Thus the formula recommended by Starkov describes only the motion of particles in the region of laminar particle streamlining, where the resistance is determined according to Stokes's law. Using this equation to describe the region $Re_{rel} \geq 5.8$ leads to a gross error, since in this region dw_g/dx is not proportional to $[(w_g/w_s) - 1]$. To be fair, is should be pointed out that here we used the data from the streamlining of a single sphere in a gas flow. According to the data of [1], in the transition and turbulent regions the relationship between the resistance factor and the Re_{rel} number for a system of moving spherical particles will differ from the analogous function for a single sphere in a gas flow.

Since Starkov did not understand how we derived the second equation in system (3), probably because we failed adequately to detail the transformation of system (1) to the form of (3), we will again present the derivation of this equation. Let us introduce the speed of sound as $a^2 = dp_g/d\gamma_g$. Substituting this expression dp/dx from the momentum equation of system (1) into the continuity equation, we obtain the condition for the inversion of the effect in the form

$$\frac{dw_g}{dx} = \frac{1}{M^2-1}\left(\frac{w_g}{F}\frac{dF}{dx} - \frac{g_s}{g_g}M^2\frac{dw_s}{dx}\right).$$

It was not the purpose of our article to describe the methods involved in the numerical calculation, and in particular, the passage of the point M = 1, since this method is sufficiently well known. One of the sources [2] in which the method for the passage of the point M = 1 is cited in the Starkov article.

As regards the specific impulse, in certain cases it is convenient to refer it to the gas phase. This makes it possible to more completely describe the essential nature of the process. It is clearly indicated in our paper that the specific impulse is referred to the gas phase.

In conclusion, it should be pointed out that, unlike other papers, in our article we present an evaluation of the effect exerted by the transfer of heat between the phases on the efficiency of the discharge process for the two-phase flow and that the specific impulse is referred to the gas phase; we have introduced the concept of an adiabatic efficiency for the discharge process of the two-phase flow and we indicate the relationship of this coefficient to the weight composition of the two-phase flow and to the dimensions of the particles; analysis of the conditions for the inversion of the effect and of the executed calculations provides the basis for an explanation of the influence exerted by the particle dimensions and the weight composition of the two-phase flow on the magnitude of the shift in the critical cross section in the diverging portion of the nozzle; on the basis of the calculational results we provide a qualitative explanation for the experimental data of Komov [3].

These are our thoughts in connection with the problems touched on in the article by Starkov.

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REPLY TO STARKOV'S COMMENTS ON THE ARTICLE: "THE FLOW OF A GAS–LIQUID MIXTURE IN A SHAPED NOZZLE, WITH A CONSTANT PHASE VELOCITY DIFFERENCE"

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In a number of papers dealing with the study of gas flows with particles in rocket nozzles (including the works of Hassan and Kliegel, cited by Starkov), the equations governing the nature of the energy

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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 $(w_m - w_d)/w_m = \text{const}$ 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 $w_m - w_d = \text{const}$, 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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