variations of $P_1$ (see Figs. 6 and 8) occur primarily in the 3 to 7 year range, and for this period were all followed by changes in $S_T$. I also interpret $L$ as consisting of long-term variations associated with $P_1$ and quasi-biennial variations associated with $P_2$. The lack of significance of the correlation between $L$ and $S_T$ at zero lag therefore results from the interference and cancelling effects of the positive correlation of $L$ with $P_1$ and negative correlation of $L$ with $P_2$. However, at 12 months lag, these effects are additive.

Although Pittock's interpretation is the reverse of this, it appears that the above is in fact 'neater physically' by sorting the main variations into quasi-biennial and longer term periodicities. The latter may be related to air-sea interaction effects. The cause of the QBO is unknown. The positive correlation of $P_2$ with $S_T$, as noted in section 8, implies that when $S_T$ is positive (pressures low over Indonesia, high over the East Pacific), so that the Hadley cell is weak and the Walker circulation is strong, then the mid-latitude westerlies are weak and blocking is prevalent south-east of New Zealand ($P_2$ positive). The long-term mean behaviour of the QBO, as noted by Dr. Pittock, is apparently one where it strongly manifests itself for periods of a decade or so, and may die out at times, but there is ample evidence to suggest that it has been present in the near and distant past, over most of the globe, and throughout the troposphere and the stratosphere.

A major conclusion of my and Dr. Pittock's work seems to be that more research is required.

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DISCUSSION OF MASON AND JONAS' MODEL OF DROPLET GROWTH IN CUMULUS CLOUDS

By J. Warner

1. INTRODUCTION

The principles underlying the evolution of cloud droplet spectra and the formation of precipitation in warm clouds are well known. It is also generally recognized that these processes will be affected by mixing between a growing cloud and its environment; further, the nature of the changes resulting from the mixing process is well understood. This being the case, a model formulated to describe in quantitative terms the process of droplet development and rain formation is only of value to the extent that one can have confidence in the soundness of the dynamics and thermodynamics of the model itself. The model described by Mason and Jonas (1974) and Jonas and Mason (1974) (hereinafter I and II) inspires no confidence in this respect; further, the quantitative nature of the droplet spectra predicted from it are likely to be valueless: In what follows I would like to look at these predictions in some detail and examine their significance in terms of both the dynamics and thermodynamics of the model and of its microphysics.

2. DYNAMICS AND THERMODYNAMICS

In I a spherical thermal of known initial size, temperature excess and upward velocity is postulated; it rises through essentially non-turbulent surroundings, loses its buoyancy as a result of mixing,
descends still mixing and comes to rest; it is followed immediately thereafter by a second spherical thermal having properties identical to those the first had initially. In all cases it is assumed that mixing produces an increase in radius of the thermal directly proportional to its changes in height; i.e. that the entrainment rate is proportional to the vertical velocity of the thermal.

There are many reasons why such a model is likely to result in an unrealistic description of the mixing process, particularly when the motion of the thermal it describes is small in comparison with its size. In this regard we may note that the first thermal considered in Fig. 1 of I rises a distance less than its radius before coming to rest and descending and that the second thermal rises through the turbulent residue of the first, not through still air as assumed. However, rather than discussing these matters in detail, let us look at the use the authors have made of the model and at its predictions.

In I the authors claim that their model predicts liquid water content in agreement with observations and that it is better than the usual one-dimensional models at predicting simultaneously vertical cloud depth and liquid water content. Both claims are false. As shown by their own Figs. 1 and 2, the ratio of the liquid water content to its adiabatic value during the ascent of the second thermal is everywhere greater than the values I have reported (e.g. Warner 1970). This is, of course, particularly true in the lower levels, where the thermal is rising through the saturated air left behind by its predecessor and where the ratio \( W/W_a \) is close to 0.8 over the lowest 400m in the maritime cloud and the lowest 1400m in the continental cloud. With the ascent of a third thermal through the residue of the second the depth of the layer in which the liquid water content would be nearly abiotic clearly increases, even though \( W/W_a \) decreases again in the upper levels which have not been modified by earlier thermals.

