1. Introduction

A workshop on the parameterization of cumulus convection was held at Key Biscayne, Florida, on 3–5 May 1991. The workshop involved about 40 individuals (see Fig. 1) with expertise in cumulus parameterization and in areas of atmospheric science on which cumulus parameterization depends.

The purpose of this paper is to report on areas where the workshop participants generally agreed that progress has been made, and to indicate aspects of the problem that are still poorly understood and require further work.

As the spatial resolution of a model decreases, smaller-scale phenomena such as cumulus convection become unresolved. The cumulus parameterization problem consists of formulating the collective effects of unresolved cumulus clouds, associated circulations, and precipitation in terms of prognostic variables resolved by a dynamical model. As model resolution degrades, a distorted version of cumulus convection may be aliased onto the coarse resolution of the model. In parameterizing convection, not only must the collective effects of convection be represented by the parameterization with reasonable veracity, but the biased effects of distorted explicit convection must somehow be avoided.

As diverse as attempts to parameterize convection are, they all seem to have two common elements—namely, a cloud model that determines how convection affects the environment and a closure scheme that governs how the environment controls the convection. The workshop considered these two general problems, as well as several more specific considerations.

In the following sections, we consider the issues of cloud models and closure, and the problems of representing mesoscale circulations and momentum fluxes.

We conclude with a summary of general points that emerged from the workshop.

2. Cloud models for cumulus parameterization

In adapting cloud models for cumulus parameterization, it was felt that it would be desirable to have a cumulus model that was flexible enough to span the full range of observed clouds, from small trade wind and fair weather cumuli, and possibly even stratocumulus, to giant cumulonimbi. The reasoning is that clouds occupy a continuous size spectrum in the atmosphere, and it is difficult to decide where to draw the line between, say, shallow clouds and deep clouds in a model. A few schemes, such as the Arakawa–Schubert scheme, already attempt to do this. Stratocumulus clouds are particularly important to the earth's radiation balance, and probably cannot be treated independently of cumulus clouds, as illustrated in a talk by Chris Bretherton.

Given recent progress in cloud dynamics, it was felt that alternatives to the entraining plume model of a cumulus cloud should be explored. The lack of saturated downdrafts in this model is in conflict with observations, as discussed at length by Marcia Baker and Alan Blyth. Some investigators have attacked this problem by assuming that an ensemble of entraining plumes with differing entrainment rates, and, hence, different detrainment levels, exist at each grid point. However, downdrafts induced by mixing of cloud material and environmental air are still excluded from this model. The entraining plume model may nonetheless yield correct heating profiles because they detrain finite amounts of condensed water, whose subsequent evaporation cools the environment. But the moistening produced by the model may differ from that of a real cloud that experiences cloud-top entrainment (see Fig. 2). Of course, downdraft entraining plumes can be and are incorporated in many cumulus representations.

Participants in the workshop agreed that far more
attention needs to be paid to cloud microphysics. For deep, tropical convection, the heating profile is largely determined by the requirement that large-scale destabilization balance convective stabilization. In contrast, the moistening profile depends sensitively on the microphysical processes that determine how much condensed water is ultimately reevaporated into the environment versus being precipitated out of the system. It was felt that existing representations of cumulus convection are far from adequate for use in climate models, as their representation of cloud microphysics is too crude to allow realistic prediction of atmospheric water vapor and of convectively produced stratiform clouds (such as cirrus anvils), both of which are crucial to the radiation balance.

Moreover, the evaporation of falling precipitation drives mesoscale and convective-scale downdrafts, which control both the mesoscale dynamics of convective systems and the mean entropy content of the subcloud layer. These downdrafts may also strongly influence the heat balance of the ocean, since in light mean wind conditions they may have a profound influence on surface heat and moisture fluxes.

Some existing schemes already include moist downdrafts. Inclusion of downdrafts in the Arakawa–Schubert scheme has been accomplished by Ming-Dean Cheng, who reported results at the workshop.

Aspects of cloud microphysics that must be properly accounted for include the precipitation efficiency in warm clouds and the partition of frozen water into graupel, which tends to fall out quickly, and essentially unrimed crystals and aggregates, which are carried upward and outward to form the mesoscale anvil. Explicit representation of detrainment over cloud water, ice, and precipitation will probably be needed in higher-resolution dynamical models.

