Reiser, H. (1962). Baroclinic forecasts with the primitive equations. *In* "Proc. International Symposium on Numerical Weather Prediction in Tokyo," Nov. 1960, pp. 77–84. Meteor. Soc. Japan.

Rosenthal, S. L. (1970a). Experiments with a numerical model of tropical cyclone development: Some effects of radial resolution. *Mon. Wea. Rev.* **98,** 106–120.

Rosenthal, S. L. (1970b). A circularly symmetric primitive equation model of tropical cyclone development containing an explicit water vapor cycle. *Mon. Wea. Rev.* **98,** 643–663.

Shuman, F. G. (1962). Numerical experiments with the primitive equations. *In* "Proc. International Symposium on Numerical Weather Prediction in Tokyo," Nov. 1960, pp. 85–107. Meteor. Soc. Japan.

Shuman, F. G., and J. B. Hovermale (1968). An operational six-layer primitive equation model. *J. Appl. Meteor.* **7,** 525–547.

Smagorinsky, J. (1958). On the numerical integration of the primitive equations of motion for baroclinic flow in a closed region. *Mon. Wea. Rev.* **86,** 457–466.

Smagorinsky, J. (1963). General circulation experiments with the primitive equations. I. The basic experiment. *Mon. Wea. Rev.* **91,** 99–164.

Smagorinsky, J., S. Manabe, and J. L. Holloway, Jr. (1965). Numerical results from a nine-level general circulation model of the atmosphere. *Mon. Wea. Rev.* **93,** 727–768.

Smith, R. K. (1997a). On the theory of CISK. *Quart. J. Roy. Meteor. Soc.* **123,** 407–418.

Smith, R. K. (ed.) (1997b). "The Physics and Parameterization of Moist Atmospheric Convection." NATO ASI Series C. Math. and Phys. Sci. Vol. 506. Kluwer Academic Publishers, Boston.

Sundqvist, H. (1970a). Numerical simulation of the development of tropical cyclones with a ten-level model. Part I. *Tellus* **22,** 359–390.

Sundqvist, H. (1970b). Numerical simulation of the development of tropical cyclones with a ten-level model. Part II. *Tellus* **22,** 504–510.

Syōno, S. (1951). On the structure of atmospheric vortices. *J. Meteor.* **8,** 103–110.

Syōno, S. (1962). A numerical experiment of the formation of tropical cyclone. *In* "Proc. International Symposium on Numerical Weather Prediction in Tokyo," Nov.1960, pp. 405–418. Meteor. Soc. Japan.

Syōno, S., and M. Yamasaki (1966). Stability of symmetrical motions driven by latent heat release by cumulus convection under the existence of surface friction. *J. Meteor. Soc. Japan* **44,** 353–375.

Syōno, S., Y. Ogura, K. Gambo, and A. Kasahara (1951). On the negative vorticity in a typhoon. *J. Meteor. Soc. Japan* **29,** 397–415.

Tsutsui, J., and A. Kasahara (1996). Simulated tropical cyclones using the National Center for Atmospheric Research community climate model. *J. Geophys. Res.* **101,** D10, 15,013–15,032.

Yamasaki, M. (1968a). Numerical simulation of tropical cyclone development with the use of primitive equations. *J. Meteor. Soc. Japan* **46,** 178–201.

Yamasaki, M. (1968b). A tropical cyclone model with parameterized vertical partition of released latent heat. *J. Meteor. Soc. Japan* **46,** 202–214.

Yamasaki, M. (1968c). Detailed analysis of a tropical cyclone simulated with a 13-layer model. *Papers Met. Geophys.* **19,** 559–585.

Yanai, M. (1964). Formation of tropical cyclones. *Rev. Geophys.* **2,** 367–414.

Yanai, M., S. Esbensen, and J.-H. Chu (1973). Determination of bulk properties of tropical cloud clusters from large-scale heat and moisture budgets. *J. Atmos. Sci.* **30,** 611–627.

Zhang, G. J., and N. A. McFarlane (1995). Sensitivity of climate simulations to the parameterization of cumulus convection in the Canadian Climate Centre general circulation model. *Atmos. Ocean* **33,** 407–446.

Chapter 8

Quasi-Equilibrium Thinking

Kerry Emanuel
Program in Atmospheres, Oceans and Climate
Massachusetts Institute of Technology
Cambridge, Massachusetts

I. INTRODUCTION

Statistical equilibrium thinking is natural to us in most contexts. In fluid problems for which the scales of interest are much larger than the mean free path between molecular collisions, we are comfortable dealing with the statistics of molecules rather than their individual dynamics, so that concepts such as pressure and temperature are natural and well developed. The great conceptual simplification brought about by statistical thinking arises from explicit assumptions that the space and time scales we are interested in are large compared to those characterizing the elementary particles or processes we are averaging over.

In large-scale geophysical fluid dynamics, we have become comfortable with a variety of scaling assumptions that greatly simplify thinking about the dynamics and formulating them in numerical models. Among the first approximations we become acquainted with are the hydrostatic and anelastic approximations, which filter out sound waves. It is important to remember here that these approximations are by no means equivalent to pretending that sound waves do not exist or that they are not important; rather, we

General Circulation Model Development

simply assume that adjustments brought about by them are so fast compared to weather systems that we may think of them as occurring infinitely fast. If we had to consider flows with speeds close to or exceeding the speed of sound, we would be forced to abandon these approximations and the special kind of thinking that goes with them. Similarly, for flows of small Rossby number, we can stop thinking about gravity waves and take it for granted that they bring about adjustments that are very fast compared to the time scale of weather systems of synoptic scale and larger. Once again, this mode of thinking should not be regarded as pretending that gravity waves do not exist; rather, we approximate their adjustment time scale as zero. The enormous simplification that this and a few other assumptions bring about is beautifully entailed in "PV thinking," as described by Hoskins *et al.* (1985).

Statistical equilibrium ideas play a crucial role in thinking about and accounting for turbulence at all scales. Almost all successful theories and parameterizations of three-dimensional turbulence rely on the idea that kinetic energy is cascaded so rapidly to small scales, where it is dissipated, that generation and dissipation are nearly in equilibrium. Even the so-called "one-and-a-half order" closure, popular in a variety of applications, allows for only small departures from this kind of equilibrium. Turbulence kinetic energy can respond with a small time lag to changes in generation and dissipation, and can be advected away from regions where it is generated.

Statistical equilibrium thinking is also the natural mode of thinking about ordinary dry convection. We regard the intensity of boundary layer convection as a statistical quantity that is directly related to the intensity of the surface heat flux. To a first approximation, we think of such convection establishing on a very short time scale a dry adiabatic lapse rate through the depth of the convecting layer. If we were asked why a circulation develops between a dry, sandy field and an adjacent irrigated pasture, we reply that the air over the pasture is cooler, owing to evaporation. We take it for granted that small-scale convection over the dry field distributes heat through the boundary layer on a short time scale. Few would state that the field-scale circulation arises from a spontaneous organization of small-scale convective elements.

In 1974, Arakawa and Schubert formally introduced their application of statistical equilibrium to wet convection, using virtually the same general idea that had met with some success in ordinary three-dimensional turbulence: the idea that generation and dissipation of turbulence kinetic energy are nearly in equilibrium. This followed more than a decade of false starts by quite a few distinguished researchers, grappling with the construction of an appropriate closure for wet convection. These failed largely because they did not regard convection as an equilibrium process, or because they assumed that water vapor, rather than energy, is the quantity in equilibrium. Among the few physically consistent representations of convection that appeared before this time was moist convective adjustment (Manabe *et al.*, 1965), which, although not based on statistical equilibrium, acts in such a way as to preserve it.

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. It is still very common to hear statements to the effect that latent heating drives tropical circulations, or that such circulations arise from a cooperative interaction among cumulus clouds. In the following sections, I attempt to show why such statements are inconsistent with the notion of quasi-equilibrium and to trace the history of thinking about the interaction of cumulus convection with large-scale circulations.

II. IS "LATENT HEATING" A USEFUL CONCEPT?

We are all taught that the condensation of water vapor releases a comparatively large quantity of heat to the air in which the condensate is suspended, and we are used to thinking of this just like any other heat source, like radiation, for example. The problem with this way of thinking is that it fails to recognize and take advantage of the fact that virtually all condensation in the atmosphere is very nearly reversible, and so may be usefully incorporated into the definition of the entropy of a system consisting of dry air, water vapor, and condensed water. (Of course, the fallout of condensate is irreversible, but that is another matter. Note also that in contrast to condensation, freezing is not usually reversible because it occurs at temperatures well below 0°C.) In such a system, there is no "latent heating"; phase changes between vapor and liquid droplets hardly affect the entropy of the system.

The distinction between external heating and internal rearrangements of the terms that comprise the specific entropy is far from academic. For example, external heating applied to rotating, stratified fluid will result in a local increase in the temperature of the fluid in the vicinity of the source. But the presence of deep, precipitating cumulus convection in a rotating, stratified fluid may very well be associated with local reduction of temperature. In the first case, the correlation between heating and temperature is virtually guaranteed to be positive, while in the second case it is quite possible for the "latent heating" to be negatively correlated with temperature, resulting in a reduction of kinetic energy. Thus the "organization of convection" need not lead to the amplification of a disturbance. Despite this, the idea that certain types of tropical disturbance arise from an organization of convection persists.

We illustrate the fallacy of regarding latent heating as the cause of phenomena associated with convection by a few examples.

A. Dry Convective Turbulence

It is helpful to start out with a problem uncomplicated by the presence of moisture. One relatively simple paradigm, a version of which dates back to Prandtl (1925), consists of a shallow layer of dry soil continuously heated by a constant imposed solar radiation, underlying a fluid cooled through a finite depth by a constant imposed radiative cooling. In equilibrium, the incoming solar radiation at the top of the system matches the total outgoing radiation. But conduction of heat from the soil to the overlying fluid will destabilize the latter, resulting in convection. In statistical equilibrium, the convergence of the convective heat flux matches the radiative cooling of the fluid. This paradigm is illustrated in Fig. 1.

