Reply to Reviewer John Molinari on "Interaction between dynamics and thermodynamics during tropical cyclogenesis" by S. Gjorgjievska and D. J. Raymond

August 13, 2013

We thank John Molinari for reviewing the paper and for his insight.
Reviewer's comments are in black. Author's comments are in blue.

J. Molinari (Referee)

molinari@atmos.albany.edu

Received and published: 30 July 2013

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.

This is a good question that I have considered before. In an earlier draft of our manuscript was a thought experiment regarding this question, but that part did not make it to the final draft. Your comment made me go back to the Raymond and Sessions 2007 (RS07) simulations and do some diagnostics. Changes in the manuscript will be made accordingly, and here is the explanation for the differences in the vertical mass flux profiles from the RS07 paper. Figures that follow correspond to the control case and to the $d\theta$=2 K case. The black lines in these figures all correspond to the control case and the red lines correspond to the perturbed case.

The applied temperature perturbations (Fig. 1a) affect the thermodynamic stratification, the relative humidity, and the surface entropy fluxes. The perturbed case has higher relative humidity in the lower troposphere and lower relative humidity in the upper troposphere (Fig. 2a). The surface entropy fluxes are larger in the perturbed case, due to the lowered atmospheric temperature near the sea surface (Fig. 2c). The parcel buoyancy in the perturbed case exhibits a maximum at lower elevations (see Fig. 2b). (Buoyancy here is calculated as explained in the appendix of our manuscript). Based on these diagnostics, the shape of the mass flux profile is dictated by the thermodynamic stratification. More precisely, lowered elevation of the vertical mass flux maximum is associated with the lowered elevation of the maximum buoyancy. The larger magnitude of the vertical mass flux profile for the perturbed case is probably due to the larger surface entropy fluxes. We are now doing experiments where we apply different surface forcing for the same vertical temperature profile: larger surface forcing increases the magnitude of the resulting mass flux, but the shape of the mass flux remains almost the same. The drier upper troposphere in the perturbed case may cause a larger buoyancy decrease rate due to entrainment and mixing.

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?

They do have bottom heavy mass flux profiles before tropical cyclone formation. That is what we have observed and documented in the manuscript. But I assume your question pertains to the convection associated with these type of disturbances much earlier prior a tropical storm forms, if one forms at all. I am not aware of observational studies that show or suggest vertical mass flux profiles within these disturbances. I expect to see cycles. The mid-level cyclonic vorticity within these disturbances often forms as a result of convection that exhibits a strong positive vertical gradient in the middle troposphere. Once the negative temperature anomaly in the lower troposphere is created by the mid-level vortex, I expect to see increased vertical gradient of mass flux profile in a shallow layer near the surface. Whether tropical storm forms is a different question. Assuming environmental factors are conducive and the mid-level vortex exists long enough to provide thermodynamic stratification conducive to bottom-heavy mass fluxes, genesis will probably occur. Where does this switch happen, we do not know yet.

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.

This is an issue. Some basis for how we selected the areas are provided on page 18914, second paragraph. It is impossible to find more specific criteria for selecting areas among such a variety of disturbances, with the limitations already imposed by the observational area. For that reason, we have altered the selected areas in several reasonable ways, including not selecting areas at all. The analysis of each set of results led to the same conclusions, which suggests that the results presented here are robust. For example, Fig. 3 below shows how Fig. 5 of the manuscript looks for not selecting areas beyond that of the aircraft.

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.

Both Gaston 3 and Gaston 5 did have top-heavy vertical mass fluxes. Though, stronger convection may have been expected with the large CAPE from those missions. It makes sense that entrainment reduces CAPE more so in drier environment. This is probably worth it an investigation, but it is beyond the scope of our manuscript, as we ascribe the low relative humidity that was observed during the last three missions in Gaston to the decay of the mid-level vortex that occurred between Gaston 1 and Gaston 2.

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.