Thus as the number of successive thermals increases this model is progressively less able to reconcile the internal properties of the final thermal and the height to which it rises with field observations of liquid water content and cloud depth. At all levels, particularly the lowest, the model cloud is far too wet.

A minor point regarding I is that the authors claim that the model is largely insensitive to the initial buoyancy. However, their Table 2 shows that for a 350m radius thermal temperature excesses of 0.8, 0.6, 0.2 and 0.1 degC result in rises of 1900, 1550, 550 and 330m respectively when all other dynamic and thermodynamic conditions are equal.

While Paper I makes some attempt to take account of the thermodynamics of mixing between thermal and environment, II ignores it completely. The supersaturation is arbitrarily held constant and the calculation is one in which height and updraft do not enter. The authors of II state in their paper that they 'compute the further evolution of the droplet spectrum by coalescence and condensation acting simultaneously but with values of supersaturation, mixing rate and cloud lifetimes taken from the calculations of I', but this is clearly not the case. How, for instance, is one to interpret the results shown in Fig. 3 of II when continued growth of the droplet spectrum is inferred 10min after the thermal in the thermodynamic model in I has reached its summit and the supersaturation has fallen to zero?

3. Microphysics

Two claims are made in I regarding predicted cloud droplet size distributions: a bimodal spectrum is produced and droplets of radius 25\( \mu \)m are produced by condensation in concentrations of order 100m\(^{-3}\) within half an hour.

Apart from depletion to the environment as a result of mixing, the concentration of the largest drops is governed solely by the number of giant condensation nuclei assumed to be present at condensation level. Further, provided the total number of activated nuclei and drops is small any process which allows giant nuclei to continue their growth in cloudy air for a long enough time will enable them to grow to 25\( \mu \)m radius or more: a steady updraught of a little under 1m s\(^{-1}\) would produce such a result in maritime air in the time taken for a parcel to rise to 1-6km, the top of the model cloud described by Fig. 1 in I.

In an earlier paper (Warner 1969) I described in qualitative terms how a bimodal spectrum might arise by growth, partial evaporation and regrowth of a cloudy parcel. Any such double process - including that defined by the thermal model described in I - will do this. But the model in I predicts bulk properties, such as liquid water, even more at variance with field observations than earlier, simpler, models; it is difficult therefore to see that any significance can be attached to the quantitative nature of predictions of droplet spectra derived from it.

In II the only account taken of mixing between the model cloud and its environment comes from the last term in Eq. (1). Its use implies loss of a fraction of existing drops of all sizes to the
environment and gain from it of a number of nuclei which are thereafter exposed to the constant supersaturation assumed. It is difficult to see how the growth of these entrained nuclei can be properly computed when the nucleus mass term in Eq. (3) has been replaced by the empirical relation suggested. However, the role of mixing in II is of minor importance and errors in treating the growth of entrained nuclei are probably unimportant to its conclusions.

The droplet spectra calculated in II all show marked peaks and valleys in the shape of the distribution at the larger radii as cloud development proceeds. It is difficult to think of a physical explanation for this behaviour and it would be of interest if the authors could suggest one. As is well known, such oscillatory behaviour is a common result of numerical instability which is very difficult to avoid in calculations of this sort where comparatively large class intervals have been employed.

4. Conclusions

There seems little doubt about the validity in qualitative terms of the results given by the two papers under discussion for the development of the droplet size distribution. But these aspects of the droplet distribution have been known and discussed for many years and have appeared in the literature repeatedly.

The real question would then appear to be whether the quantitative results given by the present model are any more reliable than those obtained from earlier models. Unfortunately, it is here where one must have most doubts. The dynamics and thermodynamics of the model described in I appear to yield an even less satisfactory prediction of bulk cloud properties than the one-dimensional model I discussed a few years ago (Warner 1970). Hence it would seem unwise to rely too much on the model in I for predictions of the magnitude of changes in the droplet size distribution resulting from mixing between cloud and environment. Similarly, the assumption of constant supersaturation in the calculations given in II must cast doubt on the quantitative values of the droplet spectra which, it is predicted, would result from growth by simultaneous condensation and coalescence – even if the qualitative nature is valid.

