Trapp model assumes isentropic flow (not friction). Gravity and friction are said to be omitted from Eq. 5.28 for clarity ... but.... "They can be easily added on and are included in the code." If so this is not the Ransom-Trapp model. Integration of 5.28 is done with some undefined average $\alpha\rho$ products. The mixture energy equation 5.30 is integrated to obtain an additional relation. The procedure is then completely ad hoc. It is completed by using the Ransom- Trapp choking criterion and assuming that slip is constant. If virtual mass is ever important it is in the approach to critical where the acceleration is maximum. The assumption of constant slip is ridiculous! And what are we to think of the gravity and friction, not shown in the equations, but actually in the code.
I have read what follows but the confusion is only compounded. SPC should be asked to explain this mess at the meeting. I can only hope that NRR has already put this in the form of an RAI.

Assessment

Assessment of the code is offered in the form of comparison of S-RELAP5 against RELAP5/MOD3 and INEL Table of HEM critical flow vs. stagnation state for a fictitious geometry Figure 5.4. Noding is not mentioned. Geometry chosen for the INEL tables is not mentioned. The break junction (151) is not shown in the figure but it is described as the "abrupt area option" so I suppose it could be regarded as an orifice. If this is the case I would expect to see a discharge coefficient. None is mentioned. Some pride seems to be taken in the fact that S-RELAP5 is closer to HEM than is RELAP-5/MOD3. I presume quality in the tables means equilibrium quality, but this is not stated. Additional assessment is provided by S-RELAP5 predictions of the MARVIKEN data from runs 22 and 24. Geometric details are omitted from the report. Fig. 5.5 shows a straight discharge pipe but no nozzle. Calculations using smooth area change and abrupt area change gave essentially the same result. The modeling of "the short nozzle as a volume of small size' is said to be impractical because the upstream properties cannot be properly computed by RELAP5 (it doesn't say which one). I have difficulty making any sense out of these comparison. It is true that MARVIKEN is not a clean test of the critical flow model, at least not as done, because the behavior in the vessel is complicated and is a part of the code calculation. It is not a clean comparison for a specific flow geometry and fixed upstream state. In

any case, there are many laboratory experiments available for use in assessment. SPC should do more comprehensive assessment.

Appendix Excerpts from my Feb 1997 report

RELAP5/MOD3

The documentation was published prematurely (June 1995), i. e., before all changes in models and correlations were ready for inclusion. There is a need for an in-depth review of the final product. It is unclear what form the final documentation will take. Presumably some sort of addendum is to be prepared. What will assure that all users have the addendum and full information on how to use the combined documents. I expressed concern about the critical flow model when we reviewed the earlier documentation of the code and again during the planning for ROSA use for confirmatory research. Each time the response has been that it is complete and fully assessed. The new documentation appears to be minimally changed, and I believe the code is still inadequate for calculating critical flow for all situations to which it is applied. It does have some discussion about the fact that Ransom/Trapp used in the code predicts higher values that Henry/Fauske (not used in the code) and says that appropriate use of user supplied discharge coefficients takes care of the difference. This somehow implies that the Henry/Fauske is a standard against which the RELAP5 results should be compared.

Critical flow depends upon the geometry of the flow channel and the stagnation state of the fluid entering the channel. Classical gas dynamics provides a guide (insight) to the thermodynamic processes that may be involved leading to choked state. The three physical effects that may drive state change are area change, friction and heat exchange. The processes associated with the individual effects are, respectively, isentropic, Fanno and Rayleigh. The case of heat exchange (Rayleigh process) does not normally arise in reactor systems. The other two do and there may be cases where the both area change and friction simultaneously influence the evolving thermodynamic process. The calculation problem has two parts, the evolving flow (velocity and fluid state) and the calculation of sound speed (to locate the position of choking). In the case of single component two-phase systems the assumption of homogeneous equilibrium renders the isentropic (nozzle geometry) and Fanno (friction in a straight channel) processes analogous to the ideal gas problem. Only the equation of state is more complex, requiring a numerical approach to the calculation. The isentropic case is simpler because

it is reduced to a simple thermodynamic problem. The numerical evaluation of the Fanno flow requires a scheme of fine noding as choking is approached.

In actual systems the critical flow problem is further complicated by nonequilibrium (mechanical and thermal) effects. Both parts of the problem are affected. The flow evolution involves flashing inception (for liquid stagnation states) and phase disequilibrium (mechanical and thermal). In addition, the sound speed is strongly influenced by nonequilibrium, particularly for low quality states. The literature on the subject is very large. Still, there is a lack of general agreement on what is the best method for the very complex physics involved. There is a large body of experimental data for critical flows, but it still falls short of covering the range of geometries and flow conditions that are encountered in reactor accidents. Specifically, the available data are essentially all for flow situations having axial symmetry. There are no data for flow out irregular pipe ruptures such as split openings. Flow through valves is frequently asymmetric. Data for valves is very limited and results are highly geometry dependent. The location of choking has sometimes been thought to move from one position to another. There are no general methods available to deal with these cases. This lack needs to be acknowledged and addressed in some way other than ignoring the problem.

