data for certain quantities such as axial slip and droplet mass-transfer coefficients are not available.

5 Applying the equation, with the axial slip ratio equal to one, to experimental burnout data in fog flow, results in an order-of-magnitude agreement between calculated mass-transfer coefficients and single-phase friction factors.

6 It is recommended that future investigations of burnout in turbulent flows concentrate on gaining insight into processes related to the diffusion of droplets and their evaporation at heated walls.

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DISCUSSION

H. S. Isbin[3] and H. Fauske[3]

We are in agreement with the authors' Conclusion No. 6, and we appreciate the value of investigating physical models for gaining insight on the steam-water burnout phenomenon. At the University of Minnesota, studies have been under way for about five years on burnout models. Our approach has included a droplet diffusion, and we have also included re-entrainment and vaporization in our balances on the liquid film. The condition that we have used for establishing burnout is the disappearance of the liquid film. At burnout, liquid water is still present but is dispersed as droplets in the vapor core. The details of the model and the comparisons with data at 2000 psia have been presented at the June, AIChE Meeting, Mexico City, and the paper is being submitted for publication along with the authors' paper.[4]

We have defined our concentrations of water in the core in a more rigorous manner than the equation (4) used by the authors, and we have introduced a droplet diffusion coefficient k_G, similar to the authors'. We were not as optimistic as the authors in thinking that we could utilize molecular transport in interpreting the droplet diffusion. Our interpretations of burnout data available to us yielded the result that k_G was proportional to $G^{-0.5}$: Explanations of this relationship have been offered.

Further, applications of our model have been made successfully to hot-patch burnout tests.

With the experience we have gained in our model studies, we would suggest to our colleagues that Conclusions 1, 2, and 3 have been too enthusiastically stated for the actual results presented.

Authors' Closure

We agree with Professor Isbin and Mr. Fauske that we are enthusiastic about our model but believe that we stated our conclusions fairly.

We do not believe that the diffusion of droplets is handled more rigorously by them in the paper referred to in Footnote 4 of the Discussion than by us in our paper. For instance, their assumption of constant velocity throughout the core at any cross section, leading to their equation (5), is quite equivalent to our assumption of a constant droplet concentration at any cross section, leading to our equation (4). Neither assumption is correct. Experimental evidence suggests that droplet concentration and velocity profiles are intimately related.

Professor Isbin and Mr. Fauske state that they are not as optimistic as the authors in thinking that they could utilize molecular transport in interpreting the droplet diffusion. We do not know what prompted their statement, since we too believe that molecular effects are negligible, except possibly for the smallest droplets.

We agree that there are burnout data which indicate that k_g is proportional to $G^{-0.5}$. However, there are also other data which indicate that the exponent has a value other than -0.5 and, in fact, can be positive.

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