

Comments on ‘On large-scale circulations in convecting atmospheres’ by
Kerry A. Emanuel, J. David Neelin and Christopher S. Bretherton (July B, 1994, **120**,
1111–1143)

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(Received 18 December 1995; revised 4 October 1996)

KEYWORDS: CISK Convection WISHE

1. INTRODUCTION

The review paper by Emanuel *et al.* (1994; hereafter ENB) provides a stimulating and at times provocative overview of the dynamics of convecting systems and their interaction with larger-scale circulations. As such it prompted us to organize a semester-long seminar devoted to the study of these interactions, and to ENB’s ideas in particular. The following comments emerged from discussions and criticisms raised during the course of our seminar.

To summarize, ENB take a critical position toward what they characterize as a prevalent view of tropical convection, namely that the Hadley cell and other large-scale circulations are driven by latent heating. They argue that, instead, the large-scale meridional temperature gradient imposed by latitudinal variations in solar insolation is the driving force behind the Hadley cell. They go on to argue that tropical convection consumes CAPE (convective available potential energy) as rapidly as it is produced by large-scale forcing, leading to a state of quasi-equilibrium. The resulting quasi-invariance of the CAPE implies a one-to-one relationship between the local time-rates of change of sub-cloud entropy and the temperature of the free atmosphere. Moreover, by accounting for the finite lag time of convection (i.e. allowing the equilibrium to be ‘soft’), they find that large-scale perturbations organize convection in a manner which tends to damp the organizing disturbance. Consequently, conditional instability of the second kind (CISK) is not a mode of instability in the physical system, and the view that large-scale disturbances cooperate with convection is, in the absence of certain qualifications, wrong.

Although there is a great deal to learn from ENB’s paper, many of their ideas require further discussion. Here we focus on three:

(i) ENB’s criticisms of CISK ignore earlier and broader interpretations of the idea which cast the theory in a better light.

(ii) The view of convection as a source of kinetic energy for large-scale disturbances has an observational basis that ENB make no reference to. These observations are also relevant to a number of ENB’s other arguments and thus deserve some discussion.

(iii) ENB’s endorsement of Arakawa and Schubert’s (1974) equilibrium hypothesis is, in our view, well considered—but would benefit from a few clarifying comments.

2. A HISTORICAL VIEW OF CISK AND RELATED IDEAS

ENB criticize CISK (an enigmatic acronym whose variant interpretations are discussed below) for failing to recognize the importance of surface fluxes in generating instability, and go so far as to call it ‘an influential and lengthy dead-end road’. In so doing they contrast it with WISHE (wind-induced surface heat exchange), a theory formulated to take explicit account of surface fluxes. Their characterization of CISK appears to be based on a narrow definition of the term[†]. More general interpretations (e.g. Ooyama 1982; hereafter O82) lead to more favourable views of the theory. For instance, CISK more broadly interpreted has long acknowledged the important role of surface fluxes. In the context of such broader interpretations, WISHE does not exist in opposition to CISK, but is instead an important refinement of Ooyama’s (1969; hereafter O69) articulation of the theory.

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† Actually, one weakness of ENB’s article is that they never clearly define CISK, leaving it for the reader to decide exactly what it is.

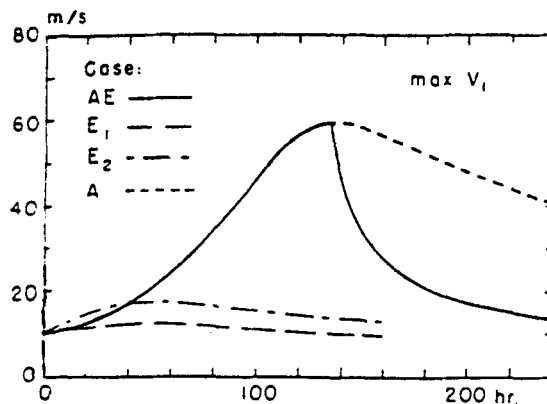


Figure 1. Maximum tangential winds versus time for four simulations. Cases E1 and E2 have no surface fluxes but increasing amounts of convective available potential energy in the initial conditions, where E2 has a nearly saturated boundary layer. Case AE is the control run with surface fluxes stopped at 134 hours. Case A is the control. All cases are initially unstable as measured by the linear theory. Taken from upper panel of Fig. 15 of Ooyama (1969).

