

Reply to Reviewer Patrick Harr on “Interaction between dynamics and thermodynamics during tropical cyclogenesis” by S. Gjorgjievska and D. J. Raymond

October 17, 2013

Reviewer’s comments are in black. Author’s comments are in blue.

The authors thank P. Harr for taking time to review the manuscript and give an insightful and constructive feedback.

Much of the comments, critiques and the suggestions are similar to those of C. Davis (the second referee) and thus same responses are given to those.

P. Harr (Referee)

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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.

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).

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”.

We received similar comments/critiques/suggestions by the second referee, C. Davis. As we responded to his comments, we take this as a very valid critique. The theory presented is not a compromise between two opposed hypotheses, nor do we evaluate either one. The introduction of the revision is changed accordingly, to clarify what the focus of the paper is. There is still some “top-down” and “bottom-up” jargon present, but it is in the contexts of recent findings regarding tropical cyclogenesis. We also decided it would be the best if we leave out the term “hybrid” in the revision.

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.

We added a section in the revision (#4) that gives an extensive summary on Raymond and Sessions (2007) and discusses the means by which bottom-heavy mass flux is produced.

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 ocean conditions were very different. However, I don’t think we can claim that the ocean conditions themselves were the dominant factor, as tropical storms do develop over ocean conditions like those of Gaston. I do believe though that it was a big factor, together with the trade wind inversion that the dropsondes registered (see our response to C. Davis’s comment #3). 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? The observed difference in saturated moist entropy in the lowest 0.5 km between Gaston 1 and Karl is probably due to both different ocean conditions and the fact that Gaston 1 had a strong mid-level vortex. Regarding the lack of variation in saturation moist entropy for Karl in the lowest troposphere, I think is a result of the generally warm ocean. The SST were high on all the dates that Karl was observed. However, the SST is not the best indicator for that, as you may already suspect. On your suggestion, I looked at ocean content related data. Below, in Fig. 1 is shown Tropical Cyclone Heat Potential (TCHP) for Karl on the dates that are noted on the figures. Karl’s approximate positions on the respective days are marked with a letter K and a number next to it. Karl developed the mid-level vortex

between the 11th and 12th of September. Before the mid-level vortex developed, its position was over lower TCHP. Then it moved over higher TCHP, but meanwhile it also developed a mid-level vortex. The effects of the cooling in the lower troposphere, associated with the mid-level vortex, were compensated with the effects of the warming because of the higher ocean heat content. As a result, the saturated moist entropy in the lowest troposphere did not change. It may be nice to include some information about ocean heat content in the regions of each storm.

In the revision we included a paragraph on the ocean heat content in the context of comparing Gaston 1 and Karl 3, in section 5.2.3.

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 with respect to the environmental conditions would be very important.

This would be a very hard think to do, considering the size of the area for each observational case, and considering that we only use dropsonde data. We are interested in the meso scale, so looking at averages over small areas (less then 3 by 3 degrees lets say, would not be useful in context of our theory. The circulation center should also be captured within the selected areas. This requirements do not leave much freedom for varying boxes definitions enough, to provide a meaningful measure of uncertainty. We are confident in our vertical profiles calculated as averages over large areas. Varying the large areas within the constraints mentioned above, does not change the vertical profiles enough to affect our conclusions and interpretations. Another assuring thing is the fact that our vertical profiles of vorticity and mass flux agree with those of Davis and Ahijevich (2012), even though they used different techniques.

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.

The revision follows the suggested order: First the background and the theory; the evolution of Karl, the evolution of Gaston; the hypothesis on Gaston's decay and related comparisons to Karl. We are thankful for the suggestion, it did improve the flow of the paper.

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.

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.

