exchange between phases are written in a form corresponding to the Stokes regime of particle streamlining by a gas. The results derived in this case may be valid for flows with extremely fine particles and for particle velocities which are very slow relative to the gas ($Re \leq 1$).* However, for a large number of applications (nozzle assemblies forced injection of substantial amounts of liquid into the gas flow) of particular interest is the region of the substantially larger Reynolds numbers, where equations such as (9) and (10) are applicable, these describing the force and heat interrelationships between the phases. It is obvious that the solutions derived in the articles referred to by Starkov and those which we derived cannot be the consequences of one another and they each have different areas of application.

We are amazed at his reference to the Kliegel paper because the condition $(w_m - w_d)/w_m$ = const was imposed there on the flow and it is precisely this quantity which was referred to as the "lag." In our paper we assumed the condition $w_m - w_d$ = const, which corresponds to a monotonic reduction in "lag" along the nozzle. Thus, essentially we are speaking of different problems.

Indeed, we took into consideration the volume of the liquid phase in determining the cross-sectional area of the nozzle. But because of the adopted assumption (item 5) to the effect that it is exclusively the force of aerodynamic drag that exerts significant influence on the dynamics of the drop (rather than our failure to account for the volume of the drop), the area occupied by the liquid is included only in Eq. (5) for the drop flow rate.

The special case (16) cited in the article obviously does not exclude solution (15), which we derived with consideration of the transfer of heat between the phases. Elimination in (15) of the exponential term, strictly speaking, does not suggest the absence of heat transfer between the phases, but only indicates the limited extent of this transfer, since in this case we have the condition $T_{d_0} \approx T_{m_0}$.

In conclusion, we should like to apologize to the readers for our insufficiently thorough treatment, in the article under discussion, of the comments referred to in this note.

IN ANSWER TO THE REPLIES OF KAPURA et al., AND SELIVANOV AND FROLOV

V. A. Starkov

1. Kapura et al. begin their reply to the "Comments on the articles..." with the explanation that their assumption of an absence of heat transfer was needed solely to explain the mechanical effect on the process of two-phase flow in a nozzle. However, such a formulation is by no means new. Altman and Carter [1], as far back as 1956, prepared a survey of the literature on two-phase flows, and it was found here that the velocity lag of the particles exert considerably greater influence on the parameters of the mixture than does the temperature lag. This conclusion has been examined on numerous occasions and in great detail in many papers concerned with two-phase flows. Thus the authors of the article were studying a problem that had long since been resolved, widely discussed in the literature, and in no way in need of further investigation.

2. The authors contend that the equation of motion cited in the "Comments...' is a special form of their equation of motion. Apparently, the authors had not familiarized themselves with the papers from which this equation was taken. The coefficient ν is not a constant, as is erroneously assumed by Kapura et al.: it includes the function that depends on the Reynolds number, i.e., this equation of motion is written in the most general form. In this connection, it should be noted that the authors cite the relationship for

*Here and beyond we use the notations and numbering of the formulas that were adopted in the article being discussed.

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the resistance factor, when Re ≤ 1, rather than when Re ≤ 5.8; this is a well-known fact which is covered, in particular, by Selivanov and Frolov in their reply to the "Comments... ." For large Reynolds numbers we have more exact experimental relationships [2] than those cited in the "Response."

3. Kapura et al. explain the derivation of the second equation in system (3) – a system which, according to them, is solved for the derivatives. In this derivation they use a formula $a^2 = g dp/d\gamma_g$ for the speed of sound in the case of a nonisentropic flow. Usually (see references [3, 4] of the "Comments...") the speed of sound is defined as $\sqrt{\left.\frac{dp}{d\rho}\right|_{s=\text{const}}} = \sqrt{\varkappa_g R_g T_g}$. Deviation from this generally accepted relationship, in this case, leads to a situation in which the Mach number M cannot be expressed in terms of finite gas parameters. Indeed, if we express the derivative $g dp/d\gamma_g$ from the equations of energy, momentum, and state for the gas phase, we obtain the following equation:

$$a^2 = \frac{dp}{d\gamma_g} g = \varkappa_g R_g T_g - \frac{g_s}{g_g} \frac{(w_g - w_s)}{\left(1 - \frac{c_p}{R}\right)} \gamma_g \frac{dw_s}{d\gamma_g},$$

i.e., in this case the expression for the speed of sound includes the derivative and system (3) is not solved for the derivatives. It will be solved for the derivatives if we neglect the second term in (1), in which case we have derived the formula for the "frozen-in" speed of sound. However, the assumption that this term is small is by no means self-evident, and it is difficult to state the error which such an assumption will yield. Accordingly, all of the numerical calculations carried out by the authors are cast in doubt. If they had used the formula for the frozen-in speed of sound $a^2 = \varkappa_g R_g R_g \neq (g dp/d\gamma_g)$ from the very beginning, as was done by Kliegel, Glautz, and numerous others, they would have derived the second equation of the system in the form in which it is presented in the "Comments...," and system (3) would indeed be solved for the derivatives.

4. At the conclusion of their response, Kapura et al. contend that, unlike other papers, theirs gives an evaluation of the influence exerted by the transfer of heat between the phases on the specific impulse, that they provide an explanation for the effect of the weight composition and the particle dimensions on the shift of the critical cross section, etc. In this regard, we would like to say the following. First of all, as was stated earlier, all of the results from the numerical calculations are in doubt. Secondly, all of these problems are covered extensively and in great detail in the literature, e.g., in articles referred to by the authors, and in references [2-7].

Let us now turn to the objections of Selivanov and Frolov.

1. These authors contend that the results achieved by Hassan and Kliegel are valid for flows with extremely fine particles moving at slow speeds relative to the gas (Re ≤ 1). This is by no means the case.

For example, it is demonstrated by Carrier [6] that with spherical particles present in the flow the ratio $c_\varkappa$ Re/Nu $\approx$ const even for flow regimes which do not remotely obey Stokes' laws. In this event, all of the results of Hassan and Kliegel are applicable. As regards an exact consideration of the changes in the transport coefficients, Selivanov and Frolov are a long way from having solved this problem. There exist more exact experimental relationships [2], and moreover, the expressions for these coefficients include the viscosity which is a strong function of temperature, a fact which the authors fail to take into consideration.

2. Further, the authors contend $w_m - w_d$ = const, they have resolved a problem that is fundamentally different from the Kliegel problem. However, in actual fact in both of these solutions we are dealing with special cases of the inverse problem – the problem of determining a nozzle profile for a specified relation ship between velocity and nozzle length, something that had been investigated by Hassan in a more general case. We can concieve of many conditions such as $w_m - w_d$ = const and for each of these conditions we can find a solution for the inverse problem. However, there would hardly be any merit in communicating these solutions in the form of articles, particularly in view of the fact that this solution is cumbersome, exhibits no apparent physical sense, and has not been carefully formulated, as is the case here.

3. In their response, the authors say that they have taken into consideration the space occupied by the particles. However, their thoughts as to the possibility of taking into consideration the space occupied by the particles in only a single equation for the flow rate of the particles does not stand up under criticism.

It is demonstrated in [6] (see the references to the "Comments...") that consideration of this space is required both for the energy equation and for the momentum equation.

4. The special case cited by the authors – the absence of heat transfer – is exceedingly trivial and hardly worth mentioning.

In conclusion, it should be pointed out that the authors' explanations of these two articles served only to reveal additional errors on their part.

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