

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

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The present article presents both a summary of the evolution of two tropical disturbances, and a more general proposed relationship between thermodynamics and vorticity that leads to tropical cyclone formation. Overall, I believe that the computations are well done and the authors support many of their conclusions as well as the data allow. There is some room for improvement as I outline below. Especially because there could be some important reorganization of the paper, I consider the suggested revisions to be major.

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1. The organization and emphasis of the paper should be improved. There are two issues here. First, in the Introduction, the paper is phrased as a distinction of so-called “top-down” and “bottom-up” hypotheses. Indeed, these were a central part of the motivation for PREDICT. But the paper is not written as an evaluation of the two hypotheses. Evidence is presented that favors one perspective (top-down), but it is not clear how to evaluate the other from the analysis presented. So I do not think this paper is really an evaluation, and perhaps presenting the dichotomy in the Introduction is more distracting than anything else. The authors also state that the real situation is not that one is wrong and the other is right. So why frame it this way in the beginning?

A small, but related point, is that the term ‘hybrid’ needs to be defined earlier, because initially I thought it referred to some sort of compromise between top-down and bottom-up ideas, but that was not it at all.

The second point is that I found the organization awkward. There is a comparison of Karl and Gaston, then a more general investigation of parameter space, followed by more comparison of Karl and Gaston, with an emphasis on why Gaston initially weakened. Granted, the interpretation of Gaston depends somewhat on the theory, but I think it would be better to tackle all of the case comparison at once, then the theory. A short discussion after the theory could add to the interpretation of Karl and Gaston as examples of the theory. Or, the theory could be presented first.

Along these lines, the paper struggles somewhat for identity, and the authors note up front that the paper has two purposes. By reorganization of material, it could be made to flow more naturally and not seem so much like it has competing themes. The addition of 24 PREDICT cases to the results of Raymond et al. (2011 JGR) seems to be a particularly noteworthy aspect of the present paper, and perhaps the most novel aspect. There should be little doubt that the paper is primarily about this, as opposed to a case comparison of Karl and Gaston. I say this because such comparisons are already published, and apart from the controversy over the first day of Gaston, there is really not much new in the present paper on these two cases. That is reassuring in a

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way, because it supports previous analyses. In fact, that point could be made stronger. For instance, I was surprised that the mass flux calculations agreed with those in Davis and Ahijevych (2012, DA12) so well despite what seems like very different approaches.

2. Why did the authors not include data from the NASA and NOAA missions? It makes a difference. In particular, the additional missions clearly demonstrate the effects of the diurnal cycle of convection on the mass flux profile and this has implications for the vorticity. For instance, DA12 show that the mass flux profile for Karl in missions near 00 UTC is much different than for missions in the 12-18Z time frame. There is actually weak divergence above the boundary layer. And the data showed that the lower-tropospheric circulation around 00 UTC the 13th was the weakest of any observed. The circulation late on the 13th was also weaker than the circulation observed near 12Z on the 13th. The point is that Karl did not represent a monotonic march toward genesis starting with events back on the 11th.

3. The suggestion that a trade inversion, and related convective inhibition, led to the initial weakening of Gaston is not really supported by the soundings. It seems that the relative flow into the convection at low-levels is from the east-northeast (Fig. 12). The soundings in that quadrant outside the black box but inside the red box (Fig. 12) are not trade inversion soundings. They are onion soundings that clearly indicate the warming and drying in downdrafts beneath anvils. There was well-organized convection overnight before the first flight into Gaston, but I suspect that this convection was occurring in air that was still not particularly moist in the middle troposphere, and strong downdrafts resulted that stabilized the atmosphere and produced the temperature inversion signature. Since these soundings were at the edge of the domain sampled by PREDICT dropsondes and the air here was already modified by convection, it is difficult to say what the thermodynamic origin of the air really was. Probably it was dry given that dry air surrounded that system. So while the mid-tropospheric pouch may have been protected to some extent, the infiltration of unfavorable air could still have occurred at the lowest levels. It is possible that the recovery of the moist entropy in

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the lower troposphere was slow enough that deep convection could not reinforce the mid-level vortex. This aspect of the hypothesis may well be correct, namely, that only shallow convection could occur, and fairly weak at that, so that the mid-tropospheric vortex was weakened.

4. There is still an important question to be answered about how the mid-level vortex and its attendant mesoscale dynamics fit into the marsupial paradigm of tropical cyclone formation (if they do), and furthermore, whether it is consistent or not with the role of rotating convective structures (vortical hot towers) in the transformation of the pre-depression disturbance into a tropical storm. What can you say about this?

Specific points:

1. How exactly is a "bottom heavy" mass flux profile defined?

2. I am not intending to add to the paper, but I am curious just what the mid-level vortex precursor to Nicole looked like. It might be worth showing. As I recall it was hard to tell where the precursor to Nicole really was.

3. The section that justifies Raymond and Sessions (2007) could be a little more quantitative. It seems like an important parameter would be the ratio of the rotational time scale to the lifetime of organized convection.

4. Despite the emphasis on the mid-level vortex, Figure 6 shows that the mid-level vortex never gets much stronger than the low-level vortex, even in Karl. How do you reconcile the theory with this result?

5. Page 18924, lines 19-23: How can there be an increase of moist entropy simply through convergence when the radial gradient is negative?

6. The analysis of the full set of PREDICT data also appears in Komaromi (2013) and Davis and Ahijevych (2013), who show results that are consistent with the proposed ideas. It is not surprising, since it is based on much of the same data.

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