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Reply to comments by Bjorn Stevens, David A. Randall, Xin Lin and Michael T. Montgomery on 'On large-scale circulations in convecting atmospheres' (July B, 1994, **120**, 1111–1143)

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1. INTRODUCTION

We very much welcome the comments on our paper (Emanuel *et al.* 1994; hereafter ENB) by Stevens *et al.* (1997; hereafter SRLM). We had hoped to provoke discussion and are very much gratified to learn of the semester-long seminar at Colorado State University, leading to the thoughtful response by SRLM. Herewith our response to their comments.

2. CISK

In contrast to SRLM, we feel that the concept of conditional instability of the second kind (CISK), as originally defined by Charney and Eliassen (1964) and also discussed by Ooyama (1964), is conceptually clear. Simply stated, it is an instability of a large-scale circulation due *specifically* to its interaction with an ensemble of convective clouds. We find no evidence in either of the above-cited papers that their authors intended CISK to encompass a feedback between surface winds and evaporation; such a feedback is not present in the simple models presented in those papers, nor is it entailed in the term 'moisture convergence'. Moreover, in all the rather prodigious literature on CISK that has appeared since 1964 there is not, to our knowledge, a single formulation that holds surface fluxes, rather than convection, to be the 'rate-limiting' process for tropical cyclones, to use the terminology of Craig and Gray (1996).

SLRM maintain that since its original 1964 definition, the term 'CISK' has evolved to encompass a broader range of processes, including WISHE (wind-induced surface heat exchange). Their reasoning seems to be as follows:

1. Ooyama was an early proponent of CISK.
2. Ooyama's (1969) model very clearly depends on surface fluxes to produce hurricanes.
3. Therefore, the importance of surface fluxes is entailed in the definition of CISK.

We strongly concur that Ooyama's (1969) model captures the essence of hurricane physics, but we do not agree with SLRM that Ooyama's concept of CISK, as articulated in that or subsequent papers, leads to a more favourable view of the theory. Indeed, Ooyama (1982) continued to argue that tropical-cyclone intensification is due to 'a cooperative process between the organized moist convection and the cyclone-scale vortex', and his own 'broadening' of the definition of CISK was meant strictly to account for the nonlinear contraction of the local deformation radius and does not account for feedbacks involving surface fluxes. In his discussion of intensification and CISK, Ooyama (1982) mentions surface fluxes only briefly and only in connection with steady-state maintenance. We cannot discern any sense in which WISHE can possibly be viewed as a refinement of Ooyama's (or anyone else's) articulation of CISK.

One confusing aspect of Ooyama (1969) is that his initial condition contained a high degree of conditional instability, allowing his linear analysis of the model to exhibit instability. Thus, Ooyama's *initial* condition was linearly unstable to CISK while it is equally clear that all but a trivial initial amplification of the model vortex is due to WISHE. (Subsequent experiments with Ooyama's model demonstrate that it exhibits finite-amplitude instabilities to WISHE, but no linear CISK instability when the high degree of conditional instability is removed from the initial condition.) We believe that the presence of both CISK and WISHE in Ooyama's simulation, coupled with subsequent attempts to 'broaden' the definition of CISK have had the unfortunate effect of muddling what should be clear and distinct physical concepts.

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We interpret SLRM's three alternative definitions of CISK as being so broad as to encompass anything anyone has ever believed to be true about the physical process of tropical cyclogenesis, thus rendering the concept useless. We defer to Charney and Eliassen (1964), the authors of the acronym 'CISK', for its definition and that definition is clear. We stand by our original opinion that CISK has been an influential and lengthy dead-end road in atmospheric science.

3. LATENT HEATING AND KINETIC-ENERGY PRODUCTION

In ENB we attempted to point out that heating by convection cannot be regarded as necessarily being positively correlated with temperature, and gave some simple examples of models in which the reverse is true. We argued that the interaction between convection and large-scale circulations, *by itself*, would lead to a negative correlation between convective heating and temperature, implying a sink of kinetic energy.

