REVIEW ARTICLE

The Cumulus Parameterization Problem: Past, Present, and Future

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(Manuscript received 24 June 2003, in final form 16 January 2004)

ABSTRACT

A review of the cumulus parameterization problem is presented with an emphasis on its conceptual aspects covering the history of the underlying ideas, major problems existing at present, and possible directions and approaches for future climate models. Since its introduction in the early 1960s, there have been decades of controversies in posing the cumulus parameterization problem. In this paper, it is suggested that confusion between budget and advection considerations is primarily responsible for the controversies. It is also pointed out that the performance of parameterization schemes can be better understood if one is not bound by their authors' justifications. The current trend in posing cumulus parameterization is away from deterministic diagnostic closures, including instantaneous adjustments, toward prognostic or nondeterministic closures, including relaxed and/or triggered adjustments. A number of questions need to be answered, however, for the merit of this trend to be fully utilized.

Major practical and conceptual problems in the conventional approach of cumulus parameterization, which include artificial separations of processes and scales, are then discussed. It is rather obvious that for future climate models the scope of the problem must be drastically expanded from ''cumulus parameterization'' to ''unified cloud parameterization,'' or even to ''unified model physics.'' This is an extremely challenging task, both intellectually and computationally, and the use of multiple approaches is crucial even for a moderate success. ''Cloud-resolving convective parameterization'' or ''superparameterization'' is a promising new approach that can develop into a multiscale modeling framework (MMF). It is emphasized that the use of such a framework can unify our currently diversified modeling efforts and make verification of climate models against observations much more constructive than it is now.

1. Introduction

The importance of clouds in the climate system cannot be overemphasized. While there is no doubt that solar insolation is the principal energy source for the climate system, it influences the atmospheric component of the system rather indirectly. This is partly due to the coupling of the atmosphere with oceans and partly due to the existence of clouds in the atmosphere. As illustrated in Fig. 1, clouds and their associated physical processes strongly influence the climate system in the following ways (Arakawa 1975):

- by coupling dynamical and hydrological processes in the atmosphere through the heat of condensation and evaporation and through redistributions of sensible and latent heat and momentum;
- by coupling radiative and dynamical–hydrological processes in the atmosphere through the reflection, absorption, and emission of radiation;
- by influencing hydrological process in the ground through precipitation; and
- by influencing the couplings between the atmosphere and oceans (or ground) through modifications of radiation and planetary boundary layer (PBL) processes.

(Here ''dynamical'' and ''hydrological'' processes refer to those due to synoptic-scale circulations of the atmosphere.)

It is important to note that most of these interactions are two-way interactions. For example, condensation heating in clouds is strongly coupled with motion so that heating is a result of motion as well as a cause of motion. Thus, although the heat of condensation is a dominant component of the atmosphere's sensible heat budget, it is not correct to say that atmospheric motions are externally ''forced'' by the heat of condensation (see Emanuel et al. 1994).

Cumulus convection plays a central role in most of the interactions shown in Fig. 1, and the representation of cumulus convection, generally called cumulus parameterization, has almost always been at the core of our efforts to numerically model the atmosphere. In spite of the accumulated experience over the past decades, however, cumulus parameterization is still a very young subject. Besides the basic question of how to pose the problem, there are a number of uncertainties in modeling clouds and their associated processes such as those

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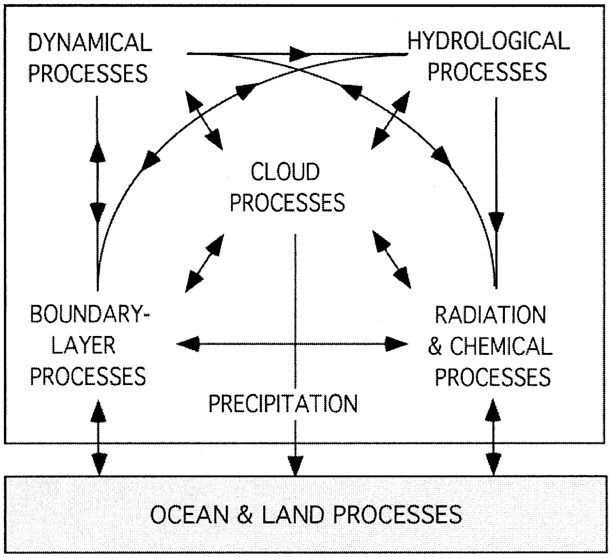

FIG. 1. Interactions between various processes in the climate system.

shown in Fig. 2. Even more seriously, we do not have a sufficiently general framework, such as a unified cloud system model, for implementing detailed formulations of these processes for the purpose of parameterization. This is a practical difficulty but it involves conceptual problems as I discuss in section 6. Improving this situation is a central task of the current phase (1990~) in the history of numerical modeling of the atmosphere, which Arakawa (2000a) called the great-challenge third phase.

During the previous phase (1960~1990), which Arakawa (2000a) called the magnificent second phase, many cumulus parameterization schemes were constructed. In spite of quite different reasoning behind them, however, GCMs with these schemes can produce comparable climatologies when the geographical distribution of sea surface temperature (SST) is prescribed. A number of papers have been published that compared GCM results with different cumulus parameterizations. While these papers usually show considerable sensitivities of model results to parameterization schemes, especially for the variability of the tropical atmosphere (e.g., Slingo et al. 1994), the existence of these papers by itself indicates that the results are ''comparable'' to start with as far as the mean fields are concerned.

As I discuss later in sections 4 and 5, all surviving cumulus parameterizations can produce a negative feedback to large-scale destabilization, which we call adjustment, in one way or another. When SST is fixed, the model is subject to another negative feedback by adjusting the air temperature near the surface to the prescribed values through the surface heat flux. As Arakawa (2000b) emphasized, these negative feedbacks combined together tend to hide model deficiencies and model differences in simulating climate. Due to this apparent insensitivity to the basic reasoning used, the scientifically very demanding nature of the cumulus parameterization problem has often been forgotten. Consequently, a large part of our modeling effort in the past has been spent on the engineering aspect rather than the scientific aspect of the problem.

It is clear that we cannot rely on this kind of ''luck'' in developing coupled atmosphere–ocean GCMs and certainly not in developing future climate models. In the climate system, SST is a product of the complicated interactions shown in Fig. 1. We then have no other choice than realistically simulating all of these interactions as far as the most advanced climate models are concerned. Accordingly, the objectives of cumulus parameterization must be drastically expanded. I classify the objectives into two categories: classical objectives and nonclassical objectives. The quantities to be determined under these objectives are listed below.

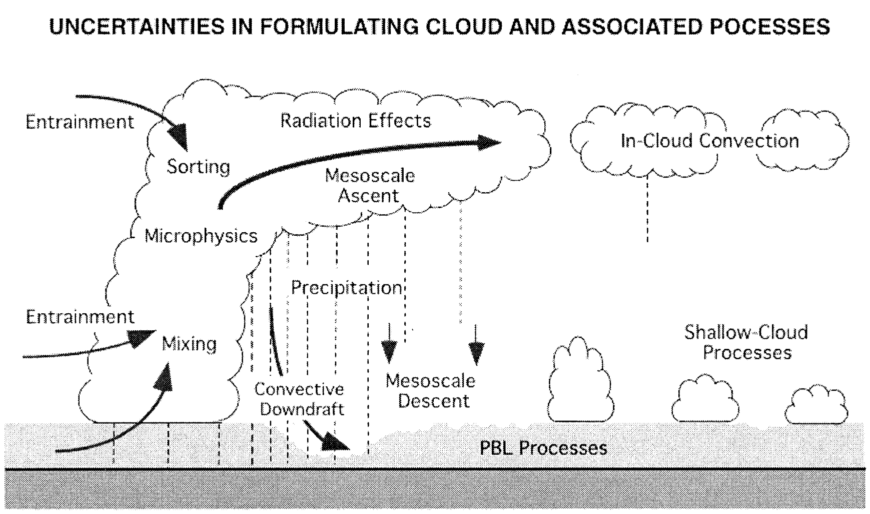

FIG. 2. Cloud and associated processes for which major uncertainties in formulation exist.

a. Classical objectives

1) Vertically integrated cumulus heating

Finding cumulus heating vertically integrated with respect to mass remains the most basic one to determine the existence and overall intensity of cumulus activity under given synoptic-scale conditions. It is also closely related to surface precipitation due to cumulus convection, which is one of the most important outputs of weather and climate models.

2) Vertical distributions of cumulus heating (cooling) and drying (moistening)

These are the quantities the dynamics core of multilevel atmospheric models demands that the cumulus parameterization subprogram provide at each time step of implementing physics to arrive at a closed system for prediction. When our concern is limited to the interaction with synoptic-scale dynamical processes shown near the upper-left corner in Fig. 1, the heating (cooling) determined by cumulus parameterization provides nearly sufficient information about the cumulus effect. When properly tuned, most of the existing schemes do more or less acceptable jobs in this aspect. Determination of drying (moistening) by cumulus parameterization, which is necessary to predict the interaction with synoptic-scale hydrological processes shown near the upper-right corner in Fig. 1, is a different story partly because the interaction involves processes described in objectives b1and b2 below, which I classify into nonclassical objectives.

b. Nonclassical objectives

1) Mass transport by cumulus convection

To predict redistribution of tracers in the atmosphere, cumulus parameterization must be able to realistically determine mass transport due to cumulus convection. This objective involves the determination of mass flux and mass detrainment associated with different types and organizations of cumulus updrafts and downdrafts. Achieving this objective, which is much more demanding than meeting the classical objectives alone, is necessary for implementing cloud microphysics, as I emphasize in objective b2 below, and for implementing atmospheric chemistry, which is becoming an important issue in climate modeling.

2) Generation of liquid and ice phases of water

Obviously, inclusion of realistic cloud microphysics is crucial for this objective. Objective b1 is an important prerequisite for this, however. What we need is microphysics in a dynamically active system. Especially when we have this objective in mind, there is not much point in distinguishing cumuliform and stratiform clouds. What we then need is a unified *cloud* parameterization instead of a *cumulus* parameterization.

3) Interactions with PBL

Objective b1 is again an important prerequisite for this objective in view of the mass exchange between the cloud layer and PBL through updrafts and downdrafts. At least in the Tropics, PBL processes are known to be crucial in regulating moist convection above (e.g., see Raymond 1995). In addition, formulations of the effects of vertical and horizontal inhomogeneities in the PBL on cumulus clouds are included in this objective. Over land, PBL processes associated with diurnal change must also be considered in determining convective activities above. Furthermore, in many situations a layer of shallow clouds can be regarded as an extension of convectively active PBL. To achieve this objective, therefore, the modeling of cloud and PBL processes should be unified.

4) Interactions with radiation

In most conventional models, radiation and clouds interact only through grid-scale prognostic variables. Naturally, what we should do is couple radiation and cloud processes on the cloud scale, which is subgrid scale in most climate models. Then separate modeling of radiation and cloud processes become almost meaningless for many cloud types including the cumulonimbus-anvil and shallow clouds. The effect of unresolved local circulations driven by cloud-induced radiation must also be considered in the *net* effect of cloud–radiation interaction on the large-scale fields. Needless to say, b2 is an important prerequiste for b4 to determine the optical properties of cloud. For shallow clouds over land, b3 is important also from the point of view of b4.

5) Mechanical interactions with mean flow

In spite of the contributions to this problem by various authors (e.g., Moncrieff 1981, 1992, 1997; Wu and Yanai 1994), I list this item with the nonclassical objectives because there are still a number of difficulties in parameterizing the effects of momentum transports. The difficulties arise mainly because the momentum is not a conservative property and even the direction of the transport depends on the degree and geometry of the cloud organization. The problem then becomes that of predicting regimes of cloud organization and their transitions.

6) Inclusion of nondeterministic effects

Most existing parameterization schemes attempt to formulate cumulus effects deterministically. Because of the statistical nature of the parameterization problem, it is rather obvious that we should introduce stochastic effects at some point in the future development of parameterizations. For further discussion of this problem, see section 5.

When all of these objectives are met, we will have unified almost the entire model physics, as indicated by a single box in the atmospheric part of Fig. 1. Naturally, this is too ambitious a task to be called parameterization. Only after successful completion of this task, however, can the atmospheric sciences become a truly unified science.

It should be emphasized here that the need for parameterizations is not limited to ''numerical'' models. Formulating the statistical behavior of small-scale processes is needed for understanding large-scale phenomena regardless of weather we use numerical, theoretical, or conceptual models. Even under a hypothetical situation in which we have a model that resolves all scales, it alone does not automatically give us an *understanding* of scale interactions. Understanding inevitably requires simplifications, including various levels of ''parameterizations,'' either explicitly or implicitly, which are quantitative statements on the statistical behavior of the processes involved. Parameterizations thus have their own scientific merits.

In the title of this paper the word cumulus is used only for convenience in the ''past'' and ''present'' parts of the paper. One of the main points of this review is to recognize that the scope of the problem should be drastically expanded for the ''future'' from the cumulus parameterization problem to the unified cloud parameterization problem or even to the unified model physics problem.

Throughout the entire paper, emphasis is placed on the conceptual and logical aspects of the problem. Section 2 of the paper presents a definition of the cumulus parameterization problem and some basic considerations on its nature. The material of the rest of the paper is organized roughly in a historical order: sections 3 and 4 for the past with the classical objectives in mind, section 5 for the present, and section 6 for the future with the nonclassical objectives in mind. Section 7 gives a summary and conclusions.

2. The nature of the cumulus parameterization problem

This section first presents a definition of the cumulus parameterization problem. I have tried to make the definition as general as possible without losing the essence of what we usually mean by the words cumulus parameterization. The logical structure of the problem is then discussed, including three conditions that should be satisfied by the principal closure of a parameterization scheme. Finally, it is pointed out that, while budget considerations are useful for many purposes, they can easily mislead our judgment of cause and effect. This point is illustrated using the equations for the boundary layer humidity.

a. Definition of the cumulus parameterization problem

It is not easy to rigorously define the cumulus parameterization problem because the problem has not become a mature scientific subject yet. Quite generally, however, the cumulus parameterization problem may be defined as *the problem of formulating the statistical effects of moist convection to obtain a closed system for predicting weather and climate.* In this definition, I have avoided to use the words large-scale processes and small-scale processes. This is because those words are not well defined when there is no gap in the spectrum of atmospheric processes, which is usually the case. The need for cumulus parameterization is independent of the existence of such a gap, although without it parameterizability and, therefore, predictability of the atmosphere in the deterministic sense should be seriously questioned. Also in this definition, I have avoided the words grid scale and subgrid scale because the concept of parameterization is not unique to a numerical model as I pointed out in section 1. This point is clearer in the theories of radiative and turbulent transfers, in which the physical problem of parameterization usually comes first and then discretization is introduced for the purpose of computation. This order is reversed when the words subgrid-scale parameterization are used. I have further avoided the word cumulus in this definition because moist convection that needs to be parameterized in a model is not necessarily typical cumulus convection.

In a discrete model, however, there is a clear a posteriori distinction between resolved processes for which the local and instantaneous effects are explicitly formulated and unresolved processes for which only the statistical effects can be implicitly considered. In this paper, I follow the tradition of using the words large-scale to mean the former and cumulus to mean moist convection in the latter sense. For related discussions on this distinction, see section 6d.

It is important to remember that the objective of cumulus parameterization is *to obtain a closed system for predicting weather and climate.* Figure 3a schematically shows the logical structure of cumulus parameterization. In the figure, the upper half of the loop represents the effects of the resolved processes on the unresolved component of the moist convection, while the lower half represents those of the latter on the former. In a discrete model, it is a common practice to let the grid size separate the resolved and unresolved processes. For the loop to be closed, it must include the right half, a formulation of which is precisely the problem of cumulus parameterization. We refer to the upper half of the loop as the ''control'' or ''large-scale forcing'' and the lower half as the ''feedback'' or ''cumulus adjustment,'' although they do not necessarily mean cause and effect.

