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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropical cyclogenesis in a tropical wave critical layer: easterly waves" *by* T. J. Dunkerton et al.

Anonymous Referee #1

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Review of "Tropical cyclogenesis in a tropical wave critical layer: Easterly waves" by T. J. Dunkerton, M. T. Montgomery, and Z. Wang, published in "ACPD".

Recommendation: Accept with (possibly substantial) modifications suggested.

General comment

This paper addresses the thorny issue of tropical cyclones (TC) genesis focusing on the birth of TCs spawned by the critical layer of a wave. It is well known that waves such as African Easterly Waves in the North Atlantic are conducive to TC genesis, although the precise mechanisms remain elusive. The paper combines careful analysis of many cases of observed TC genesis with theoretical reasoning derived from critical layer theory. As such it presents a novel and unique approach to this important problem and deserves publication as a paper.

The paper presents three hypotheses providing a partial explanation for one particular avenue of TC genesis. All three hypotheses center around the question how a proto vortex is born in the critical layer of a wave. (1) Parent to the proto vortex is the wave's critical latitute in the lower troposphere (bottom up development). (2) Important to the formation of the proto vortex is the existence of closed material contours in the critical layer. (3) There is some coexistence of diabatic proto vortex and parent wave. This sequence of steps is dubbed as "marsupial paradigm".

The main part of the paper aims to verify the hypotheses based on several dozens of representative cases. Key step in the analysis is the transformation to a moving frame of reference matching the phase speed of the parent wave. It is argued that this moving frame of reference provides a perspective which is close to Lagrangian at least during the last few days prior to TC genesis, with streamlines being close to trajectories. This provides an alternative and more useful view on the topology of the flow (closed streamlines, separatrices etc.) which is more revealing than the non-moving frame of reference considered by forecasters in the operational hurricane centers.

ACPD

8, S3935–S3939, 2008

Interactive Comment

Overall I find this analysis very interesting and thought provoking. The observational analysis convincingly corroborates the hypotheses put forth at the beginning. Below I make some remarks which may help to bring out the key points more clearly.

This is probably the main issue of concern: the paper is quite exceptional in terms of length. To me it appears more like an essay than (what these days constitues) a scientific journal article — it is very verbose including lots of footnotes (all of which are intersting but some of which lead rather far away from the actual topic of the paper). To be sure, there is nothig wrong with this, indeed the paper is well written, and I enjoyed reading it. However, I had to split my reading into several pieces owing to the length of the paper. This I found somewhat annoying, because I had to remind myself of the earlier parts by repeatedly reviewing them. I suspect that many potential readers will have the same problem, and some may not get all the way to the end for this reason (what a pity!). Also, due to the paper's length there is some repetitiveness, and sometimes it was not clear to me whether a statement is part of the accepted wisdom or rather new insight derived from the present analysis. In summary I believe that the paper could be made significantly more concise without loosing substance.[1] Actually, wouldn't this be a major attraction of the paper: The current state of affairs is that there are plenty of supposedly relevant details (which the authors are well aware of), but many of these are more or less unconnected. The masupial paradigm introduces some order into this chaos! So the paper could focus mostly on those aspects which have direct relevance for and provide support to the new paradigm. Of course I am aware that this would

[1]This is the story about Thomas Mann: When he submitted his first novel "Buddenbrooks" to a publisher, the latter returned the manuscript saying that it was OK but needed substantial shortening before he would consider publication. Thomas Mann replied that it was just the right length and did not change a thing. It was published without modifications. Later, Thomas Mann was awarded the Nobel price for this novel.

ACPD
8, S3935–S3939, 2008

mean a significant revision (and I do not mind if the authors choose not to do so).

Specific comments

One key aspect of the analysis is the changed topology of the flow field in the moving frame of reference featuring closed streamlines (which in turn are approximately equal to trajectories). The authors add the argument that the closed streamelines are important for the genesis of a moist proto vortex because this region is protected from intrusion of dry air from outside. However, it is not clear to me why the air outside the separatrix is by necessity dryer than the air inside the separatrix. Why should this be so? If moistening happens through surface fluxes and associated deep convection, then the topology of the flow should not have any influence on the location of moist atmospheric columns; in fact, the moving frame of reference might be misleading, because surface fluxes depend on the surface wind in the original (unshifted) frame of reference.

The analysis is mostly kinematical (see my item above). The authors do not say much about dynamical aspects regarding the transition from a wave to a vortex (roll-up of PV or similar). Does this mean that these dynamical aspects are not important? After all, the equations describing a vortex with approximate circular geometry are significantly different from analogous equations for slab symmetry (cf. Ulrich et al. 2002).

Some of the figures (e.g. figures 25 and 26) are not really discussed in the main text. If the authors choose to shorten the paper, they should consider reducing the number of figures by showing only those which make an important point in the argument.

ACPD

8, S3935–S3939, 2008

References

Ulrich, W., R. K. Smith, and N. C. Mai, 2002: Comparison of an axisymmetric hurricane model with the corresponding slab-symmetric ITCZ model. *Quart. J. Roy. Met. Soc.*, **128**, 2335–2347.

ACPD

8, S3935–S3939, 2008