Now consider the entropy budget of the system. By dividing the first law of thermodynamics through by temperature, T, one obtains

$$C_p\left(\frac{d\ln(T)}{dt}\right) - R\left(\frac{d\ln(p)}{dt}\right) = \frac{Q_{\rm rad}}{T} + \dot{s}_{\rm irr}, \tag{1}$$

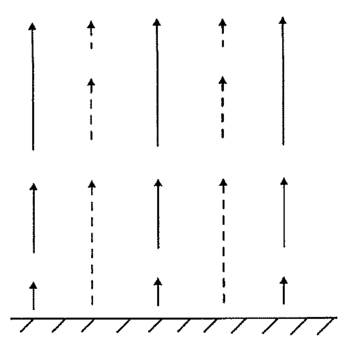

Figure 1 Radiative-convective equilibrium over dry land. Solid arrows denote long-wave radiative flux, which increases upward; dashed arrows denote turbulent convective heat flux, which decreases upward. There is no net flux divergence except at the surface, where it is balanced by absorption of solar radiation.

where C_p is the heat capacity at constant pressure, R is the gas constant for dry air, p is pressure, $Q_{\rm rad}$ is the radiative (and conductive) heating, and $\dot{s}_{\rm irr}$ represents various irreversible entropy sources. We consider the system to be closed in mass, so that integrating Eq. (1) over the entire system and over a long enough time to average out the statistical fluctuations, we get

$$\int \dot{s}_{\rm irr} = -\int \frac{Q_{\rm rad}}{T}, \tag{2}$$

where the integral is over the entire system and time. Since, in equilibrium, the surface heating balances the net atmospheric cooling, we can express Eq. (2) as

$$\int \dot{s}_{\rm irr} = F_s\left(\frac{1}{\overline{T}} - \frac{1}{T_s}\right), \tag{3}$$

where F_s is the net radiative flux at the surface, T_s is the surface temperature, and $\overline{T}$ is the average temperature at which radiative cooling occurs. Now if we assume that dissipation of kinetic energy is the dominant irreversible entropy source, then the left side of Eq. (3) is just the system integral of the dissipative heating divided by temperature. Since, in equilibrium, dissipation of kinetic energy must equal the rate of conversion of potential energy to kinetic energy, we can write Eq. (3) as

$$\left(\frac{1}{T_{\rm diss}}\right)\int \overline{w'B'} = F_s\left(\frac{1}{\overline{T}} - \frac{1}{T_s}\right), \tag{4}$$

where $\overline{w'B'}$ is the buoyancy flux, which is also the rate of conversion of potential to kinetic energy, and $T_{\rm diss}$ is the mean temperature at which kinetic energy is dissipated. Expression (4) tells us what the integrated buoyancy flux is as a function of the energy input to the system and something like a thermodynamic efficiency. Given that the temperature lapse rate is not likely to be too far off the dry adiabatic lapse rate, a very good estimate can be made of the mean temperature $\overline{T}$. The mean temperature at which kinetic energy is dissipated, $T_{\rm diss}$, is not as easy to estimate, but because it appears only as an absolute value, errors in its estimate will not have a serious effect on the evaluation of Eq. (4). Thus the energy-entropy method yields an appropriate scale for the buoyancy flux in the system. This scale is proportional to the radiation absorbed by the surface and the difference between the surface temperature and a mean temperature of the free atmosphere. We can think of the convection as a heat engine, converting the absorbed heating into mechanical work with an efficiency proportional to the difference between the input and output temperatures. The engine does no work on its environment; instead, the mechanical energy is dissipated and locally turned back into enthalpy.

Having described one aspect of the dry convection problem, let's apply the same methods to moist convection.

B. Moist Convective Turbulence: The Naive Approach

We use the same paradigm for moist convection, by replacing the dry soil used above with a thin layer of water. To make life simple, we assume that all of the net incoming radiation at the surface is balanced by evaporation, neglecting the sensible component of the turbulent surface enthalpy flux. We allow the resulting moist convective clouds to precipitate, so we expect to see tall cumulonimbi separated by regions of clear, subsiding air. In spite of the possibly impressive appearance of such clouds, we continue to treat the convection statistically. The general picture is illustrated in Fig. 2.

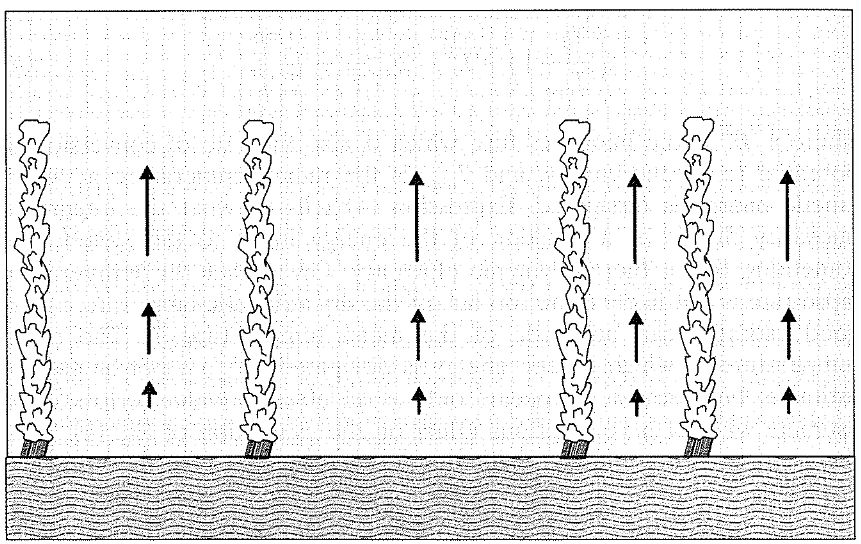

Figure 2 Radiative-convective equilibrium over a water surface. Arrows denote long-wave radiative flux.

Here we are deliberately going to engage in sloppy thermodynamics, following the habits of many large-scale dynamists when they try to do thermodynamics. In particular, we forget about the dependencies of heat capacities and gas constants on water content and do not bother to distinguish between total pressure and the partial pressure of dry air. Following the same procedure as in the previous subsection, we get, from the first law,

$$C_p\left(\frac{d\ln(T)}{dt}\right) - R\left(\frac{d\ln(p)}{dt}\right) = -\frac{L_v}{T}\left(\frac{dq}{dt}\right) + \frac{Q_{rad}}{T} + \dot{s}_{irr}, \qquad (5)$$

where L_v is the latent heat of vaporization and q is the specific humidity. The first term on the right side is the latent heating term. Once again, we integrate this over the system, conserving mass, to get

$$\int \dot{s}_{irr} = -\int\left(\frac{Q_{rad}}{T}\right) + \int\frac{L_v}{T}\left(\frac{dq}{dt}\right). \qquad (6)$$

Now we notice that, owing to the assumption that all of the absorbed solar radiation is compensated for by evaporation, the terms on the right side of Eq. (6) cancel when integrated through the thin layer of water. What we are left with is

$$\int \dot{s}_{irr} = -\int\left(\frac{Q_{cool}}{T}\right) + \int\frac{L_v}{T}\left(\frac{dq}{dt}\right)_{cloud}, \qquad (7)$$

where the remaining terms on the right are the radiative cooling of the atmosphere and the latent heating inside clouds. Inside the clouds, the latent heat release shows up as an increase of potential temperature, so that

$$-\frac{L_v}{T}\left(\frac{dq}{dt}\right) \simeq \frac{C_p}{\theta}\left(\frac{d\theta}{dt}\right),$$

where θ is the potential temperature. Outside the clouds, the radiative cooling causes a decrease in potential temperature:

$$Q_{cool} = -C_p\frac{T}{\theta}\left(\frac{d\theta}{dt}\right).$$

One can see that the two terms on the right side of Eq. (7) cancel, leaving us with no irreversible entropy production. We have gotten nowhere, except to show that radiative cooling is balanced by radiative heating. Note also that, unlike the dry problem, the surface temperature vanished and plays no role. What happened?

C. Moist Convective Turbulence: Dotting the *i*'s

Let's start over again, this time being careful with the thermodynamics. We account for the effect of water substance on heat capacities and gas constants, and we are careful to separate the total pressure into the partial pressure of dry air, p_d, and the partial pressure of water vapor (or "vapor pressure"), e. Instead of Eq. (5), we get (see Emanuel, 1994, for a derivation)

$$\left(C_{pd}(1-q_t)+C_l q_t\right)\left(\frac{d\ln(T)}{dt}\right)-R_d(1-q_t)\left(\frac{d\ln(p_d)}{dt}\right)$$
$$=-\frac{1}{T}\left(\frac{dL_v q}{dt}\right)+qR_v\left(\frac{d\ln(e)}{dt}\right)+\frac{Q_{rad}}{T}+\dot{s}_{irr}, \tag{8}$$

where C_{pd} is the heat capacity at constant pressure of dry air, C_l is the heat capacity of liquid water, q_t is the total (condensed plus vapor phase) specific water content, R_d is the gas constant for dry air, and R_v is the gas constant for water vapor. Notice that, in addition to the modifications of the effective heat capacities and gas constants, there is an extra term on the right side of Eq. (8) that we neglected in Eq. (5): the part of the work done by expansion against the vapor pressure. This term does not integrate to zero through a closed system, owing to the variability of q. We can also re-express the latent heating term:

$$\frac{1}{T}\left(\frac{dL_v q}{dt}\right)=\frac{d}{dt}\left(\frac{L_v q}{T}\right)+\frac{L_v q}{T^2}\left(\frac{dT}{dt}\right). \tag{9}$$

But, by the Clausius–Clapeyron equation (e.g., see Emanuel, 1994),

$$\frac{L_v q}{T^2}\left(\frac{dT}{dt}\right)=R_v q\left(\frac{d\ln(e^*)}{dt}\right), \tag{10}$$

where e^* is the saturation vapor pressure. We now combine Eqs. (9) and (10), substitute the result into Eq. (8), and integrate over the system as before. In doing so, we note that, because of fallout of precipitation, q_t is not conserved following the motion of the air and this results in some additional, irreversible contributions to entropy production. Using some

integrations by parts, we get

$$\int \dot{s}_{irr}=-\int\frac{Q_{rad}}{T}+R_v\ln(\mathscr{H})\left(\frac{dq}{dt}\right), \tag{11}$$

where $\mathscr{H}$ is the relative humidity, $\equiv e/e^*$. The last term in Eq. (11) is negative definite because the vapor content can only increase by evaporation into subsaturated air; condensation always occurs with $\mathscr{H}=1$. Therefore, it belongs on the left side of the equation, as part of the irreversible entropy production term.