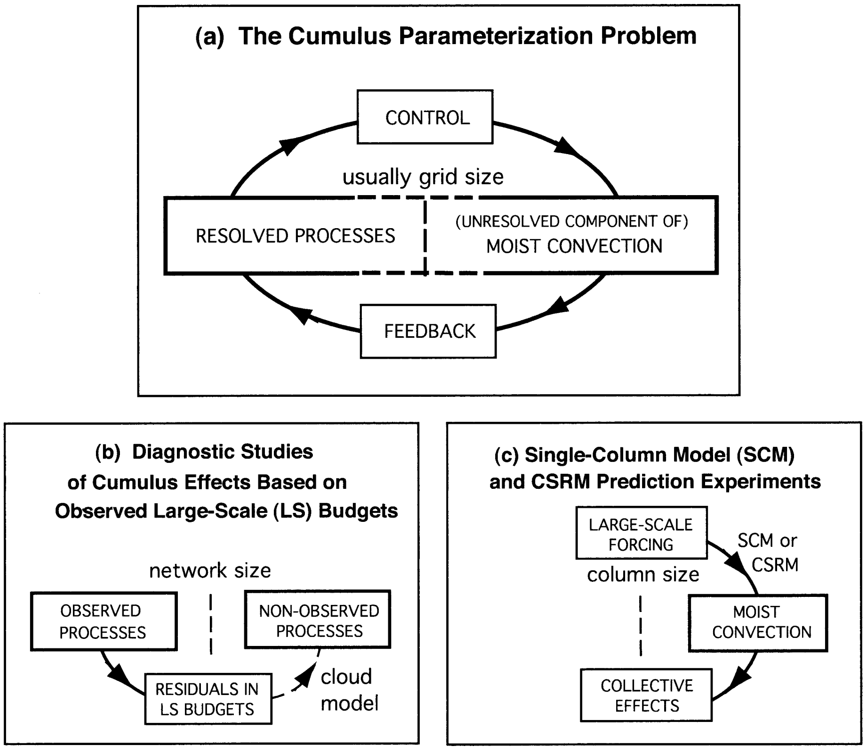

FIG. 3. (a) A schematic diagram showing interactions between resolved processes in a model and (the unresolved component of) moist convection. The formulation of the right half of the loop represents the cumulus parameterization problem. (b) A schematic diagram showing the logical structure of diagnostic studies of cumulus activity based on observed large-scale budgets. (c) Same as in (b) except for studies using SCMs or CSRMs.

These terminologies are convenient, however, when we are concentrating on the parameterization problem represented by the right half of the loop.

It should be recognized that the logical structure of the cumulus parameterization problem illustrated in Fig. 3a is crucially different from those of other related studies illustrated in Figs. 3b and 3c. In diagnostic studies of cumulus effects based on observed large-scale budgets (see, e.g., Yanai and Johnson 1993), the size of the observation network separates observed and nonobserved processes. The effects of the latter processes are then estimated as the residuals in the large-scale budgets, following the lower-left segment of the loop in the reverse direction (Fig. 3b). Some bulk features of nonobserved moist-convective processes can further be inferred using a cloud model (e.g., Yanai et al. 1973). This is again in the reverse direction and, therefore, finding a cause-and-effect relationship should not be an issue in such a study. In spite of this, or because of this, the results are useful for inferring what the effects of cumulus convection have been in the real atmosphere. In a single-column prediction experiments, either a single-column model (SCM) with cumulus parameterization or a cloud system resolving model (CSRM; sometimes called a cloud ensemble model, CEM, or simply a cloud-resolving model, CRM) is applied to a single column covering a horizontal area comparable to the usual grid size of weather prediction and climate models (for a review, see Randall et al. 1996; Somerville 2000; Randall et al. 2003a). In these experiments, the horizontal size of the column separates two processes: large-scale forcing prescribed from observations and moist-convective processes predicted by the SCM or CSRM. The experiments then follow the right half of the loop without closing the rest (Fig. 3c). While this approach has the merit of separating cumulus parameterization from the rest of the modeling problems, it is often difficult to interpret the results of these experiments, as pointed out by Hack and Pedretti (2000). Unfortunately, similar difficulties exist in any offline applications of cumulus parameterization schemes.

In the cumulus parameterization problem, we are concerned with the statistical behavior of cumulus clouds under different conditions. The problem of cumulus parameterization, therefore, is analogous to that of climate dynamics, in which we are concerned with time and space means, forced and free fluctuations around means, interactions between different temporal and spatial scales, etc. All of these have their counterparts in the cumulus parameterization problem.

b. Closure assumptions

Since cumulus parameterization is an attempt to formulate the statistical effects of cumulus convection without predicting individual clouds, it is a closure problem in which we seek a limited number of equations that govern the statistics of a system with huge dimensions. Therefore, the core of the cumulus parameterization problem, as distinguished from the dynamics and thermodynamics of individual clouds, is in the choice of appropriate closure assumptions.

With the classical objectives of the cumulus parameterization problem in mind, Arakawa and Chen (1987) and Arakawa (1993) (see also Xu 1994) identified two types of closure assumptions: type I and type II closures. Almost parallel to that classification, I use the following terminology and definitions in this paper, again with the classical objectives in mind:

- *principal closure*—a hypothesis that links the existence and overall intensity of cumulus activity (e.g., cloud-base mass flux) to large-scale processes; this kind of closure is necessary even for the most basic objective of cumulus parameterization listed as a1 in section 1; and
- *supplementary closures*—constraints on cloud properties, especially on their vertical structures, by large-scale conditions through simplified cloud models or empirical results without referring to the overall intensity of cumulus activity; this kind of closure is necessary for the classical objective a2 and for all nonclassical objectives listed in section 1; generally, more supplementary closures are needed as the scope of the cumulus parameterization expands.

These two kinds of closures are consequences of the ‘‘dynamic control’’ and ‘‘static control’’ (Schubert 1974) of cumulus activity, respectively, by the cloud environment. Although these two types of closures are not completely separable in practice, distinguishing them is useful when analyzing the logical structure of a parameterization scheme.

For a parameterization scheme to be rational, well posed, and physically reasonable, its principal closure should satisfy the following requirements:

1) *It should be based on clearly identifiable hypotheses on parameterizability.* This is important because why and to what extent cumulus convection is parameterizable is by no means obvious. The principal closure of a parameterization represents the author's own understanding of parameterizability. Hypotheses used in the closure should be made clearly identifiable so that the logical basis of the scheme is clear.
2) *It should not lose the predictability of large-scale fields.* If any assumed or observed balance in the large-scale budget equations is used as a closure, the parameterization is ill-posed because these equations are the prognostic equations of the model. Obviously, we cannot use the same equation twice, one for formulating a parameterization based on a balance and the other for a prediction based on an imbalance. If we wish to assume that a certain variable is in equilibrium, the variable should be one whose prediction is not intended by the model.
3) *It should be based on the concept of buoyancy.* This is a physically reasonable requirement since cumulus convection is buoyant convection, which recognizes its environment primarily through the buoyancy force.

c. The flux and advective forms of the advection equation

The requirements listed above are not always satisfied in parameterization schemes, causing unnecessary controversies over the justification of those schemes. As we discuss in this section, these controversies are also due to confusion between the budget consideration based on the flux form of the advection equation and the advection consideration based on the original advective form of the equation.

Here I am not going to discuss these two forms from the point of view of numerical modeling. Instead, I point out that the choice between these two forms of considerations in the basic reasoning can crucially influence one's formulation of principal closure. I particularly emphasize that, while budget considerations based on the flux form are useful for many purposes, they can easily mislead our judgment of cause and effect. This is because the flux form does not predict an intensive quantity (e.g., specific humidity, specific entropy, etc.), hiding even the simplest fact that advection never creates a new maximum or minimum of that quantity. I illustrate this point using the equations for the boundary layer specific humidity, q_M. For simplicity, I assume that it is vertically well mixed inside the boundary layer. The advective and flux forms of the equations for q_M may be written as follows:

advective form,

$$\frac{\partial}{\partial t}q_M = -\mathbf{v}_M \cdot \boldsymbol{\nabla} q_M + \frac{1}{\rho_0 h}[(Fq)_S + \cdots], \quad (1)$$

and

flux form,

$$\frac{\partial}{\partial t}[\rho_0 h q_M] = -\boldsymbol{\nabla} \cdot [\rho_0 h \mathbf{v}_M q_M] + (Fq)_S + \cdots, \quad (2)$$

respectively, where h is the depth of the boundary layer, $\mathbf{v}_M$ is the vertically averaged horizontal velocity in the boundary layer, and $(F_q)_S$ is the surface flux of the specific humidity, respectively. Here fluxes through the top of the boundary layer such as the entrainment flux are not explicitly written. The moisture convergence term on the right-hand side of (2) can be split into two terms as

$$-\nabla \cdot [\rho_0 h \mathbf{v}_M q_M] = -q_M \nabla \cdot [\rho_0 h \mathbf{v}_M] - \rho_0 h \mathbf{v}_M \cdot \nabla q_M. \tag{3}$$

As is clear from (1), it is the second term on the right-hand side of (3) that contributes to $\partial q_M/\partial t$, not the first term representing the mass convergence. When the first term is more dominant than the second term, as is often the case, *moisture convergence as a component of the moisture budget gives no information about the change of the specific humidity.*

There is little question that, at least for tropical cumulus clouds, the boundary layer humidity is crucially important in determining the cloud buoyancy above. Then, to satisfy requirement 3 given in section 2b, its effect should be considered in formulating a principal closure. Here what matters is the specific humidity as an intensive quantity governed by the advection equation (1), not the total amount of moisture in the boundary layer governed by the budget equation (2). To see the difference between the two quantities, consider a parcel of moist air. When a mass of drier air is added to this parcel, the specific humidity of the parcel decreases after mixing while the total amount of moisture increases. As in this example, budget consideration alone can mislead our judgment of cause and effect. This point is also important for understanding the role of surface evaporation in hurricanes as Malkus and Riehl (1960) put it, ''the oceanic source of water vapor, apparently so vital for the very existence of the storm, makes a negligible contribution to its water budget.''

3. Early views and controversies

From here I roughly follow the historical order, beginning with a review of the early views and controversies surrounding the cumulus parameterization problem. Although the main thrust of this paper is the problem for climate models, it is not so different from the problem for weather prediction models when we have a GCM-type climate model in mind. In this section, an emphasis is placed on modeling tropical cyclone development, partly because the early history of the cumulus parameterization problem cannot be separated from that of tropical cyclone modeling and partly because tropical cyclone development is a subject that is convenient for discussing the basic concepts behind the cumulus parameterization problem.

a. Early views on cumulus parameterization and two schools of thought

The idea of cumulus parameterization was born in the early 1960s near the beginning of the ''second phase'' (Arakawa 2000a) of numerical modeling of the atmosphere. It was introduced by Charney and Eliassen (1964, hereafter CE64) and Ooyama (1964, hereafter O64) in tropical cyclone modeling and by Manabe et al. (1965, hereafter M65) in general circulation modeling. CE64 and M65 said,

> Since a self-consistent theory of turbulent cumulus convection in an anisotropic mean field does not exist, one is forced to parameterize the process. . . . (CE64), we used a simple convective adjustment of temperature and water vapor as a substitute for the actual convective process. (M65).

Thus, they regarded parameterization as a substitute to theory or actuality. O64, on the other hand, took a more positive attitude to the problem, saying

> it is hypothesized that the statistical distribution and mean intensity of the cloud convection are controlled by the large-scale convergence of the warm and moist air in a surface layer. . . .

In both CE64 and O64, either vertically integrated cyclone-scale moisture convergence (CE64) or cyclone-scale convergence in the boundary layer (O64) determines the cumulus heating. In M65, on the other hand, parameterization is achieved through the adjustment of the vertical profiles of temperature and humidity to neutral and saturated profiles whenever they tend to become unstable and supersaturated. Although the concept of moist-convective adjustment was not necessarily new (see, e.g., Smagorinsky 1956; Mintz 1958), M65 represents the first application of the concept to a moist numerical model of the atmosphere. For further discussions of M65, see section 4a. The logical difference between the CE64 and O64 schemes and the M65 scheme led to two schools of thought in posing the cumulus parameterization problem, the ''convergence school'' and the ''adjustment school.'' In the loop shown in Fig. 3, the convergence school recognizes *synoptic-scale convergence* as ''control'' and energy supply due to heat of condensation released by cumulus convection as ''feedback.'' The Adjustment School, on the other hand, recognizes stabilization through an adjustment of the thermodynamic vertical profiles as feedback. Then control is any large-scale process that either destabilizes the profile or triggers the adjustment. Further discussion of these two schools of thought will be given in the rest of this section and in sections 4 and 5.

b. The original CISK papers

Until the early 1960s, tropical cyclone development was a big puzzle in meteorology, due to the failure in theoretically explaining and numerically simulating the growth of cyclone-scale vortices in a conditionally unstable atmosphere (see Kasahara 2000 for a review). Attempting to break this puzzle, CE64 and O64 proposed an idea that came to be known as ''conditional instability of the second kind (CISK).'' These two papers are similar to each other because both are linear theories based on the hypothesis that tropical cyclone development is due to the *cooperation* between cu-

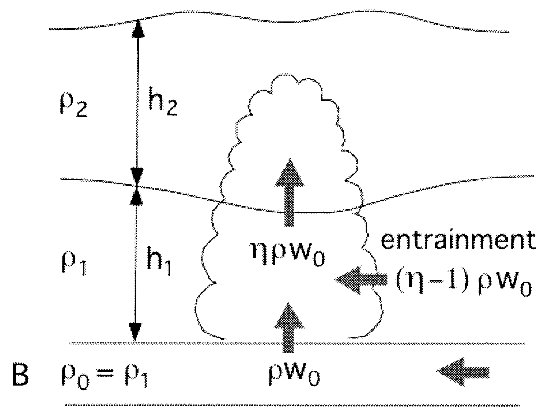

FIG. 4. Structure of the model used by O69 for simulations of tropical cyclone development.

mulus- and cyclone-scale motions, through cyclone-scale frictionally induced boundary layer convergence on one hand and energy supply by cumulus heating on the other hand. For further discussion of the CISK concept, see section 3e.

c. Simulation of tropical cyclone development by Ooyama (1969)

In this section, I discuss Ooyama's subsequent work on tropical cyclone development (Ooyama 1969, hereafter O69). The cloud model used in O69 consists of a boundary layer and two incompressible homogeneous layers above it (Fig. 4). The prognostic variables of the model are the thickness of the upper two layers, h_1 and h_2, and the boundary layer equivalent potential temperature, $(\theta_e)_B$. The azimuthal and radial components of velocity are diagnostically determined from h_1 and h_2 assuming balances of forces. Let w_0 be the (cyclone scale) vertical velocity at the boundary layer top. Clouds are assumed to exist only where $w_0 > 0$, that is, only where the boundary layer flow is convergent. The cloud mass flux from the boundary layer to the lower layer above is assumed to be equal to the cyclone-scale vertical mass flux, ρw_0, and the cloud mass flux from the lower layer into the upper layer, which is analogous to heating, is assumed to be

$$Q = \eta \rho w_0, \quad \text{if } w_0 > 0, \tag{4}$$

where $\eta - 1 (>0)$ is the coefficient for the entrainment into clouds in the lower layer (see Fig. 4).

The budget of θ_e for clouds and the nonbuoyancy condition applied to the upper layer require that the entrainment coefficient for clouds reaching the upper layer be

$$\eta - 1 = \frac{(\theta_e)_B - (\theta_e^*)_2}{(\theta_e)_1 - (\theta_e^*)_2}. \tag{5}$$

Here $(\theta_e)_1$ is the equivalent potential temperature of layer 1, which is prescribed, and $(\theta_e^*)_2$ is the saturation equivalent potential temperature of layer 2, which is inferred from the difference of the predicted perturbation pressure between layers 1 and 2. In (5), the numerator $(\theta_e)_B - (\theta_e^*)_2$ is a measure of the moist-convective instability when positive. Then $\eta > 1$ means instability when the denominator $(\theta_e)_1 - (\theta_e^*)_2$ is positive as is usually the case. In the cloud model used in O64, which is similar to the model described above, η is a prescribed constant. In O69, on the other hand, η is time dependent through the time dependencies of $(\theta_e)_B$ and $(\theta_e^*)_2$.

It is generally recognized that O69 presented the first successful simulations of tropical cyclone development. The initial condition for those simulations is a weak axisymmetric cyclonic vortex with $\eta = 2$ everywhere. Figure 5 shows the results for selected variables from one of the simulations. The top panel in Fig. 5 shows the time evolution of the radial profiles of υ_1, which is the azimuthal component of the velocity in the lower layer. The middle panel shows that of χ_0, which is the perturbation of $(\theta_e)_B$. These panels clearly show intensification of the cyclonic circulation and establishment of a warm core near the cyclone center. The bottom panel shows the time evolution of the η profiles, which decrease from the initial unstable value of 2 toward the neutral value of 1. This decrease of η demonstrates that, although the O69 scheme as originally presented belongs to the convergence school, it can be viewed as a scheme that belongs to the adjustment school as well because η is effectively predicted in these simulations. The condition $w_0 > 0$, which plays an essential role in CISK, can now be viewed as a selective adjustment coefficient.