The theory that we are proposing is on the mechanism of how the mid-level vortex leads to low-level vortex intensification. The existence of a mid-level vortex does not guarantee that genesis will occur. Gaston is not such an example, though, because the low level vortex started decaying only after the mid-level vorticity decreased drastically. Regarding the sensitivity of the results to the selected area for analysis, we discussed in the response to the comment number 3 (see above). The results from each set differ, as expected, but not enough to change the conclusions. Therefore, this theory as proposed may not be complete, but it provides an insight of the mechanisms of interaction between dynamics and convection in the tropical atmosphere.

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.

We replaced the open symbols with filled symbols. The missing word and the typographical error are also taken care of. Thank you!

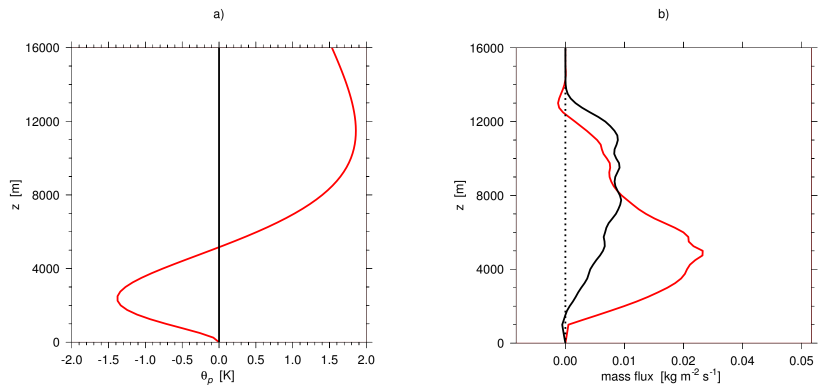

Figure 1: a) Potential temperature perturbations from RS07 and b) Vertical mass flux profiles. The black lines correspond to the control case and the red lines correspond to the perturbed case, $d\theta = 2$K.

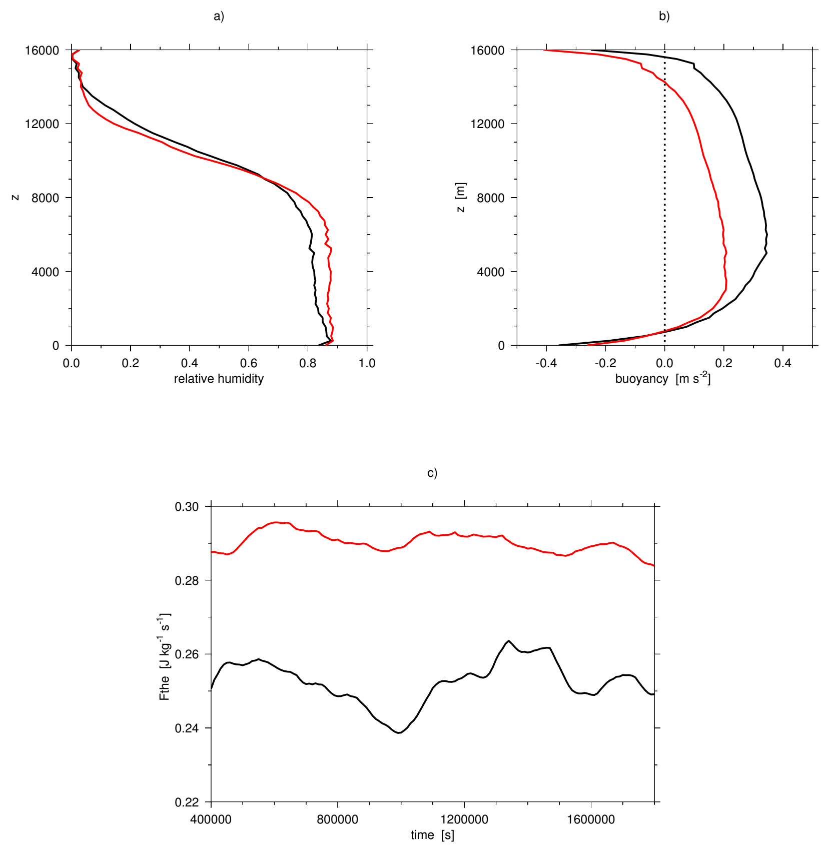

Figure 2: a) Vertical profile of relative humidity, b) vertical profile of buoyancy and c) time series of equivalent potential temperature surface fluxes.

