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We welcome Dr. Mason's comments on our paper (Lamb and Hobbs, 1971) for it provides us with the opportunity to discuss in more detail our recent measurements of the linear growth rates of the basal and prism faces of ice and our ideas concerning the variation of ice crystal habits with temperature.

Mason has misinterpreted our use of the term condensation coefficient. He considers it to be the fraction β of the vapor molecules which impinge on the surface of a crystal which simply become *adsorbed*. However, we use the term in the more usual way to refer to the fraction α of impinging molecules which become *incorporated* into the ice lattice. Thus, our empirical definition of the condensation coefficient for a crystal surface is

$$\alpha = \frac{R}{R_{\max}}, \quad (1)$$

where R is the actual linear growth rate of the surface and R_{\max} the linear growth rate if all of the impinging molecules were to become incorporated. From the kinetic theory of gases, Eq. (1) may be written as

$$\alpha = \frac{(2\pi mkT)^{\frac{1}{2}}}{\Omega \delta p} R, \quad (2)$$

where m is the mass of a water molecule occupying a volume Ω in the lattice, k the Boltzmann constant, T the temperature, and δp the excess vapor pressure above saturation. Eq. (2) may be used to determine α for the basal and prism faces of ice from the corresponding measurements of R presented by Lamb and Hobbs.

The theory of crystal growth from the vapor phase developed by Burton *et al.* (1951) predicts that at low

supersaturations the condensation coefficient is given by

$$\alpha = \frac{\beta k T \sigma}{2\pi \eta a^2} x_s, \quad (3)$$

where σ is the supersaturation, η the edge-energy of a critical two-dimensional nucleus, a the lattice parameter, and x_s the mean migration distance of molecules in the adsorbed layers. It can be seen from Eqs. (2) and (3) that, as we suggested previously, the variations with temperature of the linear growth rate R of a particular crystal face is dependent on the variation with temperature of the mean migration distance x_s on that face (and, therefore, on the surface diffusion coefficient D_s and the velocity v of steps on that face). The linear growth rate also depends on the adsorption parameter β but we suspect that this varies only slightly with temperature and monotonically.

In comparing the experimental data for the linear growth rate of the basal face and the mean migration distance of adsorbed molecules on this face, Mason places emphasis on the differences in temperatures at which the maxima and minima occur. We believe, however, that these differences are mainly of experimental origin. For example, Hallett (1961) found the velocity v of steps on the basal face to reach a maximum at about -6°C , whereas Mason *et al.* (1963) found the mean migration distance x_s to reach a maximum near -11°C , even though Mason's Eq. (1) predicts that the temperature dependences of v and x_s are the same. Of much greater significance than these differences is the similarity between the general shapes of the curves for R , x_s and v on the basal plane, and it is on this point that attention should be focused.

The linear growth rate curves for the basal and prism faces (see Mason's Fig. 1) define a series of temperature ranges in which either plate-like ($R^{\text{prism}} > R^{\text{basal}}$) or column-like ($R^{\text{basal}} > R^{\text{prism}}$) ice crystals grow. It must be emphasized, however, that the transition tempera-

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tures are not defined very accurately because of the scatter in the experimental points.³ Our results indicate that down to -5.3C plate-like crystals grow, followed by column-like crystals to -9.5C , and then plates again. A further transition is indicated at lower temperatures, but we refrained from extrapolating our results to that point. These results are, in fact, in reasonable agreement with direct observations of ice crystal habits (e.g., aufm Kampe *et al.*, 1951; Mason, 1953; Shaw and Mason, 1955).

Mason suggests that ice crystal habits may be significantly influenced by the adsorption of air molecules. However, our results show that ice crystals grown in an atmosphere of pure water vapor do not have significantly different basic habits from those grown in air. The effect of air is simply to decrease the rate of transport of water vapor through the environment to the growing face, and therefore to decrease the rate of growth by at least two orders of magnitude at atmospheric pressure (Lamb and Scott, 1972).

We turn now to the conflicting theories for the variation of the basic habit of ice crystals with temperature. In the theory proposed by Mason *et al.* it is assumed that the prism faces grow faster, so that plate-like crystals develop, when there is a net migration of adsorbed molecules from the basal to the prism face (i.e., when $x_s^{\text{basal}} > x_s^{\text{prism}}$ or $v^{\text{basal}} > v^{\text{prism}}$). Mason claims that Mason *et al.* (1963) obtained "convincing evidence that impinging water molecules do migrate to adjacent faces," but we can find no such evidence in that paper; rather, this idea is introduced as a hypothesis. Furthermore, it is implicit in the theory of Mason *et al.* [and in the theory proposed by Hobbs and Scott (1965)] that the linear growth rate of a crystal face is determined by the two-dimensional nucleation of steps near the edges of the crystal. But it is well known that two-dimensional nucleation is extremely unlikely at the low supersaturations at which ice crystals normally grow (see, for example, Cabrera and Coleman, 1963).

As we have pointed out above, both the general theory of Burton *et al.* and the experimental results for ice indicate that the linear growth rate of a crystal face is proportional to the mean migration distance x_s of the adsorbed molecules on *that* face (and therefore to v and $D_s^{1/2}$ on *that* face). If this is correct, the prism faces would grow faster and plate-like crystals would grow when $x_s^{\text{prism}} > x_s^{\text{basal}}$ (or $v^{\text{prism}} > v^{\text{basal}}$), as postulated by

³ It should be noted that superimposed on the general variations of R with temperature are fluctuations of higher degree (see our experimental data in Mason's Fig. 1). We believe these are real and are due to time-variations in the rate of advance of a face, under conditions of constant temperature and excess vapor pressure, due to the defect structure of the crystal varying with time (e.g., by the "growing-out" of emergent dislocations as the face advances). This natural variability probably does not interact significantly with the general temperature trends on which it is superimposed.