Experiments presented in O69 demonstrate that heat flux from the ocean is crucial for both the development and maintenance of the cyclone. From these experiments, O69 concluded, "the evaporation from a large area under the influence of a tropical cyclone is extremely important for supporting the convective activity in the central region of the cyclone." While everybody supports this statement, the specific role of the surface heat flux in tropical cyclone development is a matter of debate, as I discuss in the next two subsections.

d. Wind-induced surface heat exchange

Emanuel (1986) presented a new theory on tropical cyclone development, which he summarized as follows: "we advance the hypothesis that the intensification and maintenance of tropical cyclones depend *exclusively* on self-induced heat transfer from the ocean. . . . Cumulus convection is taken to redistribute heat acquired from the sea surface in such a way as to keep the environment. . .neutral. . .in a manner consistent with the quasi-equilibrium hypothesis of Arakawa and Schubert (1974)." These conjectures obviously deviate from the concept of CISK. In Emanuel's theory, called the wind-induced surface heat exchange (WISHE) theory, self-

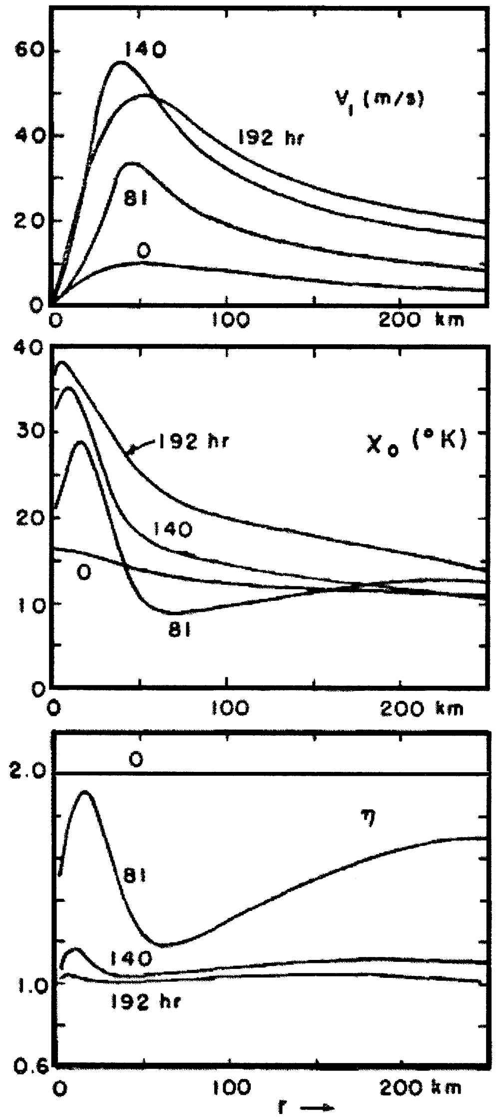

FIG. 5. Time evolution of radial profiles for the azimuthal component of velocity in (top) the lower layer, (middle) the perturbation of $(\theta_e)_B$, and (bottom) the parameter η from one of the simulations of tropical cyclone development. Taken from Fig. 13 of O69.

induced heat transfer from the surface, rather than cooperation between cyclone- and cumulus-scale circulations, is responsible for the tropical cyclone development. Conditional instability plays only a limited role as ''development of the vortex may. . . take place under nearly neutral conditions with each additional increment of latent heat put in by the ocean almost immediately redistributed upward in cumulus clouds'' (Rotunno and Emanuel 1987).

To support the theory, Rotunno and Emanuel (1987, hereafter RE87) used an axisymmetric cloud resolving model to simulate tropical cyclone development starting from a conditionally neutral initial condition. In the context of this review, the highlight of their results is the generation of a warm core near the cyclone center characterized by large values of θ_e. A warm core is also generated in O69s experiments (see the middle panel in Fig. 5). Although a conditionally unstable initial condition ($\eta = 2$) is used in Ooyama's experiments, it does not seem to matter so much for the later stage of the development because η became close to 1 (see the bottom panel of Fig. 5). In fact, Dengler and Reeder (1997) showed that the development occurred with the O69 model even when the initial condition was neutral. This and other evidence indicates that the following statement by Emanuel (1986) is valid: ''the physics implied in this view are contained in virtually all contemporary numerical models which allow for convection and air–sea heat transfer.''

e. CISK controversies

Although O69 succeeded in realistically simulating tropical cyclone development, there have been heated debates on the assumptions used in the models and the interpretation of the results, and on the concept of CISK itself (Ooyama 1982, 1997; Emanuel 1989, 1991b, 1994, 2000; Emanuel et al. 1994, 1997; Craig and Gray 1996; Stevens et al. 1997; Smith 1997). Here I present my own view of the controversies, with the objective of dissipating them rather than amplifying them.

To maintain convective activity in the central region of a tropical cyclone, O69 correctly emphasized that the high values of boundary layer θ_e must be supported. O69 argued, however, ''In order to support such activity, it is apparently necessary for the boundary layer inflow to converge, so that convectively unstable air will be continuously supplied to the convective clouds.'' This statement can be criticized because convergence does not mean an increase of θ_e, as I pointed out in section 2c, and therefore it does not necessarily support instability. What happens is the opposite: convergence brings air with lower θ_e into the inner region.

In view of the above argument, we see that the source of θ_e due to surface heat fluxes is important for tropical cyclone development in two ways: one reduces a negative feedback and the other produces a positive feedback. The negative feedback occurs if there are no surface heat fluxes in the outer region. Without such fluxes, the boundary layer θ_e in the outer region will become lower due to stronger subsidence as the cyclone intensifies. The air with low θ_e will then be advected toward the cyclone center and moist-convective instability in the inner region will not be maintained. For development of the cyclone, it is necessary to reduce this negative feedback through surface heat fluxes in the outer region. The positive feedback, on the other hand, can occur within the inner region if surface heat fluxes there are enhanced as the cyclone intensifies, for example, through an intensification of surface wind as in the WISHE theory. Ooyama deserves credit for the former and Emanuel for the latter.

The more fundamental question about the CISK concept is how can cooperation between cyclone-scale and convective-scale circulations produce their simultaneous development including the formation and intensification of a warm core? It is difficult to see how it can happen because, if there are no sources, θ_e is simply redistributed by these motions individually, and therefore by the total motion, without creating a new maximum. Conditional instability simply converts the vertical variation of θ_e to the horizontal variation while the mass distribution in θ_e space is conserved. Any instability that changes this distribution, therefore, inevitably involves processes other than cooperation between cyclone-scale circulation and convective clouds. Since the cooperation alone does not produce new instability, the concept of CISK as distinguished from the usual conditional instability can hardly be justified.

In this section, I have reviewed the controversy on the role of cumulus convection in tropical cyclone development. I did this because the subject is also relevant for better understanding the general behavior of the tropical atmosphere as summarized in section 5a.

4. Selected prototype schemes

In this section I review the logical structures of three selected schemes, each of which can be considered as a prototype of a family of schemes being used in weather prediction and climate simulation models. They are the moist-convective adjustment scheme of Manabe et al. (1965), the scheme proposed by Kuo (1974), and the scheme outlined by Arakawa (1969), which is the predecessor of Arakawa and Schubert (1974). The scheme introduced by Betts and Miller (1986) will also be briefly discussed. As I pointed out in section 1, these schemes were developed with very different reasoning; yet they or their descendants often produce similar results in practical applications, especially in climate simulations with prescribed geographical distributions of SST. The motivation for the review presented in this section partially comes from this apparent insensitivity of model results to the basic reasoning used in constructing the schemes.

a. The moist-convective adjustment scheme

The moist-convective adjustment scheme proposed by Manabe et al. (1965, hereafter M65) is the earliest and perhaps simplest cumulus parameterization scheme among those being used in climate models. Let Γ be the temperature lapse rate, Γ_m be the moist-adiabatic temperature lapse rate, and RH be the relative humidity. In this scheme, moist convection is assumed to occur when and where the air is conditionally unstable ($\Gamma > \Gamma_m$) and supersaturated (RH $>$ 100%). Then the vertical profiles of temperature and humidity are adjusted to equilibrium profiles that are neutral ($\Gamma = \Gamma_m$) and saturated (RH $=$ 100%). No cloud model is needed in this scheme; the only remaining constraint on the adjustment is the conservation of vertically integrated moist static energy during the adjustment.

The scheme can easily be criticized because it requires *grid-scale* saturation for *subgrid-scale* moist convection, and convection is confined within the unstable layer without penetrating into the stable layer above. (The unstable layer, however, may expand vertically as a result of adjustment). Nevertheless, the scheme can be considered as a prototype for a number of adjustment schemes developed later (see section 5b for examples).

Following Arakawa and Chen (1987) and Arakawa (1993), I show that the M65 adjustment scheme can also be written in a form in which the adjustment is implicit. When the scheme is applied continuously in time, the air follows a sequence of moist-adiabatic saturated states after the initial adjustment, as long as large-scale effects tend to generate a conditionally unstable, supersaturated state. Thus,

$$\frac{\partial h^*}{\partial p} = 0 \quad \text{and} \quad h - h^* = 0 \tag{6}$$

for *all t* as long as

$$\frac{\partial}{\partial p}\left(\frac{\partial h^*}{\partial t}\right)_{\mathrm{LS}} > 0 \quad \text{and} \quad \left(\frac{\partial h}{\partial t}\right)_{\mathrm{LS}} > \left(\frac{\partial h^*}{\partial t}\right)_{\mathrm{LS}}. \tag{7}$$

Here, h is the moist static energy, $s + Lq$, s is the dry static energy $c_pT + gz$, q is the water vapor mixing ratio, h^* is the saturation moist static energy, $s + Lq^*$, q^* is the saturation value of q, and $(\partial/\partial t)_{\mathrm{LS}}$ denotes the time rate of change due to large-scale effects (large-scale advection plus parameterized physics). Other symbols are standard. In writing (6) and (7), I used the relation

$$\frac{\partial h^*}{\partial p} \gtreqqless 0 \quad \text{as } \Gamma \gtreqqless \Gamma_m. \tag{8}$$

Since the M65 scheme adjusts the profile of h modified by $(\partial h/\partial t)_{\mathrm{LS}}$ back to a constant-h profile while conserving the mass-weighted vertical mean of $(\partial h/\partial t)_{\mathrm{LS}}$, the *net* time rate of change of h becomes

$$\frac{\partial h}{\partial t} = \left\langle \left(\frac{\partial h}{\partial t}\right)_{\mathrm{LS}} \right\rangle \tag{9}$$

as long as (7) is satisfied, where $\langle\ \rangle$ denotes the vertical average with respect to mass over the layer to which the adjustment is applied. Using $\partial h^*/\partial t = \partial h/\partial t$ for saturated air and (9) in the approximate relations

$$\frac{\partial T}{\partial t} \approx \frac{1}{1 + \gamma}\frac{1}{c_p}\frac{\partial h^*}{\partial t} \quad \text{and} \quad \frac{\partial q}{\partial t} \approx \frac{\gamma}{1 + \gamma}\frac{1}{L}\frac{\partial h^*}{\partial t}, \tag{10}$$

we obtain

$$\frac{\partial T}{\partial t} \approx \frac{1}{1+\gamma}\frac{1}{c_p}\left\langle\left(\frac{\partial h}{\partial t}\right)_{\mathrm{LS}}\right\rangle, \quad \text{and} \tag{11}$$

$$\frac{\partial q}{\partial t} \approx \frac{\gamma}{1+\gamma}\frac{1}{L}\left\langle\left(\frac{\partial h}{\partial t}\right)_{\mathrm{LS}}\right\rangle. \tag{12}$$

Here $\gamma \equiv (L/c_p)(\partial q^*/\partial T)_p$. Thus, with this scheme, the vertical distributions of *net* warming and *net* moistening are completely modulated by the vertical profile of γ, which usually decreases with height.

Due to (6), the effect of vertical advection on $(\partial h/\partial t)_{\mathrm{LS}}$ vanishes so that

$$\left(\frac{\partial h}{\partial t}\right)_{\mathrm{LS}} = -\mathbf{v}\cdot\nabla h + (\text{source of } h), \tag{13}$$

where $\mathbf{v}$ is horizontal velocity. It should be noted that the horizontal advection term $-\mathbf{v}\cdot\nabla h$, rather than the convergence term $-h\nabla\cdot\mathbf{v}$, appears on the right-hand side of (13). If horizontal advection is small, (9) and (13) together show that $\partial h/\partial t$ at all levels depends only on the vertically integrated source of h, which is usually dominated by the surface heat flux. Thus, with M65, WISHE can occur while conditional instability (of any kind) cannot.

b. The Kuo scheme

A number of variations of the Kuo scheme have been used in GCMs and NWP models (see section 5b for examples). There are even different interpretations of what the original Kuo scheme is. Here I present the Kuo scheme based on my own understanding of Kuo (1974, hereafter K74). For other reviews of the K74 scheme, see Cotton and Anthes (1989, section 6.5.2), Raymond and Emanuel (1993), and Emanuel (1994 section 16.2).

Let M_t be the vertically integrated moisture supply given by

$$M_t \equiv -\nabla\cdot\frac{1}{g}\int_0^{p_s}\mathbf{v}q\,dp + (F_q)_S, \tag{14}$$

where p_S is the surface pressure, F_q is the vertical flux of q due to turbulence, and the subscript S denotes the earth's surface. As in CE64, the key to the K74 scheme is the use of M_t in the principal closure. The first step of this scheme toward objective a2 given in section 1 is the separation of M_t into the following two parts:

$$\begin{cases} bM_t, & \text{the moistening part,} \\ (1-b)M_t, & \text{the precipitating part,} \end{cases} \tag{15}$$

where b is a prescribed constant. Let $(\partial s/\partial t)_c$ be the cumulus heating (denoted by aQ_c/π in K74), whose vertical integral with respect to mass over the cloud layer is given by the precipitating part of M_t. The second step toward objective a2 is the assumptions that $(\partial s/\partial t)_c$ is nonzero only where $T_c - T > 0$ and its vertical distribution follows that of $(T_c - T)/\langle T_c - T\rangle$. Here the subscript c denotes values in clouds; $\langle\ \rangle$ denotes the vertical average with respect to p for $P_T < P < P_B$; and the subscripts T and B denote cloud top and cloud bottom, respectively. Then, when $M_t > 0$,

$$\left(\frac{\partial s}{\partial t}\right)_c = \frac{(T_c - T)}{\langle T_c - T\rangle}(1-b)\frac{g}{p_B - p_T}LM_t \quad \text{for } p_T < p < p_B. \tag{16}$$

For the moistening effect, on the other hand, no separation is made between the large-scale and cumulus effects [see (3.2b) of K74], and the *net* moistening effect is expressed as

$$L\frac{\partial q}{\partial t} = \frac{(q_c - q)}{\langle q_c - q\rangle}b\frac{g}{p_B - p_T}LM_t \quad \text{for } p_T < p < p_B, \tag{17}$$

where

$$L\frac{\partial q}{\partial t} \equiv L\left[\left(\frac{\partial q}{\partial t}\right)_{\mathrm{LS}} + \left(\frac{\partial q}{\partial t}\right)_C\right], \tag{18}$$

$(\partial/\partial t)_{\mathrm{LS}}$ is the time rate of change due to large-scale effects as defined earlier, and $(\partial q/\partial t)_c$ is cumulus moistening.

One of the peculiar features of the K74 scheme is the lack of parallelism between the left-hand sides of (16) and (17). In (16), the cumulus effect is parameterized while in (17) the net effect including advection effects is parameterized. This is done intentionally [see the comments after (3.13) of Kuo (1965)], and it is philosophically consistent with the use of the moisture supply as a forcing. It is even energetically consistent in the sense that $(\partial s/\partial t)_c$ and $(\partial q/\partial t)_c$ given by (16) and (17) satisfy

$$\int_0^{p_s}\left[\left(\frac{\partial s}{\partial t}\right)_C + L\left(\frac{\partial q}{\partial t}\right)_C\right]dp = 0. \tag{19}$$

Of course, this consistency alone does not justify (16) and (17). Instead, it shows that the lack of the parallelism between the thermodynamic and moisture equations and the use of M_t as the forcing are closely tied to each other, indicating that the latter is also peculiar.

One way of removing this peculiarity is to modify the left-hand side of (16) to a form parallel to the left-hand side of (17) so that the net effect is also parameterized for temperature. [Another way of removing the peculiarity is modifying (17). This will be described toward the end of this section.] Then, to satisfy the energetic consistency (19), LM_t on the right-hand sides of (16) and (17) must be replaced by the vertically integrated supply of moist static energy, h. As a result, the scheme becomes somewhat analogous to the reinterpreted version of the M65 scheme given by (11) and (12) in the sense that $\langle(\partial h/\partial t)_{\mathrm{LS}}\rangle$ represents the forcing. There are two important differences, however. First, with the modified Kuo scheme the vertical average is over the layer in which $T_c - T > 0$ while in (11) and (12) it is over the layer in which $\Gamma - \Gamma_m > 0$. Generally, the former is deeper than the latter due to the penetration of convection into a stable layer above. Second, the expression (13) does not generally hold since $\partial h/\partial p$ is not necessarily close to zero with this scheme.