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Reply

By B. J. Mason

An unbiased, objective assessment of the present state of the subject would not, I think, support Dr. Warner’s opening statement that changes produced in droplet spectra by mixing between the cloud and its environment are well understood and even less the implication that the major characteristics of observed droplet-size spectra have been satisfactorily explained. His concluding remarks that the results obtained in our papers have been known for years and have appeared in the literature repeatedly are not true. The evidence lies in Warner’s own recent series of papers,
which we have studied very carefully, and in which he has tried, unsuccessfully, to explain the shape, dispersion and bi-modality of his own carefully measured drop-size spectra.

Warner (1969a) states unequivocally that ‘in our understanding of the initiation of the condensation-coalescence mechanism there are still large gaps in our knowledge and deficiencies in relating present theories to field observations’. In Warner (1969b) he demonstrates that ‘the concentration of cloud droplets is consistent with condensation on observed concentrations of nuclei and updraft speeds’, without invoking mixing, but he cannot account for the observed dispersion and bimodality in this way. Having carried out a calculation very similar to one published earlier by Bartlett (1968), Warner shows that broadening of the spectrum cannot be produced by turbulent fluctuations in the updraught as claimed by Russian workers but goes on to say that ‘some updraught structures, particularly those involving a general acceleration, are capable of broadening the spectrum by the repeated activation of fresh nuclei and of producing a bimodal distribution’. However, he ‘casts doubt on any belief that bimodal distributions are usually produced in this way’. A further problem was to account for the observed high concentrations of droplets of \( r < 10 \mu \text{m} \) at heights of only a few hundred metres above cloud base but he could do this only on the assumption that the condensation coefficient for small water droplets lies between 0.02 and 0.05. Finally, in order to account for the appearance of drops of \( d > 35 \mu \text{m} \) he had to invoke condensation on giant salt nuclei of \( m > 10^{-15} \text{g} \).

Returning to the problem in Warner (1973) he essentially repeats the calculations of Mason and Chien (1962) who examined the growth of droplets by condensation in a rising air mass undergoing lateral mixing with the environment and from which droplets are continually transferred to the surroundings and replaced by fresh nuclei. Warner confirms that such a model produces considerable broadening of the spectrum but rightly says that it does not reproduce the observed shape. He now comes to the surprising conclusion that ‘simple [sic] mixing between the cloud and the environment is unimportant in determining the droplet size distribution, at least in the early stages of cloud growth’. This is surprising because without mixing it is difficult to account for the low liquid water contents which Warner observes and to reconcile the observations with the fact that computed droplet spectra becomes increasingly narrow during the ascent of an unmixed parcel.

This is how we found the subject when we decided to carry out our calculations. The observed shape and dispersion of the droplet spectra were still unexplained, no model or convincing calculation had produced a bimodal spectrum by condensation, the role of mixing was unresolved and droplets of \( d > 35 \mu \text{m} \) were thought to arise by condensation on giant salt nuclei \( (m > 10^{-15} \text{g}) \) despite the fact that these are rarely present in sufficient concentrations to explain the observed concentrations of large droplets.

Warner claims to have produced a qualitative explanation of bimodal spectra but we can find only the following short statement: ‘Development of bimodal distributions is probably a result of mixing between the cloud and its environment. If a parcel of cloudy air-containing drops whose size distribution has only a single mode is mixed with environmental air, all the drops will decrease in size, the smallest most rapidly until they evaporate. If this parcel then rises further at a slow rate, condensation will take place only on the smaller number of droplets that remained. Subsequent mixing of this parcel with some of the original cloud will result in the development of a bimodal drop size distribution.’

This rather general statement, unsupported by any calculation, does not take us very far and, as stated, is not accurate. Mixing with a rising parcel will not cause evaporation unless the supersaturation falls below the critical value required to sustain the smallest droplets and all the rest may continue to grow. Evaporation of all the droplets is likely to occur only if the parcel descends when the smaller droplets will disappear leaving behind the largest to be caught up in a second rising thermal and produce a bimodal spectrum in the manner demonstrated in our paper. We cannot agree with Warner that any process of partial evaporation and regrowth will achieve the required result.

