Reply

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19 June 1986

We appreciate the commentary on our paper by McBride and Willoughby (1986, hereafter referred to as MW). We must state at the outset that the goal of a simple model such as ours is not a faithful simulation of a natural phenomenon in all its details. Rather, a simple model seeks to isolate a mechanism, which is perceived to be important in a certain aspect of the phenomenon, and tests the hypothesis of its importance. Namely, one starts off with a hypothesis that a certain process or processes are important and formulates a simple model based on this hypothesis. Since the model is not expected to reproduce all the features of the phenomenon, it is important to establish the criteria of success or failure of the simulation at the outset in terms of which features the model is expected to reproduce. After the model runs, one asks questions (i) and (iii) using the established criteria of MW. If the answers to both questions are affirmative, then we have a strong sense that the answer to the question (ii) is affirmative, and the hypothesis will be retained. On the other hand, if one of the answers is negative, then the answer to (ii) will be negative and the hypothesis rejected. Viewed in this way, one recognizes an important point that the failure of a simulation does not mean the failure of the model. Indeed, if one could conclusively reject a hypothesis through a negative answer to (i) or (iii), then the model must be considered a success, since we have narrowed the choice of possibly important processes and thus have gained a greater knowledge. We agree with MW that the question (iv) is an important but often neglected one. Often the best way to answer (iv) is to perform model runs for different initial and/or boundary conditions and see if the model yields any indication that the apparent success or failure of the principal simulation has been spurious.

In our paper (Kurihara and Kawase, 1985, hereafter designated KK) we hypothesized that the process of synoptically controlled latent heat release and nonlinear advection of momentum and temperature in the x–z plane can cause the trough phase of synoptic scale wave disturbance on the trade wind system to grow and contract, eventually to the strength and size of a tropical storm. Needless to say, our hypothesis does not hold for those stages and types of tropical disturbances in which, as envisaged by Ooyama (1982), the systems are controlled more by the inner mesoscale activities than by the synoptic scale conditions.

Accordingly, we have built a simple model that incorporated two processes we considered important. In adopting a slab geometry, we have omitted the effect of vorticity of the mean flow or nonlinear advection in the north–south direction. Omission of the mean flow vorticity was motivated by the finding of Tuleya and Kurihara (1981, hereafter, TK) that mean flow shears are not indispensable for tropical cyclogenesis. While we do not deny that tropical cyclones can grow out of barotropic instability of the mean flow, we did not consider such a process here in order to isolate the importance of processes mentioned above. Omission of nonlinear advection in the north–south direction was adopted in analogy with Hoskins' (1975) model of extratropical frontogenesis. Although this simplified the model tremendously, it did limit the model's ability to simulate many observed features.

For the principal experiment, we chose a mean flow with an easterly vertical shear. We must point out that, contrary to what MW states, we did not choose the climatological state of the trade wind system as the mean flow. Rather, we chose a mean flow that we thought was conducive to the growth of the disturbance. Tuleya and Kurihara have found that an easterly shear is favored for the development of a disturbance while a westerly shear seems to suppress it. As MW points out, most of the trade wind disturbances in the North Atlantic do not grow to become tropical storms. This may well be because the climatological flow condition is not favorable for their development. A reversal, or at least a relaxation of the westerly shear, may provide a favorable condition for cyclogenesis in that region. Furthermore, the genesis of a tropical storm is sensitive not only to the mean flow condition but also to the other factors such as the sea surface temperature and the vertical and horizontal distributions of the temperature and moisture. Accordingly, a real disturbance superposed on a mean flow that is favorable for the tropical cyclogenesis does not necessarily attain the tropical storm intensity.

For the initial disturbance, we chose one of the dry normal modes of the linearized system with the mean flow we used. While we tried to choose the mode that most resembled the observed easterly wave, again we did not choose an observed disturbance as such. If we did so, the disturbance, if not balanced, may quickly disintegrate into different Rossby and inertial–gravity modes of the system in the course of the model integration.

The initial wave had zonal wavelength of 2500 km. While this was the maximum zonal scale that could be represented by the model, the model was free to generate smaller scale vorticity and divergence down to the grid scale (62.5 km). The wave deformation due to the effect of nonlinearity, i.e., the wave cascade, was fully expected and indeed is essential for the contraction of the wave trough. Concerning the shaping of the tropical cyclone, MW suggested that a vortex becomes stiff as the positive vorticity concentrates. Our slab-symmetric model could not include such an effect related to the curvature of flow.

We used a mixture of Ekman and wave-CISK parameterization to represent latent heat release. Use of this type of parameterization is a controversial point (Ooyama, 1982). In particular, it has the disturbing feature that disturbances with the smallest scale grow fastest. We do share MW's apprehension about this point and admit that the results of this model must be qualified as reasonable only to the extent that CISK parameterization is reasonable.

