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4 August 1983

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Dear Mike:

Good to see you again at the meeting on source term estimation last week. Enjoyed the meeting, though I soon came down with a severe viral infection that I initially thought was "Potomac fever," until I found myself wanting to stay out of the office rather than wanting to put in longer hours....

I have gone through my notes to cull some of my better comments for you, comments on the meeting or the material presented the meeting:

1. The control volumes should respond to changes in concentration or temperature over times comparable to the volume divided by the volume flow rate, and such lags may not be being gotten by the current "nodalization" in time, though I cannot tell. Such lags should help spread the pressure pulses, too, thus reducing their peaks, of real importance in determining containment integrity or failure.

2. Hydrody namic interaction among particles in a confined space (hindered settling) looks less important than I thought. Enclosed are copies of pages 49-51 from Fuchs (1964). The correction for the settling velocity is thought to be somewhere between a linear or a cube-root dependence on the volume fraction of particles, which at 1 kg/m^3 would be a volume fraction of 0.001. Might be worth a little more investigation; if the Happel expression $1-1.5(\text{vol. fraction})^{1/3}$ for settling velocity is correct, the settling velocity would be reduced by 15% at v.f. = 0.001.

3. I was glad to learn that radiation effects and electrostatic effects on particle motion were to be checked out.

4. The pool suppression model (SPARC) and the code for predicting sparging should have the gas flow rates checked against the data on which the codes are based: at some volume fraction of gas in the liquid, it may look more like a foam or the bubbles may no longer move independently: the liquid may just be "blown away" in a variety of configurations ranging from a crater to mammoth bubbles.

5. Good to consider a pipe break case for Grand Gulf, just to aid comparison of design factors.

6. The SPARC code predicts less collection of 0.6 µm particles than I would have expected from such a large pressure drop (greater than 10 feet of water).

7. Sedimentation on surfaces should not be offset by an upflow velocity unless that velocity actually emanates from the surfaces: evaporation from the surface will inhibit sedimentation, but upflowing gas past a horizontal object will have much less effect...see the results for upflow through granular beds versus downflow through the same beds. (Inertial effects complicate the analysis somewhat.)

8. Assumptions for particle density such as 10 g/cm^3 need strong justification. This makes a square-root-of-density difference in aerodynamic diameter.

9. I am glad that the ice condensor analysis will be tried again, this time treating the baskets as impermeable to flow, somewhat conservative.

10. Has plugging by the collected aerosols received enough attention? Hard to model, admittedly, but one could check for closing of channels given the amount of aerosol deposited, even if one did not follow the details of the depositing.

11. The uncertainty analysis and sensitivity analysis are of interest. I am enlosing a copy of that kind of study I performed on cyclone models and a paper by Evans, Cooper and Kinney on the four major approaches to error analysis: analytic, distribution theory, experimentation, and simulation. This may be of use.

12. My review of Beal's analysis: He is clearly competant and thoughtful. The problem of different boundary layer thicknesses can be handled by using the smallest (thus, the highest rate) and ignoring the others. In Fig.2-1, the

particle-size dependence of turbulent diffusion seems opposite of what it should be. In connection with the calculations for Table 2-1, water vapor seems to be given a density greater than air, when it should be less. On page 6 it might well be noted that stagnation is unlikely for such a large volume. As noted above, upward flow should have little effect on gravitational settling onto horizontal surfaces in a well-mixed situation. On page 14, it is notable that diffusiophoresis and thermophoresis oppose each other, typically, where droplets are involved (because the heat must be transferred through the air to offset latent heat changes) but not where container surfaces are (the heat can be conducted outward through the solid). If condensation to the walls occurs while the aerosol is present, the consensus among those I have talked to is that approximately that volume fraction should be considered "filtered out" just as though it had been removed through a filtered vent, in agreement with Beal.

13. I believe it would have been more appropriate for Fuller to indicate area-by-area where IDCOR diverges from the approaches taken here. They need not have been debated and some might have been adopted by this group. Instead, there will be somewhat divergent reports having differences that --- sometimes --- will be matters of considered judgment and --- other times ---merely reflecting that one group had a better idea. All the acknowledged "better ideas" might best be in both codes, the differences remaining then reflecting differences at the boundaries of our knowledge.

14. My review of the VANESA status: I agree with the approach and noted no errors. The comment that CORCON underpredicts melt temperature concerns me, though.

15. My review of the ice condensor analysis: Much about the approach is plausible, and the aerosol mechanics and iodine chemistry are handled in detail and well, but the crucial issue is the degree of flow through these beds rather than by-passing them. Further, how much of the ice surface stays available after it melts and slumps a bit?

16 TRAP-MELT: Could well have added Stefan flow, electrostatic interactions, turbulent reentrainment. Page 19: vapor suppression at particle surface would <u>increase</u> condensation. Page 24: Agree that the argument for a net sedimentation velocity is weak.

17. NAUA-4: Lacks turbulent deposition and turbulent coagulation, electrostatic effects, gradient coagulation, justification for the choice of boundary layer thicknesses. The efficiency factor for gravitational coagulation should be a function of the Stokes parameters of the two particles, I think, rather than of their diameters. I think Equation (16) (F_m set to 1) could be solved iteratively for r (by Newton's method for example) in fewer steps than needed to solve it by time-iteration, but this is a research question. Still, it is mentioned that this time-iterating is a limiting factor in the usability of the code. Equation (15) for impaction is questionable, especially in certain Stk ranges.

18.TRACKING SYSTEM...: Some comments for which I am listed include the thoughts of others, with which I do not agree or at least do not understand, but that does not necessarily make the comments invalid. I do have trouble with E.1 where "the aerosols and vapors spiral through the pipes without touching the pipe walls," a situation I think very unlikely. For E.3 I would like to have added, "Concern is that predicted aerosol concentrations of 1 kg/m^3 are surprising, almost without analog in experience, and unlikely to persist for appreciable times in reality." E.4 should mention thermophoresis as well as convection. (Actually, F.4 could be combined with E.3.)

Well, that's it for now. Attached is my rough work sheet for expenses and fee connected with the meeting, along with my receipts. I take it this is enough, since the last such batch was not bounced back to me (June 1983). Take care. Regards to Mel et al., too.

Cordially,

Douglas W. Cooper, Ph.D.