Reply

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1. Introduction

Bin Wang presents a number of criticisms of the air–sea interaction model of intraseasonal oscillations presented by Emanuel (1987) and independently by Neelin et al. (1987). While the air–sea interaction postulate remains an observationally untested hypothesis, the author does not believe that any of Wang's criticisms rule out this mechanism as the basic cause of the 30–50 day oscillation either in general circulation models (GCMs) or in the real atmosphere. My responses to his particular comments follow.

2. The necessity of mean equatorial easterlies

The mechanism proposed by Emanuel (1987) and Neelin et al. (1987) does require mean equatorial easterlies. As Bin Wang points out, the climatology of near-equatorial zonal winds does show some small regions of mean westerlies, particularly in the western Indian Ocean in summer, reflecting the monsoon. Nevertheless, the oceans are dominated by easterlies. Wang notes that various studies have indicated that the oscillation has its maximum amplitude over the Indian and western Pacific Oceans, where the mean easterlies are weak or non-existent, but I seriously question the means by which a wavenumber 1 disturbance is localized. Many of the studies cited by Wang, for example, use outgoing longwave radiation as a measure of wave activity. This variable is closely related to vertical velocity which is a quarter wavelength out of phase with the surface zonal velocity in the linear theory. It is precisely when the vertical velocity maximum is located over the Indian and western Pacific Oceans that the maximum in easterlies occur approximately one quarter wavelength east over an area of prominent mean easterlies. Wang's implication that the air–sea interaction model predicts that amplitude should be correlated with the strength of the mean easterlies is at odds with both the theory and my description of it (Emanuel 1987) which stated that ". . . the largest amplitudes will occur over the warmest water. . . ." I do not see a conflict here between theory and observations.

3. The existence of intraseasonal oscillations in GCMs with swamp lower boundaries

Neelin et al. (1987) performed several long-term integrations of a version of the GDFL GCM. They found that when the sea surface temperature is fixed, the elimination of the wind-dependence of the surface heat fluxes dramatically reduces the amplitude of the ~24-day oscillation in the model. This result strongly supports the air–sea interaction model of intraseasonal oscillations. On the other hand, a strong peak at ~24 days occurs when the lower boundary is replaced by a swamp whether or not the surface fluxes are wind-dependent. This completely rules out the air–sea interaction model as an explanation of the oscillation in this case and is thus a much more serious criticism of the idea. Presumably, a detailed analysis of the GCM data will ultimately lead to a correct explanation of the model phenomenon, but for the present we will have to be content to speculate on the causes.

The possibility that the oscillations in the swamp model represent stable modes of the equatorial waveguide excited from higher latitudes cannot be ruled out. But it seems more likely, as Bin Wang suggests, that there is a local convective source of these waves. I should like to present some evidence that CISK-like behavior is possible and perhaps even likely in GCMs while it is probably not workable in the real atmosphere.

It should be first pointed out that diabatic heating is an exceedingly poor diagnostic to use in the tropical atmosphere. Whatever the source of large-scale motions, ascent will be accompanied by large diabatic heating which is nearly (if not exactly) cancelled by adiabatic cooling. The small difference between the two is energetically crucial, but observationally undetectable. Moreover, and contrary to what Bin Wang has implied, the structure of the correlations between heating and temperature perturbations cannot distinguish between the air–sea interaction mechanism and CISK, as I will demonstrate in the next section. But one feature of the atmosphere is entirely necessary for CISK and

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its absence would rule out CISK as an explanation of the waves. That feature is conditional instability. If the atmosphere is not baroclinic and is conditionally neutral or stable then there is no adiabatic redistribution of mass that can lower the center of gravity (unless there is evaporation of rain which in any event is not included in most GCMs). Thus there is no available potential energy. I argue that there is no conditional instability in the real tropical atmosphere (except on the scale of clouds) but that it occurs quite readily in GCMs with Kuo-type cumulus parameterizations or convective adjustment.