What happened to the latent heating term? It canceled with a term we left out when doing things the sloppy way—the work against the vapor pressure. There is no contribution of latent heating to mechanical energy production when the thermodynamics is done properly. What we are left with is an equation identical in form to Eq. (3), except that there are more contributions to the irreversible entropy production. [A relation like that of Eq. (3) was first derived for the case of moist convection by Rennó and Ingersoll, 1996.] These include mixing of moist and dry air, evaporation of rain and surface water into subsaturated air, and frictional dissipation owing to falling rain. A complete scale analysis of these terms was performed by Emanuel and Bister (1996), who showed that mechanical dissipation still dominates, so that Eq. (4) remains approximately true. The role of moisture is to some extent hidden; its primary function is possibly to modify the mean temperature, $\overline{T}$, at which radiative cooling occurs. *In no event is it sensible to regard moist convection, in equilibrium, as being driven by "latent heat release."* Thus convective scheme closures that rely on the moisture budget are doomed to fail, because they violate causality. Convection is not caused by moisture, or "moisture convergence" any more than dry convection that happens to contain mosquitoes is caused by "mosquito convergence." In neither case do we deny that there may be a very strong *association* between the two, but it is not causal in nature.

Now one might argue that, when convection is far from being in equilibrium with large-scale processes, the concept of latent heating might be more useful. After all, the first paradigm of moist convection most of us hear about is the case of explosive, deep moist convection over middle latitude continents in spring and summer, when potential energy, stored in a conditionally unstable atmosphere with a "lid," is suddenly released by some trigger. This may be true, but in that case, the interaction with the environment is largely one way and it is not meaningful to think about parameterizing the convection as a function of large-scale variables. As put very succinctly by Arakawa and Schubert, "Unless a cumulus ensemble is in quasi-equilibrium with the large-scale processes, we cannot uniquely relate the statistical properties of the ensemble to the large-scale variables."

D. What Does Equilibrium Convection Look Like?

It is fine to imagine what moist convection in equilibrium with large-scale forcing looks like (Fig. 2), but what does it *really* look like? In the last decade, it has become possible to numerically simulate whole ensembles of convection. Figure 3 shows the distribution of low-level upward motion in a doubly periodic box of 180 km^2, using a numerical cloud model developed by the Center for the Analysis and Prediction of Storms (CAPS). The model includes representations of cloud physical and turbulent processes and is here run with a horizontal resolution of 2 km. A radiative cooling of the troposphere is imposed, and the lower surface is an ocean with fixed surface temperature. The model is run long enough for the domain-average precipitation to come into statistical equilibrium.

The convection is more or less randomly distributed, but a careful analysis (Islam *et al.*, 1993) reveals that the spacing between clouds is more nearly regular than random. This means that clouds are *less* likely to clump together than would be true if their spatial distribution were random. *There is no tendency toward spontaneous organization of clouds*, at least at these scales. (One cannot rule out the possibility of spontaneous organization at scales larger than the domain size.)

Figure 4 shows what happens, on the other hand, if we now impose a background vertical shear of the horizontal wind in the domain. (This is done by relaxing the domain horizontally averaged wind toward a prescribed value at each level.) Now we have very clear mesoscale organization of convection, with squall lines (or, more accurately, arcs) lined up across the direction of the background shear. The mechanism by which this happens was delineated by Thorpe *et al.* (1982) and Rotunno *et al.* (1988); it has to do with the interaction between the background vertical shear with the density currents created by cold, downdraft air spreading out at the surface. The spacing between the squall arcs is nearly that of the domain size, so that the domain may not be large enough to detect the true spacing such lines would have in an unbounded domain. (For different magnitudes of the shear, however, there can be several arcs within the present domain.)

One may reasonably ask whether a parameterization of moist convection should be able to simulate explicitly the actual shape of the convection; that is, to distinguish between the forms of convection in Figs. 3 and 4. The answer is no. After all, the large-scale forcing imposed in both cases

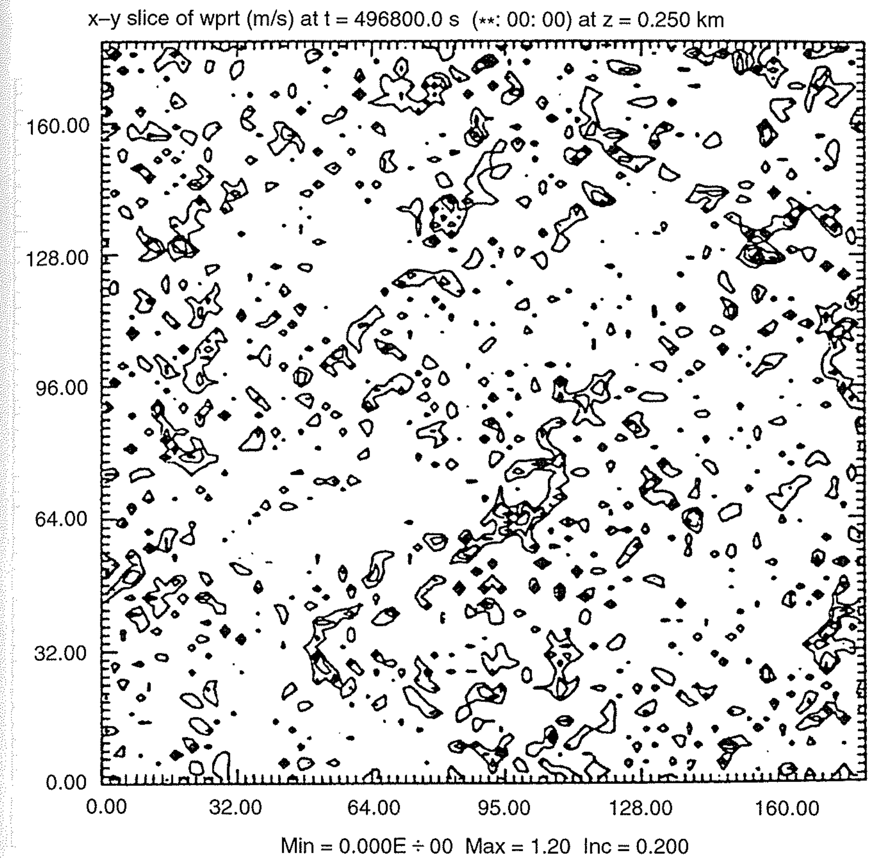

Figure 3 Distribution of upward motion at 250 m in a three-dimensional numerical simulation of radiative-convective equilibrium over a water surface. The simulation has reached statistical equilibrium at this time. (From Robe, 1996.)

is identical. (The background wind shear is not a forcing in this sense; it does not contribute to destabilizing the atmosphere to convection.) Fortunately, there is hardly any detectable difference in the equilibrium, domain-averaged vertical profiles of temperature and relative humidity between Figs. 3 and 4, so that if one is after the vertical heat and moisture fluxes, it may be permissible to neglect the background shear. The convective momentum fluxes are another matter, of course, and their parameterization remains an outstanding problem. (If the relaxation toward the background shear profile is suddenly stopped in the simulations above, the

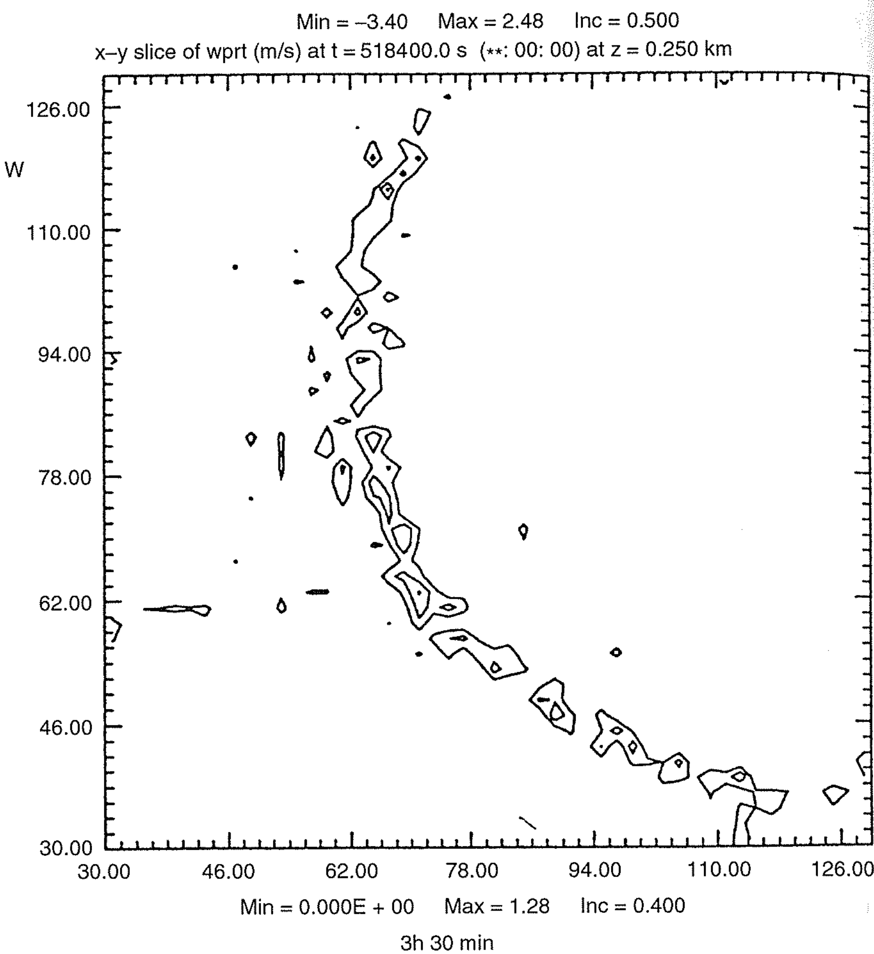

Figure 4 As in Fig. 3, but for a simulation with an imposed vertical wind shear from right to left, in the lowest 3 km. (From Robe, 1996.)

domain average shear relaxes toward zero on a surprisingly short time scale, indicating mostly down-gradient momentum transport by the convection.)

