# Comments on "Does Mixing Promote Cloud Droplet Growth?"

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The article by Paluch and Knight (1986, hereafter referred to as PK) reviews some cumulus cloud-droplet (number concentration-diameter) measurements, and presents conclusions related to entrainment and vertical mixing in clouds. Their discussion, however, is misleading, with a mixture of absolute statements and inaccurate premises from which they imply that the evidence and analysis so far developed on entrainment should be discounted, and that we return to the preentrainment status in cloud physics. They claim without any support, for example, that "the broadening of the droplet-size spectrum towards the smaller sizes is a well-established phenomena, but it is quite a different problem from the broadening towards larger sizes that is the topic here." However, we believe that this association is well established in relevant examples (e.g., Telford and Wagner, 1981), and the arguments at the end of PK also assume this quote is not true so they can reach their final conclusion.

Their approach postulates an entrainment theory which always generates big drops, and consequently plays no role and cannot be present in clouds where big drops are not found. The entrainment theory, as we studied it (Telford and Chai, 1980; Telford and Wagner, 1981; Telford et al., 1984; Rogers et al., 1985; Chai et al., 1985), shows that if there is sufficient vertical cycling in cumulus clouds and entrainment of dry air at the top of each vertical excursion of the parcels, it can produce big drops (Telford and Chai, 1980). Clearly however, if the air above has such a low wet-bulb potential temperature relative to the cloud that mixing rapidly evaporates the cloud without allowing sufficient time for several cycles to occur with a particular parcel, or the cloud turns to ice particles before this cycling is completed, then the process cannot create many big drops even though other entrainment effects may be clearly active.

Where no sideways mixing can occur, and the wet-bulb potential temperature decreases only marginally at cloud top, as in some entraining marine stratus layers, observations show that big drops occur in relative abundance (in terms of warm rain growth by coalescence) in the diluted parcels. Furthermore, the drop number concentrations are often reduced by a factor of $\sim 10$ by dilution, from roughly 500 drops $cm^{-3}$ to $\sim 50$ drops $cm^{-3}$, in parcels where the big drops are found (Telford and Wagner, 1981).

The PK approach proceeds with statements which would be better suited to the era before we had met with entrainment: "The large droplet 'tail' of the spectrum is difficult to assess. It might be due in part (or completely) to coalescence itself, and thus in this sense is not a part of the problem of how fast coalescence can get started." This is quite unfounded speculation. This subject was thoroughly explored thirty years ago, as indicated, for example, in my paper (Telford, 1955), which they quote with this question in mind. The coalescence rate for sizes below the Hocking (1959) cutoff at 36 $\mu m$ diameter is so small, as later calculations showed, that coalescence growth at smaller sizes is not a reasonable expectation, even in marine stratus clouds which continue to develop and evaporate over many hours. We observed in such a cloud drops greater than 25 $\mu m$ in diameter in regions of cloud where the mean droplet diameter was less than 13 $\mu m$ (Telford and Wagner, 1981). This cloud was about 200-m deep and most of the big drops would have fallen out in a few hours (a 20-$\mu m$ diameter drop falls at about 1 cm $s^{-1}$, so it falls 100 m in less than 3 hours), so to be found in a cloud in significant numbers, such drops would have to form in a similar time. Hence, coalescence was not, and is not now, a candidate explanation.

Their text continues: "Also, it is impossible to say what the large end of a simple condensation spectrum should look like without both detailed knowledge of the cloud condensation nuclei (CCN) and some information about the updraft history of a parcel. . . ." This subject, assuming that "simple" means growth in a uniform unmixed updraft, has been treated extensively, and "giant" nuclei were invented to give large drops beyond the very narrow computed spectrum which results when no entrainment is included in the calculation (the condensation-drop growth is by a physical process which has never been shown to be inadequate). Thus, it is *not* "impossible to say"; the necessary conditions have been examined in detail, are well-documented, and generally have been judged as being less than adequate as a useful concept to explain warm rain. If they wish to postulate the presence of giant nuclei whenever they need them, then the absolute, "impossible," might seem attractive. However, absolute statements, which are only true in the sense that they exclude absolute certainty, are not a useful basis for understanding probable outcomes.

The argument then proceeds to include together several quite contradictory approaches to "mixing" modeling (including entrainment, one presumes), and points out that they each give different results. This seems to imply that although they arise from the same physical principles, the conclusions are apt to be arbitrary, and hence equally dismissible. Such comments, ignoring the physical basis of the arguments, do not support any rational conclusion about either the theories or the nature of clouds.

In this context we should point out that in reference to the Snowmass paper of Paluch and Baumgardner, the claim is made that the entrainment process cannot produce big drops because the reactivation of nuclei in the updrafts will prevent the biggest drops from growing further. This reactivation *was* included in the model of Telford and Chai (1980) and is discussed in that paper. The small reactivated drops make no detectable change to the remainder of the drops, or to the growth of the biggest drops. The big drops still act like the "giant" nuclei of the 1950 era.