ENB take the fact that air–sea interaction does not *explicitly* appear in the early linear analyses as a fundamental flaw of CISK. However, in stating that ‘we have implicitly assumed that the depression forms over the tropical oceans where there is always a source of near-saturated air in the surface boundary layer’ Charney and Eliassen (1964; hereafter CE64) acknowledge the important role of the warm ocean, even though in attempting to demonstrate how, under certain circumstances, convection may cooperate with larger-scale disturbances their linear analysis failed to account explicitly for the energetic role of surface fluxes. Although it was not cited by ENB, O69 is clear on the issue of surface fluxes. In a series of experiments (results of which are plotted in O69’s Fig. 15 and reproduced in our Fig. 1) it is demonstrated that irrespective of either the predictions of the linear analysis or the amount of CAPE in the initial conditions, the continual generation of instability through surface fluxes is necessary for incipient tropical-storm-like vortices to amplify and maintain themselves near hurricane strength. Because of this sensitivity O69 goes on to conclude that ‘... the supply of heat and moisture directly from a warm ocean is a crucial requirement for growth and maintenance of a tropical cyclone’.

CE64 introduced the phrase ‘conditional instability of the second kind’*. Does that mean that Ooyama (1964; hereafter O64) and O69, neither of which mention the phrase or the acronym, are irrelevant to a discussion of the idea or ideas behind the acronym? Because of the similarities between O64 and CE64 (the same idea was being explored, with a similar technique) they are often cited together. O69 explores and elaborates on the idea† which CE64 and O64 first addressed using linear analysis. Because many people, and certainly Ooyama, identified the term CISK with the ideas raised in CE64 and O64 (as opposed to particular models used to explore those ideas), O69 clearly can be interpreted as an investigation of CISK. In fact, O82 makes this point explicitly when he states his view of CISK as ‘... the conceptual content that has grown and matured with advances in modeling work’.

Because definitions of CISK are ultimately matters of opinion, discussions of the merits of the theory run the risk of degenerating into semantical debates rather than discussions of real scientific issues (cf. O82). Consequently, in addressing the merits of CISK in general, and the validity of ENB’s criticisms in particular, it is best to view the acronym from a variety of perspectives, which we group into three schools as discussed below.

(i) The ‘narrow-school’ views CISK as that which is explicitly contained in some subset of the models used to investigate or demonstrate the idea of cooperative interaction between convection and large-scale circulations (CIBCLSC). Some members of this school constrain CISK to be any linear CIBCLSC theory. Others argue that CISK is any CIBCLSC theory that is predominantly driven by an advective–convective interaction in which surface fluxes are at most a second-order effect. Others might view CISK in terms of those CIBCLSC models which employ a particular type of cumulus parametrization (i.e. one which is based on moisture convergence), irrespective of the energy source of the disturbance. Still others might

* Incidentally the acronym CISK appears to have been first suggested by Rosenthal and Koss (1969).

† One can trace the origins of this idea at least as far back as Eliassen’s (1952; 1959, section 9) work.

view CISK as any CIBCLSC theory that has a specific combination of the above features. Because of fundamental flaws in a variety of the CIBCLSC models, CISK can easily be depicted as a dead-end if it is defined in a sufficiently narrow manner. For instance, linear models are unable to describe finite-amplitude instabilities and have historically neglected fundamental processes such as air–sea interaction; moreover, many of the simple models are also demonstrably sensitive to the details of their cumulus parametrizations (e.g. Stark 1976; Pedersen and Rasmussen 1985; Emanuel 1993).

