LETTERS TO THE EDITOR

To the Editor:

I have read with great interest the paper "Constitutive Equation for Drift Velocity in Two-Phase Annular Flow" (Ishii et al., 1976). The paper is a significant contribution to the drift flux theory literature. I wish, however, to point out to your readers some logical contradictions contained in the paper. I also wish to expand somewhat on some of the authors' caveats on limitations of both the drift flux theory and vapor drift velocity correlations.

A major thesis of the paper is that the simplification of the two-fluid model from six field equations to only four field equations, "makes the use of the drift flux model a very attractive and powerful technique for analyzing a number of engineering problems." The two field equations eliminated are one continuity equation and one momentum equation. This simplification process "requires some drastic constitutive assumptions . . . It is therefore natural that some of the characteristics of two-phase flow will be lost." It is emphasized that the two-fluid model is also complicated "because of the several necessary constitutive equations" which "should be formulated fairly accurately to offer any meaningful advantage of the two-fluid model over the drift flux model." Furthermore, "accurate constitutive equations for the interaction terms under transient conditions are largely unknown. Moreover, the use of existing inaccurate constitutive equations can result in numerical instabilities, since the two-fluid model is inherently unstable owing to Kelvin-Helmholtz instability unless a proper stabilization mechanism is built in to the model through constitutive equations."

The authors then proceed to derive the annular flow vapor drift velocity constitutive equation which is meant to be applied "in the area of computer code developments for transient thermohydraulic analyses" by stating that "a natural starting point for its derivation is the momentum equations for two components." The use of several empirical two-fluid steady state friction correlations are later justified by stating: "It is a common practice to apply constitutive relationships obtained under steady conditions to the transient problems, with an assumption that the parameters entering into a constitutive relationship are local variables and are functions of time." Near the end of the paper, after the annular vapor drift velocity correlation has been derived and drift velocities of up to 3 m/s are computed, it is concluded that "the basic assumption of the drift flux model is that there exists a strong coupling between the motions of two phases. Therefore, certain two-phase problems involving a sudden acceleration of one phase may not be appropriately described by this model. In these cases, inertia terms of each phase should be considered separately, that is, by use of two-fluid model." Only the stratified flow regime in horizontal flow is more loosely coupled than annular flow.

The drift flux model as presently being programmed (Wulff et al., 1975*a*, 1975*b*) can be derived from the two-fluid (Ishii, 1975) or separated flow (Lahey, 1974) model of two-phase flow. This has been shown by Lahey (1974) and Ishii (1975). If no simplifying assumptions are made, then the drift flux formulation contains no more information than the two-fluid or separated flow model. In referring to transformations of the two-fluid continuity and momentum equations, Lahey (1974) states, "all forms correctly describe the physics involved and will yield the same quantitative information about the flow field . . . integration and numerical evaluation . . . will produce the same results."

Both models would presumably have the same numerical instabilities discussed by Ishii et al. (1976). In practice, the drift flux or diffusion model does make some simplifying assumptions, some of which are discussed by Ishii et al. (1976). By actual accounting, the two-fluid model has thirty-two equations for thirty-two unknowns, and the drift flux model has twenty-seven equations for twenty-seven unknowns in order to mathematically close the system (Ishii, 1975). Five assumptions must, therefore, be made. Some of them might be considered drastic, depending on the circumstances.

Aerojet Nuclear Company (WRS Research Report, 1973) and Lyczkowski (1975*b*) present relative velocity or slip correlations for several flow regimes including annular flow. These correlations were developed for RELAP4 (Moore and Rettig, 1973) by utilizing several of the physical models developed for the two-fluid model SLOOP code effort now indefinitely suspended (Cottrell, 1976). These models have recently been presented (Solbrig et al., 1976). The general slip or momentum difference equation (Bouré et al., 1973;

Lyczkowski, 1975*b*) was used as the basis of the derivation. The following assumptions were made: wall frictions are negligible, inertial effects are negligible, quasi steady state is assumed, and momentum exchange between phases caused by mass exchange is negligible.

The relative velocity expression for annular flow was inserted into an experimental slip version of the RELAP4 code (WRS Research Report, 1973). However, when subsequent computational difficulties were experienced, empirical nonmechanistic slip correlations replaced the mechanistic annular flow slip flow correlation (Fischer, 1975). Recent work has explained that the difficulties may be due to numerical instabilities caused by the appearance of complex characteristics which persist even after further simplifying assumptions are made (Simpson and Rooney, 1975; Lyczkowski, 1975*a*; Tentner and Weisman, 1976; Stewart, 1976). It should be pointed out that the regions of instability are not as large as the complete two-fluid model (Lyczkowski et al., 1975), so the simplifications do help the situation.