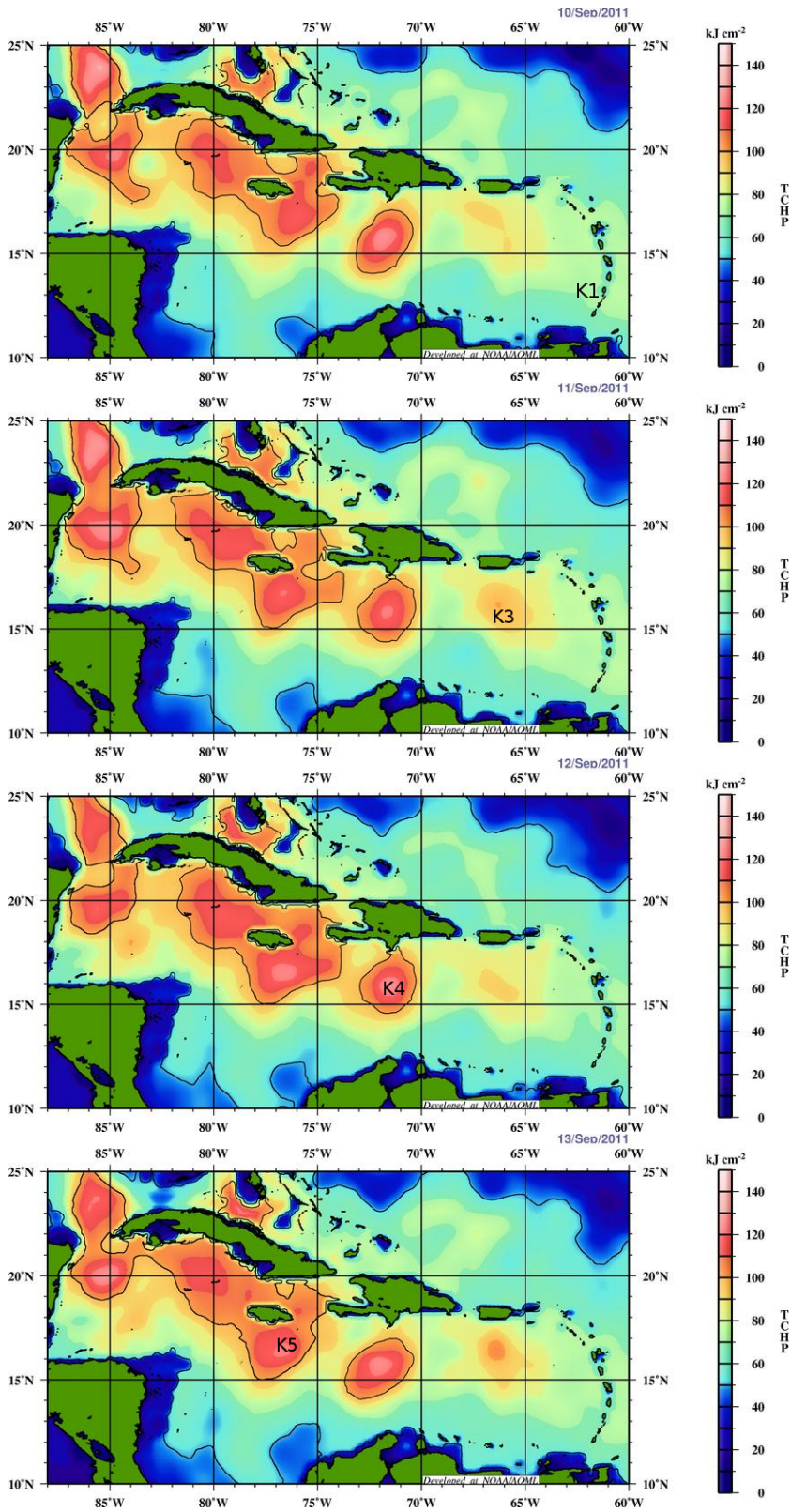


Figure 1: Tropical Cyclone Heat Potential data for Karl. The letters K# correspond to Karl's position on the respective day. The images are taken from: <http://www.aoml.noaa.gov/phod/cyclone/data/>