(ii) The ‘conceptual-school’ views CISK in terms of its conceptual content (e.g. O82) as applied to tropical storm-like vortices and as first demonstrated by O69. This school has come to view CISK as a fundamentally nonlinear process of tropical-storm *intensification* in which surface fluxes are ‘crucial’ (O69). An often-overlooked result that tends to support important elements (namely that tropical-cyclone intensification is a nonlinear process and that convection can project energy onto the slow manifold) of this school’s view of CISK is provided by geostrophic adjustment calculations (Schubert *et al.* 1980, section 9). These calculations show that because the local Rossby radius of deformation is reduced from its ambient value, tropical-storm-like vortices are characterized by an axisymmetric adjustment process which becomes increasingly efficient at adjusting the wind field to the mass field as the vortex intensifies. From this point of view the paper of Rotunno and Emanuel (1987) helps to clarify the important processes underlying conceptual CISK; it is not, however, fundamentally at odds with O69.

(iii) The last school views CISK quite generally and does not distinguish between it and CIBCLSC theories. In other words any theory which describes the cooperative interaction between convection and large-scale circulations is, in the view of this school, a CISK theory. Consequently, this school fails to differentiate between CISK as a theory of waves (e.g. Hayashi 1970; Lindzen 1974), and CISK as a theory of tropical-storm intensification. It also fails to distinguish between the roles of CAPE and surface fluxes in driving the growing disturbance.

In delineating the above three schools we have tried to represent a wide spectrum of thought. We agree that CISK may indeed be a dead-end when defined in a sufficiently narrow manner; however, when CISK is viewed more generally it better lends itself to the metaphor of a long and influential road, one littered with pot-holes and detours perhaps, but certainly not a dead-end.

3. LATENT HEATING AND KINETIC-ENERGY PRODUCTION

Closely linked to ENB’s criticism of CISK is their assertion that an important misconception in atmospheric science is that ‘heating *per se* leads to the production of kinetic energy’. They provide quotes from two textbooks as evidence of this misconception and remind us that the direct effect of heating is to alter the available potential energy (APE); if heating is positively correlated with temperature (i.e. $\overline{Q'T'} > 0$) then APE is generated. ENB argue that in the absence of certain feedbacks (namely their theory of evaporative wind feedback or WISHE) convection damps disturbances (i.e. $\overline{Q'T'} < 0$). This view of the tropics, which ENB call moist convective damping, is offered as a new or alternative paradigm.

The idea that $\overline{Q'T'} > 0$, for disturbances in the tropical-Pacific, has an observational basis that ENB make no reference to. Moreover, these observations motivate the textbook quotes which ENB call into question. They are also relevant to theories such as WISHE and deserve discussion. Latent-heat release in cumulus convection was diagnosed relatively early as the APE source for tropical storms (e.g. Riehl 1959). However, M. López’s observations* of cold cores in easterly waves prevented the generalization of this result, as it was recognized that in such a disturbance convective heating would consume, rather than produce, APE (e.g. Nitta 1970, section 1). Questions regarding the nature of the energy source for tropical waves partially motivated the pioneering studies carried out by the Tokyo University/University of California Los Angeles group (Yanai, Nitta, and colleagues) and the University of Washington group (Wallace, Reed, Recker, and colleagues). These studies indicate that although the correlation between apparent heating and temperature (i.e. $\overline{Q_1'T'}$) is negative at low levels, the overall correlation is positive over a considerable range of scales. An example is shown in Fig. 2, which we have reproduced from Nitta (1970). The same conclusion was reached in subsequent analyses of 1958 and 1967 Marshall Island data (e.g. Nitta 1972, Fig. 8; Wallace 1971, Fig. 19 and section VI B).

Although we wish to emphasize the earlier data, more recent analyses corroborate the earlier findings. For instance, Hendon and Salby (1994) use satellite data to infer that $\overline{Q'T'} > 0$ during the amplification phase of the intraseasonal oscillation (ISO). One of us (XL) has also analysed TOGA–COARE† data, the result of which is shown in Fig. 3, in which the vertical structure of the wave-averaged $-\omega'T'$ is

* M. López’s analysis was first published by Graves (1951) and then later republished by Riehl (1954).

† The Coupled Ocean–Atmosphere Response Experiment of the Tropical Ocean and Global Atmosphere programme.

