Response to "Comments on Fuel-Coolant Premixing Modeling"

Corradini's letter[1] contains two technical points and one formal point. The following summarizes the technical points:

1. Appropriate experiments to test detailed (two-dimensional, transient, multifield) premixing models may be years away and in the absence of such the "most fruitful" approach to identifying mixing limits is by looking toward overall effects that limit mixing.

2. ". . . I am troubled by predictions without some concept of dynamic fuel fragmentation [he means breakup!] and multifield modeling." The formal point has to do with the relative value of developing models (codes), undertaking comparative analyses of their results, and abstracting code names and respective "capabilities" in the form of tables.

We respond to these two technical points as follows:

1. As examples of his most fruitful approach, Corradini cites the 1981 work of Fauske and Henry and his own extension of it. This preference in contrast to multifield modeling is puzzling at best, and we certainly do not agree with it. The reasons are straightforward. First, this most fruitful approach was reviewed extensively [i.e., U.S. Nuclear Regulatory Commission (NRC) IDCOR workshop, Steam Explosions Review Group[2]], and it failed to be convincing. In fact, a good deal of the impetus for the two-dimensional, transient, multifield modeling was generated by the general dissatisfaction with such "overall effects" (most would characterize it "simplistic") approach. Indeed, this was one of the major conclusions of the NRC IDCOR Workshop on Steam Explosions several years ago, in which both Corradini and I participated. Second, the processes that limit premixing are the same in both modeling approaches, i.e., driven by vigorous steaming. Thus, the lack of data should be felt in the one just as much as in the other (approach). Third, the physics and modeling of Corradini's most fruitful approach is only a small subset of what is considered in multifield modeling. These extra dimensions (of multifield modeling) *are essential* to planning experiments and to assessing premixing with the necessary broad perspective in the interim.

2. As we have pointed out previously (Part II of our papers under discussion[3]), a large melt pour will have to pass through several perforated plates on its way to the lower reactor vessel head. This process will impose a length scale (of breakup) on the fuel stream that may be somewhat greater than the capillary length (of the order of 1 cm) but will tend toward it as it falls through the water. In our calculations we have now (Part V of our papers, in preparation) covered length scales from 1 to 5 cm. As expected, the results indicate that finer and finer breakup produces less and less premixing. Thus, by ignoring any additional breakup, due to steam flow and two-phase turbulence, we have produced conservatively high results. Until such breakup phenomena are better understood, we prefer to retain this conservatism in the predictions.

Finally, turning to the formal point, we would like to make the following comments.

Corradini states: "If I look at the details of Fletcher's results . . . and the new results of Theofanous in his letter there are large differences as well with some of the detailed predictions of the other models in Table I." The fact is there are *no* other results with multifield modeling available! Even Fletcher's results (see also our response to Fletcher in this issue of *Nuclear Science and Engineering*) referred to by Corradini are contained in a letter to the editor and they can hardly provide the basis for comparison and scrutiny envisioned by our probabilistic methodology (see Part I of our papers).[4]

Corradini quotes from our emphasis that proclaiming one's modeling capability is not any good until results are produced and documented, and he states that: "I do not think this statement is the proper way to view computational model comparisons in the *absence* of real data. We must not only scrutinize the *results* of calculations, but also the *models* that are used to produce these results." This is indeed profound! The point we are making, again, is that the viability of a modeling approach is in question until it *and its results* have been documented. For example, the Phoenics model (first entry in Corradini's table) could not produce results at low pressures, which are the primary interest in this problem. Furthermore, after the initial scrutiny of the models themselves, only through their results can one learn the impact of differences in modeling approaches, and thus be guided, synergistically, to the next improvements.

Finally, Corradini's table needs to be put into some sort of perspective. As already mentioned, the Phoenics model could not produce results of interest in this problem and it has been abandoned. The IFCI has not been utilized yet for reactor predictions; indeed, it has not even been documented (the only reference available[5] deals with the breakup model). The CHYMES model documentation is scattered among various publications, but it is reasonably complete. The first reactor predictions have just become available in a letter to the editor.[6] Due to serious shortcomings in the modeling approach taken in CHYMES, we have disputed these results (see our response to Fletcher and Thyagaraja). Our ALPHA-PM model has been fully documented together with reactor predictions and comparisons with our two-fluid model results.[7] The SIMMER and TEXAS models are not viable because the first sets fuel and water velocities equal and the latter is one-dimensional. On the lighter side, let us note that according to this table the IFCI "may allow numerical mixing," while for ALPHA-PM . . . "numerical mixing by Eulerian fuel field," and for CHYMES . . . no comment!

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