Reply

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I am thankful to Drs. Kabanov, Mazin and Smirnov for their comments (hereafter referred to as KMS) on my paper (hereafter referred to as S). The two main points in KMS are: 1) criticism of the conjecture that the rate of growth of a cloud drop is proportional to the volume occupied by it, and 2) criticism of the statistical treatment.

The first criticism is based on estimates of the "time of decay of vapor concentration fluctuations," $\tau_D = l^2/D$ (the terminology, notations and quotations are from KMS), and of the "time scale of the formation of supersaturation," τ_p. As is well known, τ_D is the time in which the effects of diffusion are felt at the distance l, and τ_p the time constant for supersaturation, *assuming* the usual theory of growth of cloud drops by condensation (see, e.g., Squires' paper referenced in S). KMS found $\tau_D \ll \tau_p$, and concluded from this that "the neighboring drops cannot grow at different terminal supersaturations." By this they presumably imply that the supersaturation is uniform between drops. In my opinion this does not necessarily follow from the condition $\tau_D \ll \tau_p$. I believe that the correct interpretation of the condition is that the quasi-static approximation is justified, which is to say that at any instant the time-dependent solution for the diffusion field is approximately the same as the steady-state solution for values of the various parameters prevailing at that instant.

As pointed out by KMS, the statistical treatment in S was wrong and, as noted above, a corrigendum has appeared in this Journal. I would like to take this

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opportunity to mention that this point was also brought to my attention by Prof. R. R. Rogers, McGill University, and Mr. J. Warner, CSIRO, Australia. The correct solution of the statistical problem requires consideration of statistical correlations and is beyond the scope of this reply. Moreover, it is not obvious that the problem of the growth of cloud drops by condensation may be divided into two parts, namely, 1) the problem of condensation on a fixed configuration of cloud drops, and 2) a statistical problem. The approach which was adopted in S for the sake of simplicity assumed implicitly that the diffusion field is at all times adjusted to the prevailing configuration of cloud drops. Since the time of rearrangement of a given configuration of cloud drops (due to gravitational settling, for example) may not be large compared to τ_D, it would appear that the problem may not be divided into two parts.

Nevertheless, the problem of condensation on a fixed configuration of drops is of interest in itself, and its solution may provide some insight into the realistic case where the configuration is changing with time. In S, the solution to the fixed configuration case was *conjectured* on the basis of the exact solution of an analogous one-dimensional problem, and on the basis of physical arguments. I believe that the physical arguments are compelling enough, and the problem is significant enough, to warrant theoretical and numerical efforts at solution.

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