Comments on "Mixing and the Evolution of Cloud Droplet Size Spectra in a Vigorous Continental Cumulus"

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1 October 1984 and 17 December 1984

Paluch and Knight (1984, hereinafter referred to as PK) present observations of the evolution of the droplet spectra in a continental cumulus cloud. These observations are of great value and should lead to the elucidation of several important questions. However, we are concerned lest a number of statements in this paper be misinterpreted by the interested reader who does not have time to research the problem for himself. There are several points in the analysis and in PK's interpretation of entity mixing described by Telford and Chai (1980) and, more recently, by Telford *et al.* (1984) which we feel need to be addressed. There are also a number of ambiguities and juxtapositions which could easily be misinterpreted and so become misleading, and hence need to be discussed and compared to other ideas.

While it is impossible to discuss all our concerns, an example of ambiguity in the paper which needs clarification is the meaning to be attached to the following three sentences. "As pointed out by Telford *et al.* (1984), in this situation mixing could be homogeneous within each air parcel but appear nearly inhomogeneous on larger scales. The diffusive model of turbulent mixing by Baker and Latham (1982) illustrates this point. In this model, cloud properties are transferred into an entrained volume (scale about 100 m) according to a prescribed turbulent mixing rate, whereas on the subgrid scale (about 10 m), the mixing is implicitly treated as homogeneous."

In regard to the first sentence quoted we certainly discuss "many air parcels mixed in different proportions" but on rereading our paper we find nothing to imply that homogeneous mixing could look like inhomogeneous mixing, except possibly in extreme contrived, conditions. What we said was that many mixing events are likely to be proceeding simultaneously at any given level, so that as the dry air blending proceeds to completion with each of the small volume pairs (cloud with dry air), a range of droplet spectra can be produced by "homogeneous mixing" which, with continued mixing between such saturated parcels of modified cloud, could produce any prescribed spectra with smaller drops. This is true except for one spectra, the spectral modification expected from "inhomogeneous mixing" which leaves the spectral distribution unchanged but reduces the concentrations.

Thus the sentence is in direct contradiction with the ideas of the paper to which it is attributed. However, we did say this process could ". . . both reduce droplet concentrations and produce many smaller droplets as are seen in actual cloud droplet spectra," so that the observed cloud spectra could be simulated without "inhomogeneous mixing," which thus, may well be, not a specific physical process but a coding procedure to simulate a certain result.

If the homogeneous mixing process were to produce only volumes which were exactly saturated and drop free, which we would expect to happen with zero probability, then a further step of mixing this volume with unmodified cloud would generate the so-called "inhomogeneous mixing" result, but it seems that inhomogeneous mixing is here, and in most other circumstances, "primarily a coding procedure . . ." as we stated, ". . . rather than a basic physical process . . .". A view we also take of other applications of "inhomogeneous mixing" processes we have seen. This particular process as discussed in Telford *et al.* (1984) is totally unconnected with scale, since each step could be within any range of scales which are smaller than the cloud and do not force too much vertical motion to change drop sizes by adiabatic processes, which latter changes should be accounted for in a separate step in any model.

The basic problem is that the concepts of "homogeneous" and "inhomogeneous" mixing are so vague and imprecise from a physical point of view that readers have no simple test of what makes sense, and so can take them to support any approach they choose, which was the point we tried to make in the quoted paper. Even referring to "homogeneous" and "inhomogeneous" mixing in the same paragraph as we did with the quoted phrases given above is liable to be confusing as we have just seen. No matter how a description is constructed, ambiguous interpretation seems possible.

As a further example, the hypothetical procedure for "homogeneous mixing" discussed above could be reproduced exactly with the same final spectra, using the two step "inhomogeneous mixing" procedure for each of the subdivided mixing steps. The only additional requirement is that the "inhomogeneous mixing" occurs at heights above the level where final mixing occurs, so that the drops change size due to the adiabatic evaporation resulting from descent. However no such proviso was specified in PK so it is not clear what they mean.