Our model, consisting of two or more thermals rising through the residues of their predecessors, is admittedly a very simplified description of what actually happens but, unlike all other one-dimensional models, it does contain the essential elements of cumulus evolution and, despite its limitations, it does produce pronounced bimodal spectra of very similar character to those observed by Warner and is able to account for the large concentrations of droplets of \( r < 10 \mu \text{m} \) at all levels without having to appeal to very low values of the condensation coefficient. Moreover, the maritime version produces, within half an hour, droplets of \( d > 35 \mu \text{m} \) on nuclei of \( m > 10^{-14} \text{g} \) and droplets of \( d > 50 \mu \text{m} \) on nuclei of \( m > 10^{-11} \text{g} \). We believe that these salient features have not been produced by any previous model calculation.
Warner sharply questions the validity of these results because during the early stages of ascent thermals have liquid water contents almost twice as high as he observes near the bases of cumulus clouds. He insists (Warner 1969a) that any acceptable model of cumulus growth must reproduce his observed average concentrations of liquid water that are only a few tenths of the adiabatic values, and not just the peak values. But if we accept the observational evidence that a cumulus is built from a succession of thermals, a model must surely distinguish between the properties of active rising thermals and the average quantities likely to be measured in horizontal traverses through a cloud composed largely of the residues of earlier thermals. Here it is relevant to note that Warner usually began his aircraft traverses near the cloud top and entered near the base some 10–20 min later by which time the whole cloud may have become considerably modified by mixing through both the top and the sides.

We have chosen to focus attention on the active thermals because it is in these that the droplet spectra develop most rapidly towards the precipitation stage. If such a thermal is to accelerate through a pre-existing cloud rather than just feed on its initial buoyancy (which, in the case of maritime clouds is likely to be very small), it is a thermodynamic necessity for its liquid water content to be larger than the average values quoted by Warner irrespective of the detailed nature of the mixing process. The average liquid water content measured near the base of a cloud composed largely of the residues of earlier thermals may be considerably lower. Once the thermal emerges from the cloud into the drier air above, it may continue to rise for a considerable distance even though it is being continually diluted and decelerated by mixing. But even with environmental humidities as high as 85% our calculations indicate that, during the second half of the ascent, the liquid water content may drop to values not much greater than the average values measured by Warner near cloud tops. We regret that in our paper we did not distinguish sufficiently between the calculated properties of the thermals and the likely properties of the cloud as a whole.

In summary, we are of the opinion that any attempt to model the growth of a substantial cumulus by the rise of one single isolated mass of air is unrealistic and is almost bound not to reproduce the requisite growth and, at the same time, the average properties measured by Warner. The apparent dilemma raised by Warner largely disappears if the cloud is regarded as building on a succession of thermals rising through an environment modified by earlier thermals. Despite the admitted simplification and limitations of our model (which can be readily extended to more than three thermals) we believe that it reproduces the main features of the observed droplet spectra quite well and that Warner’s opinion that the model is unsound because it does not reproduce his low average values of water content in the lower levels is ill founded.

Turning now to his criticism of our second paper, he appears to misunderstand or to disapprove of our objective which was to demonstrate that the apparent barrier to growth of droplets between radius 20 μm which they can reach by condensation as shown in Paper I and radius 30 μm beyond which they can grow quite rapidly by coalescence, disappears if one allows condensation to continue and reinforce the early stages of coalescence.

We therefore started with spectra produced by condensation during the ascent of the second thermals in our model of Paper I. We could, of course, have followed the detailed evolution of such a spectrum during its ingestion and ascent in a third thermal but we decided that we could amply demonstrate our thesis by using an average value of the supersaturation generated in the third thermal with coalescence suppressed, rather than computed new values of the supersaturation for each time step which would have greatly increased the degree of computation.