RELAP5 has a very simplistic approach to dealing with critical flow. Its method is inadequate for many of the real world situations. The code invites tuning via the use of discharge coefficients. Discharge coefficients have a legitimate place in flow calculations as a means of correcting the predictions of a clearly identified and simple theoretical flow model (e.g. Bernoulli) for second order effects such boundary layers and vena contracta. Values for discharge coefficients must be empirically established from an appropriate dated base and for the appropriate range of governing dimensionless parameters. It is possible to use this approach for two phase flows as well but it has not been done in a legitimate way for application in codes such as RELAP5. Instead arbitrary discharge coefficients have been used as a convenient tuning device to bring integral test predictions closer to data on global responses (usually not a direct check of critical flow dependence upon the stagnation state and flow geometry). RELAP5 lacks the capability to deal with Fanno type flow with nonequilibrium. INEL people say it can. The documentation does not show this.

Dr. Kelly has introduced what he calls a Modified Henry-Fauske Model into the new frozen version of RELAP5/MOD 3. This was judged a suitable fix to take care of the surprise that the code had calculated flow disagreeing with APEX/OSU data (I believe it was said to be of by a factor of 3). Given the prior insistence that the model had been thoroughly assessed against data and the fact that the 1995 documentation

shows the RELAP5 model to be within 10 or 15% of the Henry-Fauske prediction there appears to me to be a gross inconsistency in this story. I have not seen documentation of the model (Kelly says he presented details in the meeting in Idaho in Feb. 1996). I said in my report on the documents in preparation for that meeting (which I could not attend) that I looked forward to reviewing the details of the model but I still haven't seen a suitable description upon which to give critical comment. I believe I have legitimate claim to being among the major contributors to the literature on the problem (not the foremost expert) and I hoped Dr. Kelly would be interested in hearing my view on the proposed code fix. Evidently not. Dr. Etawila told me that this code change was intended as a "work around" . This and other information discussed in the meeting led to the suggestion (Kress I think) that there be a version of RELAP5/MOD3 designated for AP600 only. This is probably necessary and also desirable (it tends to acknowledge that the code is less than a general tool).

Interfacial drag has long been a target as the single cause of poor code predictions. Over the years numerous changes have been made, some without any bases whatsoever in theory or experiment. I am concerned that what we heard in the INEL discussions on applications indicates that this mentality continues among code developers and users. RES needs to recognize this problem and deal with it. I am referring to the concept that the predictions of global measures should be adjusted by unfounded tuning of constitutive relations. Again, I emphasize that code performance has to be examined on two levels, global measures and direct verification of constitutive relations. There has been a mind set on the former and neglect of the latter. RES implicitly gives support to this unsound approach.

INEL deserves praise for identifying the fact that entrainment from a stratified hot leg into the ADS-4 line involves geometry outside the data base range for the RELAP5 model. RES has commissioned new tests at OSU to fill the need. This is the right approach to code improvement. However, it may be noted that the AP600 geometry has been known for years while the discovery was apparently made only while trying to reconcile APEX data. There may be a lesson here for RES.

RELAP5 has two options for decay power, the Appendix K prescribed 1973/1971 ANS Draft Standard and the 1979 ANS/ANSI Standard which was cited by NRC as an appropriate model for best estimate codes. If RELAP 5 is meant to be used for both EM and BE purposes, it should have the option of the Moody critical flow model, which it evidently does not. Some money was wasted running ORIGEN2 in an attempt to "validate" the 1979 ANS/ANSI Standard for RELAP5. ORIGEN results of course depend upon which version of the nuclide data (ENDF - IV) are used as input. It and

other "depletion" codes constitute part of the basis of the 1979 Standard. It is not in itself a suitable basis for verifying the standard. In fact, the very concept of the standard is to provide a "verified" model. I am constantly amazed by misconceptions about decay power among code developers. RES should now be using the revised version (1995) of the 1979 ANS standard which provides broader coverage of the specific nuclides undergoing fission during reactor operation. The quantitative change in prediction is not very significant, but improvements in the technology should find their way into the NRC codes.

Summary

In summary, I believe the meeting provided evidence that :

1. The NRC/RES program for use of RELAP5 in audit and other in-house assessments of AP600 behavior is likely to be sufficient. Documentation to support this conclusion is still incomplete and must be given higher priority by RES.

2. RES deserves praise for there excellent approach to using test data to broaden the understanding of the AP600 responses.

3. The scaling studies for assessing the adequacy of the test programs appear to be of good quality and give considerable confidence that the experimental data base is adequate. This observation has to be qualified by the statement that the scaling methods used contain some approximations and should not be regarded as completely rigorous. Some thought needs to be given to the possible problems arising from limitations inherent in the assumptions. Also, the documentation is still incomplete. Again, RES needs to give higher priority to documentation (completion and quality).

4. RES still needs to develop better criteria/standards for judging code performance. Some more quantitative approach is suggested. RES needs to encourage a more critical approach for its staff and contractors. It is not in the interest of the public, the agency or the industry "over sell" the quality of NRC codes.

5. There remains a need for an in-depth review of the Models and Correlations in RELAP5/MOD3