It might be possible, on the other hand, to formulate a representation of convection that regards only the active clouds as the subgrid-scale elements and that takes the mesoscale cold pools to be explicitly simulated by the model. This form of mesoscale convective parameterization would be valid if quasi-equilibrium holds for the interaction between cumulus clouds and mesoscale circulations. That is, if the clouds forming at the leading edge of the cold pool behave in such a way that the rate of destabilization of the column owing to uplift at the leading edge of cold pools is nearly balanced by convective stabilization by the small-scale cumulus elements, then this kind of mesoscale parameterization is viable. But we emphasize that in this case, the cold pools must be explicitly simulated.

E. Quasi-Equilibrium and Convective Inhibition

One peculiarity of moist convection, with no analog in dry convection, is the possibility of metastable states that are stable to small perturbations but unstable to sufficiently large ones. Textbooks almost always contain examples of metastable soundings from places like Texas, where the degree of convective inhibition can be exceptionally large, even when there is a large reservoir of convective available potential energy (CAPE). To what extent is the presence of convective inhibition (hereafter CIn) consistent with statistical equilibrium?

In numerical experiments such as those described in the previous subsection, the experimental design virtually ensures statistical equilibrium when averaged over sufficiently large space-time subdomains. How small can one make the subdomain before statistical equilibrium fails? Figure 5 shows the ratio of the standard deviation of convective rainfall to the subdomain mean, as a function of the size of the space-time subdomain, for a pure convective-radiative equilibrium experiment (Islam *et al.*, 1993) Clearly, the statistics in this case are stable down to remarkably small scales. But were the same thing done for the experiment with shear (Fig. 4), surely the statistics would be less stable and bigger subdomains would be necessary for quasi-equilibrium to be valid. A careful examination of point soundings in these experiments reveals, that, indeed, there is some CIn between active clouds in all the experiments. But it is noticeably larger in the experiments with shear. In this case, strong lifting at the leading edge of the cold pools forces convection there, but the total amount of convection over the domain is constrained by the radiative cooling. Thus the convection must be suppressed between the squall lines. The magnitude of the CIn is part of the quasi-equilibrium state; it is not imposed externally. The forecaster, trying to predict the evolution of individual clouds, might profit from looking at the CIn, but those who are trying to understand the large-scale factors that determine the mesoscale structure would be looking at part of the outcome, not part of the cause.

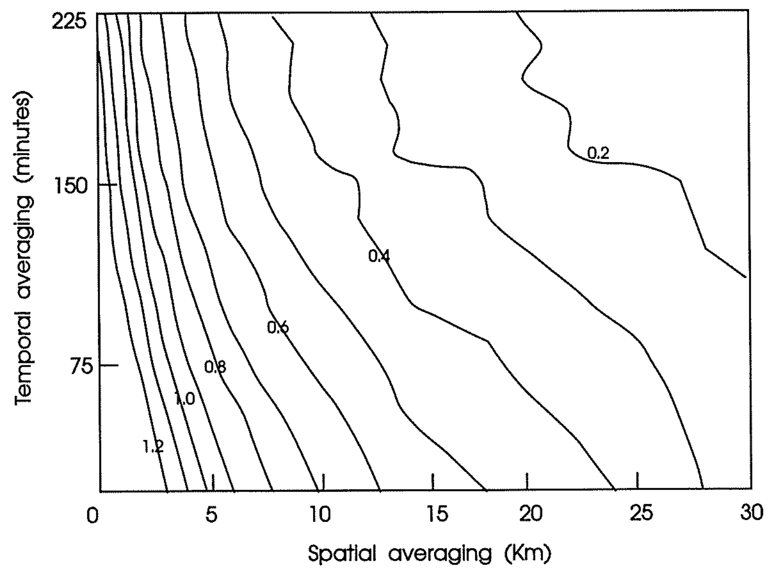

Figure 5 Ratio of the variance to the domain average of the precipitation in a three-dimensional numerical simulation of radiative-convective equilibrium over a water surface, as a function of space-time averaging. The ordinate is the length of time averaging; the abscissa is the length of averaging in space. This ratio asymptotes to $\sqrt{2}$ for short averaging intervals. (From Islam *et al.*, 1993.)

III. THE PHYSICS OF CONVECTIVE QUASI-EQUILIBRIUM

Part of the difficulty some have in accepting the quasi-equilibrium postulate may have to do with problems visualizing how it may work in nature. In the case of dry boundary layer convection, it is relatively easy to understand the process. Suppose, for example, that the rate of radiative cooling is increased in some individual atmospheric layer above the surface. At first, this layer may be expected to cool. But as soon as it does so, it is more unstable with respect to the air just below it, and less unstable with respect to the air just above it. This provides not only for an increase in the convective heat flux from the lower layer, but also for a decrease of the flux to the higher layer; both act to increase the convergence of the convective heat flux, thus warming the layer.

It is more difficult to imagine what happens in a moist convecting layer. Start with a state of pure radiative convective equilibrium and, to make life simple, specify the radiative cooling profile. Now suppose we increase the

rate of cooling in some atmospheric layer above the subcloud layer. If this layer happens to be *just* above the subcloud layer, then it is not difficult to see that the convective flux from the boundary layer will increase, just as in the dry case, and there will be a compensating warming. But what happens if the extra cooling is introduced to a layer far removed from the subcloud layer? The subcloud layer simply cannot know directly about this development and there is little or no basis for thinking that there will be a compensating increase in mass flux out of the subcloud layer. Even if there were, this would entail an extra warming not only in the layer to which we added the cooling, but to all layers below that layer. The warming of these other layers, to which we did not add extra cooling, would quickly stabilize the lower atmosphere and cut off the convection.

Nature resolves this paradox in two ways, as becomes evident on examining the response of explicit ensembles to changes in imposed cooling rates. First, the mass flux can increase in the individual layer to which we add extra cooling *without* increasing the mass flux out of the boundary layer. This occurs because of entrainment. While the exact physics of entrainment into cumulus clouds is not well understood, it is becoming increasingly clear that the rate of entrainment is sensitive to the vertical gradient of the buoyancy of the clouds (Bretherton and Smolarkiewicz, 1989). Cooling an individual layer will have the effect of increasing the buoyancy of clouds rising into that layer. This increases the upward acceleration of air in the clouds and leads to greater entrainment just below the layer of extra cooling. This in turn increases the mass flux in the layer. The increased compensating subsidence outside the cloud warms the layer, opposing the initial added cooling. The physics is very different from what happens in the dry case, but the effect is the same.

The second response to the presence of a layer of extra cooling is entailed in the precipitation physics. Adding cooling to the system means that, to reach equilibrium, there must be an increase in precipitation. How this happens is complex, but it is crucial to recognize that any increase in precipitation will also, in general, increase the magnitude of any unsaturated downdrafts driven by evaporation of precipitation. This will generally occur below the layer into which extra cooling has been added. Because no cooling has been added there, the increased downdraft mass flux must be compensated by an increased updraft mass flux. One may think of it this way: The upward mass flux compensates not just the imposed radiative cooling, but also the (interactive) evaporative cooling. So there *can* be an increase in *updraft* mass flux out of the subcloud layer. This can help warm the layer to which the extra cooling has been added.

Entrainment and adjustments of the unsaturated downdraft are together very effective in compensating for changes in the imposed forcing.

To illustrate this, Fig. 6 shows the imposed radiative cooling profiles and equilibrium convective heating profiles for a variety of experiments using a single-column model with the convective scheme of Emanuel and Ziukovic-Rothman (1999). This is not explicitly a quasi-equilibrium scheme. Instead, it calculates the cloud base updraft mass flux based on an assumption of quasi-equilibrium of subcloud layer air with respect to the air *just above* the subcloud layer, as advocated by Raymond (1995). But, unlike the general quasi-equilibrium closure of Arakawa and Schubert (1974), the mass flux above cloud base is not calculated explicitly from a quasi-equilibrium assumption; rather, the rate of entrainment into clouds is allowed to respond to vertical variations of cloud buoyancy.

It is evident in Fig. 6 that even bizarre profiles of imposed radiative cooling are compensated for by the net convective heating profiles, demonstrating the efficacy of the adjustment process. Figure 7 shows that the resulting temperature profiles are all very close to a moist adiabatic profile. Thus the assumption that convection relaxes the temperature profile of a convecting layer back toward a moist adiabat is well verified in this model. Zeng, Neelin, and others discuss in Chapter 15 the profound implications that this has for understanding tropical dynamics.

IV. NONEQUILIBRIUM THINKING

Most students of meteorology are conditioned to think of convection in nonequilibrium terms, being first introduced to the concept of conditional instability through the illustration of highly metastable soundings from places like Oklahoma. Instability accumulates under some "lid" and is released suddenly when convective temperature is attained or when some mesoscale process locally removes the potential barrier to convection. This may very well be an appropriate mode of thinking about the type of convection that often results in severe thunderstorms. But it is probably inappropriate for thinking about many tropical circulation systems.

