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Comment

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

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In this manuscript, the authors present analyses of the dynamic and thermodynamic characteristics of a developing tropical cyclone and a non-developing-weakening tropical system. Their analyses are placed in the context of a previously defined theory in which a midlevel vortex in thermal wind balance provides an environment in which vertical mass flux and horizontal convergence are maximized at low levels. The analyses of these two systems are also placed in the context of an expanded sample that includes two additional storms.

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This computations are carefully constructed and the results presented in a reasonable manner such that their interpretation is clear. My comments revolve around a few central themes, which include; i) the framework in which the study is placed; ii) the necessary background to fully understand the results; iii) the role of air-ocean processes; and iv) sampling.

1) In the Introduction, the authors place the study in the context of the “top-down”, “bottom-up” hypotheses to present a “hybrid” pathway. However, I think that this framework distracts from the primary purpose of the manuscript, which should be to present the proposed theory without the constraint of the top-down, bottom-up jargon.

1a) The Introduction leaves the reader with a sense that the authors are going to present their “hybrid” theory as an alternative hypothesis to the top-down, bottom-up hypotheses and make some definitive statements as such. However, the manuscript seems to drift with respect to this purpose. For example, there are statements such as “. . .our results provide more evidence for the top-down pathway of tropical cyclogenesis” (lines 23-24, p 18919). Then there are statements such as “In relation to the “bottom-up” development hypothesis, the theory we present here does not contradict the importance of a protected pouch where convection occurs uninterrupted.” (lines 9-10, p. 18925). While these two statements are perfectly valid, having to force these ideas into the top-down, bottom-up framework detracts from the significance and the meaning of proper interpretation.

Rather than a framework of top-down and bottom-up, could the framework be defined relative to specific characteristics that are perhaps more at the center of recent analyses of tropical cyclone formation? Place the study in the context of characteristic factors in thermodynamic and dynamic properties that occur in developing and non-developing systems at; low levels (i.e., warming, moistening, convergence-divergence, circulation changes); at mid levels (i.e., moistening, circulation changes); and with respect to disturbed and non-disturbed tropical conditions (i.e., radiative-convective equilibrium).

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1b) I am not sure that “hybrid” is the best term for a description of the proposed theory. In my mind, I expected “hybrid” to signify a combined theory, but I don’t think this is true in this context. I am not opposed to use of the term “hybrid” but it must be defined. If you want to continue to use it, then define your meaning for “hybrid”.

2) The core of the manuscript is very dependent on the work of Raymond and Sessions (2007). For example, the implication of the stabilized environment is described as “When the troposphere is more stably stratified, the convection produces a more bottom-heavy mass flux profile.” Since this is key to the theory, I think the exact means by which this bottom-heavy profile is produced should be summarized here. I concur with the earlier review that recommends this and I strongly suggest that the authors place a version of their reply to that review into their manuscript.

3) I would like to see more said about the role of the ocean in this theory and in the difference between Karl and Gaston. There is some mention but I think that it is very general. Could it be said that the very different ocean conditions in which Karl and Gaston existed was the dominant factor in the development scenarios? The lowest 0.5 km of the saturated moist entropy profile (Fig. 2a for Gaston and Fig 4a for Karl) are very different. Is the lack of variation in the profiles of Karl due to the warmer ocean, and is SST the best indicator of that? It may be nice to include some information about ocean heat content in the regions of each storm.

4) The authors make a good attempt at addressing the difficulties with respect to the fundamental issue associated with averaging to define representative conditions in an environment that is highly variable in space and time. However, in some cases some subtle differences among average profiles are key to interpretation and understanding. Could you make use of some re-sampling techniques to provide a measure of uncertainty in the profiles? This could be based on varying box definitions more so than you do, or a type of boot-strap method in which randomization could be used to omit observations or gridded regions from the analysis. Since regions over which observation are averaged is defined differently by different studies, any analysis of sampling issues

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with respect to the environmental conditions would be very important.

5) In the current version of the manuscript, the overall characteristics of Karl and Gaston are analyzed. Then the proposed theory is presented. Finally, Gaston is re-examined more fully. To me, the re-examination of Gaston was awkward. I think a better format would be to present the theory and larger-sample statistics, including a good summary of Raymond and Sessions (2007), the present the analysis of Karl and Gaston without dividing the analysis of Gaston into two sections.

In summary, the authors have presented a nice analysis and interpretation of tropical cyclone formation in the context of the PREDICT data set. The methodology is clear and complete and the results are well framed by the analysis. This manuscript provides a valuable contribution to the literature on tropical cyclone formation.

I do think that the manuscript would benefit by: i) Re-focusing the framework away from the top-down, bottom-up jargon to differences that have been recently identified using new data sets; ii) Including background information; iii) Commenting on the role of the ocean; and iv) Addressing sampling issues.

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