The K74 scheme has been severely criticized by Emanuel and Raymond (1992), Raymond and Emanuel (1993), Emanuel (1994), and Emanuel et al. (1994). Primarily based on the arguments presented in section 2c, I agree with most of their criticisms. We should not ignore, however, the fact that the K74 scheme can produce acceptable results in many practical applications. Here is another example showing that the performance of a parameterization scheme may be better understood by deviating from its author's own reasoning for the scheme.

The key to understand the performance of the K74 scheme is that the scheme can also be viewed as an adjustment scheme, as I did in section 3c for Ooyama's scheme. This can be done by interpreting M_t on the right-hand sides of (16) and (17) as a part of the coefficient for adjustments of T toward T_c and q toward q_c. Then, the K74 scheme can be considered as a member of a broad family of adjustment schemes with a finite adjustment time scale. The adjustments of temperature and moisture profiles are, however, performed in a nonparallel way. Furthermore, the condition $M_t >$ 0 for the adjustment can be disputed and the justification for choosing the variable adjustment coefficient can be questioned. In practice, the condition effectively excludes shallow clouds. This is why a Kuo-type scheme is usually supplemented by another scheme for shallow clouds, as in K74 itself and Tiedtke (1989). Also, it is likely that the general magnitude of the variable adjustment coefficient is too small. Then, unlike in the M65 scheme, a significant amount of conditional instability can remain after the adjustment even when a neutral sounding is chosen for T_c.

As I pointed out earlier, the nonparallel expressions on the left-hand sides of (16) and (17) and the use of M_t in the scheme are closely tied to each other. If we modify the left-hand side of (17) to a form parallel to the left-hand side of (16) so that the cumulus effects are parameterized for both temperature and moisture, the energetic consistency (19) can be more freely satisfied without involving M_t in the right-hand sides of (16) and (17). Then the scheme becomes closer to standard adjustment schemes such as the Betts–Miller scheme (Betts and Miller 1986; see also Betts 1986; Betts and Miller 1993).

c. Arakawa's (1969) parameterization

In many aspects, the parameterization outlined in Arakawa (1969, hereafter A69; see also Arakawa 1997, 2000a; Schubert 2000; Kasahara 2000) can be considered as the predecessor of the more sophisticated Arakawa–Schubert parameterization (Arakawa and Schubert 1974; Lord and Arakawa 1980; Lord 1982; Lord et al. 1982; Cheng and Arakawa 1997; see also reviews by Tiedtke 1988, section 4.4; Cotton and Anthes 1989, section 6.5.3; Arakawa and Cheng 1993; Emanuel 1994, section 16.4.1; Randall et al. 1997b, Schubert 2000). The A69 parameterization is designed for a three-layer GCM, in which the lowest layer represents the boundary layer (see Fig. 6). The cloud model used in this parameterization is similar to that used in Ooyama's model (see Fig. 4) but with two important differences. First, due to the use of the Lorenz vertical grid in the GCM, the model has two degrees of freedom in the vertical thermal structure above the boundary layer, while Ooyama's model has only one effective temperature. Having the two degrees of freedom led me to separate the compensating subsidence and detrainment effects on the environment. Second, the cloud-base mass flux is treated as unknown so that it is generally different from the large-scale vertical mass flux at that level (ρw_0 in Ooyama's model).

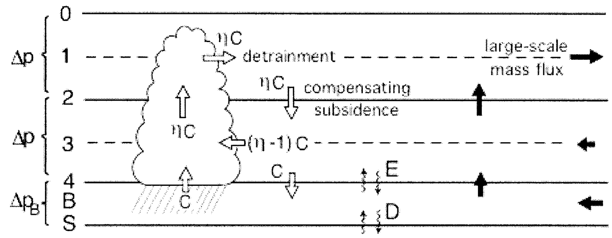

FIG. 6. One of the three cloud types considered in Arakawa's (1969) parameterization for a three-level model. Solid and open arrows show large-scale and superposed cumulus-induced mass fluxes, respectively.

One of the three cloud types considered in A69 is shown in Fig. 6. In the figure, the solid and open arrows show the large-scale mass flux and mass flux associated with cumulus convection, respectively. As in Ooyama's model [see (5)], the entrainment factor $\eta - 1$ is determined by the nonbuoyancy condition applied to the cloud top. Mass budgets then determine the detraining mass flux at cloud top and the compensating subsidence mass flux in the environment *per unit cumulus mass flux at cloud base* (denoted by C in the figure). Considering the budgets of s and q for each layer of the cloud environment and the boundary layer, A69 expresses $(\partial s/\partial t)_c$ and $(\partial q/\partial t)_c$, and therefore $(\partial h/\partial t)_c$ and $(\partial h^*/\partial t)_c$, for each layer in terms of C. The parameterization will then be closed if a principal closure is introduced to determine C.

To this end, A69 first considers the necessary condition for the existence of cumulus activity. For the cloud type shown in Fig. 6 to be at least neutrally buoyant at levels 3 and 1, it can be shown that $h_B \geq (h_1^*, h_3^*)$ is necessary. If $h_1^* > h_3^*$, as is usually the case, the necessary condition becomes

$$A = h_B - h_1^* \geq 0. \tag{20}$$

The quantity A defined here is a measure of moist-convective instability for this cloud type, analogous to the cloud work function defined by Arakawa and Schubert (1974). Using $(\partial h_B/\partial t)_c$ and $(\partial h_1^* > \partial t)_c$ expressed in terms of C, we may write, symbolically,

$$\frac{\partial A}{\partial t} = -SC + F. \tag{21}$$

Here the coefficient S on C is a combined discrete measure of $-\partial h/\partial z$ and $\partial h^*/\partial z$, the actual expression of which depends on how the vertical advection terms are finite-differenced in the model. When S is positive, which is usually the case, the SC term tends to decrease A as long as $C > 0$, representing an adjustment of the environment toward $A = 0$. The term F is the large-scale forcing for this cloud type defined by

$$F \equiv \left(\frac{\partial A}{\partial t}\right)_{\mathrm{LS}} = \left(\frac{\partial h_B}{\partial t}\right)_{\mathrm{LS}} - \left(\frac{\partial h_1^*}{\partial t}\right)_{\mathrm{LS}}. \tag{22}$$

When $F > 0$, large-scale processes tend to increase A. This can be through $(\partial h_B/\partial t)_{\mathrm{LS}} > 0$ due to upward surface heat flux or due to boundary layer horizontal advection from a region of higher moist static energy. It can also be through $(\partial h_1^*/\partial t)_{\mathrm{LS}} < 0$ due to radiative cooling, adiabatic cooling as a result of rising motion, or horizontal cold-air advection at the upper level.

The principal closure of this scheme is that of an adjustment scheme for cumulus effects, in which the cumulus term in (21) is written as

$$SC \approx \frac{A}{\tau_{\mathrm{ADJ}}}, \quad \text{if } A > 0, \tag{23}$$

where τ_{ADJ} is the time scale for the adjustment. When τ_{ADJ} is properly chosen, the cloud-base mass flux C can be obtained from (23) for a given unstable profile characterized by $A > 0$.

In practical applications of this scheme, a small value of τ_{ADJ} (1 h or less) is chosen. A69 argues that, if τ_{ADJ} is small, C obtained from (23) is virtually independent of τ_{ADJ} because the numerator of (23) is also the order of τ_{ADJ}. Following this argument, we see that the solution of this scheme with small τ_{ADJ} can be approximated by the solution with instantaneous adjustment and, therefore, by the equilibrium solution of (21) given by

$$C \approx F/S. \tag{24}$$

In this form, forcing is explicit while adjustment is implicit. This argument led to the quasi-equilibrium assumption elaborated upon in Arakawa and Schubert (1974).

When a sufficiently small τ_{ADJ} is chosen, conditional instability does not exist with this scheme because at most $A \approx 0$ for all t. For $A \approx 0$, the definition of A given by (20) gives

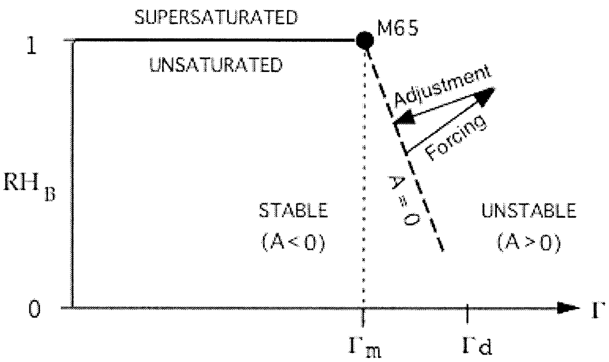

FIG. 7. A schematic illustration of the quasi-equilibrium, large-scale forcing and adjustment in the scheme by Arakawa (1969), where Γ is the mean lapse rate, Γ_d is the dry-adiabatic lapse rate, Γ_m is the moist-adiabatic lapse rate, and $(\mathrm{RH})_B$ is the relative humidity of the boundary layer. The dashed line represents the quasi-equilibrium states, which is the destination of the adjustment responding to the large-scale forcing. The dot represents the equilibrium state used in the moist-convective adjustment scheme of Manabe et al. (1965).

$$(\mathrm{RH})_B \approx 1 - c_p[(T_B - T_1) - (T_B - T_1)_m]/Lq, \tag{25}$$

where $(\mathrm{RH})_B = q_B/q_B^*$ is the relative humidity of the boundary layer and the subscript m represents the value corresponding to the moist-adiabatic vertical structure defined by $h_B^* - h_1^* = 0$. Figure 7 schematically illustrates the quasi-equilibrium states given by $A \approx 0$, large-scale forcing, and adjustment in an idealized $\Gamma - (\mathrm{RH})_B$ space, where Γ is the mean lapse rate. The figure also shows the equilibrium state used by the M65 scheme. [For observational studies related to this figure, see Arakawa and Chen (1987), Arakawa (1993), and Randall et al. (1997b). See also section 5e for the limits of such studies.]

d. Summary and conclusions for section 4

In this section, the logical structures of three selected parameterization schemes are reviewed. The principal closure of the M65 scheme is an instantaneous adjustment of conditionally unstable lapse rate and supersaturated relative humidity to the moist-adiabatic lapse rate and saturated relative humidity. Because these equilibrium states to which the adjustment is made constrain the temperature and humidity profiles so strongly, as schematically shown by the dot in Fig. 7, there is no room for a cloud model except for energetic consistency to determine the vertical mean temperature after adjustment. The principal closure of the A69 scheme is also that of a virtually instantaneous adjustment to quasi-equilibrium states, but they are defined more loosely than in M65, constraining only coupled temperature–humidity profiles as schematically shown by the dashed line in Fig. 7. This is why the A69 scheme needs a cloud model for supplementary closures to constrain the direction of adjustment. In these adjustment schemes, large-scale forcing is implicit. In the K74 scheme, on the other hand, large-scale forcing is explicit and adjustment is implicit. Its principal closure is achieved through the use of vertically integrated moisture convergence and the parameter b in formulating the large-scale forcing. A cloud model can be used for supplementary closures to determine the coefficients on the forcing.

The review presented here suggests that understanding the performance of a scheme does not have to be based on the reasoning presented in the original paper. For the two adjustment schemes, (11) and (12) for the M65 scheme and (24) with (22) for the A69 scheme give alternate interpretations of the schemes. In these interpretations, large-scale forcing is explicit and adjustment is implicit. In the opposite way, the K74 scheme can be interpreted as an adjustment scheme although the way in which adjustment is performed is peculiar. This peculiarity, however, can be removed in one way or another, as pointed out in section 4b.

When we wish to compare different parameterization schemes, it is much more meaningful to interpret the schemes based on the same logical framework, that of adjustment schemes for example. In this way the comparison can give more specific information on the differences between the schemes. I indicated in section 1 that, in GCM applications with a prescribed geographical distribution of SST, the main task of cumulus parameterization is to provide a negative feedback by adjusting the temperature lapse rate in the tropical troposphere to realistic values. The major conclusion of this section is that all of the schemes reviewed here perform this task, but in quite different ways. Such differences might not have been too serious in the past, but will not continue to be so as the scope of cumulus parameterization expands.

5. Current trends and outstanding problems

In this section, I first discuss the basic concept underlying the parameterizability of cumulus convection and review the current trends of cumulus parameterization schemes with an emphasis on their logical structures. This section is also an introduction to section 6, in which I point out outstanding problems existing at present in further developing cumulus parameterization schemes.

a. Moist-convective quasi equilibrium as the basis for parameterizability

Although the importance of cumulus parameterization for the practical problems of weather and climate prediction has always been well recognized, the scientific basis for cumulus parameterization has not attracted sufficient interest on the part of the modeling community. Due to this imbalance, cumulus parameterization has not become a mature scientific subject yet, as I pointed out in section 1. In my opinion, however, at least major controversies in posing the problem have almost dissipated. Parameterization, which is an attempt to relate the statistical effects of unresolved processes to processes that are explicitly resolved in weather and climate models (see Fig. 3), obviously requires some kind of statistical balance between them. The real question is then for what quantities, under what situations and in what way can quasi equilibrium be formulated *for the purpose of predicting weather and climate.* Quasi equilibrium of buoyancy-related quantities under moist-convective processes, such as that of the cloud work function (Arakawa and Schubert 1974), which is the rate of convective kinetic energy generation per unit cloud-base mass flux, and the convective available potential energy (CAPE; see Emanuel 1994 for definition), satisfy requirement 3 for the principal closure discussed in section 2b, and now it seems to be more widely accepted as the basis for parameterizability. Although there are arguments against the use of such quantities in the context of quasi equilibrium (e.g., Mapes 1997), the majority of the existing cumulus parameterization schemes (see section 5b) are based on the quasi-equilibrium concept, either explicitly or implicitly, through the adjustments of temperature and humidity profiles to reference profiles.

The importance of the quasi-equilibrium concept is not limited to the closure for a parameterization scheme. It may serve as a ''closure'' in the way we think (e.g., Emanuel et al. 1994; Neelin 1997; Emanuel 2000). As Emanuel (2000) pointed out, ''It is somewhat surprising that, almost a quarter century after the introduction of the idea of quasi-equilibrium, very little of its conceptual content has influenced the thinking of most tropical meteorologists, even while the parameterization itself is enjoying increasing use.'' The following points, emphasized by Emanuel (2000) as ''quasi-equilibrium thinking'' but expressed in my own language, are closely related to the quasi-equilibrium concept:

- The condensation process is strongly coupled with dynamical processes so that the heat of condensation should not be considered as a heat source external to the dynamics.
- Logically speaking, the heat of condensation does not necessarily produce available potential energy because the temperature field is not necessarily positively correlated with the heating.
- Cumulus heating primarily occurs as a result of ''cumulus adjustment,'' which is a passive response to other processes. When isolated from other processes, this produces a negative feedback, leading to the idea of ''cumulus damping'' on large-scale disturbances in the Tropics.

The last point, however, may be a matter of academic interest since in reality cumulus convection is almost always coupled with other processes.

In the sense that it may have an important conceptual content, the idea of moist-convective quasi equilibrium is analogous to quasigeostrophic dynamics for large-scale motions in the atmosphere and oceans, as briefly mentioned in Arakawa and Schubert (1974) and discussed by Schubert (2000) in detail. Quasigeostrophic dynamics filters out the Lamb and inertia–gravity waves, assuming that geostrophic adjustment due to the dispersion of these waves rapidly takes place whenever the geostrophic balance tends to be destroyed. The geostrophic potential vorticity then becomes only a prognostic variable of the system. Thus the assumption of quasigeostrophic balance can be viewed as a closure in the sense that it gives a theoretical framework with a smaller number of degrees of freedom.

The time scale of the geostrophic adjustment is roughly given by $1/f$, where f is the Coriolis parameter, and the time scale of the processes that tend to break the balance is typically given by the advective time scale L/V, where L and V are the characteristic length and velocity scales, respectively. Then the condition for applicability of quasigeostrophic dynamics is that the ratio of these two time scales is sufficiently small, that is, $\mathrm{Ro} \equiv V/fL \ll 1$, where Ro is the Rossby number. Quasigeostrophic motion then represents the first-order approximation in the power series expansion of the solution in terms of the Rossby number. Quasi-equilibrium solutions in the cumulus parameterization problem can also be interpreted as the first-order approximation to the solution when $\tau_{\mathrm{ADJ}}/\tau_{\mathrm{LS}} \ll 1$, where τ_{LS} is the time scale for the variation of large-scale processes that destabilize the atmosphere moist convectively. For higher-order approximations, however, no analogy can be made between the two problems. I will come back to this important point in the next subsection.

b. Current trend from diagnostic closures to prognostic closures

In this section, I classify parameterization schemes presented in the literature into six groups according to the formal structures of their principal closures.