Nowhere is the disparity between equilibrium and nonequilibrium thinking more on display than in discussions about hurricanes. As reviewed very thoroughly by Yanai (1964), most of the earliest attempts to model hurricanes, beginning in the early 1960s, focused on finding a particular mode by which stored conditional instability is released. As earlier theoretical studies had predicted, conditional instability is released at the scale of individual clouds. All attempts to run numerical simulations of hurricanes as modes of release of conditional instability failed to produce a hurricane-scale vortex. Earlier theoretical work by Riehl (1950) and Kleinschmidt (1951) had shown that the warmth of the eyewall could only be

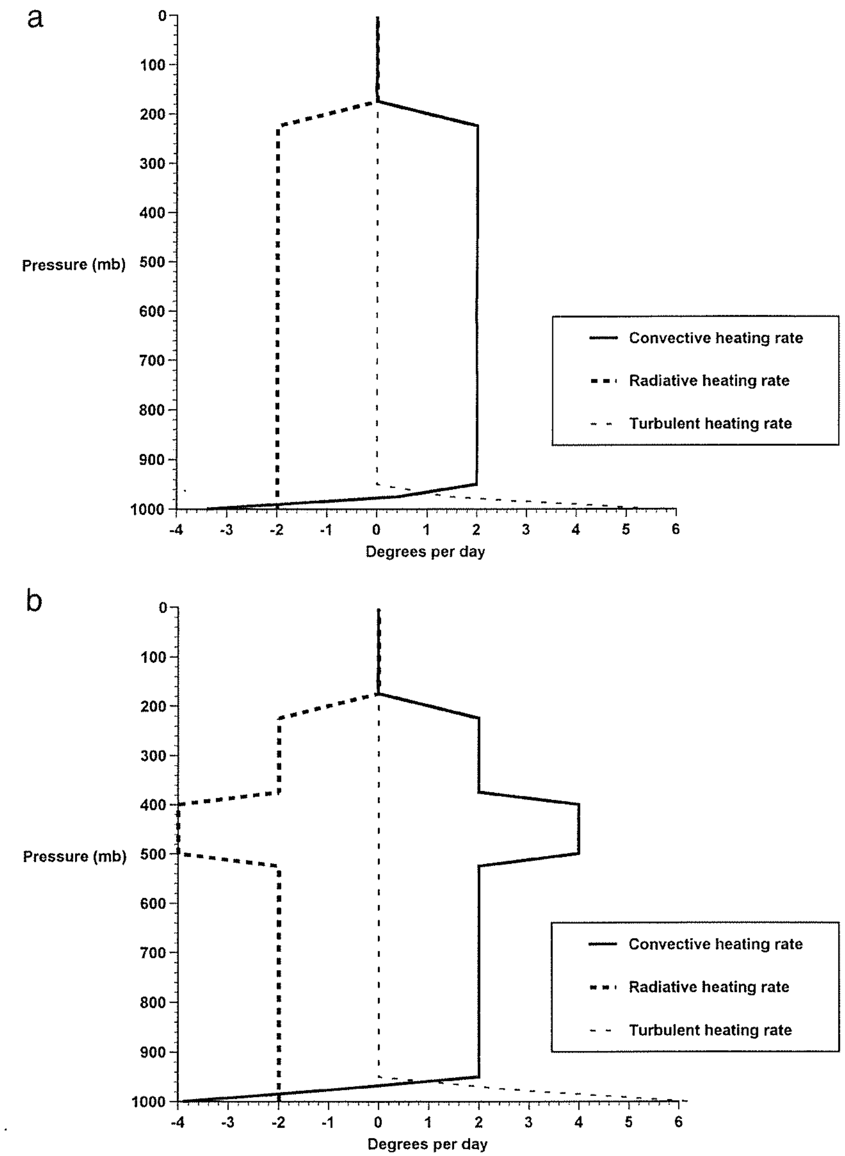

Figure 6 The heat budget of a single-column model in radiative-convective equilibrium, showing the rate of heating as a function of pressure. In each case, the solid line denotes the convective heating rate, the dashed line the (imposed) radiative heating rate, and the thin dashed line the convergence of the dry tubulent heat flux. (a) Uniform radiative cooling in the troposphere. (b) Same as (a) but with added cooling in the 400- to 500-mb layer. (c) Same as (a) but with zero cooling in the 850- to 950-mb layer. (d) No cooling in the 500- to 950-mb layer. This shows that convection can penetrate even a deep layer of no large-scale destabilization.

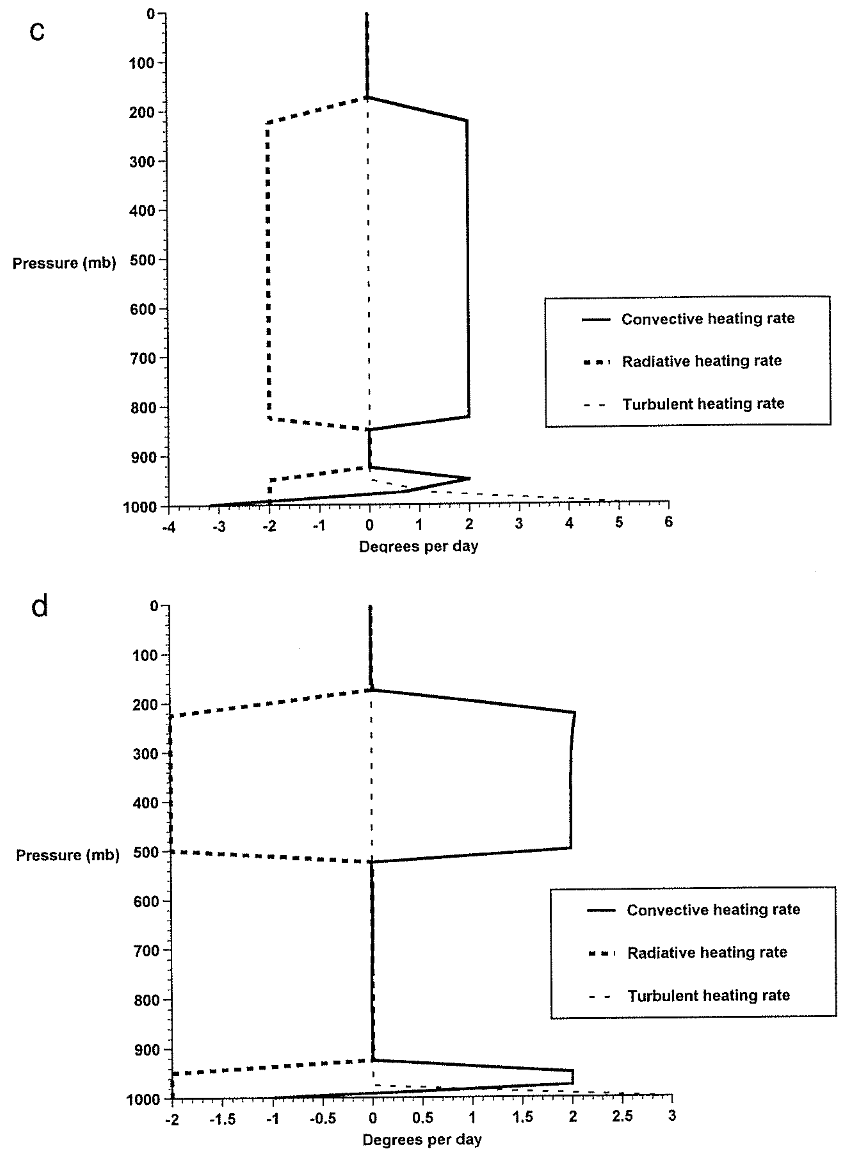

Figure 6 (*Continued*)

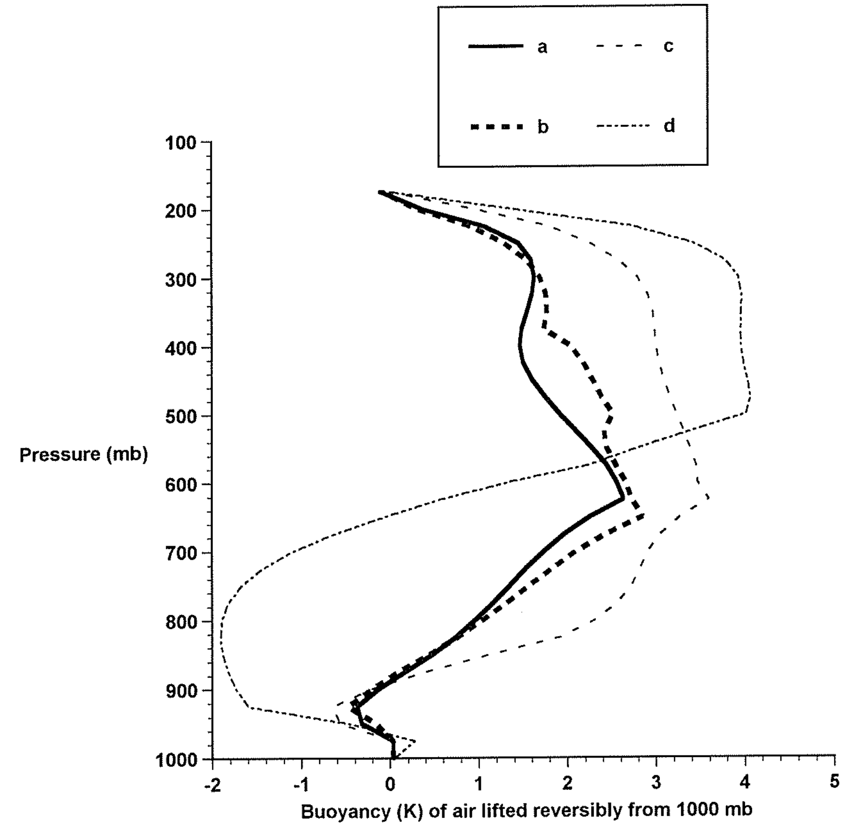

Figure 7 Departure of the ambient temperature from a reference moist adiabat for each of the experiments illustrated in Fig. 6. A positive value means that the reference adiabat is warmer than the atmosphere.

explained by the enormous enthalpy transfer from ocean to atmosphere that occurs in the high wind region of the storm. Although the principals involved in this work were undoubtedly aware of this earlier theoretical work, they evidently considered the heat transfer to be a secondary issue.