The principal experiment was carried out with heating only [EXP(0, 1)], nonlinearity only [EXP(1, 0)], and both processes incorporated [EXP(1, 1)]. The results were compared with the free run of the normal mode [EXP(0, 0)]. The principal features we sought in the results were 1) growth of a disturbance, 2) contraction of the cyclonic phase of the disturbance, and 3) formation of a warm core above the surface low pressure. To summarize the results, we obtained the growth of the wave only when heating was turned on [EXP(0, 1) and EXP(1, 1)]. Both of these growing runs developed a warm core above the surface low, centered at 600 mb. As MW mentions, this is substantially lower in height than the observed warm core in a tropical storm (at 300 mb). We may attribute this discrepancy partly to the lack of vertical resolution in the model upper atmosphere. In the model, an anticyclone appeared at 300 mb, which was coincident with the upper level divergence. Since the warm core must be contained below the level of maximum anticyclonic perturbation, this requires that the warm core cannot be situated higher than the 400 mb level of our model. The form of the function specifying the vertical distribution of heating in the parameterization may also be responsible.

When nonlinear effects were included in addition to the heating, we saw the anticipated contraction of the cyclonic phase of the wave. This also resulted in the faster increase of relative vorticity in the cyclonic phase compared with EXP(0, 1). We now feel that our description and interpretation of this result in our original paper could lead to misunderstandings. As we pointed out in our paper, if the flow remains close to geostrophic balance, nonlinear vortex stretching has the effect of enhancing cyclonic vorticity and diminishing anticyclonic vorticity. This effect can take place regardless of the heating effect. The only requirements for heating is that the flow remain close to geostrophic balance with the mass field or, in other words, that the nonlinear cascade occurs within the domain of Rossby modes. Inspection of our Fig. 3 reveals that, while EXP(1, 1) resulted in a greater cyclonic vorticity at the surface than for EXP(0, 1), the cyclonic phase of the wave contracted significantly in the former case and, in fact, the amplitude of the anticyclonic phase for the former case was less than for the latter case. This indicates that the enhanced growth of the maximum cyclonic vorticity at the surface when nonlinearity was included was primarily due to the effect of nonlinear vortex stretching, rather than to actual enhancement of heating due to nonlinearity as we implied in our paper.

In conclusion, our primary experiments have confirmed our hypothesis concerning the growth and transformation of an easterly wave: latent heat release is crucial in causing the wave to grow while nonlinear advection of vorticity and nonlinear vortex stretching cause contraction and intensification of the cyclonic phase of the wave. Such processes, if continued, can transform an easterly wave into a compact tropical storm.

Our supplementary experiments were performed to try out the model under different mean flow and initial conditions. One of the important discoveries in TK's study of the effect of environmental conditions on tropical cyclogenesis was that cyclone development occurred favorably under the condition of easterly vertical shear. Tuleya and Kurihara conjectured that the warmed upper air has to be advected in the same sense as the propagation of the low level disturbance since the presence of warm core aloft is required to keep the flow in geostrophic balance. In our paper we tried to reproduce this result, since our model seemed to contain all the factors to prove the above hypothesis. We have also tried to see whether the initial structure of the disturbance affects its subsequent development.

We have tried five different mean flow conditions (two with easterly shears, two with westerly shears, and one of no vertical shear) and three different initial disturbances (dry normal modes for an easterly shear case, a westerly shear case, and the no-shear case). For each combination of the mean flow and initial condition, EXP(1, 0), EXP(0, 1), EXP(1, 1) (see above) were performed. The results of these 45 experiments are summarized in terms of maximum surface vorticity at 36 h of integration in KK's Table 5.

It was shown that the conclusion of our primary experiments held under different mean flow conditions for three initial disturbances examined. Also, in agreement with TK, easterly vertical shear led to stronger growth of the wave. However, as MW points out, an enhancement of growth of the wave took place under westerly shear as well in our supplementary experiments; this result was not observed in TK. In order to find a clue for explaining the above discrepancy between our model and the more comprehensive three-dimensional model, we should look into physical features that were present in TK's model but missing in ours. In this respect, we note here that one of the differences between the two models is the treatment of hydrologic cycle, on which the evolution of waves can be quite sensitive. In TK, the water vapor budget was explicitly computed and a convective adjustment scheme was used. The distribution of relative humidity in the boundary layer of a three-dimensional model is generally unhomogeneous, e.g., the air to the east of a disturbance in the experiment by Kurihara and Tuleya (1981, Fig. 20) was relatively dry. In contrast, in our model in which the CISK heating was used, the moisture in the boundary layer is implicitly fixed at a constant value and the heating profile was prescribed. Thus, certain aspects of our experiments possibly suffered from simplified representation of a physical process. Also, the effect of vertical shear on the slab-symmetric wave can be different from that on a three-dimensional vortex. In any case, more study is needed on the sensitivity of the evolution of tropical disturbances on the vertical shear of the mean flow in the presence of the effects of diabatic heating and nonlinearity. When we find the answer to the above issue, we may be able to evaluate to what extent the processes incorporated in our model are at work in the real atmosphere.

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