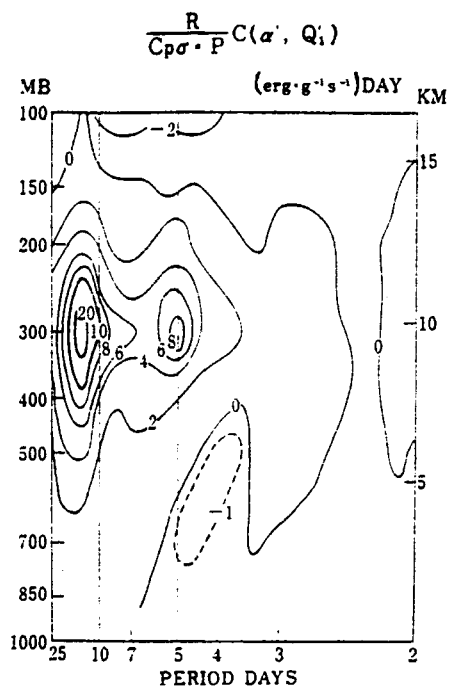


Figure 2. Vertical distribution of the co-spectra representing the generation of eddy available potential energy (from Nitta (1970) Fig. 5, which analyses Marshall Island sounding data from 1956).

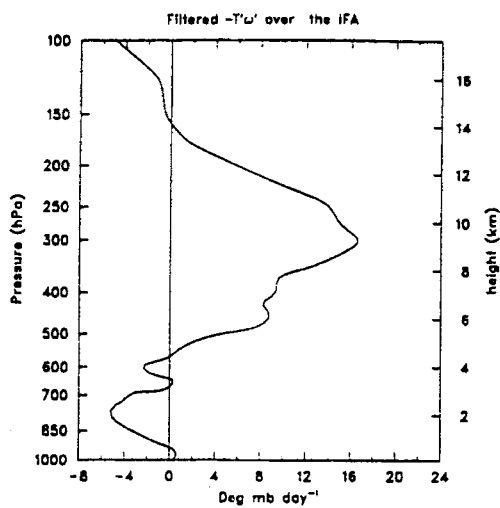


Figure 3. Vertical profile of the intensive observing period mean co-variance between temperature and vertical motion over the TOGA-COARE intensive flux array (IFA). Detailed information on data and analysis methods is discussed by Lin and Johnson (1996).

seen to be similar to that observed by Wallace. Observations of $\overline{Q'T'} > 0$ are not universal, however. For example, an analysis of GATE* data suggests $\overline{Q'T'} < 0$ over the eastern tropical Atlantic[†], indicating that convection acts to damp the observed disturbances (e.g. Norquist *et al.* 1977, section 4 and Fig. 1; Thompson *et al.* 1979, Table 7). Nevertheless, there exists an empirical basis for the idea that $\overline{Q'T'} > 0$, at least for tropical disturbances in the western Pacific. Exactly how this (i.e. $\overline{Q'T'} > 0$) comes about remains an open question.

One suggestion, advocated by ENB, is that WISHE describes the mechanism through which tropical waves generate, through the production of anomalous enthalpy fluxes, the energy necessary to maintain themselves against dissipation. In order to serve as a theory of eastward-propagating equatorially trapped waves (such as the ISO) WISHE requires mean easterlies and predicts total winds and surface fluxes leading the convection by a quarter cycle. Recent observations of the ISO based on TOGA-COARE data show that disturbances can maintain themselves and propagate eastward in mean westerlies, with the maxima in convection slightly leading surface winds and surface latent-heat fluxes (e.g. Lin and Johnson 1996[‡]). As suggested by Zhang (1996), the inability of the theory to explain these observations indicates that current models need further refinement. In addition to the equatorially trapped waves, WISHE predicts a number of other modes (Emanuel 1993), none of which are discussed in the context of current observational data sets (e.g. Reed and Recker 1971, Table 1; Thompson *et al.* 1979, Table 3). Although these observations are not necessarily fatal for the concept of WISHE, they do not bode well for the particular models which have, to date, been used to explore it. Previous work also indicates that a difference between many models, including those of ENB, and most observed disturbances is that the latter exhibit considerable vertical structure, and exist over a sea surface which varies substantially in space and time (Thompson *et al.* 1979; Nakazawa 1995; Lin and Johnson 1996).