The general problem of downdrafts and their incorporation in cloud models was discussed at length. Observations of mesoscale convective systems show that downdraft mass fluxes are comparable to, and at some levels exceed, those of updrafts. More attention needs to be paid to the correct formulation of downdraft models. The interaction of downdrafts with the planetary boundary layer also needs to be addressed.

Ed Zipser, Peggy LeMone, and others pointed out that many formulations of convective heating and moistening assume that convective updrafts ascend through cloud-free environments, but that this is often not observed. Careful attention should be directed to the problem of the interaction of convective drafts with stratiform cloud layers.

Finally, it was pointed out that short- and long-wave radiative fluxes are tightly coupled with cloud dynamics. Stratocumulus layers and mesoscale anvils are clouds for which this coupling is very important.
3. The closure problem

Debate on the closure issue was vigorous, indicating a continued lack of consensus in this area. Much of the debate focused on whether one form of closure could be used for a variety of applications (e.g., weather prediction, climate simulation), for a spectrum of large and mesoscale phenomena (e.g., oceanic cumuli, hurricanes, mesoscale convective systems), and for a spectrum of convective clouds (shallow, deep, highly sheared, slantwise), or whether a variety of closures must be used.

Closures in existing representations fall into three categories: those based on constraints provided by the large-scale water budget, on the quasi-equilibrium hypothesis, and on the release of convective available potential energy.

The first of these proved to be the most contentious. Several participants suggested that Kuo-type schemes are fundamentally ill posed and that their use should be discontinued. It was pointed out that Kuo-type schemes always moisten grid columns in which convection is active, while observations often show a net convective drying of the column. Several examples were cited of large-scale forcing that would produce convection in nature but not in schemes that require large-scale convergence of water vapor. David Raymond also argued that moisture convergence should not be used as a basis of closure, since it is as much a response to convection as its cause. There was near-universal agreement that the partitioning of the vertical distributions of heating and moistening via the “b” parameter is rather arbitrary, in spite of the appearance of some rather sophisticated diagnostic relations for this parameter. Leo Donner noted, however, that Kuo schemes are rather widely used and seem to work fairly well. He also noted that moisture convergence is statistically well correlated with the rate of large-scale destabilization.

There was vigorous discussion about the circumstances in which the quasi-equilibrium hypothesis should be valid. There is universal agreement that convection is fundamentally chaotic in nature, so that pointwise prediction of convective activity is impossible beyond a few convective overturning times. On the other hand, the ensemble-average properties of

![Fig. 2. Illustration of how apparently similar detrainment layers in identical environments can contain different water vapor mixing ratios. The plume on the left detains just below the inversion after having entrained air continuously from cloud base upward. The result is a much higher mixing ratio than for a parcel that ascends without mixing to just above the inversion, mixes with the air there, and sinks below the inversion before detraining. In both cases the mixture contains equal amounts of environmental air and cloud-base air.](image)
convection should be relatable to features of the large-scale flow and boundary conditions; this is in fact the condition for “parameterizability.” Debate focused on how to define an ensemble. Mike Fritsch argued that an ensemble may consist of a collection of individual updrafts and downdrafts that comprise a large thunderstorm, arguing that such storms might be predictable many hours in advance as a consequence of more predictable, mesoscale processes. This led to a general discussion of the circumstances under which quasi equilibrium holds.

Akio Arakawa, Michio Yanai, Kuanman Xu, and Xiaoqing Wu argued, on the basis of recent research, that quasi equilibrium works well in relating strictly convective circulations to mesoscale circulations. Fritsch countered that one goal of mesoscale models is to predict the time and location of the development of individual thunderstorms on mesoscale time scales, which he felt is incompatible with quasi equilibrium, unless it pertains to individual convective bubbles or plumes in the context of thunderstorms or complexes of thunderstorms. This raised the issue of “triggering,” which promoted a lengthy discussion among the participants.