1) DIAGNOSTIC CLOSURE SCHEMES BASED ON LARGE-SCALE MOISTURE OR MASS CONVERGENCE, OR VERTICAL ADVECTION OF MOISTURE

These schemes relate cumulus effects directly to low-level large-scale horizontal convergence or vertical moisture advection at the same instant. The schemes include Kuo (1974) and its variations such as Anthes (1977), Krishnamurti et al. (1980, 1983), Krishnamurti and Bedi (1988), Donner et al. (1982), Molinari (1982), Molinari and Corsetti (1985), Kuo and Anthes (1984), Frank and Cohen (1987), and Tiedtke (1989). However, as I pointed out in section 4b for the Kuo (1974) scheme, many of these schemes can also be interpreted as relaxed adjustment schemes with variable adjustment coefficients.

2) DIAGNOSTIC CLOSURE SCHEMES BASED ON QUASI EQUILIBRIUM

These schemes also relate cumulus effects directly to large-scale processes at the same instant. Schemes in this group, however, explicitly define moist-convective equilibrium states and assume that a sequence of quasi equilibria is followed in time as long as conditions for the existence of cumulus activity are met. Then there must be an approximate balance between the large-scale effects that tend to destroy the equilibrium and the cumulus effects that tend to restore the equilibrium. The concept of adjustment is important in the basic reasoning but it is not explicit in the actual computation. An example is the Arakawa and Schubert (1974) scheme as presented in the original paper. The closure used by Donner et al. (2001), which implements Donner's (1993) parameterization of mesoscale effects, belong to this group. For other examples, see the comments given at the end of the next group.

3) (VIRTUALLY) INSTANTANEOUS ADJUSTMENT SCHEMES

In these schemes, the adjustment toward equilibrium states is explicit and the forcing is implicit. The adjustment time scale is virtually instantaneous in the sense that it is basically the same as the computational time step for implementing the physics. I have already reviewed the schemes by Manabe et al. (1965) and Arakawa (1969) in sections 4a and 4c, respectively. Other examples are Miyakoda et al. (1969), Kurihara (1973), and Arakawa and Schubert (1974, hereafter AS) as implemented by Lord et al. (1982), Grell (1988, 1993; see also Grell et al. 1991), and Yao and DelGenio (1989; see also DelGenio and Yao 1993). These schemes can easily be converted to relaxed adjustment schemes, which will be described in section 5b(4) below, by applying the adjustment only partially on each physics time step during the integration. I have also pointed out in sections 4a and 4c that the Manabe et al. (1965) and Arakawa (1969) schemes can be written in the form of the schemes in group (2) as well.

4) RELAXED AND/OR TRIGGERED ADJUSTMENT SCHEMES

In these schemes, equilibrium states are also defined, but adjustments toward those states are either relaxed (i.e., partial at each time step) and/or performed only when certain conditions for triggering are met. I put a variety of schemes into this group. Examples are Kreitzberg and Perkey (1976, 1977; see also Perkey and Kreitzberg 1993), Fritsch and Chappel (1980; see also Fritsch and Kain 1993), Betts and Miller (1986; see also Betts 1986; Betts and Miller 1993; Janjic 1994), Gregory and Rowntree (1990; see also Gregory 1997; Gregory et al. 2000), Kain and Fritsch (1990; see also Kain and Fritsch 1993), Moorthi and Suarez [1992, 1999; see also Moorthi 2000, relaxed Arakawa–Schubert (RAS)] Hack (1994), Pan and Wu (1995), Zhang and McFarlane (1995), Sud and Walker [1999a,b microphysics of clouds with RAS (McRAS)], Cheng and Arakawa (1997), and many others. When the adjustment time scale is sufficiently short, the schemes are close to those in groups (3) and (2), in which cumulus activity is determined by the rate of generating disequilibrium. When the adjustment time scale is longer or it is performed only when triggered, cumulus activity is primarily determined by the existing amount of disequilibrium.

5) Prognostic closure schemes with explicit formulations of transient processes

In these schemes, adjustment toward quasi-equilibrium states is effectively achieved during time integration of explicitly formulated transient processes. The formulation of such processes itself can effectively serve as a principal closure. Examples are Emanuel (1991a; see also Emanuel 1993a, 1997; Emanuel and Zivkovic-Rothman 1999) and Randall and Pan (1993; see also Pan and Randall 1998; Ding and Randall 1998). I will discuss these schemes in more detail in section 5c.

6) Stochastic closure schemes

To meet objective b6 in section 1, stochastic effects can be introduced into any group of schemes mentioned above. Lin and Neelin (2000, 2002, 2003) presented a family of nondeterministic schemes based on a deterministic scheme that belongs to group (4). Lin and Neelin (2003) tested two ways of introducing stochastic components: one directly to the vertical structure of heating (VSH scheme) and the other to the cloud-base mass flux (CAPE-M_b scheme) in a mass-flux model. They found that with the CAPE-M_b scheme, the inclusion of a stochastic component increases the overall variance of precipitation with a realistic spatial pattern. I will come back to this scheme in section 5c.

Admittedly, the classification presented above is rather ambiguous, especially between groups (3) and (4), and it is quite possible that some schemes are misplaced. Nevertheless, we recognize the existence of a general trend from deterministic diagnostic closures, including instantaneous adjustments, to prognostic or nondeterministic closures, including relaxed and/or triggered adjustments. The transition from diagnostic closures to prognostic closures is formally analogous to that of the dynamics in numerical weather prediction models from the quasigeostrophic equations to the primitive equations, which took place during the 1960s, and from the primitive equations to the nonhydrostatic equations, which has already started to take place. For the most part, this transition is to avoid complications in numerical calculations when we wish to include higher-order effects while strictly enforcing the balance. The same is more or less true for the trend of the cumulus parameterization schemes discussed above, although there is evidence for the physical importance of a finite adjustment time scale for the ''cumulus damping'' effect (e.g., Emanuel 1993b; Neelin and Yu 1994; Yu and Neelin 1994; Emanuel et al. 1994; Neelin 1997; Yano et al. 1998).

It is very important to recognize, however, that we are not going back to known equations by abandoning a quasi-equilibrium assumption in the present case of cumulus parameterization. This is partly because equations governing cloud-scale dynamics and physics have not been well established and partly because cumulus parameterization is an attempt to look at clouds as a group (forest), which is more than taking averages of the dynamics and physics of individual clouds (trees). As I will review in section 6, there are a number of problems to be solved before the benefit of this trend can be fully utilized.

The trend is also formally analogous to the generalization of the K theory for turbulence to higher-order turbulence closure models (e.g., Mellor and Yamada 1974, 1982). In these models, however, the quasi-isotropic and quasi-diffusive natures of turbulence are still assumed in a relaxed way. The cumulus parameterization problem is different from the turbulence closure problem in the following sense:

- Cumulus convection is *inherently* anisotropic while turbulence is usually quasi-isotropic.
- Cumulus convection is *inherently* penetrative and the one-dimensional parcel method is still a useful starting point. This is in a sharp contrast to (microscale) turbulence, which is *inherently* diffusive and three-dimensional.
- In addition to the quadratic nonlinearity due to advection, nonlinearity involving phase changes of water is crucial for cumulus convection.
- Statistical distributions of dynamical and thermodynamical properties of air in the cloudy atmosphere are highly skewed (see Fig. 8) and, therefore, the concepts of ''mean and variance'' are less useful than those of ''cloud and environment,'' ''updrafts and downdrafts,'' or similar structural characteristics in physical space.
- Correspondingly, a large part of closures can be achieved in the cumulus parameterization problem through the use of a simplified cloud model, at least as far as the classical objectives (see section 1) are concerned. [For relations between the basic closures in mass-flux models and those in higher-order turbulence closure models, see Lappen and Randall (2001a–c).]

c. *Nondiagnostic and nondeterministic nature of cumulus mass flux*

In a model with a relatively coarse horizontal resolution, the problem of cumulus convection becomes

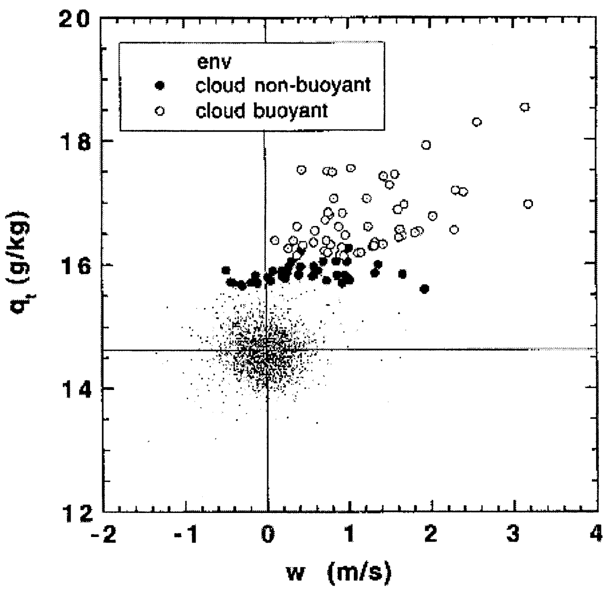

FIG. 8. Plots of total water mixing ratio against vertical velocity simulated by an LES model. From Siebesma and Holtslag (1996).

complicated due to the existence of mesoscale organization of cumulus convection. The results of CSRM simulations by Xu et al. (1992) do show fluctuations in cumulus activity not fully modulated by large-scale forcing. Obviously, these fluctuations cannot be parameterized using a quasi-equilibrium assumption, showing that there is a limit of diagnostic or deterministic parameterizability. To go beyond this limit requires the introduction of more prognostic equations to predict transient cumulus activity or stochastic components representing uncertainties on the initial conditions for those equations. The concept of quasi equilibrium can be either implicit in the prognostic system or can be used as a weaker constraint on the system. The question is, then, how to formulate such a system.

In the context of a simple mass flux model for cumulus updraft, the key quantities to be found are cloud mass flux, $M_c(z)$; entrainment into clouds, $E(z)$; and detrainment from clouds, $D(z)$, all of which are functions of height. If the storage of mass in clouds is neglected in the mass budget for clouds, as in Ooyama (1971) and AS, only two of these are independent since

$$\frac{\partial}{\partial z} M_c(z) = E(z) - D(z) \tag{26}$$

must hold. Arakawa and Schubert (1974) assume that each cloud type has its own characteristic normalized vertical profiles of these quantities so that the problem reduces to finding the spectrum of cloud-base mass flux, $m_B(\lambda)$, where λ is a parameter used to identify cloud type. Since two one-dimensional functions, $M_c(z)$ and $E(z)$, for example, are determined by one one-dimensional function, $m_B(\lambda)$, this assumption is one of the supplementary closures (see section 2 for definition) in the parameterization. The parameterization still needs a principal closure to determine $m_B(\lambda)$.

Randall and Pan (Randall and Pan 1993; Randall et al. 1997a; Pan and Randall 1998) developed a prognostic principal closure for the AS parameterization, in which the convective-scale bulk kinetic energy K is predicted for each cloud type. The starting point of the closure is the kinetic energy rate equation for the cloud type being considered, given by

$$\frac{dK}{dt} = m_B A - \frac{K}{\tau_D}, \tag{27}$$

where A is the cloud work function defined by AS and τ_D is the dissipation time scale for K. As is clear from (27), A is by definition the rate of production of K per unit cloud-base mass flux, representing a measure of moist-convective instability of a given sounding. Following Xu (1991), the prognostic closure then introduces a parameter α defined by

$$K = \alpha m_B^2. \tag{28}$$

The kinetic energy K includes the horizontal kinetic energy associated with the convective activity. Since this part of K tends to be large when the convection is organized on the mesoscale (Xu et al. 1992), we can roughly interpret a properly scaled α as a measure of the mesoscale organization of convection. If α is known, m_B can be diagnosed from K using (28). Using m_B determined in this way for all possible cloud types, the cumulus effects on the vertical profiles of temperature and humidity can be computed. Implementing these effects into the prognostic algorithm of the model, the temperature and humidity fields and, therefore, the cloud work function A can be updated. Using this A in (27), K can be predicted further. This is the procedure of the prognostic closure proposed by Randall and Pan.

Although it is not needed in actual prediction, we may interpret the above procedure along the line of (21) but now using the cloud work function tendency equation,

$$\frac{dA}{dt} = Jm_B + F, \tag{29}$$

where Jm_B and F represent the cumulus and large-scale effects on dA/dt, respectively, representing cumulus adjustment and large-scale forcing. The factor J, which symbolically represents the kernel of the integral equation in AS, is assumed to be negative (i.e., the cumulus effect is to decrease A) so that we may write $J = -|J|$. Assuming α and J are constants and eliminating A and K between (27), (28), and (29), we obtain

$$2\alpha \frac{d^2 m_B}{dt^2} + \frac{\alpha}{\tau_D}\frac{dm_B}{dt} + |J|m_B = F. \tag{30}$$

In the limit as $\alpha \to 0$, (30) gives

$$m_B = F/|J|. \tag{31}$$

This is the equilibrium solution of (29), which is a valid approximation to the *forced solution* of (30) when the time variation of the forcing F is slow. Thus the procedure described here can be considered as a generalization of the cloud work function quasi-equilibrium closure used in AS. When α is not small, on the other hand, (30) becomes a damped oscillation equation with forcing. Ensemble-average solutions of a CSRM with a prescribed time-varying F presented by Xu et al. (1992; see also Xu 1993) resemble such oscillations. It is especially interesting to see that the oscillation mechanism due to the existence of the first term on the left-hand side of (30) depends on the measure of mesoscale organization, α. At present, we do not know well what determines α in nature and to what extent it can be treated as a quasi-constant. The prognostic closure reviewed here, however, gives a clearer idea of what the quasi-equilibrium closure means in a more general context.

The scheme proposed by Emanuel (1991a) is based on the buoyancy sorting model of Raymond and Blyth (1986). The scheme and its variations described by Emanuel (1993a, 1997) and Emanuel and Zivcovic-Rothman (1999) also predict (or effectively predict) the updraft mass flux, M. For example, Emanuel (1997) uses

$$\frac{\partial M}{\partial t} = \alpha(T_{\rho p} - T_{\rho})_{\mathrm{ICB}} - \beta M, \qquad (32)$$

where α and β are constants, T_ρ is the density (or virtual) temperature of the environment, $T_{\rho p}$ is that of a reversibly lifted air parcel, and the subscript ICB denotes the first level above cloud base. The buoyancy effect at this level is used following the idea of subcloud-layer quasi equilibrium advocated by Raymond (1995). Note that, when (28) is used, (27) becomes similar to (32).

In my opinion, these approaches to prognostically determine the cumulus mass flux should be pursued further. Besides the computational and physical advantages these approaches may have in practical applications, they have conceptual merits also because without a theory on the transient behavior of cumulus activity, we cannot rigorously discuss the existence or nonexistence of quasi equilibrium. The more complete such a theory is, however, the more prognostic equations will be involved. Predictions will then become inevitably nondeterministic due to uncertainties on their initial conditions.

There are attempts to explicitly include this nondeterministic nature of the problem in parameterization schemes. The CAPE-M_b scheme of Lin and Neelin (2003), for example, introduces stochastic components into an equation similar to (23). Using the notations of (23) except for m_B in place of C for the cloud-base mass flux, the equation they used may be written as

$$Sm_B = \frac{A + \xi}{\tau_{\mathrm{ADJ}}}, \qquad (33)$$

where ξ represents the stochastic effect. The time scales of such processes are represented by including a temporal autocorrelation in ξ. Physically, these time scales should represent the natural lifetime of cloud clusters. Following a similar procedure, we can think of nondeterministic versions of more general prognostic closure schemes.

d. Factors involved in the time evolution of cumulus mass flux

At this point, let us look into factors involved in the time evolution of the cumulus mass flux. For simplicity, we assume that the cloud system consists of a population of readily-identifiable individual clouds. With this idealization, we can express the total cumulus mass flux per unit large-scale horizontal area as

$$M = \sum_i^N \rho\sigma_i w_i, \qquad (34)$$

where σ_i and w_i are the fractional cloud cover and the mean vertical velocity, respectively, of the ith cloud. We may rewrite (34) as

$$M = \rho\sigma w_I, \qquad (35)$$

where σ is the fractional cloud cover by all clouds given by

$$\sigma \equiv \sum_i^N \sigma_i \qquad (36)$$

and w_I is a weighted average of w_i given by

$$w_I \equiv \sum_i^N w_i\sigma_i \Big/ \sum_i^N \sigma_i. \qquad (37)$$

From (35) we see that M can change either through a change in the fractional cloud cover, σ, or through a change in the mean vertical velocity of individual clouds, w_I. For the classical objectives of cumulus parameterization (see section 1), there is no pressing need to determine σ and w_I separately. This is not the case, however, for the nonclassical objectives of cumulus parameterization (see also section 1). For example, no calculations of cloud microphysics and radiation can be realistic without knowing the vertical velocity and cloud cover, respectively. There is a good reason to believe that the response of a system of deep clouds to large-scale forcing is mainly through changes in σ rather than changes in w_I (see Robe and Emanuel 1996; Emanuel and Bister 1996).