The drift flux model does have great pedagogical value. Lahey (1974) points out, "the center of mass formulation . . . lends itself to a readily understandable interpretation of the various terms and is consistent with classical techniques used in the kinetic theory of gases. However, one must have information about the functional dependence of V_{gj} [the vapor drift velocity] before it is a useful formulation, a requirement which can somewhat limit the practical application of this formulation." I wish to conclude this letter by making the following points:

1. The drift flux field equations can be obtained from the two-fluid model field equations.

2. Drift flux vapor drift velocity correlations can best be derived from the two-fluid momentum equations and correlations, and hence both models experience the same uncertainties concerning phase interaction terms.

3. Acceptable vapor drift velocity correlations are presently only available for steady state gravity dominated flow in vertical ducts (Wulff et al., 1975*a*).

4. Given the same set of simplifying assumptions, both the two-fluid and drift flux models will contain exactly the same information and will produce the same description of the flow field. Certain transformations of the two-fluid or drift flux field equations may, however, be better suited to a particular numerical treatment.

5. The reduction in the number of total unknowns in the drift flux model over the two-fluid model is not significant. This reduction in the number of unknowns is at the expense of an equal number of additional assumptions, some of which may be drastic.

6. The drift flux formulation and simplifications of it do not completely eliminate numerical instabilities caused by complex characteristics.

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To The Editor:

Stability of Film Flow Down a Vertical Cylinder

Lin and Liu (1975) derived an equation of motion of the free surface of the falling liquid film over the inner or outer surface of a vertical cylinder. On the basis of this equation they obtained the stability criteria for the film coating of wires and tubes. The stability of film coating of wires has recently been reinvestigated by Zollars and Krantz (1976). In that work they made some criticisms on the work of Lin and Liu. Because of the limited space, we address ourselves only to two of their rather serious criticisms.

1. The normalization of the velocity by the film surface velocity W_o is improper. This normalization leads to unbounded wave speed and perturbation velocity in the limiting case of zero Reynolds number, *Re*, since $Re \rightarrow 0$ implies $W_o \rightarrow 0$.

2. The results of Lin and Liu are limited to the range of curvature for which $0.5 \leq 1/[1 + (h_o/r_o)] < 1$.

We give the following answers to the criticisms.

1. By use of calculus, we have

$$\lim_{W_o \to 0} \frac{\text{dimensional complex wave speed}}{W_o} = \lim_{W_o \to 0} \frac{W_o c}{W_o} = c = c_r + ic_i$$

where c_r and c_i are respectively the *finite* dimensionless wave speed and the amplification rate, the explicit expressions of which are given by Equations (17) and (18) of Lin and Liu. Similarly,

$$\lim_{W_o \to 0} (\text{dimensional perturbation velocity}/W_o) = \lim_{W_o \to 0} (W_o \psi_\eta/\eta, \; -W_o \psi_\xi/\eta)/W_o$$

$$= \lim_{Re \to 0} (\psi_\eta/\eta, \; -\psi_\xi/\eta).$$

The explicit expression of the stream function appearing in the above equation is given in the Appendix of the work by Lin and Liu. The expression is lengthy and will not be reproduced here. Note that in this expression of ψ, Re appears only in the numerator. The above limit, therefore, does not become unbounded as $W_o \to 0$ or equivalently as $Re \to 0$. Thus we have shown that the complex wave speed and the perturbation velocity remain bounded as $W_o \to 0$, contrary to the assertion of Zollars and Krantz.

2. With respect to the criticism that our results are valid only in the range of curvature for which $0.5 \leqq [1 + (h_o/r_o)] < 1$, we point out that the indicated upper bound has nothing to do with the curvature. The upper bound in the above inequality is satisfied for any finite h_o regardless of the value of the curvature $1/r_o$, however large it may be. The lower bound given in the above inequality does not apply in our analysis. Note that the radial distance r is normalized in our analysis by the film thickness h_o; that is, $r = h_o\eta$. This guarantees that the terms associated with curvature in the governing differential equation and the boundary conditions (that is, the terms with η appearing in the denominators) remain the same in order of magnitude for given flow parameters even if the curvature approaches infinity as $r_o \to 0$, as long as the film thickness h_o also approaches zero at the same rate. Since then $1/\eta = h_o/(r_o + h) = 0(h_o/r_o) = 0(1)$, for $r_o \to 0$, $h_o \to 0$. Therefore, no limitation on curvature is necessary for the validity of our results. However the "thin film thickness" condition, $h_o \to 0$ as $r_o \to 0$, must be imposed. Unfortunately, this condition was not pointed out explicitly. Nevertheless, it was implied in our "shallow water expansion." It should be pointed out that in Goren's (1962) analysis of the limiting case $Re \to 0$ of our problem, he did not have to invoke this condition. This may be the reason why our results for the limiting case of $Re \to 0$ do not agree completely with Goren's *analytical* results, although the disagreement is within the experimental scatter. The disagreement may also be due to the fact that our asymptotic solution was carried out only up to $0(\mu)$, where μ is the small ratio of the film thickness to the wave length of the disturbance.