SRLM give some examples of instances in which observed tropical circulations demonstrate positive correlations between heating and temperature. These observations neither refute nor confirm ENB's argument, because they do not and cannot treat the interaction between large-scale circulations and convective clouds in isolation from other processes, such as WISHE, or feedbacks involving radiative transfer. A hurricane is an excellent example of a storm in which potential energy is converted to kinetic energy; this is manifested by a positive correlation between convective heating and temperature. Hurricanes are driven by WISHE, however, as envisioned by Riehl (1950) and Kleinschmidt (1951) and demonstrated by the numerical simulation of Ooyama (1969). Without elevation of subcloud-layer entropy by surface fluxes, an initial warm-core vortex decays owing to its interaction with convection (Emanuel 1989). But we do not expect to observe circulations with negative growth rates in the real world.

While we do not regard the observation of positive correlations between heating and temperature as a test of any particular theory about the origin of tropical circulations, particular applications of WISHE can be tested against observations. For instance, predictions of Emanuel's (1987) model of the intraseasonal oscillation (ISO) as an equatorially trapped WISHE mode with Kelvin wave structure have been compared with data from TOGA–COARE* (Lin and Johnson 1996) and large-scale surface analyses (Zhang 1996) and upper air/satellite analysis (e.g. Hendon and Salby 1994). These studies do not support the model, showing that the ISO is prominent in a region of weak mean surface westerlies rather than easterlies, and that the strongest surface enthalpy fluxes are west rather than east of the maximum convection. However, this does not rule out the possibility of other wave-like WISHE modes (e.g. those described by Emanuel (1993)) being relevant in other contexts, or that WISHE may combine with mechanisms such as surface-drag variations to create the observed waves. In addition, the zonal structure of the ISO is more complex than a Kelvin wave, so it may be misleading to base conclusions on wind and flux measurements only near the equator, rather than over a broader region.

From the modelling perspective, WISHE does appear to rationalize the behaviour of some aquaplanet general-circulation model simulations of the ISO in which there are mean easterlies around the equator (Neelin 1988). Xie *et al.* (1993) found ISO-like WISHE modes even in a model with no mean wind. On the other hand, linear studies using the Betts–Miller scheme (Neelin and Yu 1994) and the Emanuel scheme (Brown and Bretherton 1995) fail to produce CISK.

4. QUASI-EQUILIBRIUM

We are well aware of the distinction between quasi-balance approximations and exact balance, and ENB introduced the term 'strict quasi-equilibrium (SQE)' to distinguish it from quasi-equilibrium. Our discussion of SQE was intended as a conceptual aid in understanding tropical dynamics, not as an appropriate closure for a convective scheme, except perhaps as a leading order balance. Indeed, one of ENB's central points was that the departure of the atmosphere from SQE has important dynamical consequences, including moist convective damping. Even the simplest models of convection with a very small time lag are consistent with substantial fractional fluctions of convective available potential energy (CAPE). For example, the linear model of Emanuel (1993) with a small but non-zero convective time lag produces fluctuations of CAPE of about 200 J Kg^{-1} for moist wave number 1 Kelvin-type waves, given a zonal wind perturbation amplitude of 4 m s^{-1}. This is consistent with the amplitude of CAPE fluctuations shown in Fig. 4 of SLRM. Even so, we urge caution in evaluating CAPE from actual soundings. Figure 1 of this paper shows reversibly defined CAPE calculated using over 344 soundings from one of the same sounding stations used by SLRM to construct their Fig. 1. (All sounding parcel-origin levels associated with negative