The failure of these earliest attempts at numerical simulation formed a large part of the motivation behind the development of the theory of conditional instability of the second kind (CISK) by Charney and Eliassen (1964) and Ooyama (1964). The history of the development of CISK is reviewed very nicely by Kasahara in Chapter 7 of this volume. The idea of

CISK was stated very beautifully by Charney and Eliassen (1964):

> ...we should look upon the pre-hurricane depression and the cumulus cell not as competing for the same energy, for in this competition the cumulus cell must win; rather we should consider the two as supporting one another—the cumulus cell by supplying the heat energy for driving the depression, and the depression by producing the low-level convergence of moisture into the cumulus cell.

In my view, a fatal flaw was introduced into thinking about tropical circulations by this enormously influential work. It is the idea that latent heat release can ever be an energy source for equilibrium circulations, an idea disproved earlier in Section II. This flaw was exacerbated by later work that also introduced the incorrect notion that the vertical profile of convective heating is an internal property of the convective clouds that can, to a first approximation, be specified independently of the environment.

The Charney and Eliassen work attempted to demonstrate CISK by posing a balanced model in which, as in the case of unbalanced models, the latent heat release is taken to be proportional to vertical velocity but, unlike unbalanced models, the vertical velocity was constrained to be that associated with Ekman pumping. Thus constrained, the model dutifully produced a linear instability with tropical cyclone-like characteristics, but even in this case the most rapidly growing modes were of small scale.

The difference between nonequilibrium (CISK) thinking and equilibrium thinking, in the case of a tropical cyclone, is illustrated in Fig. 8. In nonequilibrium thinking, the ambient atmosphere has a reservoir of usable potential energy for convection. The tropical cyclone is a means of releasing that instability on a large scale. In equilibrium thinking, the storm passes through an infinite sequence of convective equilibrium states, and the warmth of the eyewall is a consequence of the energy flux from the ocean. In reality, there is always some stored potential energy to balance dissipation in clouds (see Emanuel and Bister, 1996), and there is never perfect equilibrium in an evolving system. Nevertheless, as subsequent work (Emanuel, 1989) showed, approximating the evolution as passing through an infinite sequence of equilibrium states yields a realistic numerical simulation.

Five years after the CISK papers were published, Ooyama (1969) presented the first genuinely successful numerical simulation of a tropical cyclone. It captured the essential physics of the intensification process, and documented the sensitive dependence of the vortex evolution on the exchange coefficients of enthalpy and momentum at the sea surface. It confirmed the deductions of Riehl (1950) and others that surface enthalpy

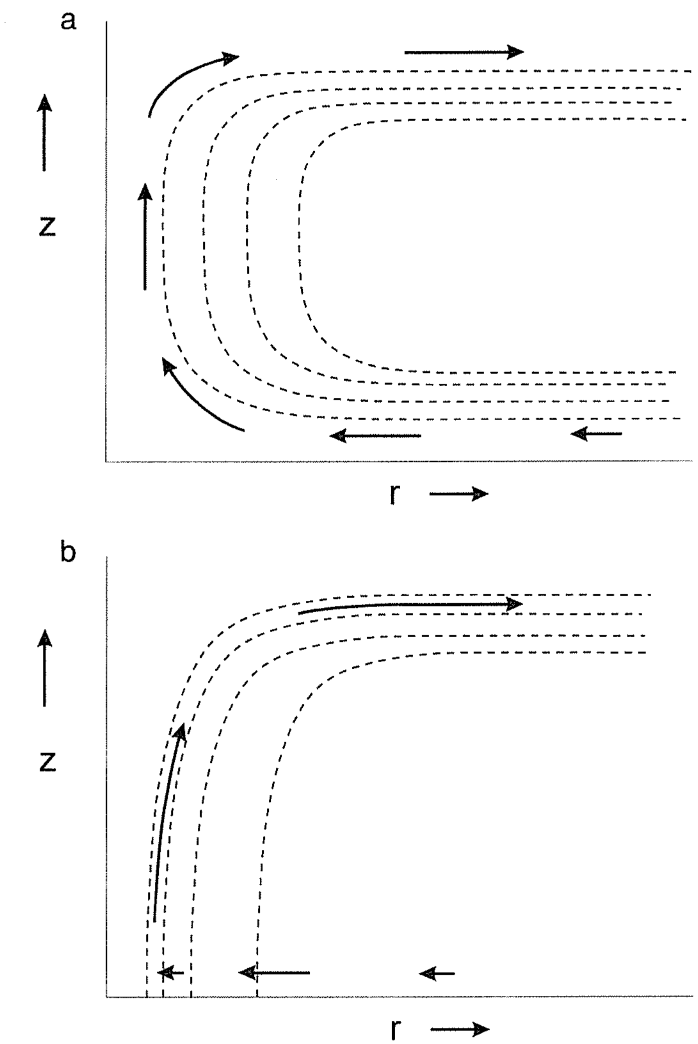

Figure 8 Two views of tropical cyclone physics. In each case, the dashed lines indicate surfaces of constant saturation θ_e. (a) The CISK view. The frictionally induced inflow allows for convection in the core. The core region is warm because the convection is there and not in the outer region. The role of the ocean is to keep the subcloud layer "stoked"; i.e., to prevent its θ_e from decreasing in the face of convective downdrafts. Intensification stops when the free troposphere saturation θ_e is the core increases to the initial value of θ_e in the boundary layer. (b) The WISHE view. The saturation θ_e is tied to the subcloud layer θ_e. The core is warm because surface fluxes have increased θ_e there. The rate of intensification is limited by the surface fluxes, not by the convection, which is very fast by comparison. Intensification stops when the frictionally induced radial advection of low θ_e air balances the surface enthalpy flux.

fluxes are essential; when they are excluded from the model very little happens.

One might have thought that Ooyama's and subsequent numerical simulations would finally lay to rest the notion that the controlling process in tropical cyclone intensification is some kind of cooperation between the cyclone circulation and cumulus clouds. That this has not happened is a testament to the attractiveness of nonequilibrium thinking and, I think, the eloquence of the Charney and Eliassen paper. The Ooyama simulation left an opening for CISK adherents: It began with a highly unstable vertical sounding and used a cumulus parameterization that could not switch on unless there was moisture convergence in the boundary layer. Together, this meant that the resting state was indeed linearly unstable to CISK, as shown by Ooyama in the same paper. But it was also clear from the nonlinear simulations that, even with the loaded gun, CISK could account for no more than a trivial initial intensification. In a later paper, Ooyama (1982) downplayed the significance of the linear instability, and Klaus and Reeder (1997) showed that Ooyama's original model, when initialized with a neutral sounding, is metastable in the same sense as the model of Emanuel (1989), which also begins with a neutral sounding. The latter model showed that a perfectly acceptable numerical simulation of a tropical cyclone results even under the extreme assumption that the model is always precisely neutral to cumulus convection.

Craig and Gray (1996), using a cloud-resolving, nonhydrostatic primitive equation model of a tropical cyclone, showed that, at all stages of intensification, the rate of development is directly proportional to the surface enthalpy exchange coefficient and inversely proportional to the drag coefficient, in direct contradiction to the predictions of CISK.

The most recent variation of nonequilibrium thinking holds that tropical cyclones cannot develop until the rotation is strong enough that the local deformation radius is of the same order as the scale of convective clouds. This notion was introduced by Ooyama (1982) and furthered by Hack and Schubert (1986). The general idea is that an isolated heat source cannot produce finite warming in a nonrotating, infinite fluid, because the warming is spread over an infinite region. Warming only commences when the deformation radius becomes appreciably small.

The problem with this idea is that it postulates a single, noninteractive cloud, whereas equilibrium thinking demands that we consider an ensemble of clouds always in equilibrium with the forcing. A large-scale circulation with a large deformation radius can be efficiently warmed by an array of clouds and there is no physical reason to postulate a single cloud. On the contrary, doing so is unphysical. In a nonrotating unbounded domain initially in radiative-convective equilibrium, a sudden increase in the surface fluxes will be accompanied by a rapid increase in the domain temperature at all levels up to the tropopause, in spite of the total absence of rotation. There are many dynamical reasons why the rate of intensification of tropical cyclones may depend on their intensity, but the ratio of the cloud scale to the local deformation radius is not one of them, as demonstrated by tropical cyclone models that constrain the free atmospheric temperature to always lie on a moist adiabat determined by the thermodynamic properties of boundary layer air.

Nonequilibrium thinkers continue to hold that tropical cyclones are powered by latent heat release in cumulus clouds, though surface energy fluxes are of course necessary to maintain the reservoir of conditional instability. This is analogous to claiming that cars are powered by their drive trains, though engines are of course necessary to keep torque on them. In both cases the awkwardness arises from a failure to separate time scales—in the case of a car, the time scale for transmission of torque through the drive train is tiny compared to the time scale determined by the power of the engine and the inertia of the car; in the case of the hurricane, the time scale for transmission of enthalpy by convection, on the order of a few hours, is small compared to the time scale determined by the rate of surface enthalpy flux and the inertia of the cyclone. No numerical simulation has ever demonstrated a critical role for convective time scales in the evolution of the vortex.

One final point to be made about nonequilibrium thinking concerns ordinary conditional instability itself. In nonequilibrium thinking, the degree of conditional instability is thought of as an external condition that determines various properties of the convection. In equilibrium thinking, by contrast, the degree of conditional instability is determined by the convection itself, together with the forcing. Quasi-equilibrium does not imply actual invariance of CAPE, any more than quasi-geostrophy implies invariance of ageostrophic velocities. In the quasi-equilibrium view, both the intensity of the convection and CAPE are determined by the forcing and, to second order, the time rate of change of the forcing. CAPE is not a predictor, though it can be an indicator.

V. EQUILIBRIUM THINKING

The underlying proposition in quasi-equilibrium thinking is that convection rapidly adjusts the temperature profile back toward a moist adiabat in a way that preserves the vertically integrated enthalpy. To a first order of approximation, convection keeps the atmospheric temperature profile on a moist adiabat tied to the subcloud layer entropy. This strongly constrains the vertical structure of the horizontal and vertical velocities as well as the temperature perturbations associated with large-scale disturbances in convecting atmospheres, as discussed by Neelin and Yu (1994) and Emanuel *et al.* (1994). It also means that, in convecting atmospheres, the problem of predicting the evolution in three dimensions of atmospheric variables reduces largely to the problem of predicting the evolution of subcloud layer entropy.