4. QUASI-EQUILIBRIUM

ENB devote considerable space to reviewing, explaining, and advocating the concept of quasi-equilibrium (QE). For reasons that are not clear, they choose to rename this concept 'statistical equilibrium thinking'. We applaud their efforts to clarify the concept of QE, which continues to be misunderstood more than twenty years after it was proposed (Arakawa and Schubert 1974). We would, however, like to make two additional points.

First, one form of QE states that the cloud work function (which is closely related to the CAPE) 'is quasi-invariant with time' and that 'the changes in the CAPE are small' in convectively active regimes. How can we reconcile these assertions with data like those shown in Fig. 4, in which the CAPE ranges from several hundred Joules per kilogram (or more depending on the method of computation) to much smaller values, over a period of days? This point of view appears to be based on the tacit assumption that the assertion that the changes in the CAPE are 'small' means that they are small compared with the time average of the CAPE. In fact, however, this is not what is meant at all. Instead, the idea is that the changes in the CAPE are small compared with those that would occur if the convection were somehow suppressed while the non-convective processes continued to increase the CAPE with time. Mathematically, we write (following Arakawa and Schubert 1974):

$$\frac{dA}{dt} = \frac{dA}{dt} \Big|_{\text{cu}} + \frac{dA}{dt} \Big|_{\text{LS}} \approx 0. \quad (1)$$

Here A is the cloud work function, whose time derivative is partitioned, in (1), between contributions due to convective and non-convective processes. The QE closure is essentially a scaling approximation in which the left-hand side of (1) is neglected, on the grounds that convective and non-convective contributions to the time derivative are nearly equal in magnitude but of opposite sign. Observations such as those discussed by ENB show that this requirement is well satisfied. Consequently the observed day-to-day

* GARP (Global Atmospheric Research Program) Atlantic Tropical Experiment.

† The Norquist *et al.* (1977) data are more ambiguous in this respect, because at upper levels in the combined ocean/land region there appears to be considerable positive correlation between heating and temperature perturbations. Here we rely on their budget analysis over the ocean from which they infer a negative value of vertically integrated $\overline{Q'T'}$.

‡ Using a different analysis technique and a much coarser data set with much of the variance in the signal removed, Zhang (1996) suggests that latent-heat fluxes lead convection (by much less than a quarter cycle) and that the winds are in phase with convection. As indicated by Zhang these results are not consistent with explanations suggested by the simple WISHE models—even if the slight phase lead of the latent-heat fluxes is encouraging.

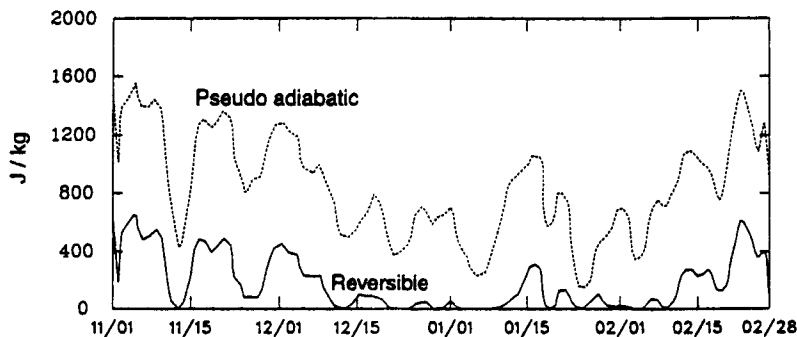


Figure 4. Time variations of convective available potential energy (J kg^{-1} , 5-day running mean) over the intensive flux array during the TOGA-COARE intensive observing period. The solid and dashed lines represent air parcels originating from 1000 hPa undergoing reversible and pseudo adiabatic processes, respectively; ice effects are not considered. See Lin and Johnson (1996) for a discussion of data and analysis methods.

changes in A , such as those shown in Fig. 1, are not necessarily in conflict with QE, as QE requires only that the variability of A be small compared with what it would be in the absence of convection. This interpretation of QE was clearly stated by Arakawa and Schubert, and has now been clearly re-stated by ENB, but somehow misunderstandings linger.