Hobbs and Scott. This theory makes it unnecessary to suppose that adsorbed molecules migrate around crystal edges from one face to another. We further suggest that, since ice crystals almost certainly contain dislocations, the mechanisms for the propagation of steps and the advance of a crystal face is by the growth of spiral steps rather than two-dimensional nucleation. If just one spiral dislocation emerges from a face it should give rise to a growth rate greatly in excess of that which could be produced by two-dimensional nucleation alone. Moreover, in this case the introduction of a higher density of emergent dislocations on a crystal face, which may be produced by growing a crystal on a substrate such as that used by Mason *et al.* or Lamb and Hobbs, may not produce spurious results because the growth rate of a face is determined largely by the single most active step source.

Finally, we note that most of Mason's objections to our theory for the variation of ice crystal habits with temperature are invalid because of his misinterpretation of our use of the term condensation coefficient. Thus, like Mason, we would ascribe the growth of a plate on the end of a columnar crystal when the temperature is changed appropriately to changes in the relative magnitudes of x_s (and, therefore, v and D_s) on the basal and prism faces. A necessary requirement seems to be the existence of steep gradients of supersaturation across the various crystal faces, but this exists whenever crystals of extreme geometry grow in a vapor diffusion field. In this case, we envisage, as do Mason *et al.*, the bunching of steps as they move away from the region of comparatively high supersaturation at the corners of the crystal leading to the growth of plates on the ends of columns. Again, however, we believe the steps will be generated by dislocations rather than by two-dimensional nucleation.

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Comments on “Collisions of Small Cloud Droplets: Gas Kinetic Effects”

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Davis (1972) has obtained the long-awaited solution of the Stokes flow interaction of two spheres moving along their line of centers in the case when slip may occur at the sphere surfaces and has applied this solution in calculating collision efficiencies of cloud droplets. For the problem he considers, i.e., cloud droplets $> 3 \mu\text{m}$ diameter, his calculations and discussion appear to be correct.

However, Davis's solution should also apply to smaller spheres for which his discussion of the validity of the slip flow method is incomplete and may be misleading. Specifically, for spheres with $\text{Kn} = l/R > 0.1$, the slip flow method fails, apparently because the slip flow boundary condition (for a flat surface) is no longer valid. Thus, Davis's solution may be valid only so long as the sphere Knudsen number l/R does not exceed about 0.1 (the gap Knudsen number l/h , where h is the separation of the spheres, may apparently be much larger, as pointed out by Davis). This conclusion is suggested by the following evidence.

The drag on a body in slip flow can, in principle, be calculated by applying the normal conservation equations and boundary conditions of continuum fluid mechanics with one alteration. Instead of equating the relative tangential fluid velocity to zero at the body's surface, this quantity is assigned a finite value: the slip velocity. Of course, the relative fluid velocity in the normal direction is still equated to zero at the surface.

To calculate the magnitude of the slip velocity, Basset (1888) assumed it to be proportional to the tangential stress with the constant of proportionality β called the “coefficient of sliding friction.” Thus, in Cartesian coordinates fixed to a surface element with the x axis pointing in the outward normal direction, the surface boundary conditions for slip flow are

$$u=0, \quad \beta v = \mu \left(\frac{\partial u}{\partial y} + \frac{\partial v}{\partial x} \right), \quad \beta w = \mu \left(\frac{\partial u}{\partial z} + \frac{\partial w}{\partial x} \right), \quad (1)$$

where μ is the gas viscosity.

Epstein (1924) showed by a kinetic theory investigation, that *so long as surface curvature is slight*, Eqs. (1)

are valid and

$$\mu/\beta = (2/f - 1)l = Al, \quad (2)$$

where f is the fraction of molecules diffusely reflected from the surface and l is the gas mean free path. Although the precise dependence of A on the nature of the gas-surface interactions has been questioned, measured values of A , typically 1.2 for slip drag on a sphere, are in good agreement with (2) assuming a reasonable value for f .

A simple case of slip flow, in which curvature effects are negligible, is the slip flow between concentric cylinders in relative tangential motion. Investigations of slip flow drag between concentric cylinders have found agreement of theory and experiment. For other internal flows, such as flow of a gas through a capillary, the slip flow method is also successful. In these internal flows, curvature of the surface may be substantial, but not generally in the direction in which the slip velocity occurs. For slip flow over small bodies, on the other hand, significant values on Kn generally occur only with significant curvature of the surface in both tangential directions. For the case of a sphere, application of the slip flow method gives for the slip correction factor $C [= 6\pi\mu RV/\text{drag force}]$:

$$C = (1 + 3A \text{Kn}) / (1 + 2A \text{Kn}), \quad (3)$$

where $\text{Kn} = l/R$. This result is most familiar in its low Kn limit approximation

$$C = 1 + A \text{Kn}, \quad (4)$$

which is valid (according to theory) only when $A \text{Kn} \ll 1$. However, there is no question that (4) is in superior agreement with experiment, giving the correct results to Kn values of at least 0.4 or 0.5 while Eq. (3) is no longer accurate when $\text{Kn} > 0.1$. Thus, Eq. (4) must be regarded as an empirical expression whose form, to be sure, is suggested by the low Kn limit of the theoretical result, but without present theoretical basis except in the low Kn limit. Indeed, present theory predicts an incorrect expression for $\text{Kn} > 0.1$.