In forecasting the initial appearance of deep convection in an atmosphere that is decidedly conditionally unstable, one must be able to forecast the local disappearance or reduction of the capping inversion that restrains the convection. This is the “triggering” problem. Several participants felt that this event could be forecast explicitly, at least by mesoscale models. John Molinari and Zipser, on the other hand, thought that there is enough variance in the inversion strength and the entropy of subcloud-layer air within a grid box that the outbreak of convection somewhere in the grid box must depend on an adequate account of these variances. Don Perkey felt that this is a more difficult problem than when convection is triggered by well-defined mesoscale circulations such as those associated with fronts. Zipser stated that this problem was for the most part a different problem from what we usually call closure. Kerry Emanuel remarked that the triggering problem was being exaggerated by the historical fact that most research on thunderstorms was conducted in the United States, where large conditional instability restrained by capping inversions is not uncommon. He felt that this situation is pathological in the global context and that most convection could be regarded as being in equilibrium with larger-scale destabilization.

In effect, two separate problems were discussed: that of forecasting the onset of convection in a conditionally unstable atmosphere with a potential barrier to convection, and that of predicting the evolution of existing convective systems. Perhaps it is true that, once triggered, convection rapidly comes into equilibrium with large-scale destabilization, even if by large-scale we mean the comparatively local forcing due to, for example, the circulation around a mesoscale cold pool.

Finally, there was some discussion of closures that relate the amount of convective available potential energy at the beginning of the time step to the amount of convective available potential energy. These include the schemes of Fritsch and Chappell (also as recently modified by Jack Kain), Kreitzberg and Perkey, and Emanuel. Raymond claimed that in actual practice, such schemes replicate quasi-equilibrium convection by insuring that the model evolves through a sequence of equilibrium states, except when a metastable atmosphere has just become unstable, in which case such schemes may produce very large mass fluxes over one or a few time steps until quasi equilibrium is regained. Emanuel’s and Randall’s schemes are explicitly formulated to drive the atmosphere toward quasi equilibrium, even though quasi equilibrium is not strictly enforced. (Indeed, even the Arakawa–Schubert scheme cannot strictly impose quasi equilibrium, for this would in general require some negative cloud mass fluxes. Instead, as pointed out by Steve Lord, mass fluxes are determined uniquely so as to minimize the rate of change of the cloud work function.) David Randall presented a closure in which the cumulus kinetic energy replaces the cloud work function as an explicit prognostic variable, relaxing the rigid enforcement of quasi equilibrium.

It would appear that closures that relate cumulus activity to the instantaneous magnitude of convective available potential energy have much more in common with the quasi-equilibrium closure than either has with Kuo-type schemes, with the caveat that quasi equilibrium cannot hold under the comparatively unusual circumstance of the release of finite stored energy. The trend at the moment is toward closures that one might call “soft” quasi equilibrium, which tend
to drive convective mass fluxes toward a state of quasi equilibrium without attempting to strictly enforce it.

There was general agreement that a good closure should be able to handle shallow as well as deep convective clouds, in spite of important differences in their characteristics. There was some dissent from this view, however; for example, Bruce Albrecht suggested that neither the quasi-equilibrium nor moisture-convergence closures were adequate for shallow convection. Concern was expressed about the ability of convection schemes to handle differences between oceanic and continental convection; Baker suggested that the concentration of cloud condensation nuclei might need to be a prognostic variable in models of the future. There was far less agreement about whether slantwise and upright convection could be treated by a single scheme (see section 5). Participants suggested that as data assimilation becomes an increasingly important tool in constructing analysis for climate studies, the distinction between closures for global weather prediction models and those used in climate models will probably lessen.

4. Incorporation of mesoscale processes

It is now well known that much of the precipitation that falls in mesoscale convective systems comes from mesoscale stratified clouds, such as anvils, rather than directly from convective clouds. The workshop took up the questions of whether and how mesoscale processes should be included in representations of ensemble convective effects.

There is a growing body of evidence that parameterized convection can be used successfully in mesoscale numerical models. Wu and Yanai presented research results suggesting that quasi equilibrium is achieved even at the mesoscale, and there has been considerable success in explicitly modeling mesoscale convective complexes using parameterized convection.

There appeared to be a consensus among workshop participants that representations of convection used in mesoscale models should actively supply condensed water as well as water vapor to the mesoscale model. Some even suggested that this should occur in global models as well; indeed, some weather forecast models now carry cloud water and precipitation as explicit prognostic variables. Bob Houze commented that the scale of mesoscale anvils might be related to the kinematics of ice and snow fallout; thus, mesoscale models must receive from the convective representation some condensed water if one wishes to simulate the anvil system. Donner and Dean Churchill pointed out that such anvils have a large effect on radiative transfer, which is sensitive to the shape and size distributions of the ice particles.