For further clarification, let us rewrite (35) as

$$M = \rho N\sigma_I w_I, \qquad (38)$$

where N is the number of clouds and σ_I is the mean σ_i given by

$$\sigma_I \equiv \frac{\sigma}{N}. \qquad (39)$$

Since both σ_I and w_I are strongly constrained by the dynamics governing the structure and intensity of *individual* clouds, it is reasonable to assume that the change of σ in response to large-scale processes is mainly through an increase in N, which may well involve stochastic processes. A statistical theory that governs the time evolution of N is then badly needed. Equation (34) itself is, however, oversimplified because it is based on a highly idealized cloud ensemble model. This leads to the problem of modeling cloud systems, which I will discuss in section 6a.

e. Interpretation of observational studies on quasi equilibrium

In view of the potential conceptual importance of moist-convective quasi equilibrium, it is natural to examine observed data from that point of view. Recent examples of such studies include Brown and Bretherton (1997), Cripe and Randall (2001), and Zhang (2002). It is not straightforward to interpret the results of such studies, however, due to the following reasons:

1) Diagnostic quasi equilibrium that can be seen in time averages should be clearly distinguished from the physical constraint of moist-convective quasi equilibrium for closing prognostic equations (see Yano 2000). The former may or may not be a consequence of the latter (see also Mapes 1997).
2) If the dataset includes cases with no moist convection, its statistics do not address the problem of moist-convective quasi equilibrium.
3) In reality, cumulus convection is strongly coupled with other physical processes, PBL processes in particular. Then, any test of quasi equilibrium should not ignore the possibility of ''boundary layer quasi equilibrium'' (Raymond 1995, 1997).
4) The moist-convective quasi-equilibrium hypothesis is a means of filtering out the nonparameterizable components of moist convection so that the prognostic equations are closed. How strong the nonparameterizable components are in a particular dataset is independent of the validity of the hypothesis for the parameterizable component.
5) The results of such studies depend on the adopted expression for quasi equilibrium, which may not be adequate for quasi equilibrium in nature.

Discussions presented in this section further motivate us to look into more basic problems in formulating model physics, as I attempt to do in the next section.

6. Major problems in conventional model physics and future approaches

During the last decades, the scope of numerical modeling of the atmosphere has magnificently expanded. Numerical models are now indispensable tools for studying and predicting the atmosphere. Further development of model physics for future climate models is, however, now facing very difficult problems, both practical and conceptual. In this section, I will emphasize that we need multiple approaches to overcome these difficulties, including the development and use of a new framework for modeling.

a. A major practical problem in conventional model physics

There are a number of uncertainties in formulating processes associated with clouds such as those illustrated in Fig. 2. These processes are the major subjects of cloud dynamics as a branch of atmospheric sciences, which should be investigated regardless of whether we wish to parameterize them or not. From the point of view of parameterization, however, there is an additional problem: as I pointed out in section 1, we do not have a sufficiently general framework for implementing detailed formulations of these processes for the purpose of cloud parameterization. In my opinion, this represents the most serious practical problem in the conventional approach of formulating model physics.

The need for such a framework is most obvious for the unified cloud parameterization problem, in which both cumuliform and stratiform clouds are treated as special cases. Parameterizations of these two forms of clouds have a different history of development using different geometrical structures: column physics for cumuliform clouds and layer physics for stratiform clouds. [For classical papers on the latter, see Sundqvist (1978) and Tiedke (1993).] I realize that a unified cloud parameterization needs a cloud system model that inevitably has a multicolumn (i.e., horizontally nonlocal) and multilayer (i.e., vertically nonlocal) structure.

Even for parameterizing typical cumulus clouds based on column physics, there is no established model that can be used to describe organizations of different types of clouds. Here ''cloud types'' can be interpreted in a very general way as the potential degree of freedom existing in the vertical structure of clouds for a given environmental sounding. When cloud-base height is specified, cloud-top height is the simplest example that can be used to identify cloud types. A majority of the cumulus parameterization schemes currently being used assume a single cloud type either explicitly or implicitly. The spectral cloud ensemble model of AS and its variations including Ding and Randall (1998) are exceptions. Even there, however, different cloud types can interact only through their common environment. When we wish to include direct interactions between clouds, we should find a replacement for the concept of cloud types to represent the vertical degree of freedom. There is also the problem of distinguishing the active core and surrounding inactive part of clouds (for a related problem, see Lin and Arakawa 1997a,b). It seems, therefore, that a nonconventional cloud system model is needed even in the conventional approach of cumulus parameterization.

b. *Major conceptual problems: Artificial separation of processes*

In the conventional approach, there are even more fundamental problems in formulating model physics: *artificial separation of processes* and *artificial separation of scales.* In the past, our modeling efforts have been spent mainly on coupling each physical process (radiation, PBL, large-scale condensation, cumulus convection, stratiform cloudiness, chemistry, etc.) with the dynamics core more or less independently from the others. Consequently, the model physics in conventional models has a modular structure, in which interactions between individual processes take place mainly through the model's prognostic variables, missing most of the small-scale direct interactions between those processes. This is a serious problem particularly in achieving the nonclassical objectives discussed in section 1.

Here I take radiation–cloud interaction through fractional cloudiness as an example. In most conventional models, mean cloudiness is diagnosed for each grid box from the model's prognostic variables, typically using empirical relationships between mean cloudiness and mean relative humidity (e.g., Slingo 1987). CSRM-simulated data can also be used to find such relationships (e.g., Xu and Krueger 1991; Xu and Randall 1996). This diagnostic procedure, however, simply replaces the problem of predicting mean cloudiness by that of predicting mean relative humidity. Moreover, radiative cooling (heating) concentrated near the cloud top (bottom) inevitably modifies in-cloud microphysical–turbulent–convective processes (e.g., Köhler 1999) and/or induces cloud-scale local circulations. These processes can in turn influence the cloudiness itself so that the radiation–cloud interaction is a two-way interaction involving multiple processes.

One of the future emphases in climate modeling should be on a formulation of the entire spectrum of these interactions, or development of a ''physics coupler'' in which these processes are fully coupled. This is an extremely challenging task, but we should remember that it is our eventual goal.

c. *Major conceptual problems: Artificial separation of scales*

The second major conceptual problem is associated with the truncation of the spectrum of atmospheric processes. Although truncation is introduced for a computational purpose, it inevitably influences the model physics by separating it into resolved and unresolved processes. Depending on where truncated, atmospheric models can be roughly classified into two groups: low-resolution models such as climate and global NWP models and high-resolution models such as CSRMs (or CRMs) and large-eddy simulation (LES) models. Models in the former group are usually (but not necessarily in the future) quasi-static while models in the latter group are necessarily nonhydrostatic. Besides, the two groups have distinctly different model physics: low-resolution model physics, in which moist-convective processes are parameterized, and high-resolution model physics, in which moist-convective processes are explicitly treated at least partially. Each of these model physics is designed and tuned for a certain range of resolutions, not to formulate physics as a function of resolution covering a broad spectrum.

The heavy line in the left panel of Fig. 9 illustrates a typical vertical profile of the moist static energy source, $Q_{1c} - Q_{2c}$, for low-resolution models as suggested by observed large-scale budgets. Here Q_{1c} and Q_{2c} denote the ''apparent heat source'' and ''apparent moisture sink'' (Yanai et al., 1973) due to convection. The right panel of Fig. 9, on the other hand, illustrates typical vertical profiles of the moist static energy source as expected from local cloud microphysics. Since moist static energy is conserved during condensation–evaporation processes, its source away from the surface is primarily determined by phase changes involving ice. For a nonprecipitating updraft, there are sources due to the latent heat release above the freezing level through the depositional growth of cloud ice. For a downdraft/precipitation, there are sinks below the freezing level due to the latent heat absorption through the melting of snow and graupel. Here it is important to recognize that *any space–time–ensemble averages of the profiles in the right panel do not give the profile shown by the thick solid line in the left panel.*

Conventional cumulus parameterizations do recognize the difference between these two kinds of physics at least qualitatively. For the purpose of illustration, let us consider a simple cumulus parameterization, in which only the effect of the compensating subsidence in the cloud environment is considered. The parameterized cumulus effects can then be expressed as

$$\text{cumulus warming,} \qquad Q_{1C} \sim \mathrm{Mc}\partial s/\partial z; \quad \text{and} \tag{40}$$

$$\text{cumulus drying,} \qquad Q_{2C} \sim -\mathrm{Mc}L\partial q/\partial z, \tag{41}$$

where Mc is the cumulus mass flux or equivalently the compensating subsidence in the cloud environment, $s \equiv c_pT + gz$ is the dry static energy, L is the heat of the condensation per unit mass of water vapor, and q is the water vapor mixing ratio. The parameterized source of the moist static energy, $h \equiv s + Lq$, is the given by

$$Q_{1c} - Q_{2c} \sim \mathrm{Mc}\ \partial h/\partial z. \tag{42}$$

For the typical tropical atmosphere, $\partial h/\partial z < 0$ in the lower troposphere and $\partial h/\partial z > 0$ in the upper atmosphere. Then, when a vertical profile of Mc for deep cumulus convection is used, (42) gives $Q_{1C} - Q_{2C}$, which has a vertical profile similar to the one shown in

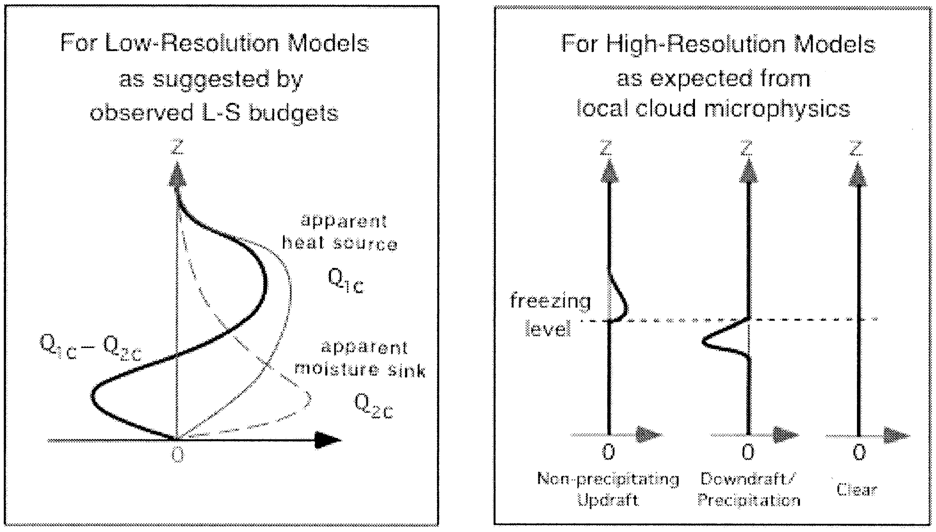

FIG. 9. Schematic figures illustrating typical vertical profiles of the moist static energy source required for low-resolution models as suggested by (left) observed large-scale budgets (the thick solid line) and (right) that required for high-resolution models as expected from local cloud microphysics.

the left panel of Fig. 9. Thus even this highly idealized parameterization is on the right track; the problem is that it does not produce scale-dependent results that are in between the left and right panels of Fig. 9.

d. The convergence problem of model physics

The argument presented in the last subsection raises a serious question about the convergence of model physics. Justification of a discrete model relies on the hope that its solution converges to the solution of the original system as the resolution is refined. The convergence problem for conventional model physics is, however, different from the standard convergence problem in numerical analysis since the governing equation is *modified, rather than approximated,* through the use of parameterized expressions.

To proceed, we first discuss the relation between the ''real'' source that appears in the original governing equations and the ''required'' source that must be used in the model's governing equations. Let the original, local, and instantaneous governing equation and the model's governing equation applied to the evolution of the averaged field be

$$\frac{\partial \Psi}{\partial t} = -\mathbf{V}\cdot\nabla\Psi + \text{``real'' source} \quad \text{and} \qquad (43)$$

$$\frac{\partial \overline{\Psi}}{\partial t} = -\overline{\mathbf{V}}\cdot\nabla\overline{\Psi} + \text{parameterized source}, \qquad (44)$$

respectively, where Ψ is an arbitrary prognostic variable and an overbar denotes a space–time–ensemble averaging. For the model to correctly predict the averaged field, (44) must be consistent with (43) so that it is necessary to satisfy

$$\text{``required'' (parameterized) source} = \overline{\text{``real'' source}} - [\overline{\mathbf{V}\cdot\nabla\Psi} - \overline{\mathbf{V}}\cdot\nabla\overline{\Psi}]. \qquad (45)$$

From now we call the required parameterized source simply the required source. As is clear from (45), *the required source for accurately predicting the mean field is not the mean of the ''real'' source.* The terms in the brackets in (45) represent the mean transport due to the deviation from the mean. Obviously, this transport depends on how we define the mean. In discrete models, the average is usually over each of the grid boxes, rather than a continuous running average. The expression for the mean transport is then more complicated since it depends on the discretized form of the advection term $\overline{\mathbf{V}}\cdot\nabla\overline{\Psi}$ used in the model. We can still say that *the required source for accurately predicting the mean field is not the mean of the real source,* and their difference depends on model resolution. Naturally, the required source converges to (an ensemble average of) the real source as the resolution is refined.

Similarly to the case of total physics presented above, we can identify the required source due to a particular component of physics. For cloud microphysics, for example, we have the following:

$$\begin{aligned} &\text{required cloud microphysical source} \\ &= \overline{\text{real cloud microphysical source}} \\ &\quad + (\text{advection, turbulence, and radiation effects} \\ &\qquad \text{induced by unresolved cloud microphysics}). \end{aligned} \qquad (46)$$

Thus, *the required cloud microphysical source for accurately predicting the mean field is not the mean of the real cloud microphysical source.* This point is clear even in the schematic figure, Fig. 9. Again, the required microphysical source converges to (an ensemble average of) the real microphysical source as the resolution is refined.

e. Resolution dependency of model physics: Illustrations from nonhydrostatic model experiments

Jung and Arakawa (2004) presented a systematic analysis of ''required'' sources and their resolution dependencies inferred from nonhydrostatic model experiments. The analysis procedure used is parallel to the diagnosis of the ''apparent source'' from the residuals in observed large-scale budgets, but here the procedure is more refined especially in identifying the advection effects. The model used is a two-dimensional (x–z) nonhydrostatic anelastic model originally developed by Krueger (1988) and subsequently revised and applied to a variety of cloud regimes (see, e.g., Xu and Krueger 1991; Krueger et al. 1995a,b). Jung and Arakawa (2004) first integrated the model as a CSRM with full physics under a variety of idealized tropical conditions (CONTROL). Next, a low-resolution model that is consistent with the CSRM but with only partial physics is integrated starting from a selected realization from CONTROL over a short time interval. The result of this integration is then compared to that of CONTROL for the corresponding time interval and the error of the low-resolution run due to the missing physics is identified. From this error, they diagnosed the required source needed for the low-resolution run to become correct as far as the resolvable scales are concerned. This procedure is repeated over many realizations selected from CONTROL and then the ensemble average is taken. For illustration, the results are further space averaged over the entire model domain.

Here I present the required source due to cloud microphysics. Figure 10a shows an example of the domain- and ensemble-averaged profiles of the required cloud microphysical source of the moist static energy. Figure 10a (left) shows those with the same horizontal grid size as that used for CONTROL (2 km) but with different time intervals for implementing the cloud microphysics. The thick solid line is for the case with a small time interval (2 min), approximately representing the real source. Note that, as expected, this line resembles a mixture of those shown in the right panel of Fig. 9. We see that, as the time interval becomes longer, the peaks localized in the real source are dispersed in the required source both upward and downward. This dependence of the required source on the physics time interval must be due to the upward transports of air in updrafts with high moist static energy produced by freezing and downward transports of air in precipitating downdrafts with low moist static energy produced by melting.

