

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

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

The authors thank C. Davis for his insightful comments and suggestions.

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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.

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 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.

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. We substantially restructured the revision, and we also backed away from the term “hybrid”. We appreciate your comments and suggestions.

We stand by the two purposes of the paper though. We agree that the addition of the 24 cases (30 in the revision) to the Raymond et al. (2011) is probably the most noteworthy aspect, but nevertheless, the case of Gaston deserves to be presented too, as there is evidence for its initial decay different than what has been published. The comparison between Karl and Gaston are down the lines of arguments of the hypothesis we propose on the Gaston’s initial decay, whereas both Karl and Gaston are presented as supporting cases to the presented theory in the paper.

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.

In the revision we included two Karl missions from NASA. The rest of them were conducted when Karl was a strong tropical storm and therefore are not included. We didn’t include the NOAA missions. There are 2 appropriate missions that could be added, they precede the two NASA missions, and they would not significantly add to the analysis.

It makes sense that Karl did not monotonically march toward genesis. I assume no storms have a monotonic march toward genesis, as in the pre-genesis phase the diurnal cycle must take its course. However, the negative vorticity tendencies associated with the over night mass fluxes are relatively small (see Fig. 2c in the revised version, yellow and green line). Small magnitudes of vorticity tendencies calculated in a snapshot should not be considered reliable for predicting the following day vorticity. Strong convection can occur not long after the snapshot and that would considerably change the vorticity tendency. Furthermore, the time scales of relative vorticity are much larger than the convection time scales. For those reasons we don’t see as obvious diurnal cycle in the relative vorticity as we see in the mass flux profiles. To conclude the point: Even though Karl did not present a monotonic

march to genesis since it was initially observed on the September 10th, it did present an obvious pattern in the relative vorticity evolution (see Fig. 4b in the revised manuscript). The initially observed low-level relative vorticity was decreasing while the mid-level relative vorticity was increasing. The huge convective event during Karl 3 produced strong mid-level vorticity tendencies which was followed by significant increase in the mid-level vorticity by the following day. After that point the low-level vorticity also started increasing and Karl eventually reached a tropical storm status. It agrees well with our theory.

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 is a hypothesis, which the soundings do not dispute. 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. Those are all soundings in the downdraft area. Some actually do show very stable layer (see sounding 3 and 4 here in Fig. 1.) 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. There is evidence of temperature inversion over a large area ahead of Gaston 1, which suggests that this inversion was not produced by the downdrafts. On the way back from the Gaston 1 mission, G-V launched a dropsonde at 13N, 47W. This was several degrees west of Gaston at that time. This dropsonde registered trade wind inversion (see here Fig. 1, drop 20). All the drops launched to the west of the circulation center and outside the convective region indicated inversion. This witnesses against the idea that the inversion was created by the downdrafts. 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. Convection and downdrafts modify buoyancy locally. The absence of inversion in the soundings in the convection and downdraft area does not eliminate the possibility that convection within the disturbance was affected by trade wind inversion. From the facts presented here and in the manuscript, we are led to believe that convection was affected by inversion and low surface entropy fluxes. 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 the lower troposphere was slow enough that deep convection could not reinforce the mid-level vortex. The fact that the relative humidity did not change between Gaston 1 and Gaston 2, strongly suggests that it was not dry air that was killing Gaston 1. Both Gaston 1 and Gaston 2 were equally moist. The fact that trade wind inversion was registered in a big region that extended to the west and northwest of the circulation center and that low surface fluxes were associated with Gaston (see Fig. 9 in the revised manuscript), makes these better suspects for suppressing convection in Gaston 1, rather than dry air intrusion. 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. Yes.

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?

We suspect the mid-level vortex and the marsupial paradigm are related, but don't know yet how. Perhaps the mid-level vortex is setting up the thermodynamic stratification within the pouch. The mid-level vortex creates the mid-level pouch. A paragraph in the conclusions briefly addresses this. Though it will require a focussed investigation to find out how the two exactly fit together. Regarding the VHT, we don't have anything to say.

Specific points:

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

Bottom-heavy refers to a mass flux profile that exhibits the strongest positive vertical gradient in the lowest 2-3 km. We clarified that in the last paragraph of section 5 in the revised manuscript.

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.

It was hard to tell. We have only one observation prior genesis and it does not capture it well. Here Fig. 2 shows the relative vorticity and the relative wind at 5 km elevation and at 1.5 km elevation. It appears from here that the low-level vortex is out of the picture. I looked at FNL data and as much as we can rely on these data, there is not much low-level vorticity around that time. Nicole is included in our scatter plots, but as far as the evolution goes, I think we don't have enough worth showing it.

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.

In the revised version we do give an estimate of the smallest ratio of the rotational time scale to the convective time scale.

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?

Now that we included the GRIP missions and Karl on September 12 is documented, you can see that the mid-level vortex does get much stronger.

