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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Intermediate and high resolution numerical simulations of the transition of a tropical wave critical layer to a tropical storm" *by* M. T. Montgomery et al.

Anonymous Referee #1

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Summary:

This paper describes numerical simulations of tropical cyclogenesis from a cyclonic disturbance associated with a zonally periodic wave meant to represent an Easterly wave progating along an easterly jet. Both low-resolution and high resolution simulations are analyzed, and their differences discussed. It is argued that many features of the simulations support the authors previously published paradigms about TC genesis, that 1) genesis occurs along the critical line of the wave where the flow speed of the wave matches the local zonal flow speed, and 2) genesis is a "bottom up" process defined by the formation and merger of vortices generated by localized deep convective

events, the vortical hot towers (VHTs).

The method of developing the basic state and the accompanying easterly-like wave is quite nice. However, I believe there are some substantial flaws in the model configurations that render the simulations, and their interpretations, subject to considerable doubt. Unfortunately, additional simulations will be required to address these problems. There are also many problems with the analysis and discussion that are outlined below. For these reasons I would say that substantial revisions are required before publication.

Major comments:

1) The authors have made a noble effort in reproducing the basic state of Kurihara and Tuleya (1981). But is that what they really want to do? The KT81 basic state jet has peak easterly flow at the surface, as does their initial wave disturbance, and these features are well-reproduced in this study. But the real easterly jet has its peak wind speed at 700 hPa (see KT81 Fig 2a), and it is (now) well known that real easterly wave disturbances have their peak flow near this height too (sometimes lower, sometimes higher). If these results are intended to make a "quantum leap" in dynamical understanding of TC genesis, why not use a basic state and perturbation that is much closer to reality?

2) There are several aspects of the parameterizations and nesting that raise concerns.

a) First, regarding the 28 km simulation with BMJ cumulus parameterization, is there no microphysics scheme active at all? This is not clear. If the warm-rain scheme is on, see (b).

b) For the high-resolution simulations with warm-rain microphysics, do these also have BMJ on the large grid? That would be somewhat concerning, because there is a mismatch between these two schemes. The BMJ scheme makes precipitation by relaxing the moisture and temperature profiles back to some pre-determined sounding (in some energetically consistent way). But that sounding is based on an atmosphere that does have ice...while the warm rain scheme operates as if ice does not exist.

c) In both the warm-rain and ice physics simulations, the inner nested grids are not "turned on" until 46 h into the simulation. The authors are to be commended for providing this important detail, and describing the "adjustment" that occurs after the high-resolution nests are activated. However, this adjustment occurs at a critical time during the genesis process, especially for the warm-rain simulation. More on this below.

3) The authors point out that these simulations do not appear to have the same sequence of events leading up to genesis as that described by Nolan (2007), most notably an increase in humidity at middle and upper levels before substantial surface vortex intensification. To make this point, a plot of area-averaged RH at 500 hPA is shown in comparison to plots of area-averaged vorticity at 900 and 500 hPa. There are several problems with this:

a) It is stated that the increase in vorticity at 900 precedes the increase in RH at 500. But, RH at 500 does indeed increase substantially from 65% to near 80% from hours 48 to 54 (it may have come up from an even lower value but this can't be seen from the figure). This rise in RH stops for almost 12 hours. But more importantly, both this rise and the interruption are suspect because they occur during the adjustment time after the inner grids have been introduced.

b) It seems strange to use RH at 500 hPa, because from the time-height diagrams it is clear that this is the level with the lowest values and slowest increase in RH. Nolan (2007) did not specificy this or any level as the most important, referring mostly to the general saturation of the core at middle and upper levels. Furthermore, it is evident that RH at 500 does reach very high levels after 72 hours. When comparing to Figure 7, this time precedes both the time when the surface pressure begins to fall and the beginning of the rises in maximum 850 hPA wind and vorticity; the pressure fall would be the only "observable" measure of genesis for most cyclones.

Although they do not increase faster than the low-level values, note also that substantial increases in mid-level vorticity do occur before "observable genesis" for both the warm-rain and ice physics cases.

c) There is a mismatch in the RH figures between the warm-rain and ice-physics cases. RH reaches very high values at high altitudes for the warm-rain case, where it does not for the ice-physics case. My guess is that RH is being computed w.r.t. to water only in both cases. For the ice case, it is clear that the atmosphere is nearly saturated up to 600 hPa by t = 120 hours, and it may also be as high or higher above if the right RH formula is used. This again is well before "genesis" would be estimated from Figure 7.

I suppose (1) above is a philiosophical matter, but the points raised in (2) and (3) are serious. Frankly speaking, my advice to the authors would be to abandon the warm rain simulation. It does not offer sufficient simplification (such as allowing the use of analytical techniques) to be justified in the face of the important differences in the structure of convection. If the results of the ice-physics case are basically the same, why not just use them? Further, I hope the authors will not that this reviewer is not rejecting the primary assertions of the paper. After revision, this paper may be able to make essentially the same points, although I suspect that more overlap with other ideas about TC genesis will be found.

Minor comments:

p. 26152/fig 4f: I propose showing profiles of RH instead of mixing ratio, who can possibly look at a plot of mixing ratio and see if the air is "dry" or "moist." Is there a mid-level minimum in RH? (vs. ice or water?)

pp. 26160-26161: The authors state that the low values of theta-e near the surface deny the importance of surface fluxes and the WISHE mechanism before genesis. This is a mis-interpretation of WISHE: it is not about high theta-e values, but about large fluxes. Lower theta-e values actually cause larger fluxes, so that the ocean can more rapidly communicate its heat energy to the atmosphere. Without the fluxes, the low-level theta-e values would be even lower (like for convection over land).

p. 26164: It will be interesting to see the vorticity budget analysis for the ice physics simulation.

p. 26166: It is not clear what the convective-versus stratiform analysis adds to the paper. Furthermore, it is generally known that numerical models have too much deep versus stratiform convection in the tropics and tropical cyclones. The relative absence of stratiform precipitation before t = 96 h is very surprising. There could be some bias here.

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