The next sentence states, "The diffusive model of turbulent mixing by Baker and Latham (1982) illustrates this point," which seems to us to assume a lot of unexplained preconditions. The Baker and Latham model uses diffusion "K" theory (which has very little support from observations) to transfer drops and air into a central clear air sphere from a concentric surrounding spherical cloud. This process advances in the arithmetic by infinitesimal steps in space and time, and does not seem to have much connection with the use of the terms "homogeneous" or "inhomogeneous" as first defined by those authors, which presupposed mixing across the whole area of an updraft, instantaneously.

From the physical point of view it is not clear why the advancing diffusive interface between cloud and entrained dry blob should be considered homogeneous, when the inhomogeneous variations between points along the concentration and vapor density gradients drive the process. We are well aware that there is a "one step" or "two step" mixing procedure (Telford *et al.,* 1984), but a distinction can only be made between them if the arithmetic is considered to be physically important. How the procedure used could be related to a physical description is not clear. For an infinitesimal step there are basic problems related in implementing the inhomogeneous process to removing a fraction of one drop, for example, if the "two step" operation is accepted as the definition of inhomogeneous mixing. There is a real problem as well in describing how small scale turbulence to support gradient diffusion is maintained without large scale turbulence to generate it, unless we are referring to the ETEM process. Furthermore it is difficult to understand how an "inhomogeneous mixing" event could come into existence except as a diffusive process where drops were diffused towards the interface of a subsaturated volume, where they can be largely evaporated before they diffuse back into the cloud again. It appears that when such a sharp interface is involved, a reasonable physical picture of "inhomogeneous mixing" is the same as "homogeneous mixing."

The final results in that paper are close to the results to be expected on the earlier "inhomogeneous mixing" type calculations without in any way justifying the process other than as a convenient arithmetic procedure. Thus the whole reference to scales can really only be connected to the idea that the dilution of spherical blobs may occur in conditions where the scale of the turbulence is very small compared to the size of the blob, so that gradual diffusion from the outside inwards can be physically implied to support the correctness of the form of the arithmetic assumed. Since, however, there is no evidence to support the idea that turbulence only exists at the small scales, and indeed a feature of the PK paper is to claim there is evidence suggesting that the small scale diffusion does not exist (see below), the whole applicability and logical coherence of these three sentences should be considered very carefully.

As a further comment the "subgrid scale" idea introduced in the third sentence is a concept associated with the numerical evaluation of differential equations and is completely unconnected with the physical properties of clouds, or the way the Telford and Chai model is formulated. Furthermore, the Telford and Chai papers (Telford and Chai, 1980; Telford and Wagner, 1981; Telford *et al.,* 1984) specifically state that the entity type entrainment mixing (ETEM) is not related to the question of homogeneous or inhomogeneous mixing but will give virtually the same results regardless of the detailed process. We hope the contrary implications in PK are now properly brought into question, since we consider the association of these contradictory ideas less than fortunate.

The observations discussed in PK illustrate the constancy of the radius of the larger droplet spectral peak at constant altitude regardless of major concentration variations. This is clearly an important result. Paluch and Knight conclude that this occurs because of the existence of "major, small scale non-uniformities in the droplet population" which, they suggest, occurs because "the turbulence is not fully developed to the smallest scales" in the cloud studied. They imply that the cloud field consists of a series of cloud volumes containing high droplet concentrations indispersed by unmixed entrained dry air volumes; that is, the cloud is filled, effectively, with patches of dry air which remain distinct because small scale turbulence does not exist, and, consequently, "the observed droplet spectra consist primarily of droplets from cloud volumes where the local concentrations are high", and are then averaged in sampling to give the observed results.

Although PK acknowledge that the unvarying drop radius at the large droplet concentration spectral peak is consistent with entity type entrainment mixing (ETEM) (Telford and Chai, 1980) their subsequent analysis that the mixing process may resemble that of Broadwell and Breidenthal (1982) is open to question.

