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Discussions

Interactive comment on "Interaction between dynamics and thermodynamics during tropical cyclogenesis" *by* S. Gjorgjievska and D. J. Raymond

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This represents a stimulating and original contribution that has grown out of several previous papers by Raymond and his colleagues. The basis for Gjorgjievska and Raymond's theory of tropical cyclogenesis arises from cloud-permitting model simulations by Raymond and Sessions (2007). These showed maximum vertical mass flux near 10 km altitude in the undisturbed tropics, but simply by cooling the lower troposphere about 1K and warming the upper troposphere by 1K, the authors found that maximum vertical mass flux shifted downward to near the 5 km level. Assuming balanced dynamics, a midlevel vorticity maximum in a pre-tropical cyclone disturbance contains

analogous temperature anomalies, cool below and warm above, to those tested by Raymond and Sessions. As a result, the authors argue that the presence of a midlevel vortex favors maximum mass flux below the midtroposphere. Via mass conservation principles, this provides a mechanism for lower tropospheric spinup of vorticity. The authors have built a theory for tropical cyclogenesis by adding measures of convective instability, moisture content, and gross moist stability to the basic concept derived from the cloud-permitting model results. These ideas provide an alternative mechanism for previous arguments of the importance of midlevel vortices (e.g., Ritchie and Holland 1997; Bister and Emanuel 1997). I especially like the evidence in Fig. 5 that deep convective instability does not produce low-level increases in vorticity; rather, low levels spin up when instability is relatively low. This is a good insight.

Having said this, I have a few questions that would be worthy of debate. Because of the importance of the Raymond and Sessions arguments in this paper, the first question relates to the use of the 2007 results.

1. It is difficult to grasp the nature of the shallower vertical mass flux maximum in the Raymond and Sessions (2007) paper. Why does cooling below and warming above increase the midlevel mass flux? Does it relate to variations in parcel buoyancy, altered background relative humidity, and/or changes in stratiform fraction? I would like to see clearer explanations for the differences in vertical mass flux from the 2007 paper, although this paper is not necessarily the place to do so.

The remaining questions are directly concerning this manuscript.

2. Easterly waves and other pre-existing wave disturbances prior to tropical cyclone formation are usually cold-core in the lower troposphere, since their maximum vorticity often lies at 600-700 hPa. The magnitude of the temperature anomaly is typically about 1K, similar to that tested by Raymond and Sessions. Why wouldn't these disturbances already have maximum mass flux near 5 km?

3. The areas selected for averaging (see, for instance, Figs. 8-9) seem arbitrary. At first

I thought it would be best to define a circulation center, calculate azimuthally-averaged tangential velocity, and choose the averaging area as encompassed by the outermost edge of cyclonic mean flow. But the vortices were not completely captured by the dropsonde distribution, and this solution is not feasible (this comment is not a criticism of the PREDICT choices of lawnmower or square spiral patterns, which I believe are optimal for measuring these disturbances). As a result, I accept the choices that were made, but at least some basis must be provided for how the averaging areas were defined.

4. The definition of instability in this paper is roughly equivalent to pseudoadiabatic CAPE. In a recent paper (Molinari et al. 2012 JAS), we showed that in a fairly dry column CAPE estimates can be reduced by up to 90% when entrainment is included. This might be one reason that Gaston never experienced a top-heavy mass flux, even though during three missions, its instability was larger than the maximum instability reached in Karl.

5. The theory as proposed must be considered incomplete. The results are dependent upon the area chosen, as this paper shows. Sometimes a midlevel vortex was present and development did not occur, such as time 1 in Gaston. Although the authors' explanation for Gaston makes sense, it means there is still not a formal prediction possible from this approach. Nevertheless, the concepts presented are original and provocative.

Minor comments 1. p. 18919, line 22: missing word 2. p. 18923, line 13: typographical error 3. Open and filled symbols are difficult to discern in Fig. 6.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 18905, 2013.