The right panel of Fig. 10a shows the horizontal resolution dependence of the domain- and ensemble-averaged profiles of the required source of moist static energy for a fixed physics time interval (60 min) but for different horizontal grid sizes. We see that the peaks of the required source for larger grid sizes deviate considerably from those for the 2-km grid size. With the 32-km grid size shown by the thick solid dotted line, for example, we see a pronounced sink in the lower troposphere and a pronounced source spread throughout the middle and upper tropospheres, due to the existence of vertical transports by deep convective systems, which now involve subsidence in the clear environment.

Figure 10b is the same as Fig. 10a but for the required cloud microphysical source of the total (airborne) water mixing ratio. Figure 10b (left) shows the dominant sinks in the middle troposphere due to the generation of precipitating particles and small peaks near the surface due to evaporation from precipitation. These features do not depend significantly on the physics time interval. The right panel, however, shows a strong dependency of the (negative) source on the horizontal grid size, again due to the existence of vertical transports by deep convective systems.

These results strongly suggest that, for the results of the coarser-resolution model to be the same as those of CONTROL as far as the resolved scales are concerned, a parameterization of the combined effects of microphysics and unresolved transports needs to be included to produce the required sources. As we have seen in these figures, the required sources depend highly on the horizontal resolution of the model, as well as on the time interval for implementing the physics in the case of moist static energy. Ideally, a formulation of the model physics should automatically reproduce these dependences as it is applied to different resolutions. Conventional cloud parameterization schemes cannot do this since they assume either explicitly or implicitly that the horizontal grid size and the time interval for implementing physics are sufficiently larger and longer than the size and lifetime of individual moist-convective elements. This generates difficulties in high-resolution models, in which grid-scale and subgrid-scale moist processes are not well separable, as is typical in mesoscale models (Molinari and Dudek 1992; see also Molinari 1993; Frank 1993). The future formulation of the model physics should include these dependencies at least for high-resolution models.

f. Motivation and strategy for a new approach

Here I should emphasize that the argument presented so far in this section is not meant to be pessimistic about the future of climate modeling. Instead, I am pointing out that the problem is so challenging that multiple approaches are needed for the development of future climate models. Also, it is by no means my intention to deemphasize the progress the modeling community has made during the last decades, especially in the area of medium-range weather forecasting. Cloud parameteri-

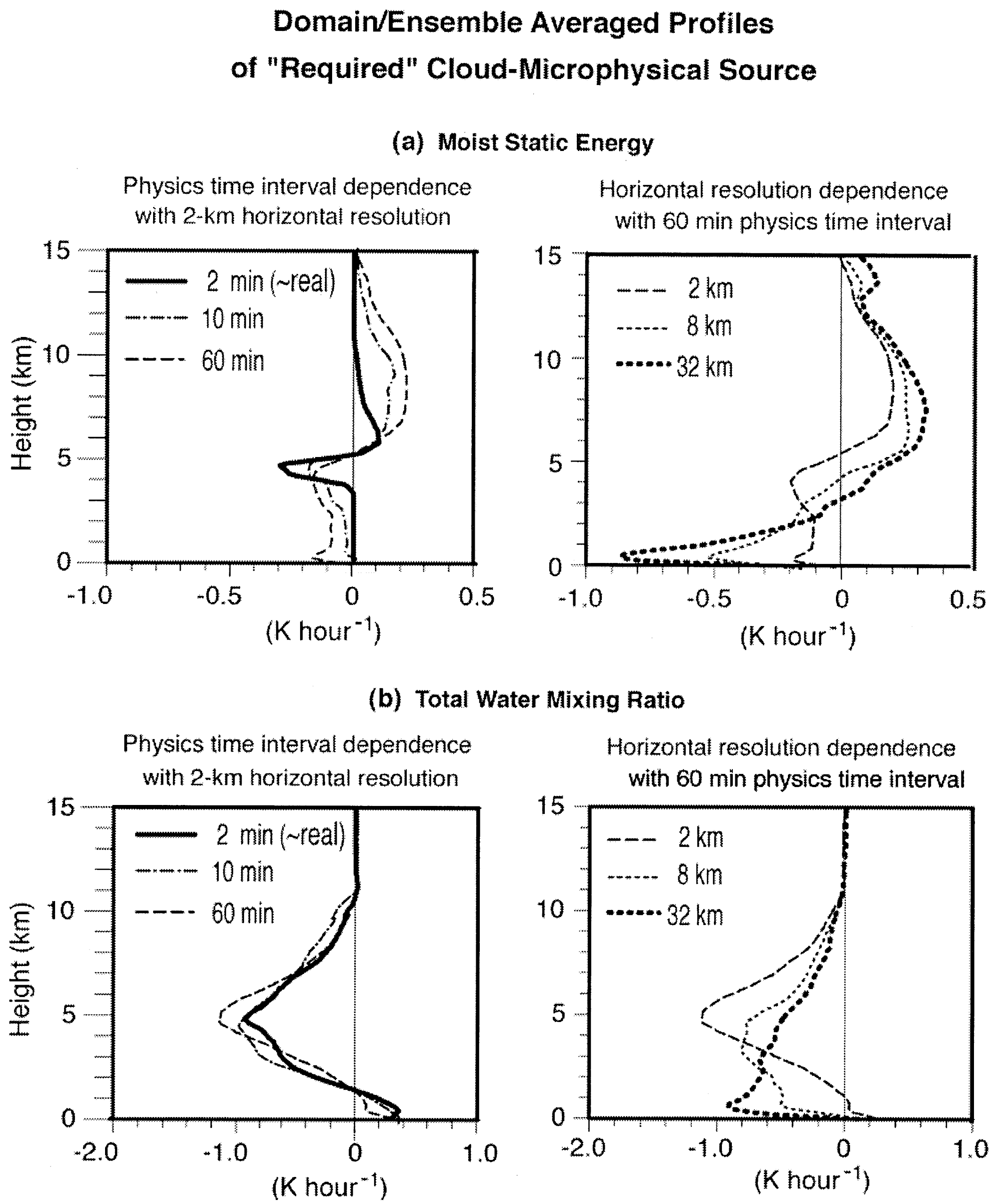

FIG. 10. (a) Domain- and ensemble-averaged profiles of the "required" source for moist static energy *due to cloud microphysics* under strong large-scale forcing over land. (left) Dependency on the physics time interval with a fixed horizontal grid size (2 km). (right) Dependency on the horizontal grid size with a fixed physics time interval (60 min). (b) Same as in (a) but for total water. Redrawn from Jung and Arakawa (2004).

zations constructed using the conventional approach are basically on the right track and can continue to produce useful results in many practical applications. Besides, they have their own scientific merits as I pointed out in section 1. Further development of cloud parameterizations is, however, now facing very difficult problems as I reviewed above, while the improvement of weather prediction and climate models cannot be delayed until all of these problems are solved. In the mean time, computer technology is rapidly advancing. Taking these together, I now feel that the time is ripe to introduce multiple approaches, including a new approach outlined below.

Let us ignore practicality for the time being. We can think of two extreme approaches in developing unified model physics that eliminate the problems discussed so far in this section. One is the "parameterize everything" approach, in which the entire spectrum of the interactions shown in Fig. 1 are parameterized in a unified way. An attempt toward this goal requires the maximum use of human brainpower. In this approach, all formulations can be written in the original continuous equa-

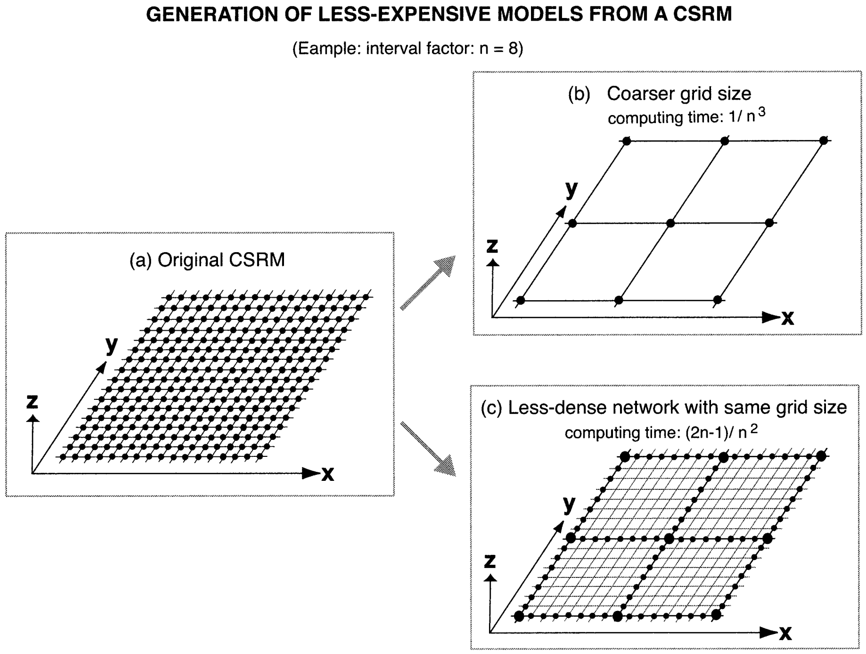

FIG. 11. (a) Horizontal grid of a 3D CSRM. (b) Same as in (a) but with a coarser grid size. (c) Same as in (a) but for a quasi-3D gridpoint network with the same grid size.

tions before discretization is introduced so that the concept of ''subgrid-scale parameterization'' is abandoned. Discretization is then introduced as an approximation in the computation. The other is the ''resolve everything'' approach, in which the entire spectrum of the interactions shown in Fig. 1 are explicitly resolved. In practice, this approach would mean covering the entire globe by a large-eddy simulation (LES) model of turbulence with detailed cloud microphysics and radiation. An attempt toward this goal requires the maximum use of computer power. The idealistic goal of the conventional approach is the former, while that of the new approach proposed here is the latter. It is important to remember that these two approaches complement each other.

While the eventual target of the latter approach mentioned above is a global LES, a global 3D CSRM can be a compromise if the physics of existing CSRMs continues to improve. A global 3D CSRM is, however, still very expensive to run and, even when it becomes possible to use such a model for some limited purposes, it is not wise to do so for all purposes. In climate studies, there are a number of issues to be studied, requiring long-term integrations or many integrations under different initial/boundary conditions, different model parameters, and different model configurations. Based on this reason, I believe that we should attempt to develop a new modeling framework that satisfies all of the following requirements:

1) it can be used as a global 3D CSRM if we wish,
2) it is flexible enough to have less expensive options, and
3) it is based on the same formulation of the model physics for all options.

In a framework that satisfies these requirements, convergence to a global 3D CSRM is guaranteed. Moreover, through satisfying requirement 3, we can better coordinate or even unify our currently diversified modeling efforts along the line of improving CSRMs. Sections 6g and 6h describe an example satisfying the three requirements mentioned above to show the technical feasibility of developing such a framework, while section 6i further discusses the merit of using such a framework.

g. *Generation of quasi-3D CSRMs from a 3D CSRM*

This section presents an example that satisfies the three requirements given above. Let us first consider the problem of generating less expensive versions of a given 3D CSRM. Figure 11a shows the horizontal grid of the original CSRM. An obvious way of deriving a less expensive version of the model is simply to increase its grid size, say, by the factor of n, as shown in Fig. 11b for the case of $n = 8$. In fact, this is a very efficient way of economizing a model since the computing time decreases with the factor $1/n^3$. (Here I have assumed that a longer time step can be used for a larger grid size maintaining the same Courant number.) It is well known, however, that the model performance rapidly degrades as n increases if cumulus parameterization is not introduced into the model (see, e.g., Weisman et al. 1997; Nasuno and Saito 2002; Jung and Arakawa 2004). Attempting to reduce this kind of errors with an improved

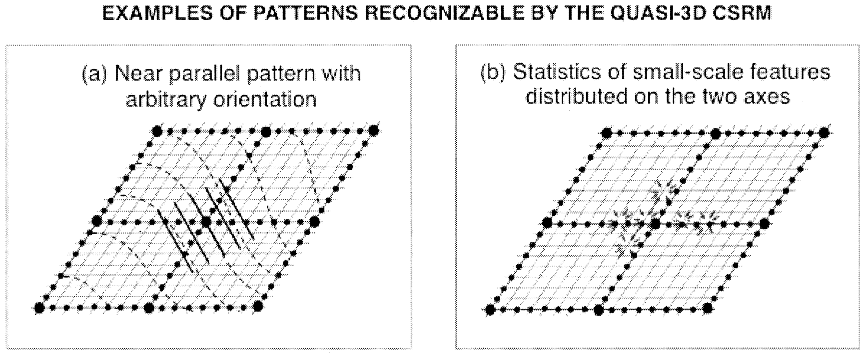

FIG. 12. Patterns that can be recognized by the quasi-3D gridpoint network.

parameterization represents the conventional approach applied to mesoscale modeling.

Another way of generating less expensive versions of the CSRM is to stay with the same grid size and the same formulation of the model physics but to use a less dense network of grid points as shown by the large and small dots in Fig. 11c. This introduces gaps in the grid system and the model cannot be 3D except at the large dots, where two axes of grid points intersect. A ‘‘quasi-3D CSRM’’ can be developed, however, by introducing bogus grid points at the intersections of the thin lines. Values at these points can be determined by a 2D regression–interpolation technique applied to the predicted values at neighboring grid points.

The important point here is that a system constructed in this way can satisfy all three of the requirements presented in the last subsection, including convergence to a global CSRM. The magnification factor n; which is now the ratio of the interval between two neighboring gridpoint axes to the grid size of the original CSRM, is an arbitrary integer, and it is guaranteed that the framework becomes closer to the original CSRM as n decreases. The system is then a mathematical approximation to the original CSRM, in which the error due to the use of a less dense network can be made arbitrarily small. This is in sharp contrast to the conventional approach, in which the original system is modified, rather than approximated, through the replacement of explicit physics by parameterized physics.

The amount of computation with a quasi-3D CSRM relative to that of the original CSRM is given by the factor $(2n - 1)/n^2$, which is approximately $2/n$ for a large n. In principle, this factor indicates only one order of magnitude saving for the case of $n = 20$ and only two orders of magnitude saving for the case of $n = 200$. (If the grid size of the original CSRM is 1 km, these cases correspond to the interval of 20 km and 200 km, respectively.) Parallelized computing, however, may substantially reduce this factor.

While values at the bogus grid points cannot give any meaningful information for small scales, a quasi-3D CSRM can recognize gross 3D structures of cloud organization within a network, such as a locally near-parallel pattern with an arbitrary orientation as shown by solid lines in Fig. 12a. The ability to represent the orientation is important especially from the point of view of vertical momentum transport by organized convection (see the nonclassical objective b5 in section 1). The pattern identified at each large dot can then be interpolated to all other points, including the bogus points, to obtain a smooth, locally parallel pattern covering the entire network as shown by the dashed lines in Fig. 12a. In addition to this pattern, the model can simultaneously recognize small-scale features distributed along the gridpoint axes, as shown in Fig. 12b, through the deviation of predicted values from the smooth pattern obtained by the regression–interpolation technique. Recognizing these features is also important from the point of view of parameterization as long as they statistically represent small-scale features that are distributed within the network. This last point is in the same spirit as Grabowski's cloud resolving convective parameterization, which I will discuss in the next subsection.

h. Quasi-3D multiscale modeling framework

If we have a quasi-3D CSRM covering the entire globe, in principle it can replace GCMs entirely. It is better, however, to use it as a parameterization in a GCM by constructing a coupled GCM–quasi-3D CSRM system as shown in Fig. 13d. This is because GCMs have more uniform gridpoint distributions for large scales and it is good to maintain compatibility with GCMs with conventional parameterizations. In the coupled system, which we call the quasi-3D multiscale modeling framework (MMF), the interval between two neighboring gridpoint axes can be (but does not have to be) the same as the grid size of the GCM. Then, as the GCM grid size becomes small, the quasi-3D MMF converges to the original 3D CSRM. Even with larger GCM grid sizes, the quasi-3D MMF can explicitly simulate interactions between three categories of scales: scales resolved by the GCM, scales of cloud organizations in a GCM grid box as schematically shown in Fig. 12a, and

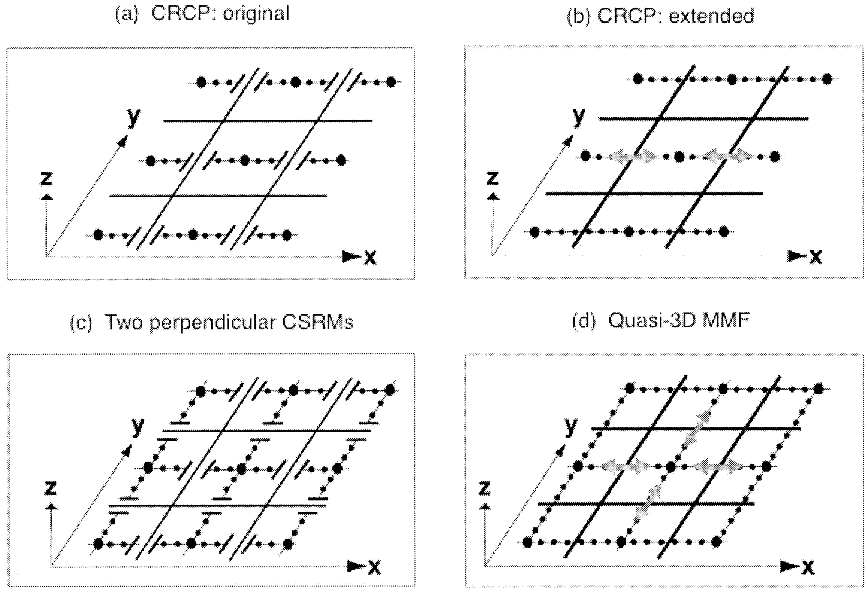

FIG. 13. (a) An illustration of the framework used in Grabowski's CRCP. (b) Same as in (a) but for a revised version in which a GCM and a 2D CRM are coupled at the large dots for scalar variables and at the arrows for velocity components. (c) Same as in (a) but for a revised version in which two perpendicular CRMs are embedded into a GCM grid box. (d) An illustration of a quasi-3D MMF.

statistical samples of small-scale features as schematically shown in Fig. 12b.