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Reply

We state in Krantz and Zollars (1976) that the no-flow case is an improper limit for the scaling employed by Lin and Liu (1975) since ". . . the perturbation velocities and complex wave velocity are nondimensionalized with the surface velocity, and thus are not well-behaved in the limit of no-flow." The scaling employed in Krantz and Zollars (1976) follows the formalism of the method of multiple scales as described in various standard references. [See, for example, Cole (1968).] A cardinal rule in such scaling techniques is that the dimensionless variables be well-behaved in any limit process considered. Nondimensionalizing with a parameter which is allowed to approach zero will lead to erroneous results, as evidenced by Figures 3, 4, and 5 in Lin and Liu (1975). These figures indicate a nonzero dimensionless phase velocity at zero Reynolds number, thus implying traveling waves on a nonflowing film. This physically inconceivable prediction emanates from Lin and Liu's improper scaling. Further confirmation of the error incurred by Lin and Liu's improper scaling is indicated by the unnumbered equation given for the dimensionless amplification factor c_i on page 779 of their paper. If one recasts this equation for c_i in dimensional form and considers the limit process suggested by Professor Lin in the preceding letter, one finds that c_i^* (dimensional) $= 0$ for all wave numbers when the surface velocity of the film $W_o = 0$! That is, all disturbances for the no-flow case are predicted to be neutrally stable. This result of Lin and Liu's analysis disagrees with the predictions of Goren (1962) and Krantz and Zollars (1976).

The limit process considered by Professor Lin in the preceding letter is applied incorrectly. Indeed, $\lim_{W_o \to 0} (W_o c/W_o) = \lim_{W_o \to 0} (c^*/W_o)$, where c^* is the dimensional complex wave speed, is undefined, since c^* is in general nonzero when W_o approaches zero. One cannot assume that c remains constant as W_o approaches zero, since c is nondimensionalized with respect to W_o. Permitting W_o to approach a limit in one part of the argument while at the same time holding it constant elsewhere in the same argument violates the basic rules of the calculus! Therefore, we maintain that our criticism of Lin and Liu's scaling is justified.

We also maintain that Lin and Liu's results for film flow down wires can be compared with those of Goren (1962) only over the range $\frac{1}{2} \leqq r_o/(r_o + h_o) < 1$, where r_o is the wire radius and h_o is the film thickness. [Note that these bounds are stated incorrectly in the preceding letter of Professor Lin.] These bounds emanate from the bound which must be placed on the curvature group in the scaling. Consider, for example, Equation (9) in Lin and Liu (1975). A number of terms in this equation are proportional to $1/\eta$ raised to some power, where $\eta \equiv r/h_o$. Thus when η becomes very small these terms can become very large even though the wave number $\mu \equiv 2\pi h_o/\lambda$ is small. For example, if the term containing $\mu^2\eta^{-2}$ is considered to be of $0(\mu^2)$, then η^{-2} must be of $0(1)$; however, if this term is considered to be of $0(\mu)$, then $\eta^{-2} = 0(1/\mu)$. Thus, there must be a bound on the curvature. The bound $\eta_o^{-1} \equiv h_o/r_o = 0(1)$ must be inferred in the analysis of Lin and Liu in order for their results to be obtained. In terms of Goren's (1962) dimensionless radial coordinate this bound translates to bounds we have indicated for Lin and Liu's results. Professor Lin maintains that no bound need be placed on r_o/h_o because h_o approaches zero at the same rate as does r_o. We disagree with this contention since h_o and r_o are independent parameters. Clearly, in the case of long waves μ can be small without h_o being zero. Similarly r_o can be zero for nonzero h_o; consider, for example, the limiting case of film flow over a wire corresponding to a column of fluid. On physical grounds it is unreasonable to contend that the film thickness is determined uniquely by the wire radius. Thus, we do not accept Professor Lin's ordering arguments regarding h_o/r_o.

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