We again meet up with a crucial scientific point in analyzing the data referring to the number of big cloud drops observed in samples. The limit of the number of "largest" drops ". . . was chosen so that the maximum largest droplet concentration falls between 10 and 30 $cm^{-3}$, because very small counts are susceptible to statistical fluctuations and also may not be inherently trustworthy." The statistical fluctuations are a simple case of Poisson statistics. If, in a particular sample category, we count 9 drops, then the standard deviation of the distribution of this sample for repeated observations from identical statistical ensembles is $[(\sqrt{9})/9]$ times the mean value.

Thus, approximately half the time the true mean value for the number of drops observed in each sample in such a size range will be between 6 and 12 drops. In a correlation analysis this is ample accuracy to detect trends. The statistical uncertainty of a best-fit line on a scatter diagram decreases as more data is plotted, so the individual inaccuracies become small in the final evaluation. Such trends require physical explanations. Thus, since a FSSP sample typically observes about 10 000 drops in a second when there are about 500 drops $cm^{-3}$, a statistically significant number of drops, 9, say, corresponds to about 0.5 drops $cm^{-3}$, *not* 10 to 30 as offered by PK. The phrase ". . . may not be inherently trustworthy" can be ignored as unsupported bias towards the conclusion they seek, but is is quite clear that the choice they made—examining the largest 10% of the droplet spectrum—has little to do with either the observational capability of the instrument, or the capability of a cloud to rain by the coalescence process. For a cloud to produce rain, about one drop in $10^5$ growing to millimeter sizes would amply account for any rainfall. No valid conclusion about the possibility of entrainment processes being able to produce enough big drops to start coalescence growth can be drawn from either the argument or from much of the data analysis. Even the comment that no drops were ever seen in the largest size range suggests no more than that this cloud may have been sampled across the shear, so the entrainment cycling was not fully mature in the part of the cloud encountered, rather than being conclusive evidence related to the whole cloud.

There is no doubt that big drops do not readily develop in the continental cumulus clouds studied here, and that if the cloud is big enough to survive the necessary cycles of dilution, the ice process usually takes over. This is, however, no basis for the conclusions reached in PK.

Thus, readers should be very cautious about reaching conclusions from PK. Their last sentence states that because cloud droplet spectra have been observed (for many years previously, *not* just since Manton and Warner, 1982) to be much broader than condensation theory can predict without mixing and entrainment, and since many small drops provide most of the broadening, it follows that the theory ". . . invites speculation that there may exist a spectral broadening process not related to mixing." This quoted sentence itself would seem to be a non sequitur wherein the rejection of entrainment in the role of producing big drops is also taken to show it cannot produce small drops or spectral broadening.

Furthermore, it would seem that PK are asking readers to accept their discussion on the basis that observed small drops are not connected with the production of big drops (as stated at the start of PK presentation and mentioned previously). It thus seems that we are being asked to accept conclusions, reached in PK on the premise that there is no connection between the big and small drops, that because big drops (they claim) are not produced by entrainment, so then small drops related to the broadening of the spectrum cannot be produced by entrainment, either. For the absence of big drops to establish that the small drops could only occur from the effects of another process, it is necessary to accept that the process of entrainment we are considering always produces big drops whenever it forms small drops. This not only contradicts the basis of their discussion, but is also untrue.

PK do not address the question that entrainment may well initially broaden the droplet spectrum, or further, that dilution makes little difference after this initial phase, except to gradually produce a few bigger drops which their analysis might well not have detected. Rogers et al. (1985) did see some evidence of a few big drops as the parcels diluted and aged, but again there was no abundance of big drops found before the clouds turned to ice. The theory of Telford and Chai (1980) shows that the first downward motion after the initial entrainment event produces a broad spectrum by creating small drops. The readers will have to decide for themselves what acceptable conclusions can be supported from PK's approach.

It should be added that the obvious limitations of entity-type entrainment mixing (ETEM, see Telford and Chai, 1980; Telford and Wagner, 1981; Telford and Chai, 1984) in producing big drops, when mixing (dilution) is rapid, is that a cloud may totally evaporate before the necessary vertical cycling needed to produce big drops can occur; this is illustrated by recently published data (see Telford et al., 1987). The heating over land where continental clouds form, raises the wet-bulb potential temperature of the cloud, so for similar overlying clear air temperatures, the entrainment at cloud tops dries out the cloud and evaporates it much faster than for clouds over the sea. Thus, continental clouds tend to be narrower, and have more small drops in total, and fewer big drops, than similar clouds over the sea. Over the sea most of a cloud has been diluted and cycled several times before the total evaporation reaches a stage where the cloud particles are completely removed so that only clear air remains. Continental clouds, however, tend to be relatively new, partly modified cloud, with relatively large numbers of small drops and nothing else.

In their reply, PK say they hesitate to accept ". . . that any deviations in the observed droplet spectra . . . must be attributed to the entrainment process," particularly when ". . . it appears that little or no entrainment has taken place. . . ." Just so! However, if a cloud does not entrain dry air it will last virtually forever, as I have said before. The only indication that entrainment has not taken place is a so-called "adiabatic" liquid-water content, which is quite rare in observational data, so very few samples actually correspond to "no entrainment," and the implication that spectrum broadening often occurs without entrainment is rather doubtful.

If PK is to be read as a plea for more studies of small warm cumuli, then we are in strong agreement, provided the justification is not based on a pretense that entrainment processes do not exist.

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