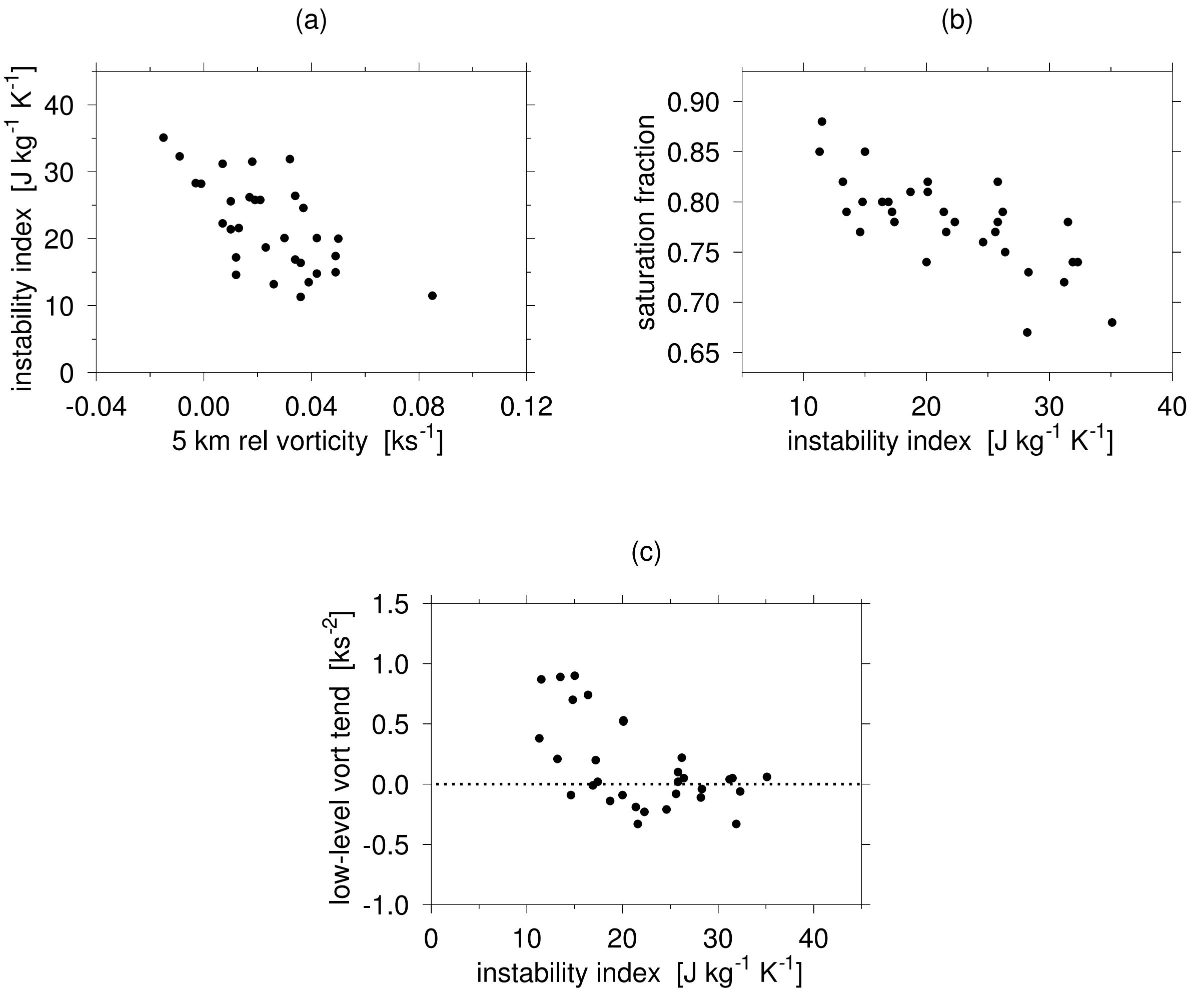

Figure 3: Scatter plots created for no area selected. These are analogous plots to those in Fig. 5 in the manuscript.