The idea that turbulence is not fully developed appears to be inappropriate if it is recognized that clouds are not a single, uniform, well-mixed environment but consist of smaller, well-defined entities which represent buoyant (or negatively buoyant) plumes or blobs, with their intervening downdrafts. In this way the cloud is diluted from above. This approach to a description of a turbulent field explicitly determines the scale on which mixing may be considered homogeneous, and provides details of the organized structure without recourse to a model which depends on shear instability, as in the application of the model of Broadwell and Breidenthal (1982) to mixing in clouds by Baker *et al.* (1984), for example.

Undoubtedly, shear instability between a turbulent cloud and the ambient non-turbulent air at cloud top may lead to the well-known Kelvin–Helmholz instability, studied in detail by Broadwell and Breidenthal, but entrainment need not depend on it to any great extent.

As pointed out by Townsend (1970) in discussing entrainment in "fully sheared turbulence", ". . . the level of turbulent motion and the entrainment rate are set by the structure of the whole flow. The actual entrainment mechanism is a folding and engulfing of ambient fluid by movements of the interface which in general have two origins. The basic origin is the velocity fields of the eddies of the main turbulent motion but, particularly in flows with large entrainment rates, entrainment by the basic mechanism alone leads to instability of the interface and to the initiation of a period of active, more rapid entrainment where the mean shear is largest at the entrainment interface. The rapid entrainment destroys the velocity profile that first caused the instability and the flow reverts to a quiescent condition with the basic entrainment rate."

In any case, the structure deep within the cloud layer does not depend on the details of the entrainment process at cloud top since the vertical scale over which the instability acts is limited by the size of the largest eddy, which is the size of the descending entity. It is important to point out that vertical transport throughout the depth of the cloud layer can only occur if there are variations in density between one blob of air and another. In turn this accounts for the observed, so-called, coherent structures or entities within the cloud layer. If PK are implying that the two components, dry air and undiluted cloud, subside deep into the cloud without mixing, as an extension of the Broadwell and Breidenthal (1982) statement ". . . showed that the fluid elements from the two streams are distributed unmixed throughout the layer by large-scale inviscid motions," then we would like to point out that dry air parcels can never sink unmixed deep into a cloud because they would follow the dry adiabat and hence the extremely stable and resist vertical motion.

However, this is not clearly stated, although their reference to Broadwell and Breidenthal is followed in the next paragraph by ". . . the large droplet peak presented in Figure 10 seems to show that mixing remains incomplete at smaller scales," it might possibly be another reference to the ETEM process modifying a dwell phase (for the development of the dwell phase concept see, Telford, 1975, Telford and Wagner, 1980) cloud population. The ETEM process employs an entity which continually entrains surrounding cloud water and hence can descend along the wet adiabat.

The essence of the matter is that the boundaries of the entities are determined in extent by the largest wavelength driven by the turbulent generation process. Because of this, the turbulent motion tends to maintain uniformity within the buoyant parcel by providing enough mixing on this same scale, so that the parcel remains uniform in composition and is thus self-perpetuating. The size of the buoyant blob then determines the wavelength scale of the turbulence created during entrainment of the fluid moving past it, so this process in turn, regenerates the right scale of mixing to maintain the entity. This uniformity is maintained as entrainment slowly changes the entity's composition.

Since the largest scale turbulence maintains a relatively sharp outside interface to the entity, with a relatively uniform internal structure, the entrainment rate is dominated by the internal transport of the entrained material away inside from this interface towards the inside of the volume. Thus strictly surface entrainment phenomena are irrelevant, once the entity structure is established.

The ETEM process also obviates the need to distinguish between "homogeneous mixing" and "inhomogeneous mixing" which can be ambiguous in interpretation, since both extremes in the mixing process may exist. Paluch and Knight note that the spectral peaks shift to smaller sizes at very low concentrations in about 3% of their observations. Telford *et al.* (1984) showed that this probably corresponds to a small region of dissipating cloud where entrainment of dry air reduces the size of all of the drops so that this volume would qualify as a homogeneous mixing region. Alternatively, this might also be interpreted as parcels descending and the drops evaporating in an area where there is no surrounding wet cloud.