The issue of how and whether to represent mesoscale processes in global models that cannot now resolve them proved somewhat more contentious. Arakawa and Xu presented evidence from explicit ensemble numerical integrations of moist convection that in spite of the presence of mesoscale organization, convection is basically parameterizable. These simulations showed that even when the domain size is large (512 km) there is still reasonably strong modulation of unresolvable moist-convective processes by resolvable processes. They did point out, however, that for such a large domain size there appear unmodulated high-frequency fluctuations and, when the vertical shear is strong, a few hours delay in the modulation. These become less significant as the averaging domain decreases in size, down to the point where the scale separation between the averaging domain and the cumulus scale becomes problematic.

A few participants expressed concern that the presence of mesoscale circulations very much changes the rules of the game. David Raymond and Jack Kain pointed out that mesoscale distributions of clouds and water vapor change what convective plumes “see” as their environment, as opposed to what parameterized convection encounters in large-scale models. Moreover, the mesoscale circulations associated with the evaporation of rain might substantially alter the distributions of heating and moistening by the ensemble of convective plumes and mesoscale circulations, compared to what would happen without the mesoscale. Emanuel presented evidence from running his own scheme in a radiative-convective equilibrium mode that the equilibrium relative-humidity profile is extremely sensitive to assumptions about how much precipitation falls through unsaturated air as opposed to saturated, cloudy air.

In summary, it was generally felt that convective schemes will prove viable in models that explicitly simulate mesoscale circulations, provided that the schemes actively provide correct detrainment of hydrometeors to the explicitly simulated mesoscale environment. The representation of convection in global models that do not resolve mesoscale phenomena should account for mesoscale processes for a variety of reasons, including the effect of the mesoscale distributions of water substance on the convective plumes, the possibly large effects of the mesoscale circulations on heat, water, and momentum fluxes, and the decided influence of mesoscale anvils on shortwave and longwave radiation. The incorporation of such effects in large-scale models was identified as an issue that needs much more work.
5. Momentum fluxes

Compared to vertical transport of thermodynamic quantities by cumulus clouds, the transport of horizontal momentum is even more poorly understood. Momentum is not a conserved quantity, and momentum transport does not obey simple laws, as demonstrated by mounting observational evidence. LeMone reviewed data showing decided countergradient momentum fluxes in some tropical squall lines.

The linear theory of dry, parallel-plate convection in shear shows that momentum transport is downgradient and behaves like a simple passive scalar when the convection occurs in lines oriented along the shear. But when the convective lines are aligned orthogonal to the shear, the transport is strongly upgradient. Nonlinearity and latent heat release are well known to favor three-dimensional convection in the absence of shear, leaving unresolved the favored mode of moist convection or the sign of the momentum transport in shear. There is also observational and theoretical evidence that the presence of precipitation makes it possible for downgradient transport of the cross-line component of momentum to occur.

Observations presented by LeMone are consistent with the above, showing downgradient transport of the along-line component of momentum and of the cross-line component in the lower troposphere, with upgradient transport of the cross-line component in the upper troposphere. Wu and Yanai showed a systematic difference in the direction of vertical momentum transport between MCCs, for which the momentum flux is downgradient, and squall lines, which transport the cross-line component upgradient. Mitch Moncrieff discussed momentum fluxes in analytic models of convection.

Virtually everyone agreed that the observational and theoretical evidence weighs against simplistic treatments of convective momentum transports and that much more work is needed in this area. Several participants cited evidence of poor performance of models that use simple momentum transport schemes. Martin Miller stated that numerical simulations of tropical cyclones perform much better when cumulus friction is omitted, attaining central pressures 25–40 mb lower, and closer to observations, than in simulations with momentum transports. Wu showed evidence of poor performance of a simple momentum transport scheme based on the entrainment assumption for squall lines, although the model appears to work well for MCCs. Raymond suggested that the resolved flow response to convection may contain most of the important momentum effects associated with convection. There was some discussion of the possibility that potential vorticity conservation principles would impose constraints on convective momentum transfer.

