Comments on "Flow Around Spheres at Intermediate Reynolds Numbers"

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On the basis of their experimental study, Seeley *et al.* (1975) conjectured that the flow fields determined theoretically by LeClair *et al.* (1970), and further discussed by Pruppacher *et al.* (1970), are inaccurate. As these theoretical flow fields were obtained from a numerical solution to the complete steady-state Navier-Stokes equation of motion for incompressible flow around a sphere, Seeley *et al.* charged that the inaccuracies resulted from an insufficient resolution in the numerical grid close to the sphere surface.

In studying the charges made in the article by Seeley *et al.*, we found that their own as well as our work was significantly misinterpreted. We feel that some clarifying comments are in order, particularly in view of several facts:

1) Our numerical method developed to determine the flow field about rigid spheres has also been used to determine the flow field about liquid water spheres in air (LeClair *et al.*, 1972) to simulate the flow inside and outside water drops falling in air, to determine the flow field about a thin oblate spheroid (Pitter *et al.*, 1973) to simulate the flow of air about falling ice crystal plates, and to determine the flow field about a circular cylinder (Schlamp *et al.*, 1975; Schlamp and Pruppacher, 1977) to simulate the flow past a columnar ice crystal in air.

2) These flow fields have been used by us to successfully determine various quantities of cloud physical importance such as the collision efficiency of cloud drops colliding with other cloud drops in the presence and absence of electric fields and charges (Schlamp *et al.*, 1976, 1979), the collision efficiency of plate-like ice crystals (Pitter and Pruppacher, 1974) and of columnar ice crystals (Schlamp *et al.*, 1975) colliding with supercooled cloud drops in air, the effect of ventillation on the evaporation of cloud drops and on the evaporation of platelike ice crystals (Woo and Hamielec, 1971; Pitter *et al.*, 1974), the terminal velocity of cloud and raindrops at various levels in the atmosphere (Beard, 1976), the shape of cloud and raindrops (Pruppacher and Pitter, 1971), and the efficiency with which aerosol particles are captured by cloud drops (Beard, 1974; Beard and Grover, 1974; Grover and Beard, 1975; Grover, 1976; Grover *et al.*, 1977; Wang *et al.*, 1978). Our comments on the paper by Seeley *et al.* are as follows:

1) Our theoretical flow fields have been formulated to apply to $0.01 \leq N_{\mathrm{Re}} \leq 400$ (N_{Re} is the Reynolds number). Only *one* experimental measurement was made by Seeley *et al.* (1975) in this Reynolds number range, namely, that at $N_{\mathrm{Re}} = 290$. It is hardly justifiable to criticize the accuracy of all of our flow fields on the basis of some disagreements believed to exist at one single Reynolds number at the upper end of the Reynolds number range investigated by us.

2) The experimentally derived flow fields (streamfunctions, radial and tangential velocity) of Seeley *et al.* agree satisfactorily with our theory in the range $0 \leq \theta \leq 20°$ and $60 \leq \theta \leq 180°$, and up to 0.2 sphere radii from the surface (θ is the angle measured from the forward stagnation point on the sphere). At other angles and at larger distances from the sphere the agreement is unsatisfactory. In particular, the disagreement appears to become more pronounced with increasing distance from the sphere surface, the velocities found in the experiment being lower than those predicted by our numerical flow fields.

This latter fact is inconsistent with the charge made by Seeley *et al.* that the disagreement between their experimental and our theoretical results is due to an insufficient number of grid points in our numerical solution near the sphere surface. If the charge of Seeley *et al.* were justified, one would expect that experiment and theory show greatest disagreement closest to the surface which, as mentioned above, is *not* the case. We therefore suggest that the findings of Seeley *et al.* are due to some shortcomings in their experiment. These shortcomings are listed below:

(i) The experimental studies of Seeley *et al.* were carried out in a cylindrical vessel of diameter $D = 30$ cm, inside of which the flow around a sphere of diameter $d = 6$ cm, (and 3.5 cm) was studied. The spheres were held in the flow by a rigid support. According to Fidleris and Whitmore (1961) the wall effect at $N_{\mathrm{Re}} = 300$ for $D/d = 5$ (and 8.6), as in the experimental set up, is still considerable, while for $D/d = 12$, as in our numerical study, the wall effect is zero.

(ii) The sphere support, used by Seeley *et al.* may have had a considerable effect on the flow field. In fact on p. 598, ¶2 of their paper Seeley *et al.* admit;

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"The flow patterns at $N_{\mathrm{Re}} = 155$ and 290 was seen to be affected to some extent by the sphere holder." Our numerical study did *not* suffer from this problem.

(iii) In the experiment the sphere was studied inside a *cylindrical* confinement. This is an entirely different situation compared to that considered in our numerical study, where the sphere was situated inside a *spherical* confinement.

(iv) Most seriously, the velocity in the experimental set-up decreased to *zero* at the wall of the cylindrical confinement. In contrast, in our numerical study, the velocity *did not* decrease to zero, but assumed *free stream velocity* at the spherical boundary. Thus, it is *not* surprising that the velocities observed by Seeley *et al.* are lower than those predicted by our numerical model and are increasingly lower the farther away from the sphere.

3) Our numerically derived flow fields have quantitatively been verified by us in a number of ways. Although it is granted that by these verifications so called "integrated" quantities of the flow field are tested, it must be stressed that these quantities—contrary to the belief of Seeley *et al.*—are very sensitive to the correctness of the flow field. Thus:

(i) The length of the eddy at the downstream side of a sphere, oblate spheroid and cylinder, numerically determined by us, agree well with the eddy lengths determined experimentally by various authors (Pruppacher *et al.,* 1970; Pitter *et al.,* 1973).

(ii) The surface velocity theoretically predicted by us for a water sphere in air agree well with the surface velocities experimentally determined by us for water drops freely falling in air of our wind tunnel (LeClair *et al.,* 1972).

(iii) The shape theoretically predicted by us for water drops in air agree well with the shape observed by us on water drops freely falling in the air stream of our wind tunnel (Pitter and Pruppacher, 1971).

(iv) The drag numerically predicted by us for a rigid sphere and for a water sphere in air agrees well with the drag observed by us on rigid spheres falling in oil and on water drops falling in air (LeClair *et al.,* 1970).

(v) The collision efficiencies determined from our theoretical flow fields for the case of drop-drop collision, and for the case of drop-aerosol particle collision agree well with the results of our wind tunnel studies (Beard and Grover, 1974), and with our Rain Shaft Studies (Wang *et al.,* 1978).

(vi) The onset of riming on platelike and columnar ice crystals theoretically predicted on the basis of our numerical flow fields agrees well with the onset of riming in atmospheric clouds as observed by various scientists (Pitter and Pruppacher, 1974; Schlamp *et al.,* 1975).

The evidence given above suggests that the theoretically determined flow fields around spheres for $N_{\mathrm{Re}} \leqslant 300$ are considerably more accurate than the flow fields inferred from the experiments of Seeley *et al.* (1975), as these experiments appear to be plagued by numerous insufficiencies. On the other hand, it is gratifying to see that the differences are not severe at all and, in fact, are quite small near the sphere and at certain angles.

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