The sentence in PK, "If it depends in some way on any mixing process, it should not be so constant and well-defined," taking "it" to mean the size of the peak droplets, is worth some discussion. The fact that the size is constant and well-defined, despite the great variability in droplet concentration, falls short, in our view, of implying that this size does not depend on the nature of some mixing process. We would contend that the protected initial adiabatic growth with practically no dilutions during that brief stage, is a feature of our description of the process leading to the subadiabatic liquid water in the cloud. This, in our view, can be considered part of the mixing process when the cloud is viewed as a whole. There are two points involved. The constancy of the diameter of the maximum concentration peak across the cloud at a given height regardless of total droplet concentration, and the fact that this diameter is approximately that expected in an undiluted updraft. It appears to us that the particular mixing process we describe here (and previously), can lead to both of these observations, even though dilution process is continually in progress. We agree that the specific droplet diameter at the larger droplet peak concentration is due to updrafts of fresh cloudy air originating below cloud base, and assume here that there is at first a rapid rise to the parcel and then initially entrainment rapidly modifies the cloud towards a dwell phase, where many parcels remain at almost constant altitude. This concept was discussed in detail previously (Telford, 1975; Telford and Wagner, 1980).

Thus a large fraction of the cloud has a composition which only slowly changes, except where it is finally dissipating. Consequently, mixing within the cloud will tend to occur between the initially formed parcels supplying a specific spectrum, and low concentration saturated descending entities (which also appear to have a continuing identity like the updrafts). The latter have lower drop counts and partly evaporated drops which supply nuclei and small drops to the surrounding cloud to produce the observed increase in smaller sized drops.

The descending diluted thermals entrain the droplet spectra from the dwell phase parcels which were formed rapidly at the initial entrainment stage. Here the original undiluted updrafts rapidly establish, by initial dilution, approximate equilibrium with their surroundings. Therefore the penetration of the diluted volumes formed by entrainment at the cloud top is slower, because the near equilibrium density balance of the cloud with the outside surroundings means that the buoyancy differences are less. Hence the droplet concentration peak size is only slightly modified, until the cloud parcels dilute to the stage where they start to dissipate, when the whole depth of the cloud column becomes unstable. Re-elevation of the low concentration entities at various stages could then grow bigger drops by the condensation process.

This explanation does not imply that larger drops are not formed, but rather that the cumulus cloud droplet spectral development is dominated by a process that provides a lot of drops at the narrow spectral window of the maximum size concentration. This would be expected from the relatively unmodified condensation process in parcels resulting from the dwell phase due to the tendency towards equilibrium with the pressure gradient set by the surroundings, when the other diluted recirculating parcles only occupy a small fraction of the total cloud volume. This should be valid even in a particularly active cloud development. As the cloud ages larger drops are more likely to be formed. Marine stratus clouds do not seem to have this particular restriction requiring a constant peak frequency radius at the same altitude (Telford and Wagner, 1981), presumably due to their long life and lack of sides.

"It is difficult to account for the correlation of the prominent peaks in the computed supersaturation with those of small droplet concentrations except by appealing to droplet activation." There is ample evidence from calculations that extensive reactivation does not occur except as a result following dilution by entrainment of some sort. Such as when drops are replaced by nuclei of the same size by entrainment, or by the ETEM process. As a special case the bimodal structure computed by Warner (1969) was obtained by applying accommodation coefficients less than 0.05. Even then an accelerating updraft was needed. The transient supersaturation was produced by restricting the rate of condensation and increasing the rate of adiabatic water release. However, there seem to be fewer small drops produced than are necessary to match the observations.