Thor Erik Nordeng spoke about the related issue of slantwise convective parameterization. He believes that the slantwise convection scheme he used in the Norwegian forecast model did improve the performance of the model. Miller thought that the European Centre for Medium-Range Weather Forecasting T106 model did not exhibit much effect from a similar slantwise convection scheme, because the model rarely exhibited any regions of instability. He felt that the model might be removing the instability by resolved motions.

Jean François Geleyn and Emanuel pointed out that slantwise neutrality is the end state in a spindown experiment starting from conditional instability in a baroclinic atmosphere. This end state must reflect both the heat and momentum fluxes by convection. They felt that the explicit incorporation of slantwise neutrality as an end state in convection schemes might take care of the momentum fluxes, and that, therefore, slantwise and upright convection should be incorporated in a unified scheme. Other participants felt that this might not work well if the route to slantwise neutrality from (upright) conditional instability is indirect.

No single method of accounting for momentum effects of convection was preferred by the participants. Clearly, significant progress in fundamental understanding of convective effects on momentum must be made before improvements in representing such effects are forthcoming.

6. Evaluating convective representations

Verification of cumulus parameterizations has so far relied mostly on comparison of predicted to observed vertical profiles of heating and moistening. At this workshop, Arakawa and Xu also presented comparisons between heating and moistening profiles
produced by the Arakawa–Schubert scheme and those produced by an explicit cloud ensemble numerical model. It has become conventional to call the apparent heat source, consisting of the actual latent heating together with the convergence of the eddy flux of sensible heat, \( Q_1 \), while the apparent moisture sink, multiplied by the heat of vaporization divided by heat capacity, is called \( Q_2 \). In the context of parameterizations, these represent the total contributions of convection to the thermodynamic and water conservation equations.

There was considerable discussion about the validity of using deductions of \( Q_1 \) and \( Q_2 \) from observations to evaluate convective representations. Bill Frank suggested that such comparisons might be misleading in the case of schemes that use the amount of conditional instability to calculate instantaneous tendencies of temperature and water vapor, since these may simply represent the scheme’s attempt to establish new equilibria. If these schemes were run for several time steps using observed forcing, they might rapidly establish quasi-equilibrium tendencies with better agreement between the observed and predicted \( Q_1 \) and \( Q_2 \). The failure of such schemes to produce good profiles of \( Q_1 \) and \( Q_2 \) in the first time step would then indicate a mismatch between the profile of conditional instability that gives the correct mass fluxes in the scheme and the observed profile, a mismatch that is quickly corrected by slight alterations in the temperature profile during the first few time steps of an integration. Such adjustments may, however, cause problems such as artificial excitation of internal waves when used in time-marching models. Frank’s remark does not pertain to schemes using the quasi-equilibrium closure.

On the other hand, Emanuel felt that good comparisons between the observed and predicted \( Q_1 \) and \( Q_2 \) profiles might be insufficent tests of cumulus representations, because actual tendencies of temperature and water vapor represent very small differences between \( Q_1 \) and \( Q_2 \) and the tendencies produced by large-scale processes such as advection and radiation. Thus, relatively small differences between correct and incorrect \( Q_1 \) and \( Q_2 \) would lead to large errors in predicted tendencies. He recommended a more stringent test in which time series of large-scale forcing (advection, radiation, surface fluxes) supplied by observations from mesoscale arrays (such as that used during GATE) would be used in conjunction with a cumulus scheme to make predictions of the time evolution of temperature and relative humidity over a period of several days. A comparison between the observed and predicted temperature and relative humidity over such a time period would provide a sensitive test of the scheme.

Fritsch pointed out, however, that the instantaneous \( Q_1 \) and \( Q_2 \) from semiprognostic calculations are important diagnostics for evaluating the utility of convective schemes in mesoscale models. Fritsch’s point underscores a theme voiced frequently in the workshop: convective schemes for mesoscale models probably require quite different validation than those designed for climate models. Small errors in moistening may have important effects in climate simulations, while they may be largely irrelevant for 6-h forecasts; conversely, the heating profile may be less of an issue in climate simulations, which will almost always be in the quasi-equilibrium regime, but may have a large effect on the short time-scale release of stored convective energy, of great importance in mesoscale forecasts.

