Review of

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

Ref.: acp-2018-316

Recommendation: Minor revision

The authors investigate the reasons why a mid-level vortex develops during tropical cyclogenesis by isolating the effects of the stratiform components of heating and cooling. They find that the cooling from sublimation at mid-levels is primarily responsible for the bulk of the inflow at this height. As air parcels moving inwards at mid-levels conserve their absolute angular momentum, a mid-level vortex subsequently develops. This is a very useful study that sheds light on why mid-level vortices form in some numerical models, although there is some debate as to whether mid-level vortices are necessary for genesis. I recommend that the paper be accepted with minor comments and I have some specific questions/recommendations for the authors.

General comments:

The article is well written and presented, although I would request more discussion and description in the results section. Most of my comments are relatively minor.

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 profiles and run the Sawyer-Eliassen model with a balanced vortex?
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.
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 *etal*. 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).
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 by 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?

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.
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 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.

Minor comments:

- Page 3, line 4. "cod pool"
- 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.
- Page 7, line 13. "conservative" should be "conserved".
- 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?
- 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?
- Page 9, lines 4-5. Can this low level cooling be from the initial vortex, even after 48 h has passed?
- Page 9, line 6. Do you have an explanation for the thin layer of cooling at a height of 13 km?
- Page 10, line 5: "other two in this figure". Do you mean three?
- 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.

References:

Haynes P, McIntyre ME. 1987: On the evolution of vorticity and potential vorticity in the presence of diabatic heating and frictional or other forces. J. Atmos. Sci. 44: 828–841.

Kilroy G, Smith RK and Montgomery MT, 2018: The role of heating and cooling associated with ice processes on tropical cyclogenesis and intensification. Quart. J. Roy. Meteor. Soc. 144, 99–114.