Recently, an innovative approach to cumulus parameterization called cloud resolving convective parameterization (CRCP) was proposed by Grabowski (Grabowski and Smolarkiewicz 1999; Grabowski 2001). In this approach, a 2D CSRM with cyclic horizontal boundary conditions is applied to each vertical column of the GCM as shown in Fig. 13a. At the large dots in the figure, the GCM forces the CSRM through large-scale advective tendencies and the CSRM forces the GCM through updated domain-averaged thermodynamic variables. The velocity components of the GCM and the domain-averaged velocity components of the CSRM are nudged toward each other also at this point. The approach is still called parameterization because the GCM recognizes only the domain-averaged values of the CSRM fields. Yet the approach is promising for future climate models because it will eventually play the role of a physics coupler that enables us to treat all interacting physical processes within a unified framework. In this way, when the CSRM includes all of the necessary components of the model physics, the major conceptual problem in the conventional parameterizations discussed in section 6b can be essentially eliminated. From this point of view, the approach represents a milestone in the history of developing model physics.

Khairoutdinov and Randall (2001; see also Randall et al. 2003b) applied CRCP, which they called superparameterization, to the climate simulation problem and presented very encouraging results. The results are very encouraging especially because they are obtained with almost no tuning. The simulation needed two orders of magnitude more computer time than with conventional parameterization even though the CSRM used was highly simplified. There are a number of problems to be overcome, however, before the merit of the approach can be fully appreciated. In particular, we note that CRCP as originally proposed has the following problems:

1) As Grabowski (2001) pointed out, CSRMs for neighboring GCM grid boxes can communicate with each other only through the GCM due to the use of a cyclic horizontal boundary condition.
2) Also due to the use of a cyclic horizontal boundary condition, each CSRM converges toward a 1D cloud model with no vertical velocity as the GCM grid size approaches the CSRM grid size.
3) The two-dimensionality of the CSRM on which the superparameterization is based is obviously an artificial constraint.

To relax the constraints mentioned in points 1 and 2 above, one can extend the 2D CSRMs by replacing the cyclic boundary condition with a coupling of the velocity components with the GCM at the arrow points in Fig. 13b. To relax the 2D constraint mentioned in point 3 above, on the other hand, one can use two perpendicular 2D CSRMs as shown in Fig. 13c. At the large dot points in the figure where the two 2D CSRMs intersect, we can do 3D calculations by adding terms representing interactions between the two directions, which are missing in the original 2D CSRMs. The two CSRMs are still confined in a single GCM grid box in this version.

At this point we recognize that the quasi-3D MMF shown in Fig. 13d is a combination of Figs. 13b and 13c. In this system, the GCM and the quasi-3D CSRM communicate with each other by sharing approximately the same horizontal fluxes at the arrow points while sharing approximately the same mean thermodynamic variables at the large dot points. In practice, these couplings can be achieved through a proper nudging of the variables between the two models. A forthcoming paper by Jung and Arakawa (manuscript submitted to *Mon. Wea. Rev.*) investigates the sensitivity of the results to different ways and different strengths of coupling in an idealized context. The best algorithm for the MMF is, however, still to be developed.

i. Improvement of the MMF and its verification against observations

There are two sources of errors for the quasi-3D MMF: one is due to the existence of gaps in the gridpoint network and the other is due to the deficiency of the CSRM itself. In the sense that it is a consequence of using a coarser overall resolution, the former can be regarded as a kind of truncation error. As in the case of convergent finite-difference approximations based on a regular grid, the truncation error of the MMF can also be made arbitrarily small by using a denser gridpoint network. There is, however, the technical issue of designing a regression–interpolation algorithm. Optimum methods of coupling the quasi-3D CSRM with the GCM must also be developed. An important point here is that these issues are mathematical in nature, not a mixture of mathematical and physical issues as in coarse-resolution models with conventional parameterizations of the physics.

As pointed out above, the deficiency of the CSRM represents the second source of error in the MMF framework. There are a number of issues to be seriously considered for the improvement of CSRMs themselves. An important benefit of the MMF approach is that it provides a link between the low-resolution and high-resolution model physics discussed in section 6c so that we can better coordinate or even unify our currently diversified modeling efforts along the line of improving CSRMs. For conventional climate models, testing a specific component of model physics, such as cumulus parameterization, is difficult and can even be deceiving due to spurious or incomplete interactions with other processes (see section 6b). The merits of testing parameterization schemes offline are also limited because ''large scale'' advective processes must be prescribed based on an artificial separation of scales. Verification of an MMF-based climate model against observations, on the other hand, can be done on multiple scales: verification of GCM results against large-scale observations and CSRM results against local and instantaneous observations, with explicitly simulated links between the two. This is a much more constructive way of verifying climate models, and in this way the observation and modeling communities can become much closer than they are now.

7. Summary and conclusions

A review of the cumulus parameterization problem is presented with an emphasis on its conceptual aspects, covering the nature of the problem, the history of the underlying ideas, the current trend in the logical structure of parameterization schemes, major conceptual problems existing at present, and possible directions and approaches in developing model physics for future climate models.

It is important to keep it in mind that the need for parameterizing physical processes is not limited to ''numerical'' models. Formulating the statistical behavior of small-scale processes is needed for understanding large-scale phenomena regardless of whether we are using numerical, theoretical, or conceptual models. Such formulations inevitably require simplifications, including various levels of ''parameterizations'' either explicitly or implicitly. Parameterizations thus have their own scientific merits. These merits are, however, rather limited at present because cumulus parameterization is still a very young subject. Besides the basic question of how to pose the problem, there are a number of uncertainties in modeling clouds and their associated processes. Even more seriously, we do not have a sufficiently general framework, such as a unified cloud system model, for implementing detailed formulations of these processes. Improving this situation is a central task of the current phase in the history of numerical modeling of the atmosphere.

During the last decades, many cumulus parameterization schemes have been constructed and a number of papers have been published that compared GCM results obtained with different cumulus parameterizations. The existence of these papers, however, by itself indicates that the results are ''comparable'' to start with, as far as the mean fields are concerned, in spite of the diversity in basic reasoning used in constructing the schemes. This is because all cumulus parameterization schemes surviving at present can produce a negative feedback to large-scale destabilization, which we call adjustment, in one way or another. When the SST is fixed, the model is subject to another negative feedback by adjusting the air temperature near the surface to the prescribed values of the SST. These two negative feedbacks combined tend to hide model deficiencies and model differences. Due to this apparent insensitivity of the results to basic reasoning, cumulus parameterization did not appear to be a scientifically demanding problem.

Clearly, we cannot rely on this kind of ''luck'' in developing coupled atmosphere–ocean GCMs and certainly not in developing future climate models. Accordingly, the objectives of cumulus parameterization must be drastically expanded from the classical objective to nonclassical objectives, which include 1) mass transport by cumulus convection, 2) generation of liquid and ice phases of water, 3) interactions with the subcloud layer, 4) interactions with radiation, and 5) mechanical interactions with mean flow. When all of these objectives are met, we will have unified almost the entire model physics. This is obviously too ambitious a task to be called parameterization. Only after a successful completion of this task, however, will the atmospheric sciences become a truly unified science.

After these introductory comments in section 1, section 2 defines the cumulus parameterization problem as *the problem of formulating the statistical effects of moist convection to obtain a closed system for predicting weather and climate.* The logical structure of the parameterization problem as a closure problem is then discussed, including three requirements that should be satisfied by the principal closure of a parameterization. Section 2 further points out that, while budget considerations are useful for many purposes, such considerations alone can easily mislead our judgment of cause and effect, hiding even the simplest fact that advection never creates a new maximum or minimum.

The material presented in section 3–6 is arranged roughly in a historical order. There have been decades of controversies in posing the cumulus parameterization problem since its introduction in the early 1960s. The existence of such controversies is especially apparent in understanding the role of cumulus convection in tropical cyclone development. There is no question about the importance of the surface heat flux in this problem. The real question is whether the surface heat flux can produce a positive feedback leading to a new instability or if it only reduces or eliminates a negative feedback on the existing instability. Without the surface heat flux, both cyclone-scale and cumulus-scale motions, and, therefore, the total motion, simply redistribute the equivalent potential temperature, θ_e, without creating new values. Conditional instability (of any kind) alone, therefore, which simply converts the vertical variation of θ_e to its horizontal variation, cannot produce the development of the combined system including the formation and intensification of a warm core. This is true regardless of how cyclone-scale and convective-scale motions might cooperate. The concept of CISK as a cooperative instability, therefore, can hardly be justified.

The motivation behind the review presented in section 4 on selected prototype schemes comes from the apparent insensitivity of model results to the basic reasoning used in constructing the schemes. It is concluded that the performance of some parameterization schemes can be better understood if one is not bound by their authors' justifications. The review presented in this section suggests that all surviving schemes, including Kuo's scheme, can be interpreted as adjustment schemes. When we wish to compare different parameterization schemes, it is much more meaningful to interpret the schemes based on the same logical framework, that of adjustment schemes for example. In this way the comparison can give more specific information on the differences between the schemes.

Section 5 first points out that major controversies in posing the cumulus parameterization problem have been essentially dissipated and the concept of quasi equilibrium seems to be more widely accepted among existing schemes as the basis for the closure. The quasi-equilibrium concept also serves as a ''closure'' in the way we think, as emphasized by Emanuel (2000).

In the sense that it has an important conceptual content, the idea of quasi-equilibrium in the cumulus parameterization problem is analogous to quasigeostrophic dynamics for large-scale motions in the atmosphere and oceans. Section 5 then points out that the current trend in posing the cumulus parameterization is from deterministic and diagnostic closures, including instantaneous adjustment, to prognostic or stochastic closures, including relaxed or triggered adjustment. The trend from diagnostic closures to prognostic closures is formally analogous to the transition of dynamics in numerical weather prediction models from the quasigeostrophic equations to the primitive equations and from the primitive equations to nonhydrostatic equations. In the case of the cumulus parameterization problem, however, we are not simply going back to known equations. A number of problems must be solved before the merits of this trend can be fully utilized. The trend is also formally analogous to the generalization of the K theory for turbulence to higher-order turbulence closure models. In these models, however, the quasi-isotropic and quasi-diffusive nature of turbulence is still assumed in a relaxed way, while cumulus convection is inherently anisotropic, penetrative rather than diffusive, and the parcel method is still a useful starting point for inferring the vertical structure of clouds. Section 5 further points out that a statistical theory that governs the time evolution of the number of clouds is badly needed. In closing section 5, the problems in interpreting observations from the point of view of quasi equilibrium are discussed.

Finally, section 6 points out that the conventional approach of formulating model physics is now facing very difficult problems, and emphasizes that multiple approaches are needed for developing future climate models. The lack of a sufficiently general framework for implementing detailed formulations of individual processes involves even conceptual problems in conventional model physics: *artificial separation of processes* and *artificial separation of scales.*

In the past, our modeling effort has been spent mainly on coupling individual physical processes with the dynamics core more or less independently from the others. This has resulted in a modular structure of models, in which different physical processes interact mainly through model's prognostic variables, missing most of the small-scale interactions between the processes. One of the future emphases in climate modeling should be on a formulation of the entire spectrum of these interactions.

Artificial separation of scales occurs as truncation introduced for computational purposes influences the model physics by separating the spectrum of the processes into resolved and unresolved scales. Existing models use either of the following two kinds of distinctly different model physics: low-resolution model physics, in which moist-convective processes are parameterized, and high-resolution model physics, in which moist convective processes are explicitly treated at least partially. Each of these model physics is designed and tuned for a certain range of resolutions, not to formulate physics as a function of resolution covering a broad spectrum. This raises the question of convergence for the model physics. Justification of a discrete model relies on the hope that its solution converges to the solution of the original system as the resolution is refined. The convergence problem for model physics in conventional models is, however, different from the standard convergence problem in numerical analysis since the governing equation is modified, rather than approximated, through the use of parameterized expressions in the model physics.

Section 6 further points out that the source terms in the model's governing equation ''required'' for accurately predicting the mean field are not a time–space–ensemble average of local and instantaneous ''real'' sources, and their difference depends on model resolution. Similarly, the required cloud microphysical source is not a time/space/ensemble average of real cloud microphysical source. Results obtained from a systematic analysis of required sources and their resolution dependencies inferred from nonhydrostatic model experiments are presented. It is shown that required sources depend highly on the horizontal resolution of the model, as well as on the time interval for implementing the physics. Ideally, a formulation of the model physics should automatically reproduce these dependences as it is applied to different resolutions. Conventional cloud parameterization schemes cannot do this since they assume either explicitly or implicitly that the horizontal grid size and the time interval for implementing the physics are sufficiently larger and longer than the size and lifetime of individual moist-convective systems.

Section 6 then points out that the time is now ripe to introduce multiple approaches, including the development of a new framework that can be used as a global 3D cloud system resolving model (CSRM) while it is flexible enough to include lessexpensive options with the same formulation of the model physics. Section 6 further presents an example of such a framework called the quasi-3D multiscale modeling framework (MMF), which extends the idea proposed by Grabowski (Grabowski and Smolarkiewicz 1999; Grabowski 2001). MMF can simulate interactions between the following three categories of scales: scales resolved by the GCM, scales of cloud organizations within a GCM grid box, and statistical samples of small-scale features. Most importantly, convergence to a global 3D CSRM as the GCM grid size becomes small is guaranteed with the framework.

To conclude, section 6 points out that there are a number of issues to be seriously considered for the improvement of CSRMs themselves. A real benefit of the MMF approach is to provide a link between the low-resolution and high-resolution models discussed in section 6c so that we can better coordinate or even unify our currently diversified modeling efforts along the line of improving CSRMs. In particular, verification of a MMF-based climate model against observations can be done on multiple scales with explicitly simulated links between them: verification of GCM results against large-scale observations and CSRM results against local and instantaneous observations. This is a much more constructive way of verifying climate models, and in this way the observation and modeling communities can become much closer than they are now.

In concluding this paper, I reemphasize the importance of following multiple approaches for further developing the subject. This is analogous to the importance of using a hierarchy of models in climate dynamics (Ghil and Robertson 2000). MMF discussed in section 6 is a brute-force approach, but will be found to be useful even scientifically, as GCMs are in climate studies.

Acknowledgments. The author wishes to thank Leo Donner, Kerry Emanuel, Joon-Hee Jung, Brian Mapes, Chin-Hoh Moeng, David Randall, Wayne Schubert, Bjorn Stevens, Kuan-Man Xu, Kenichi Yano, and three anonymous reviewers, who carefully read the original manuscript and gave me a number of useful suggestions for its revision. This review is based on invited papers I presented at the Cumulus Parameterization Workshop (Tao et al. 2003) at NASA GSFC, December 2001, and the Workshop on Future Cloud Parameterizations at Kauai, Hawaii, May 2003. I thank the organizers of these workshops for giving me opportunities to organize my thoughts on this subject. This work is supported by NSF Grant ATM 00-71345 and NASA Grant NNG04GA76G.

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