The primary evidence against the existence of large-scale conditional instability was first pointed out by Betts (1982). If a parcel is lifted reversibly from the subcloud layer and all its condensed water is retained in the definition of buoyancy, the parcel is almost exactly neutrally buoyant up to about the 0°C isotherm and stable above that level. We have systematically examined a large number of rawinsoundings from the western equatorial Pacific and found this condition to be robust. In all soundings that were not decidedly stable, the standard deviation of the parcel buoyancy was nearly equal to the measurement error associated with rawinsondes, illustrating that the tropical atmosphere is not only neutral, but systematically so. In view of the fact that the convective adjustment time scale is much less than the time scale of most tropical circulations, we should not be surprised to see near-neutrality in regions of active convection, any more than we should be surprised to see that the subcloud layer is almost exactly dry adiabatic. Even if there remains some small residual conditional instability, theory strongly suggests that it will be released on the smallest scales (even simple CISK models suggest this, notwithstanding that their formulation makes no account of the actual conditional stability of the basic state).

In spite of the observed near-absence of conditional instability in the tropics, the tropical atmosphere of GCMs can be quite unstable. This is so, for example, in the ECMWF model (Martin Miller, personal communication). The reason for this is contained in the representations of cumulus convection in the models. In Kuo-type schemes (as in the ECMWF model) convection is not permitted to occur without moisture convergence. Thus large instability can build up in regions of weak moisture divergence, to be released suddenly when and where the large scale becomes convergent. There is a chicken-and-egg problem here: if convection were permitted in unstable but initially divergent conditions it would lead to large-scale moisture convergence by the induced circulation, but since moisture convergence is a prerequisite, nothing happens until other factors cause large-scale convergence. This artificial aspect of Kuo-schemes causes "grid-point storms" and probably other problems as well. Convective adjustment schemes (as in the GFDL model) require the explicitly computed relative humidity to exceed some threshold value; if it does not, no convection occurs in spite of possible large amounts of conditional instability.

In the author's view, the difference between the observed small conditional instability of the real atmosphere and the often large instability of GCM atmospheres is one of the principle obstacles to a clear understanding of the dynamics of large-scale tropical circulations. In particular, the presence of conditional instability in GCMs makes CISK a distinct possibility in those models while it can probably be ruled out in the real atmosphere. Perhaps this is why the intraseasonal oscillations of GCMs are somewhat faster than their observed counterparts.

4. Wave energetics and the use of heating and temperature perturbation correlations as a diagnostic of wave dynamics

Wang, at the beginning of his section 3, misquotes me as saying that temperature anomalies lead surface convergence by one quarter wavelength. What I actually said was (Emanuel 1987) that "tropospheric heating [I should have said "warming"] will lead the wave vertical velocity by a quarter wavelength." Moreover, "the negative zonal velocity perturbations lead the θ_e^* (i.e., temperature) perturbations by a little less than 45°." The development at the beginning of Wang's section 3 is thus unnecessary.

Wang's statement that in the air–sea interaction model "there is no conversion between eddy available potential energy and eddy kinetic energy" is simply wrong; were it correct there would be no unstable modes. He seems to be confused about the difference between warming and heating, as illustrated by his Fig. 3 which, as far as I can tell, shows the correlation between temperature and evaporation rather than heating. Had he shown the correlation of temperature and heating (which was carefully stated in Emanuel (1987) to be mostly correlated with vertical velocity, but with warming $\sim 45°$ out of phase) he would have avoided the false conclusions.

