Reply

JUDITH A. CURRY

Department of Earth and Atmospheric Sciences, Purdue University, West Lafayette, IN 47907

12 May 1986

I appreciate the comments that Dr. Twomey has made on my recent paper, Curry (1986). The main criticism that is made is one regarding my determination of S, the supersaturation ratio. I welcome the opportunity to clarify section 5 of Curry (1986), and to present further examples of how the processes occurring in arctic stratus appear to differ from the classical theory of condensational growth in an updraft.

The value of S was determined in Curry (1986) from

$$S = \frac{q_v}{q_s} - 1 \tag{1}$$

where q_v and q_s are respectively the liquid water vapor density and the saturation water vapor density with respect to liquid water. Dr. Twomey is entirely correct to state that errors in the aircraft measurements of water vapor density and temperature are likely to be far too large to make a reliable determination of the absolute value of S. In fact, the accuracy of the Rosemount thermometer and the thermoelectric dewpoint sensor (from which the water vapor density fluctuations were determined) is typically $\pm 0.5°C$ for determining mean temperatures. In Table 1 the mean value of S, in addition to the maximum and minimum values and the standard deviation of S, are given for the horizontal traverses in deck 2 of Curry (1986) for those samples where the liquid water content exceeded 0.01 g m^{-3}. It is readily seen from Table 1 that most of the measured values of S determined from (1) are slightly negative (subsaturation). Since it seems unlikely that virtually all of the parcels containing significant amounts of liquid water were evaporating, we can only assume that at least one of the instruments was in error.

However, it seems reasonable to expect that the instrument(s) were subject only to a slight systematic error, perhaps an "offset" in calibration, which would affect the mean values of S, but not the deviations from the mean. In Curry (1986) I stated: "Although the absolute values of S as determined from the high frequency measurements of temperatures and water vapor concentration may be inexact due to small calibration errors in either instrument, the fluctuations in S should be reliably determined." To determine the correlations given in Table 6, only the fluctuations of S are needed; neither the mean value of S nor the absolute value of S are required. Although one (or both) of the instruments appears to be subject to a systematic error, is the random error "noise" of these two instruments sufficiently small at 1 Hz to give reliable determination of the temperature and humidity fluctuations, and thus the supersaturation fluctuations? The Rosemount thermometer is accurate to 10 Hz for temperature fluctuations, which is well beyond the requirements for the 1 Hz supersaturation fluctuations. However, for fluctuations in dewpoint temperature greater than $2°C\ s^{-1}$, the thermoelectric dewpoint sensor may show a 1 or a 2 second time lag. The temperature fluctuations in the arctic stratus are small away from cloud top, and in the range where any time lag effect should not be noticable. In retrospect, it would have been more appropriate to have determined the high frequency fluctuations of water vapor density from the Lyman-alpha hygrometer (20 Hz). However given the relatively small magnitude of the fluctuations, it appears that the measurements made by the thermoelectric sensor are adequate.

This method for determining supersaturation fluctuations is not the conventional one. An alternative to using (1) in determining S from aircraft measurements would have been to use the formula of Squires (1952), which was employed by Warner (1968) in cumulus clouds. Squire's approximate relationship is based on the assumptions that no mass or heat is exchanged with the environment, and that the droplets are well beyond the activation stage and a quasi-steady state exists in which the rate of release of water due to lifting and the rate of condensation on the drops are such that the supersaturation is changing only slowly with time. Since one of the goals of my paper was to investigate possible influences of radiative heating and turbulent mixing/entrainment on the cloud microphysics, I felt that it was preferable not to make the assumptions required by Squire's formula if it could be avoided. Since I was more interested anyway in the fluctuations of S rather than the absolute values of S, it seemed pref-

TABLE 1. Values of the supersaturation, S, for deck 2 in Curry (1986)[1].

Height (m)	$\bar{S}$ (%)	σ_s	S_{min} (%)	S_{max} (%)
1082 (top)				
940	−2.2	.0097	−4.6	0.2
819	−3.1	.0099	−5.3	−1.0
696	−3.8	.0084	−5.3	−2.0
258 (top)				
221	−3.5	.0090	−5.5	−1.7
66	−3.0	.0113	−5.5	−0.5

[1] Definitions: $\bar{S}$, mean value of the supersaturation; σ_s, standard deviation of the supersaturation; S_{min}, minimum value of the supersaturation; S_{max}, maximum value of the supersaturation.

erable to use (1) provided that the determined values of the fluctuations of S were reasonable.

