

Interactive comment on “A numerical modeling investigation of the role of diabatic heating and cooling in the development of a mid-level vortex prior to tropical cyclogenesis. Part I: The response to stratiform components of diabatic forcing” by Melville E. Nicholls et al.

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1. “The method employed by the authors to isolate the effects of the diabatic heating/cooling profiles seems particularly messy. I wonder whether it is the “best approach”. The authors suggest that the vortex will be “not too different from the quasi-balanced state ...” although there appears to me to be lots of arbitrary decisions made to achieve this state. Is it not simpler and cleaner to take the averaged diabatic heating

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profiles and run the Sawyer-Eliassen model with a balanced vortex?”

Yes, one approach would be to run the Sawyer-Eliassen model, however, that does involve making approximations to the governing equations. The conclusion that the stratiform heating components are not able to fully account for the mid-level inflow may not have been so obvious with the Sawyer-Eliassen model approach. Moreover the balanced state can be used to examine the hypothesis that the top-heavy heating profile of the convective cells in their late stage of development can contribute to the mid-level inflow. For instance, in the second part of this study an experiment is run where multiple cells are initiated with low-level warm bubbles. Then it can be seen that a weak mid-level inflow develops prior to a stratiform region forming aloft. This indicates the diabatic heating in the convective cells is playing a role in causing the mid-level inflow.

2. “In the introduction (page 3, lines 7-14) the authors describe a theory where a mid-level vortex descends to the surface. A vortex descending to the surface would violate Haynes and McIntyre (1987). There can be no net downward transport of vorticity from the middle level vortex, ruling out the possibility of genesis being a “top down” process. In any case, down flow from mid-levels would produce outflow near the surface, leading to a dilution of the vertical vorticity transported downward.”

Here we are summarizing the results of Bister and Emanuel (1997) in regards to the descent of the vortex. In their words “After some time, the midlevel vortex expands downward toward the boundary layer”. Although the mechanism responsible is perhaps not clear from their discussion we see no reason to discount their numerical modeling results. It is a theory that has been put forward and we think it should be mentioned in the introduction. While we agree that according to Haynes and McIntyre (1987) there can be no net downward transport of vorticity from the middle level vortex, there could be a lowering of the diabatic cooling with time, which could cause the inflow to become lower. This would be expected to cause spin up of the winds at lower levels and therefore give the appearance of a descent of the vortex. It is possible that

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as sublimation and evaporation leads to increased humidity at the base of the developing mid-level vortex that this increased humidity reduces subsequent sublimation and evaporation of hydrometeors falling from aloft and therefore more hydrometeors reach lower levels, thereby shifting the level of maximum diabatic cooling to lower levels with time. However this is a hypothesis and we are not seeing significant descent of the mid-level vortex in this early 12-hour period.

3. “Also in the introduction (page 3, lines 20-24) the authors introduce the theory that the mid-level vortex is conducive to convection with a more bottom heavy mass profile. A recent paper by Kilroy et al. 2018 found the opposite: namely, a simulation with warm-rain-only microphysics did not develop a mid-level vortex, but the convection had a bottom heavy mass flux profile. The simulation with ice-microphysics developed a mid-level vortex, but did not have a bottom heavy mass flux profile (see their Fig. 13).”

This is a theory that has been put forward by other researchers and there is some observational support for it, so we think it should be mentioned in the introduction. We appreciate the reviewer drawing attention to this new modeling result and it is probably worth adding a sentence pointing out the implications for this theory.

4. Page 4, lines 17-19: “Overall we consider that there is ample evidence that mid-level vortices may at times be playing an active role in tropical cyclogenesis, and even though they are unlikely to always be essential to the process, there are going to be consequences when a mid-level vortex develops which need to be further investigated” This sentence is very vague and confusing. How can there be ample evidence that it may at times play an active role? What is the active role a mid-level vortex plays? What are the consequences? Unlikely to always be essential, is there any evidence they are ever essential? I won't argue that mid-level vortices occur in numerical models (and in nature), but is their formation a consequence of ice microphysics (or other model parameters) and not a necessity for genesis?”