There was some dissent from this view, however. Tony Del Genio compared global climate simulations with the Goddard Institute for Space Studies (GISS) climate model, using two quite different convective schemes. While the schemes differed in their physics, they produced similar upper-tropospheric humidity distributions, largely because of a strong compensation provided by large-scale moisture advection. He suggested that one-dimensional tests may exaggerate differences in the schemes. But Del Genio did find that the overall cloud feedback was strongly positive in one version of the model, and almost neutral in the other. The reduced cloud feedback appeared to be due to the inclusion of downdrafts, which tend to produce more reasonable low-level humidity (and, thus, low-level cloud) distributions, and to the inclusion of a representation of cirrus anvils via a cloud water/ice budget, which causes high clouds to become optically thicker in warmer climates, giving a negative solar radiative feedback.

Alan Betts cited several instances in which missing observational data are seriously curtailing the validation of convective schemes. He emphasized the need for much better humidity measurements in the upper troposphere, improved measurements of hydrometeor distributions in convective clouds, and global precipitation data. There was a general consensus that improved validation of convective schemes will depend critically on better measurements of this kind.

The main points that emerged from the session included a concern that conventional semiprognostic tests may be insufficient for assessing the validity of convective schemes in climate models, where small errors in \( Q_1 \) and/or \( Q_2 \) may accumulate over time and lead to grossly inaccurate humidity distributions, and a consensus that better measurements of upper-tropospheric humidity, cloud microphysical parameters, and global precipitation will be crucial to better validation of convective schemes.
7. Summary

A consensus view of several issues emerged from the workshop, though in almost every case there were dissenting opinions. The following represents the authors' attempt to summarize the main findings of the workshop.

Recent observations of cumulus clouds point to certain limitations of the entraining plume model. It was generally felt that alternatives to this model, such as the buoyancy-sorting model of Raymond and Blyth, deserve serious consideration.

More serious attempts should be made to couple cumulus parameterizations with representations of stratocumulus and with boundary-layer formulations.

Saturated and unsaturated downdrafts are important components of convection and must be properly accounted for in representations of cumulus clouds.

It is essential to include accurate representations of cloud microphysical and radiative processes in formulations of the ensemble effects of convection, bearing in mind that these two processes depend on one another. We need to work much more closely with cloud physicists and experts in radiative transfer.

Along the lines of the Arakawa-Schubert scheme, it is desirable to formulate a unified treatment of the whole spectrum of convective clouds, from small trade cumuli to cumulonimbi, in spite of their profound differences, recognizing that the spectrum is continuous.

We need to carefully reconsider the assumption that cloud drafts ascend and descend through gridbox average environments.

Advances in large-scale and mesoscale models may make it desirable to include the detrainment of hydrometeors as an output variable of convective schemes.

More research needs to be directed at incorporating the effects of mesoscale convective circulations in models that do not resolve them, and to explore the problem of coupling convective representations with mesoscale circulations in models in which the latter are resolved.

There are strong reservations about the physical justification for using the water vapor budget as a closure for convective schemes.

In practice, there is a strong similarity between closures based on quasi equilibrium and those based on the amount of convective potential energy present at a time step. The idea of formulating "soft" quasi-equilibrium closures that drive convective fluxes in the direction of quasi equilibrium should be pursued further.

There is no consensus on how or whether to incorporate cumulus momentum fluxes in parameterizations. It is felt that much more fundamental research on this question needs to be done.

It became clear to workshop participants that cumulus representations in different kinds of models may have to satisfy different requirements. For instance, accurate triggering of convection in the presence of capping inversions is exceedingly important for a mesoscale forecast model, but of less importance to a coarse-resolution global model. These distinctions need to be kept in mind when comparing different schemes.

There was considerable sentiment in favor of doing much more comparative testing of cumulus parameterization schemes in models. Toward facilitating this, many felt that existing and new parameterizations should be phrased as well-documented, transportable subroutines.

Workshop participants were unanimous in asserting the importance of observations that would put cumulus parameterization on a sounder physical basis. Critical issues include better understanding of detrained moisture fluxes, upper-tropospheric humidity, mesoscale organization and its effects on parameterization, and ice and precipitation processes as they affect larger scales. Considerable importance was put on removing regional blinders (e.g., the American High Plains) and on developing global climatologies of convective behavior.

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