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RECEIVED
ADVISORY COMMITTEE
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Dr. Ivan Catton
Chairman, Thermal Hydraulic Phenomena Subcommitee
Advisory Committee on Reactor Safeguards
U. S. Nuclear Regulatory Commission
Washington, D.C. 20555

ATTN: Paul Boehnert

Dear Ivan,

RE: Subcommittee Meeting November 8 & 9
San Francisco Airport

You requested my comments on the meeting and the related background material. I have been ruled in conflict-of-interest for GE and INEL so my comments should be taken with that in mind. Also, I was able to attend the meeting on November 8 only.

Regarding the TRAC-G code capability to address the BWR stability issue, I have the following thoughts and questions. First, I thought that the GE presentation was exceptionally well planned and as a result this was the most informative and productive interactions of a vendor with ACRS that I can recall. I say this, not because I'm interested in seeing GE get some brownie points, but because I have been quite concerned about the quality of technical content of presentations made to the committee both by the industry and the NRC staff. I hope you understand why this happened and can entice others to do as well.

In his review of the background of TRAC-B codes, Dr. Shiralkar made the point that at the conclusion of their joint NRC funded program with EG&G (1984) several models were offered to EG&G for the NRC code, but they were not implemented. I believe these were listed as:

Hot rod model
3-D Kinetics
Numerical efficiency

My recollection of the 3-D kinetics issue in 1984 is that it was NRC-RES that made the decision not to include 3-D kinetics in TRAC-B. I was recommending to EG&G

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that it was needed if the code was to handle scenarios where space-time kinetics are important. Now GE appears to own a better code for a small fraction of the cost. It is another example of what I call a "reactive mode" of planning at RES.

Concerning the TRAC "non-conservative momentum equations", I'm not sure that this is a deficiency peculiar to the stability problem. It is a problem in general for the codes and I'm not convinced that anyone really understands its impact. They are always validated against very global measures and these results are often dominated by compensating errors in the constitutive models. An in depth study of this would be a good task for someone like W. Wulff.

The assumption of quasi-steady drift flux parameters for stability analysis I believe is questionable. This was used to apply Ishii's drift flux oriented interfacial drag correlations to the two fluid TRAC models. I don't think this is very sound where 0.5 H_t oscillations are present. I think there will be very significant profile distortions and local slip will deviate from the steady state. However, I recognize that there are no reliable data to carefully test this premise. Like it or not, the "transient" thermal hydraulics codes are quasi-steady tools. To make them otherwise would require experimental research that RES has felt is unnecessary. I don't agree.

I thought the data from a top blowdown experiment (PSTF-Test 5801-15) was not a good choice for the assessment of the predictor-corrector method. That pressure history is strongly influenced by the details inside the vessel where the code application uses very coarse noding.

Jens Anderson thought exit temperature fluctuations seen in code results but not in the data are caused by a computational interaction between droplet concentration and heat transfer. This seemed quite vague and I would agree with the comment made by John Lee that this difference ought to be carefully examined. S. Z. Rouhani's point about the experimental problem is correct but I don't think this provides a definite explanation.

In the broad view the GE 3-D kinetics model as implemented is fine node axial and course node radial. The 700 plus channels are grouped into 20 transverse nodes. Thus, although GE concludes that "TRAC-G predicts regional oscillations observed under test conditions with no external forcing-perturbation" they also acknowledge that it is necessary to use control rod patterns to guide selection of noding (appropriate for large numbers of channels to be represented by a single neutronic and T-H characterization) for successful simulation. This is something less than a full predictive capability. It would be interesting to know what the code could do if 730 transverse nodes could be run.

The discussions on numerical diffusion and the need for higher order numerics in the stability problem were informative but there is not a clear concensus and I gather that more will be done on this by both GE and EG&G.

The TRAC-BF1 capabilities (1-D kinetics) are clearly inferior to those of TRAC-G. Still, it appears that it will be of some help in meeting NRC's needs.

The conclusion in the presentation by Wilson that the data base is insufficient for assessment of limit cycle amplitude is disturbing as is the conclusion attributed to March-LEUBA concernig bifurcation and chaotic regimes.

I presume we will revisit the stability problem at a future meeting. I am encouraged that there does appear to be a strong drive to get a good technical solution.

Sincerely,

Virgil E. Schrock
Professor

VES/jmh