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

That entire subsection was replaced and modified substantially, so the mentioned statement is not in the revised manuscript.

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.

It is good that there is an agreement.

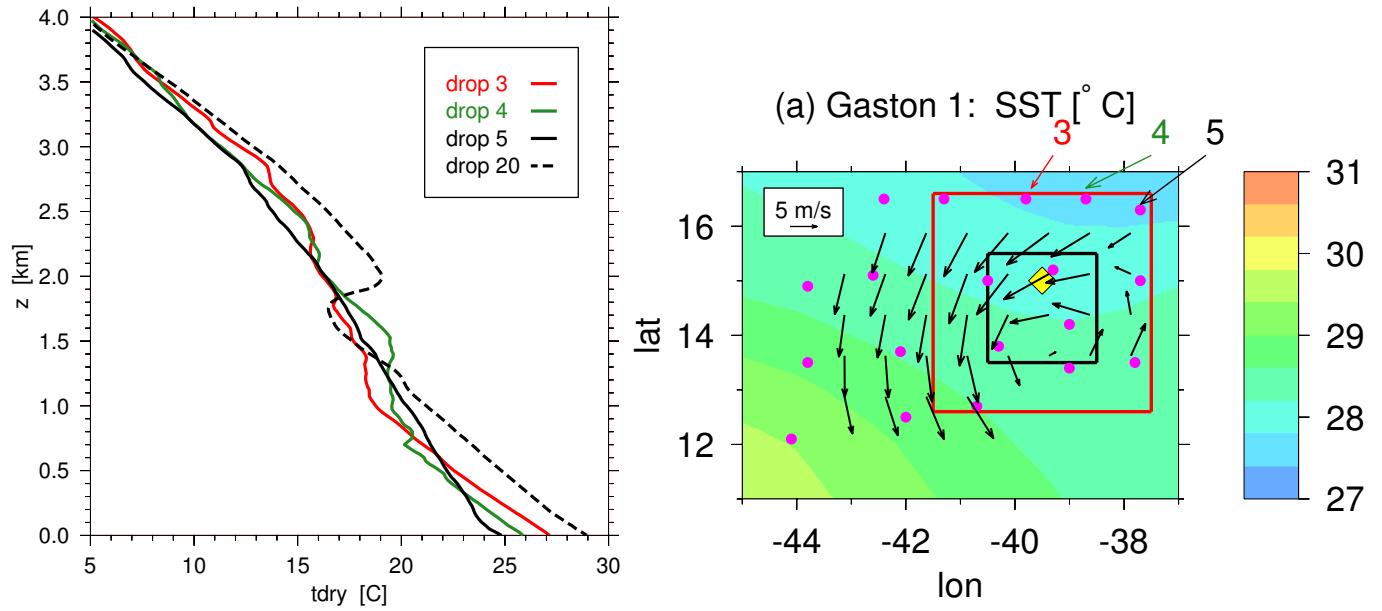


Figure 1: Some vertical soundings of temperature for Gaston 1 (left panel) and their corresponding positions (right panel). The position of drop 20 is not marked, as it is at longitude 47W.

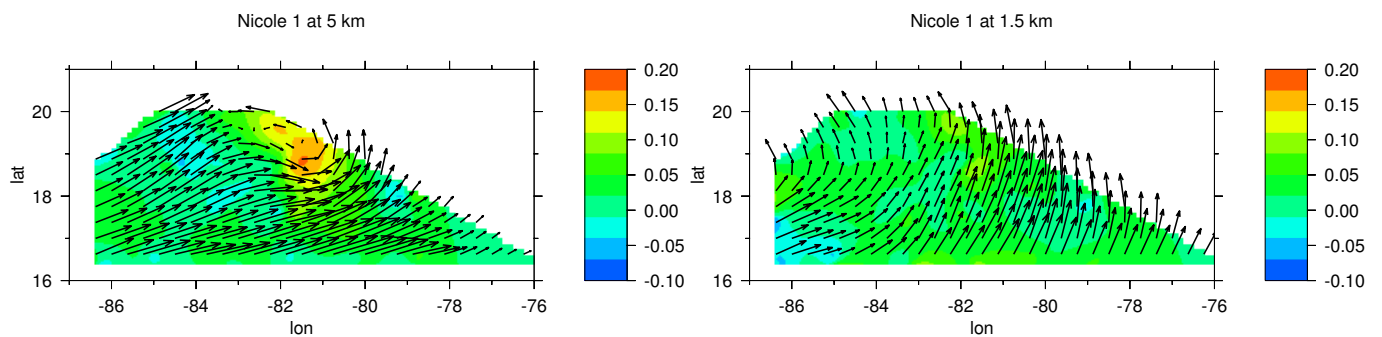


Figure 2: Nicole 1. a) relative wind and relative vorticity at 5 km (left panel) and at 1.5 km (right panel). The units of relative vorticity are ks^{-1} .