Section 4 of PK begins with the sentence, "In numerical models, bimodal spectra often result when the supersaturation in an air parcel exceeds the peak supersaturation it had experienced near cloud base." It seems safe to assume from the discussion following in PK, that the models mentioned are related to dilution of the parcel followed by further upward motion. Thus dilution should be considered as the possible key to the explanation. If we look at PK's Fig. 2b, c, d, we see that these represent three successive samples beginning on entering cloud, as shown in Fig. 3 where the sampling positions are marked. The three samples show in succession a great increase in number of droplets of about 3 μm radius, and a great decrease in the number of droplets at the 10 μm droplet peak in sample *c*. There is an average decrease in droplet concentration from about 200 cm^{-3} at the entry position through to the other side of the cloud where the droplet concentration is very much smaller. Thus at the cloud-edge-sample, b, there are very few 3 μm radius droplets, while at sample d, about a third of the way into the cloud there are about ten times more droplets at the 3 μm concentration peak than at the main 10 μm radius peak. This is quite consistent with the ETEM model regardless of updraft velocity or supersaturation. The progressive aging of the cloud by continued penetration of diluted entities from the cloud top down into the cloud, brings small drops to every level, because during descent these turbules (if we can refer to the diluted turbulent descending entities as turbules) tend to evaporate rapidly the drops they carry, which are continually being replenished in number by entrainment from the less diluted surroundings, and hence the entrained drops become quite small as they decrease in size under evaporation.

At the same time these small drops are eroded from the descending turbules into the surrounding cloud, so that as the surrounding cloud becomes more dilute it also receives an increasing population of small drops, and vice verse, the diluted turbules entrain drops at the size of the main spectrum peak. The increasing dilution from the edge of the cloud inwards, corresponds to the aging of the cloud parcels as new growth renews the almost undiluted outside edge. This raises the question of whether the updrafts discussed in PK as the source of the reactivated nuclei they postulate to account for the small drops, are in fact indicative of sustained vertical motion, or are a transient oscillation representing a turbulent fluctuation. If it is indeed a sustained vertical motion, which we agree will often reactivate nuclei, then the question which must be answered is why the low concentration regions do not experience a growth of the main spectral peak to larger sizes. This would appear to be a concept separate to the idea that the main peak in the drop size spectrum corresponds to the adiabatic uplift of undiluted parcels.

Thus the statement "It is difficult to account for the correlation of the prominent peaks in the computed supersaturation with those of small droplet concentrations except by appealing to droplet activation" should not be taken to mean that there is not another explanation which can account for correlation of the prominent peaks with small droplet concentrations. The correlation with updraft velocity is a separate question which we would suggest requires more evidence.

Another way supersaturation can activate more nuclei also occurs in the ETEM process. The nucleation of new drops during upward motion may be assisted by the recycling of nuclei, because the totally evaporated drops leave nuclei, and since the drops evaporated during the descent contain those formed on big nuclei as well as on small, nuclei able to reactivate at very low supersaturations will be available at this stage. However, these reactivated nuclei may well not be very important because of the numbers of small drops generated in the descending parcels, and which appear everywhere after horizontal entrainment between adjacent entities has been active.

A further comment about the discussions of supersaturation appears relevant. In essence a cloud is growing bigger drops if the air is moving upwards, and evaporating drops if there is downward motion. There are small hysteresis effects of a few tens of seconds lag and a small fraction of 1% in supersaturation but essentially the process is reversible. The process can be modeled to good accuracy when needed but in discussions it makes no sense to refer to supersaturation as though it were a variable independent of the vertical velocity. Comments that there may be large volumes of subsaturated air in a cloud is effectively equivalent to saying about half of the cloud is moving downwards at any one instant. The implication that there are thereby large parts of the cloud subsaturated and therefore without any drops, does not follow.

In summary we wish to draw attention to the relationships of the entity type entrainment mixing (ETEM) process to a number of issues raised by the data discussed in PK. The explanations offered by PK do not adequately take advantage of the ETEM process as it was explained in previous papers. It has, we submit, more to recommend it than some of the ideas discussed in PK, parts of which we feel could be quite misleading. However, we also would like to add that in our opinion, presenting ideas like these, as PK have done, in a concrete form which can be discussed and compared to other ideas, is an important part of developing these concepts and it will lead to a better understanding of this new approach.

Acknowledgment. This discussion was prepared under CCOPE funding from the Atmospheric Research Section of the National Science Foundation under Grant ATM-8206558.

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