There were similar reasons for using the parameterized version of Eq. (3) for the growth of a droplet by condensation on soluble nuclei. Again we are confident that this simplification did not significantly affect the results because when this same approximation was made in following the evolution of the spectrum by condensation alone the results were very similar to those obtained by the more exact treatment of Paper I.

We are also confident that the peaks in the computed droplet spectra, greatly exaggerated by the logarithmic scale, are not the result of computational instability because when the radius class intervals were reduced from 2^{1/6} (55 classes) to 2^{1/6} and, recently, to 2^{1/12} (85 classes with the maximum radius decreased from 400 μm to 100 μm), the results were almost identical.

The peaks at the small radius end of the spectrum arise from the complex (bimodal) character of the initial spectrum. The secondary peak at the tail of the spectrum arises from the fact that the larger droplets grown predominantly by coalescence with small droplets rather than by coalescences between themselves; this is a consequence of the fact that the collection kernels vary only slowly with the radii of the small droplets over the critical part of the range (see Fig. 1 of Paper II) whereas the concentration of large drops decreases very rapidly with increasing size.
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SOURCES OF ERROR IN THE USE OF THE MARSHALL-PALMER DROP-SIZE DISTRIBUTION EQUATION

By R. F. Griffiths

SUMMARY

The standard method for evaluating the Marshall-Palmer equation is shown to overestimate the area beneath the curve. The error is a function only of the precipitation rate, p, and the diameter interval, δD, and can assume large values. This source of error can be eliminated by using an integral form of the equation. It is suggested that this method be adopted in preference to the one commonly used, both for the Marshall-Palmer equation as well as for other similar expressions.

A number of expressions have been proposed by various workers to represent the distribution of raindrops with size. One of the best known is that due to Marshall and Palmer (1948). The basic equation is written

\[ N(D) = N_0 \exp(-kD) \]  

(1)

where \( N(D) \) is the number of raindrops per unit volume having diameters in the range \( D \) to \( D + \delta D \) cm, where \( N_0 = 0.08 \text{cm}^{-4} \) and \( k = 4 \) \( \times \) 0.01, \( p \) being the precipitation rate in mm h\(^{-1}\).

The expression was derived to fit the drop-size distribution data collected by Marshall, Langillle and Palmer (1947) in the course of a study on the use of radar to detect and measure rainfall. In the 1948 paper the original data were presented as a series of points on a graph, which is in fact a histogram; clearly, each point on the graph represents the number of drops per unit volume having diameters in the range \( D - \frac{1}{2} \delta D \) to \( D + \frac{1}{2} \delta D \), where \( \delta D \) is the width of each histogram class, centred on diameter \( D \). Eq. (1) was devised to pass through these central points as closely as possible. The above interpretation must be true since the expression for \( N(D) \) is independent of the interval \( \delta D \), and is a reflection of standard practice in the use of histograms to generate distribution functions.

In using the Marshall-Palmer expression we are required to take \( N(D) \delta D \) as the number of drops per unit volume in the range \( D \) to \( D + \delta D \). This is rather different from evaluating the number in the range \( D - \frac{1}{2} \delta D \) to \( D + \frac{1}{2} \delta D \), since in the former case there is always an overestimate which arises from the inclusion of the small area \( A \), shown shaded in Fig. 1. It is common practice to take for granted that this error is negligible, i.e. that the functional dependence of \( N(D) \) on \( D \) can be ignored over the range \( \delta D \). (Indeed, this is also assumed when evaluating over the range \( D - \frac{1}{2} \delta D \) to \( D + \frac{1}{2} \delta D \), but since the value of the area under the curve is slightly underestimated in the range \( D - \frac{1}{2} \delta D \) to \( D \), and slightly overestimated in the range \( D \) to \( D + \frac{1}{2} \delta D \), the two errors compensate very closely.) If we evaluate the number according to the standard use of the Marshall-Palmer equation, then the error is always positive, and it is pertinent to ask under what conditions this error is negligible.

To be rigorous we should evaluate the integral of Eq. (1) between the limits \( D \) and \( D + \delta D \) to obtain \( I \), the true value of the number of drops in the range. Hence,