There are many observational studies that find mid-level vortices develop prior to trop-

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ical cyclogenesis. Our simulations with vertical wind shear suggest that there can be ramifications for the further development of the system once a prominent mid-level vortex forms. The mid-level vortex tends to be moist compared to the surroundings since it is a region where considerable sublimation and evaporation has occurred. Moreover we find a cooling anomaly centered near the melting level, which can be expected to reduce low-level stability. Both factors could favor new convective development. In an environment with vertical wind shear a misalignment occurs between the low-level and mid-level circulations. This misalignment results in a region of enhanced local shear between the centers of the low-level and mid-level circulations. This strong local shear could potentially influence the intensity and longevity of convective cells. It is possible that once a mid-level vortex has formed it could influence development a day or two later either through merger with other mid-level vortices or by providing an environment favorable for convective development. Since we are not discussing these simulations in this paper we should probably reword this sentence, although we do think that the role of mid-level vortices in tropical cyclogenesis is worthy of further investigation.

5. “On that note, have the authors considered using/redoing one of their simulations from NM13 which followed Pathway 1 (and included ice-microphysics) and doing a similar analysis? It would be interesting to understand why a similar simulation with ice did not develop a mid-level vortex. Surely these simulations would also contain ample mid-level cooling from sublimation.”

Most of our simulations with ice develop a mid-level vortex, but in some cases they are not very strong and genesis follows Pathway 1. This could be sensitive to several factors that have yet to be fully explored. For instance if the initial environmental sounding is made warmer aloft then convective cells are less deep and the ice layer aloft is not so extensive or thick, leading to a weaker vortex.

6. “Start of section 4. I found the results section a little difficult to follow at first, as there was little to no description of the vortex evolution. I have no feel for vortex strength or development. There should be a better segue into the main results. Perhaps the

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authors could think about including a plot showing the time evolution of the maximum tangential winds, radial winds, etc. and give a description of the vortex evolution. Another section that would be greatly improved by proper introduction is from line 19, page 10. Why the jump from a system analysis to describing a single cell in detail. Why is this cell in particular important? I wasn't sure of the significance of these results."

We shall take into consideration this comment. We thought it was important to show a convective cell in detail although we agree there could be a better introduction. This particular cell was chosen since it was isolated and so less impacted by strong circulations induced by nearby cells.

Minor comments:

1. Page 3, line 4. "cod pool"

Will correct.

2. Page 4, lines 12-13. "Also Hurricane Guillermo (1991) in the Eastern Pacific by Bister and Emanuel...". Perhaps you can rephrase this, it reads like Bister and Emanuel were responsible for the hurricane.

Will rephrase.

3. Page 7, line 13. "conservative" should be "conserved".

Will correct.

4. Page 8, line 31 and page 9, line 14: I dislike the phrase "descending inflow". The plot is not showing any vertical motion. Do you mean that the height of the inflow is decreasing with decreasing radius?

Yes, this is a more correct statement.

5. Page 9, line 1: "This mid-level maximum of tangential winds appears to be associated with the midlevel inflow". Why "appears" to be, can there be any other reason?

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Yes, this statement could be more decisive.

6. Page 9, lines 4-5. Can this low level cooling be from the initial vortex, even after 48 h has passed?

Yes, the signature of the initial vortex is still quite strong even at 48 h.

7. Page 9, line 6. Do you have an explanation for the thin layer of cooling at a height of 13 km?

It appears to be partly due to the stratiform diabatic heating aloft (Fig. 14d). Possibly it is also partly due to cells overshooting their equilibrium buoyancy level causing some adiabatic cooling. It is an interesting feature, but we don't have a good theoretical explanation at this time.

8. Page 10, line 5: "other two in this figure". Do you mean three?

This sentence will be reworded.

9. Page 11, section 4.2: Would difference plots be better here, rather than showing the fields in both the original and modified runs. It would be easier for the reader to spot how the modified simulation differs, as comparing the figures is not so easy to do.

We will carefully consider this recommendation.

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