A second point is that, as already mentioned in the preceding paragraph, the mathematical formulation of QE, as proposed by Arakawa and Schubert, is based on a conceptual division of the menagerie of processes at work in an atmospheric column into two groups: 'convective processes' and 'non-convective processes'. As discussed by Randall and Pan (1993), such a distinction cannot be rigorously justified. For example, the stratiform clouds produced by the outflow from deep convection are often associated with additional latent-heat release, precipitation, and strong radiative effects. Should these be considered as convective or non-convective processes? Such stratiform clouds are strongly coupled with the convection, but on the other hand they have their own distinct life cycles and may persist long after the convection dies away. As a second example, surface fluxes of sensible and latent heat promote convection, but at the same time they can be directly coupled to convection through the action of convective downdraughts. It is thus difficult to classify these fluxes as either purely non-convective or convective. The need to classify unambiguously all processes as convective or non-convective appears to be an important conceptual weakness of using strict QE (as given by Eq. (1)) as a closure in a mass-flux representation of convection. Randall and Pan (1993) showed that such a classification is unnecessary when strict QE is replaced by a prognostic closure based on the convective kinetic-energy equation, through which QE is closely approximated. They argued that this is an important advantage of the prognostic closure, which can be interpreted as a kind of generalized QE.

5. SUMMARY

We have endeavoured to make three points, which we re-capitulate as follows:

- ENB's criticisms of CISK may apply to some narrow definitions of the theory. Nonetheless, for a long time now, the meaning of the term 'CISK' has been unclear, as over the years different schools of thought have interpreted the acronym differently and, not surprisingly, come to different conclusions regarding its merits. ENB's point that early theories over-emphasize ambient conditional instability is important and well taken; nonetheless, to characterize CISK as an '... influential and lengthy dead-end road ...' solely on this basis is unwarranted. This is particularly true in light of the contributions of O69, which emphasized the importance of surface fluxes to tropical-storm intensification.

- Observations have played an important role in determining how we think about tropical convection and its interactions with large-scale circulations. Observations show that latent heating is positively correlated with temperature in disturbances found in the tropical western Pacific (i.e. they are not moist convectively damped). The observations are also relevant to a number of explanations as to why this is so.

- Although Arakawa and Schubert's (1974) concept of QE is well supported by the observations, strict QE closure does not appear to be the best way to use their idea.

These comments notwithstanding, we reiterate that ENB offer a valuable and challenging view of the dynamics of convecting systems.

ACKNOWLEDGEMENTS

BS and XL are thankful for National Aeronautics and Space Administration Graduate Fellowships on Global Change (grants NGT-30231 and NGT-30190 respectively). DAR acknowledges support from National Science Foundation (USA) (NSF) grant ATM-9214981. MTM acknowledges support from NSF grant ATM-9312655 and ONR from grant N00014-93-0456 as well as enlightening discussions with K. Ooyama, L. Shapiro and M. Handel. A National Center for Atmospheric Research Mesoscale and Microscale Meteorology seminar given by R. Smith, and post-seminar discussions with R. Rotunno improved this manuscript, as did exchanges with K. A. Emanuel, J. D. Neelin, and C. S. Bretherton at a recent NATO summer school on moist convective processes. BS and XL are grateful to Wayne Schubert, Richard Johnson, and Brian Mapes whose wisdom substantially improved this comment. Our seminar also benefitted from the participation of Dave Alexander, Ligia Bernardet, Mark Branson, Ching-Hsuan Chen, Robert Cifelli, Donald Dazlich, Charlotte DeMott, Ping Ding, Cathy Finley, Tara Jensen, Hongli Jiang, Colin Jones, Michael Kelly, Jason Nachamkin, Scott Rafkin, Tom Rickenbach, Stan Trier, and Kuanman Xu; their collective insights greatly enriched our discussions. The FORTRAN code used to compute CAPE (as displayed in Fig. 4) was kindly provided by K. A. Emanuel (Massachusetts Institute of Technology).

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