Dr. Twomey cites a reference from Fletcher (1962) that for most of the lifetime of a cloudy parcel, the supersaturation is less than one-tenth of a percent. The discussion by Fletcher is centered around Howell (1949), who employed an adiabatic model of droplet growth in a constant updraft. I would like to direct Dr. Twomey's attention to the paper by Warner (1968), who determined S from observations in cumulus clouds using Squire's (1952) formula. Warner found that the median value of S was 0.1%, while one percent of the samples were in air supersaturated by more than 1%, and one percent of the samples were in air that was subsaturated by more than 2%. In addition, Warner found that for droplet samples taken at 1 second intervals (corresponding to 60 m travel distance of the aircraft), differences in supersaturation of 1% were not unusual between successive sampling points. The differences between the maximum and minimum values of S in Table 1 do not exceed 5% for a given traverse. In view of Warner's values, the standard deviations of S in Table 1 do not seem unreasonable.

The discrepancies between the values of S cited by Fletcher and those observed by Warner (and also in the arctic stratus) suggest that perhaps the conditions occurring in real clouds—radiative transfer and turbulent fluctuations of heat, moisture and updraft velocity—may result in a much larger range of supersaturation than predicted by classical theory. The recent study by Davies (1985) has addressed the general response of cloud supersaturation and droplet growth to radiative forcing, and has provided solutions for the steady state supersaturation in terms of total radiative flux divergences. Davies found that radiative cooling increases the equilibrium saturation and allows condensational growth of the droplets, with the steady state supersaturation increasing with an increased radiative deficit. An extrapolation of Davies results to the magnitude of cooling observed near cloud top in the arctic stratus ($R \sim 25$ W m^{-3} in Fig. 1 of Davies, 1985) would yield steady state values of S exceeding 1%.

Dr. Twomey further questioned my interpretations of the correlations between updraft velocity w, droplet concentration N, and the supersaturation S. I agree with Dr. Twomey that, for a cloud parcel that has not exchanged any heat or mass with its environment, supersaturation will show a strong positive correlation with updraft velocity; also after a droplet population has been growing, there will be little correlation of supersaturation with the maximum value of supersaturation or with droplet concentration. In Curry (1986) I state: ". . . once a population of droplets has been growing the initial relationship between supersaturation and concentration will be obscured, since supersaturation is reduced as a result of the condensation." But then why were significantly positive correlations between updraft velocity and droplet concentration observed for the arctic stratus at nearly all levels? The process of entity entrainment (Telford and Chai, 1980), whereby parcels of different properties mix and result in a continuous cycle of evaporation and reactivation of condensation nuclei, is a possible explanation. Curry (1986) also found that the correlation between vertical velocity and supersaturation is either zero or negative, in contradiction to what one would expect from a consideration of classical condensation theory. If the supersaturation fluctuations have been correctly determined, these results suggest that there has been an exchange of heat and/or momentum between the parcel and the environment, perhaps via the processes of turbulent mixing or radiative cooling. These exchanges with the environment would act to break the link between supersaturation and vertical velocity, and broaden the droplet distribution in accordance with Manton's (1979) theory. According to Manton's theory, turbulent fluctuations completely uncorrelated with the vertical velocity can result in supersaturation fluctuations and thus modify the drop spectra.

In summary, I would like to note that I make no claim to be able to explain all of the correlations that I presented in the tables of Curry (1986). The correlations involving the supersaturation may be somewhat questionable, but the supersaturation fluctuations appear to have been reliably determined. Nevertheless, it may not be appropriate to put too much emphasis on them until this method of determining supersaturation fluctuations has been compared with other methods. However, even if the correlations involving supersaturation are ignored, the remaining correlations presented in Curry (1986) present a picture of the condensation process occurring in the arctic stratus that is not strictly in accord with the traditional theory of adiabatic droplet growth due to condensation in a uniform updraft. The correlations that involve the supersaturation fluctuations further support this view. I do not know the answer to some of the questions that these observations have raised, but surely these observations are food for thought.

Acknowledgments. Comments from Roger Davies and Paul Spyers-Duran are appreciated.

REFERENCES

Curry, J. A., 1986: Interactions among turbulence, radiation and microphysics in arctic stratus clouds. *J. Atmos. Sci.*, **43**, 90–106.

Davies, R., 1985: Response of cloud supersaturation to radiative forcing. *J. Atmos. Sci.*, **42**, 2820–2825.

Fletcher, N. T., 1962: *The Physics of Rain Clouds*. University Press, Cambridge, 390 pp.

Howell, W. E., 1949: The growth of cloud drops in uniformly cooled air. *J. Meteor.*, **6**, 134–149.

Manton, M. J., 1979: On the broadening of a droplet distribution by turbulence near cloud base. *Quart. J. Roy. Meteor. Soc.*, **105**, 899–914.

Squires, P., 1952: The growth of cloud drops before condensation. *Aust. J. Sci. Res.*, **A5**, 59.

Telford, J. W., and S. K. Chai, 1980: A new aspect of condensation theory. *Pure Appl. Geophys.*, **118**, 720–742.

Warner, J., 1968: The supersaturation in natural clouds. *J. Rech. Atmos.*, **3**, 233–237.