For all disturbances with time scales appreciably greater than the time scale of convective adjustment, the vertical structure of the disturbance is completely determined by the shape, in $T - p$ space, of a moist adiabat (Neelin and Yu, 1994). Moreover, the static stability felt by such disturbances is not related to the degree of conditional instability but rather to a "gross moist stability" that depends on the shape of the vertical moist static energy profile (Neelin and Yu, 1994). Calculations of the distribution and magnitude of this gross stability have been presented by Yu and Neelin (1997). The exact magnitude of this stability measure, on the other hand, depends not only on the shape of the moist static energy profile but on the relationship between static energy and moisture fluctuations. This relationship, in turn, depends on the details of cloud microphysical processes, which are poorly understood.

One of the most basic issues we may address in quasi-equilibrium thinking is what happens when an internal gravity wave passes through a background atmosphere in radiative-convective equilibrium. (Note that the equivalent question in nonequilibrium thinking is what happens when an internal wave passes through a cloudless background atmosphere that contains stored CAPE.) To assert that the convection remains close to a state of statistical equilibrium with the large scale, we must assert that the horizontal wavelength is much larger than the intercloud spacing of the background state and that the wave period turns out to be much longer than the convective adjustment time scale.

Note first that, as of this writing, the answer to the question posed above has not been obtained by direct numerical simulation with cloud-resolving models. Such models, at least in three dimensions, are not quite capable of simultaneously containing a reasonably large gravity wave and resolving individual clouds.

The first and most basic question is how the radiative-convective equilibrium atmosphere responds to large-scale ascent and descent. To begin with, we make use of the fact that, as long as all convective kinetic energy is locally dissipated, convection does not alter the mass integral of the system moist state energy, h:

$$h = C_p T + L_v q + gz.$$

Thus, if we neglect perturbations to the radiative heating, the equation for the vertically integrated moist static energy perturbation from the mean state is

$$\frac{\partial [h']}{\partial t} = -\left[\omega' \frac{\partial \bar{h}}{\partial \mathrm{p}}\right], \tag{12}$$

where the brackets denote an integral over the mass of the convecting layer, and the overbar signifies the background state. As discussed earlier, the vertical structure of ω' is determined by the condition that the temperature profile remains moist adiabatic. Thus, the sign of the response of h' to ascent or descent depends on the convolution of the vertical structure function of ω with $\partial \bar{h}/\partial \mathrm{p}$, as pointed out by Neelin and Yu (1994). It is straightforward to calculate this from atmospheric soundings, and this has been done (Yu and Neelin, 1997). The conclusion is that this measure of stratification is stable throughout the atmosphere, meaning that upward motion will be associated with decreasing h'.

Now if h' decreases, then either T' decreases, q' decreases, or both. In all of the observational studies of which I am aware, ascent is associated with increasing q', implying that T' must decrease with h'. Moreover, even if the extreme assumption is made that the relative humidity is invariant with large-scale vertical motion, it is easy to show that T' must have the same sign as h':

$$\begin{aligned} h' &= c_p T' + L_v q' \\ &= c_p T' + L_v \mathscr{H}\left(\frac{\partial q^*}{\partial T}\right)_p T', \end{aligned} \tag{13}$$

where $\mathscr{H}$ is the relative humidity. Because $(\partial q^*/\partial T)_p$ is positive, T' and h' must have the same sign when $\mathscr{H}$ is constant. For T' to have the opposite sign as h', $\mathscr{H}$ would have to have a rather strong negative correlation with ascent, which is certainly not observed.

Thus, although the exact value of the response of temperature to ascent depends on the details of how clouds humidify the atmosphere, there is little question that ascent causes cooling, just as in a dry, stable atmosphere. The magnitude of this cooling is less, and sometimes much less, than in a nonconvecting atmosphere.

It pays to consider the same process from another angle, introduced by the author (Emanuel, 1989). It begins with the observation that the temperature of convecting atmospheres lies close to an adiabat originating in the subcloud layer. Thus, in strict statistical equilibrium as defined by Emanuel *et al.* (1994), the temperature of the troposphere is tied to the moist static energy of the subcloud layer. Now, if large-scale ascent occurs, convergence will occur in the subcloud layer, but this does *not* change the moist static energy, which is, after all, a conserved variable. But to keep the deep troposphere approximately moist adiabatic, there must be enhanced convection to counter the adiabatic cooling associated with the large-scale ascent. Enhanced convection will be associated also with enhanced downdrafts, which import low static energy into the subcloud layer. The reduction of subcloud-layer moist static energy will then be associated with a net cooling of the lower troposphere. This argument is fully equivalent to the preceding one, but makes the importance of downdrafts more explicit. Reducing the relative magnitude of the response of the convective downdraft moist static energy flux reduces the effective stratification. When this flux vanishes, as in a saturated atmosphere, so does the effective stratification.

Thus we may expect a large-scale gravity wave, propagating through a radiative-convective equilibrium background state, to propagate much more slowly than an equivalent wave propagating through a cloudless atmosphere with the same stratification.

But there is, after all, a difference. In the case of a dry atmosphere, there is no time lag between vertical displacement and buoyancy. On the other hand, it takes time for convection to respond to changes in its environment. This time lag is probably on the order of hours, but its effect on the large-scale dynamics is important. Consider again the problem of an internal gravity wave passing through a background state in radiative-convective equilibrium. As described before, upward motion will be associated with cooling, and, ordinarily, the lowest temperatures will occur 1/4 cycle after the strongest upward motion. Now take into account the small lag in the response of the convection. Now the convection, rather than being precisely in phase with the wave ascent, will lag slightly, thus shifting slightly toward the cold phase of the disturbance. This will cause the convective heating to have a negative correlation with temperature, thereby draining perturbation potential energy from the wave. This effect is called *moist convective damping*. This was shown by the author (Emanuel, 1993) to damp linear equatorial waves of all kinds in a simple model with a quasi-equilibrium-type convective closure. Neelin and Yu (1994) showed it more generally to be true of linear disturbances in a vertically continuous atmosphere using the Betts (1986) convection scheme, and Brown and Bretherton (1995) demonstrated it in a linear model using the Emanuel (1991) convection scheme. Here I demonstrate that it applies to a quasi-linear, two-column model. The model is linear in the sense that it ignores wave–wave interaction, and retains strictly sinusoidal spatial distribution of the wave variables, but it allows for arbitrary amplitude of the wave.

Nonlinear terms in the momentum equation are ignored, but are retained in the thermodynamic equation. The linearized inviscid, hydrostatic vorticity equation in a nonrotating atmosphere can be written

$$\frac{\partial^2}{\partial z^2}\frac{\partial w}{\partial t} = \nabla_2^2 B, \tag{14}$$

where w is the vertical velocity, B is the buoyancy, and ∇_2^2 is the horizontal Laplacian operator. Assuming horizontally sinusoidal disturbances with combined horizontal wave number K, Eq. (14) becomes

$$\frac{\partial^2}{\partial z^2}\frac{\partial w}{\partial t} = -K^2 B. \tag{15}$$

This equation can be solved in a single column if K is specified. One problem that arises in doing this, however, is that to define the buoyancy, B, one must define a background state. This is easy enough for infinitesimal disturbances, but for real disturbances the background state may itself change. To avoid this difficulty, we solve Eq. (15) in a two-column model, where each column is out of phase with the other ($w_1 = -w_2$) and define B as

$$B_i = \frac{1}{2}\frac{g}{\overline{T}_v}(T_{vi} - T_{vj}), \tag{16}$$

where the subscript i denotes the column in question and the subscript j denotes the other column, while $\overline{T}_v \equiv \frac{1}{2}(T_{vi} + T_{vj})$. Each column is identical to the single-column model described in Section III, except that vertical advection terms are added to the heat and moisture equations, and Eq. (15) is also solved in each column. Moreover, to guarantee global energy conservation, heat and moisture are advected from one column to the other according to the horizontal circulation implied by mass continuity and w.

The two-column model is initialized with an arbitrary buoyancy perturbation to the radioactive convective equilibrium state. Figure 9 shows the time evolution of the vertical velocity at a particular level in one of the two columns, for wave number 1. One observes a decaying oscillation with a period of roughly 40 days. Examination of the behavior of the system for other specified horizontal wave numbers, K, shows the same general behavior, with the period decreasing and the rate of decay increasing with increasing K.

This is completely consistent with earlier results by Emanuel (1993), Neelin and Yu (1994), and Brown and Bretherton (1995). We can say with

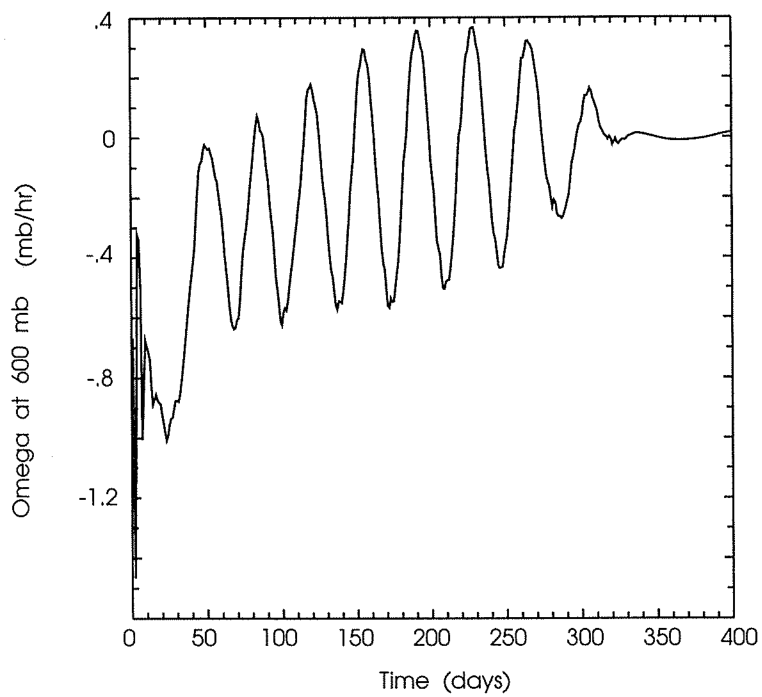

Figure 9 Variation with time of the pressure velocity (ω) at 600 mb in a two-column model that has been perturbed away from radiative-convective equilibrium.

some certainty that internal gravity waves propagating through a background state in radiative-convective equilibrium will be damped.

