Reply

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It is difficult to accept Weinstein's statement that a steady-state model intended to be sufficiently realistic to predict cloud-top height and precipitation should not be held accountable for predictions of internal cloud structure. Any calculations of cloud growth and precipitation result directly from the model calculations of temperature excess, liquid water and updraft within the cloud. If the latter differ grossly from observations one might almost regard it as coincidental that there is agreement between predicted and observed cloud depth and precipitation.

The main question, surely, is how we can compare predictions of the model with observations. Weinstein states that such comparisons are valid only for observations in "the leading parcels of the cloud;" Cotton suggests that the observations should relate to the maximum water content at a given level. Both statements refer to situations that cannot be accurately defined, much less observed, and which seem to me to have no relationship at all to the one-dimensional steady-state model under discussion. At what point in the "leading parcel" is a comparison valid? Presumably not the leading edge, but maybe a radius down from the top; and if this is the case it is difficult to see how the data I used is grossly in error, coming as it does from a sample biased toward the growing and mature stages of cloud development. If one is to use a maximum water content it is still essential to specify a distance over which the measurement is averaged, and clearly the smaller the averaging distance the greater chance there is of obtaining a high value of the water content, until in the absurd limit one averages over the diameter of a single droplet. The only averaging distance that appears to make sense is the diameter of a single cell, and my own observations indicate that the liquid water is often roughly constant over that distance. I should also remark that the values I used for "observed Q/Q_a" in my paper were taken directly from the 1955 paper and thus seem perfectly applicable to the considerations above.

In my paper I attempted to choose the simplest possible case for comparison with the model, a non-precipitating cloud containing no ice. Such a choice avoids the necessity for any consideration of the parameterization of the cloud physics terms and focuses attention on the lateral entrainment hypothesis, which Weinstein admits is the weakest point in these simple cloud models. I submit that it is weak on physical as well as on observational grounds, but I agree with Weinstein that it is the best that is currently available and that it has been shown to be useful in certain cloud-seeding experiments.

Both Cotton and Weinstein correctly draw attention to the importance of precipitation in removing water from a cloud. I stated that the clouds with which I was comparing the model produced little rain and that their liquid water content was comparable to that of clouds referred to in the larger body of data reported in my 1955 paper. If precipitation were the reason for the difference between my observations and the model predictions it would have had to amount to 2.7 mm for the 670 m radius cloud in Fig. 2 of my paper and 1 mm for the 200 m cloud (which barely penetrated the stable layer). This amount of rain is easily visible and could hardly have been missed.

I consider that the clouds I observed were relatively dry because environmental air had been mixed with them. However, I do not think that most of the mixing occurred as though the cloud were a steady-state jet or plume or even a bubble; my observations do not fit such concepts. The velocity observations, in particular, suggest that turbulence dominates the picture and I have no data suggesting that a real cloud resembles Weinstein's Fig. 2. This is surely a concept dictated by resulting ease of computation rather than by physical reality.

My claim is that the model is in essence empirical and is not a valid representation in mathematical terms of the physical processes which result in cloud growth and precipitation. In particular, I claim that the lateral entrainment hypothesis employed is invalid, in that its use cannot satisfactorily account for observations in non-precipitating cumuli. The empirical nature of the model does not necessarily detract from its usefulness; however, in its present form the model is unlikely to add much to our understanding of the physics of precipitation processes.