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

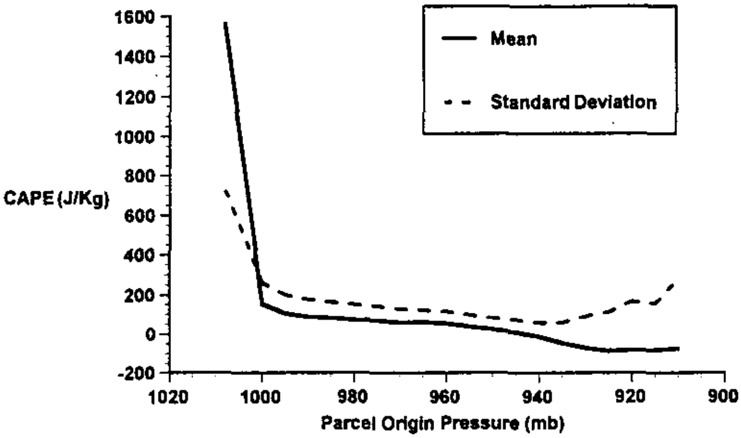

Figure 1. The mean and standard deviation of reversibly defined convective available potential energy (CAPE) as functions of the parcel-origin level, calculated using over 344 soundings from Kapangamarangi during TOGA–COARE. Sounding origin levels showing no positive buoyancy have been excluded from the averaging, and no freezing occurs in the lifted parcel.

values of CAPE were excluded from the averaging.) Clearly, the magnitude of CAPE is very sensitive to the assumed origin level of lifted air, with almost no CAPE for air lifted reversibly from above the surface layer. (We note, however, that serious flaws in the calibration and launch procedures of rawinsondes used in TOGA–COARE may strongly affect CAPE calculations using the first 10 or 20 mb of data.) Figure 1 also shows that the magnitude of fluctuations in CAPE is sensitive to the assumed parcel-origin level. The magnitude of CAPE fluctuations for air originating above the surface layer does not exceed that which arises solely from measurement noise, adding to problems of interpretation. A better understanding of the thermodynamics of convection as well as its connection to the boundary and surface layers is clearly desirable.

5. SUMMARY

ENB introduced 'statistical equilibrium thinking' as an alternative to the extant view of the interaction between convection and large-scale flows, a view that might be called 'CISK thinking'. We do not agree with SLRM that one aspect of statistical equilibrium thinking, WISHE, can in any sensible way be viewed as a refinement of CISK theory, which has always entailed a cooperative interaction between large-scale flows and convection, with surface fluxes playing, at most, a background role, never entering as an important feedback mechanism. We do agree with SRLM that there is a large range of what might constitute CISK thinking, but we believe that it ranges between wrong, in the worst cases, to merely awkward in others. The awkwardness arises, we believe, from a failure to take advantage of the fact that the buoyancy adjustment time-scale of convection is small compared with that of many larger-scale circulations. We hold statements such as 'hurricanes are driven by latent-heat release in cumulus clouds' to be analogous to the statement that automobiles are driven by the torque transmitted through the drive train. Neither statement is wrong; both are misleading. Both attribute causation to processes with relatively short time-scales. But hurricanes would not exist without convection any more than automobiles would operate without drive trains.

The advantage of statistical equilibrium thinking is that it places the emphasis on processes with comparatively long time-scales and regards convection as a fast, if complex, connector. In this view of the Hadley and Walker circulations, for instance, convection acts quickly to distribute the effect of the underlying sea-surface-temperature gradient over a deep layer, resulting in baroclinic pressure gradients that maintain the circulation (Held and Hou 1980). We find this more appealing than making a conceptual division between a dry circulation first being driven by latent-heat release and then feeding back on it. Similarly, the long-time-scale process that drives a hurricane is the feedback between the storm's circulation and evaporation from the ocean; no nonlinear model (beginning with that of Ooyama (1969)) has contradicted this. While our view discounts convection as the *cause* of large-scale circulations, we by no means wish to understate its fundamental importance to their dynamics. The relative magnitude of convective updraughts and downdraughts, which is governed by large-scale dynamical and cloud microphysical processes, strongly influences the development of tropical cyclones, for example, and no proper description of large-scale tropical circulations can be made without accounting for convective downdraughts. But we believe that the fundamental premise of statistical equilibrium thinking, that convection acts as a fast equilibrator of thermodynamic profiles, offers a simple and physically consistent view of tropical circulations that is in agreement with some, but not all, available observations. We hope that its predictions, for example that the interaction between large-scale waves and convection *per se* damps the former, will be subjected to rigorous tests using observational data and ensemble convection models.

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