I would now like to argue that the covariance between heating and temperature is an exceedingly poor diagnostic of tropical wave dynamics. This is because the phase relationship between temperature and vertical velocity (w) (and therefore heating since heating and vertical velocity are always strongly correlated in the tropics) will be the same no matter what causes the wave as long as that mechanism produces approximately the right oscillation frequency and wavenumber. To see this, it is only necessary to examine the linearized zonal momentum equation at the equator. Given that the surface pressure perturbation is in phase with the tropospheric mean temperature perturbation (as observed and as necessary hydrostatically), the linear zonal momentum equation can be written

$$(D + 2F)u = ikT, \tag{1}$$

where D is the complex growth rate, F is proportional to the surface drag coefficient, k is the zonal wavenumber, u is the perturbation zonal velocity and T is the tropospheric mean temperature perturbation (see Emanuel, 1987 for normalizations). An equation like this is used in virtually all linear models; it contains no information about the thermodynamics. Along the equator, the continuity equation demands (for the meridionally gravest modes) that

$$w \sim -iku. \qquad (2)$$

Combining (1) and (2) we obtain

$$(D + 2F)w = k^2T. \qquad (3)$$

Thus models that produce the same complex growth rate D and use the same friction coefficient F will produce the same phase relationship between w and T regardless of the model thermodynamics. The air–sea interaction model thus produces patterns of Q and T similar to Wang's Fig. 4b since it gives oscillation frequencies close to those observed. (Although the model uses vertically integrated temperature, the assumption of moist adiabatic lapse rates directly implies that the strongest temperature perturbations will be in the upper troposphere where the temperature difference between adjacent moist adiabats is largest. This is consistent with Wang's Fig. 4.) I conclude that phase relationships between heating and temperature are no better tests of model accuracy than is the comparison of model-produced with observed oscillation frequencies.

Finally, Wang notes that the perturbation evaporation rate in GCM simulations is an order-or-magnitude smaller than the precipitation rates, as also pointed out by Emanuel (1987) in reference to the air–sea interaction model. He then goes on to state that because diabatic heating is nearly in phase with rising motion, "the GCM results suggest that the condensational heating associated with moisture convergence plays a more important part than that of evaporation anomalies." This non sequitur is in direct contradiction with the GCM experimental results of Neelin et al. (1987), and once again results from a mistaken identification of heating with kinetic energy generation. Such energy generation requires a correlation of heating and temperature perturbation, the latter of which can only arise in a convectively adjusted atmosphere by perturbing the boundary layer entropy.

5. Summary

In response to Bin Wang's comments I have argued that (i) although the fluctuations of cloudiness associated with the 30–50 day oscillation are maximum over the Indian and western Pacific Oceans, the zonal velocity perturbation would be expected to have their maximum values ¼ wavelength east in regions of moderate mean easterlies; (ii) the real atmosphere appears to be nearly conditionally neutral and thus energetically incapable of supporting CISK even though GCM atmospheres can be and are conditionally unstable by virtue of artificial restraints on parameterized convective activity; (iii) therefore GCMs may very well contain CISK modes that are artifacts of the cumulus parameterizations; and (iv) any model that produces approximately the right oscillation frequencies and wavenumbers will perforce have the correct phase relationship between heating and temperature regardless of the physical mechanism, so that these phase relationships are not useful diagnostics of the model dynamics.

The same GCM that Wang refers to was used by Neelin et al. (1987) to show that evaporation–wind feedback is crucial to producing reasonable spectral peaks in the intraseasonal range when a fixed ocean is used. Since the convective parameterizations used by GCMs permit large artificial build-ups of conditional instability, however, CISK modes are permitted and may be the source of the model oscillations when a swamp lower boundary condition is used.

REFERENCES

Betts, A. K., 1982: Saturation point analysis of moist convective overturning. *J. Atmos. Sci.*, **39**, 1484–1505.

Emanuel, K. A., 1987: An air–sea interaction model of intraseasonal oscillations in the tropics. *J. Atmos. Sci.*, **44**, 2324–2340.

Neelin, J. D., I. M. Held and K. H. Cook, 1987: Evaporation–wind feedback and low frequency variability in the tropical atmosphere. *J. Atmos. Sci.*, **44**, 2241–2248.