This finding is at odds with CISK models, dating back at least to Lindzen (1974). What is different about these models? Two key elements of CISK models allow them to produce unstable internal waves. The first is the partial or complete decoupling of convective heating from buoyancy, which occurs when the rate of heating is related to moisture convergence. This permits buoyancy to accumulate in one phase of a wave and be reduced in another and so is explicitly contrary to the quasi-equilibrium hypothesis. The second is, in the case of some models, a specification of the vertical profile of convective heating that is independent of the vertical profile of cloud buoyancy. This also allows for in-phase fluctuations of buoyancy and heating.

Other aspects of quasi-equilibrium thinking are discussed in Emanuel *et al.* (1994). The two most important conclusions that emerge from the

quasi-equilibrium point of view are these:

- Large-scale disturbances in convecting atmosphere "feel" a reduced, but still positive, effective static stability.
- Such disturbances are also damped in proportion to their frequency.

If this mode of thinking is correct, then we must turn away from convection per se as an "explanation" for large-scale disturbances and think of it instead as a means of rapidly redistributing enthalpy in the vertical. Potential candidates for the sources of large-scale disturbances in the tropics include the following:

- Horizontal gradients of the radiative-convective equilibrium temperature. These are responsible for the Walker–Hadley and monsoon circulations, for example (Held and Hou, 1980; Plumb and Hou, 1992).
- Wind-induced surface heat exchange (WISHE). The feedback between wind and surface enthalpy flux is responsible for tropical cyclones and many play a role in other tropical phenomena.
- Transmission of wave energy from outside the tropics. There are now well-documented instances of this phenomenon (Kiladis, 1998).
- Dynamical instabilities. The instability of the African easterly jet in summer is the source of easterly waves, for example.

VI. SUMMARY

Until quite recently, quasi-equilibrium has been thought of primarily as a closure for convective parameterizations; its effect on the way we think about convection has been relatively slow to come about. In this paper I have reviewed and in some small ways extended Emanuel *et al.*'s (1994) exploration of the full implications of quasi-equilibrium. The main structure of quasi-equilibrium thinking emphasizes the following points:

- Latent heating is a concept that applies to the dynamics of individual clouds. In contrast, it plays *no role* in the energetics of cumulus ensembles.
- The state of radiative-convective equilibrium serves as the basic equilibrium state for quasi-equilibrium thinking in the same way that an east–west baroclinic flow serves as the basic state for quasi-geostrophic thinking about many midlatitude flows.
- Disturbances with space and time scales much larger than convective overturning time scales, and intercloud spacing characterizing the

radiative-convective equilibrium state may be considered to be in quasi-equilibrium with the convective clouds.

- Such disturbances "feel" an effective stratification that, while positive, is much less than typical dry stratifications. The stratification may also be related physically to drying of the subcloud layer by convective downdrafts. The effective stratification vanishes when the large scale becomes saturated, as happens in the core of tropical cyclones.
- Such disturbances are also damped in proportion to their frequency. This tends to filter high-frequency disturbances and to damp most nascent tropical depressions.
- Convection that is not close to being in equilibrium with explicitly simulated flows cannot be parameterized as a function of the explicitly resolved variables.

A full appreciation of the consequences of quasi-equilibrium will no doubt lead to important advances in understanding and predicting large-scale disturbances in convecting atmospheres.

REFERENCES

Arakawa, A., and W. H. Schubert (1974). Interaction of a cumulus cloud ensemble with the large-scale environment, Part I. *J. Atmos. Sci.* **31,** 674–701.

Betts, A. K. (1986). A new convective adjustment scheme. Part I: Observational and theoretical basis. *Quart. J. Roy. Meteor. Soc.* **112,** 677–691.

Bretherton, C. S., and P. K. Smolarkiewicz (1989). Gravity waves, compensating subsidence and detrainment around cumulus clouds. *J. Atmos. Sci.* **46,** 740–759.

Brown, R. G., and C. S. Bretherton (1995). Tropical wave instabilities. *J. Atmos. Sci.* **51,** 67–82.

Charney, J. G., and A. Eliassen (1964). On the growth of the hurricane depression. *J. Atmos. Sci.* **21,** 68–75.

Craig, G. C., and S. L. Gray (1996). CISK or WISHE as the mechanism for tropical cyclone intensification. *J. Atmos. Sci.* **53,** 3528–3540.

Emanuel, K. A. (1989). The finite-amplitude nature of tropical cyclogenesis. *J. Atmos. Sci.* **46,** 3431–3456.

Emanuel, K. A. (1991). A scheme for representing cumulus convection in large-scale models. *J. Atmos. Sci.* **48,** 2313–2335.

Emanuel, K. A. (1993). The effect of convective response time on WISHE modes. *J. Atmos. Sci.* **50,** 1763–1775.

Emanuel, K. A. (1994). "Atmospheric Convection." Oxford Univ. Press, New York.

Emanuel, K. A., and M. Ziukovic-Rothman (1999). Development and evaluation of a convective scheme for use in climate models. *J. Atmos. Sci.* **56,** 1766–1782.

Emanuel, K. A., and M. Bister (1996). Moist convective velocity and buoyancy scales. *J. Atmos. Sci.* **53,** 3276–3285.

Emanuel, K. A., J. D. Neelin, and C. S. Bretherton (1994). On large-scale circulations in convecting atmospheres. *Quart. J. Roy. Meteor. Soc.* **120,** 1111–1143.

Hack, J., and W. H. Schubert (1986). Nonlinear response of atmospheric vortices to heating by organized cumulus convection. *J. Atmos. Sci.* **43,** 1559–1573.

Held, I. M., and A. Y. Hou (1980). Nonlinear axially symmetric circulations in a nearly inviscid atmosphere. *J. Atmos. Sci.* **37,** 515–533.

Hoskins, B. J., M. E. McIntyre, and A. W. Robertson (1985). On the use and significance of isentropic potential vorticity maps. *Quart. J. Roy. Meteor. Soc.* **111,** 877–946.

Islam, S., R. L. Bras, and K. A. Emanuel (1993). Predictability of mesoscale rainfall in the tropics. *J. Appl. Meteor.* **32,** 297–310.

Kiladis, G. N. (1998). Observations of Rossby waves linked to convection over the eastern tropical Pacific. *J. Atmos. Sci.* **55,** 321–339.

Klaus, D., and M. J. Reeder (1997). The effects of convection and baroclinicity on the motion of tropical-cyclone-like vortices *Quart. J. Roy. Meteor. Soc.* **123,** 699–725.

Kleinschmidt, E., Jr. (1951). Gundlagen einer Theorie des tropischen Zyklonen. *Arch. Meteorologie, Geophysik Bioklimatologie,* Series A, **4,** 53–72.

Lindzen, R. S. (1974). Wave-CISK in the tropics. *J. Atmos. Sci.* **31,** 156–179.

Manabe, S., J. Smagorinsky, and R. F. Strickler (1965). Simulated climatology of a general circulation model with a hydrologic cycle. *Mon. Wea. Rev.* **93,** 769–798.

Neelin, J. D., and J. Yu (1994). Modes of tropical variability under convective adjustment and the Madden–Julian oscillation. Part I: Analytical theory. *J. Atmos. Sci.* **51,** 1876–1894.

Ooyama, K. (1964). A dynamical model for the study of tropical cyclone development. *Geofis. Int.* **4,** 187–198.

Ooyama, K. (1969). Numerical simulation of the life-cycle of tropical cyclones. *J. Atmos. Sci.* **26,** 3–40.

Ooyama, K. (1982). Conceptual evolution of the theory and modeling of the tropical cyclone. *J. Meteor. Soc. Japan* **60,** 369–379.

Plumb, R. A., and A. Y. Hou (1992). The response of a zonally symmetric atmosphere to subtropical thermal forcing: Threshold behavior. *J. Atmos. Sci.* **49,** 1790–1799.

Prandtl, L. (1925). Bericht über Untersuchungen zur ausgebildeten Turbulenz. *Zs. Angew. Math. Mech.* **5,** 136–139.

Raymond, D. J. (1995). Regulation of moist convection over the west Pacific warm pool. *J. Atmos. Sci.* **52,** 3945–3959.

Rennó, N. O., and A. P. Ingersoll (1996). Natural convection as a heat engine: A theory for CAPE. *J. Atmos. Sci.* **53,** 572–585.

Riehl, H. (1950). A model for hurricane formation. *J. Appl. Phys.* **21,** 917–925.

Robe, F. R. (1996). Sea, sun and shear: A recipe for precipitating convection, tropical rainbands and hurricane spiral arms, Ph.D. thesis. Mass. Inst. Tech., Cambridge.

Robe, F. R., and K. A. Emanuel (1996). Dependence of tropical convection on radiative forcing. *J. Atmos. Sci.* **53,** 3265–3275.

Rotunno, R., J. B. Klemp, and M. L. Weisman (1988). A theory for strong, long-lived squall lines. *J. Atmos. Sci.* **45,** 463–485.

Thorpe, A. J., M. J. Miller, and M. W. Moncrieff (1982). Two-dimensional convection in non-constant shear: A model of mid-latitude squall lines. *Quart. J. Roy. Meteor. Soc.* **108,** 739–762.

Yanai, M. (1964). Formation of tropical cyclones. *Rev. Geophys.* **2,** 367–414.

Yu, J.-Y., and J. D. Neelin (1997). Estimating the gross moist stability of the tropical atmosphere. *J. Atmos. Sci.* **55,** 1354–1372.