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Review of “Further examination of the thermodynamic modification of the inflow layer of tropical cyclones by vertical wind shear”. Authors: M. Riemer, M. T. Montgomery, and M. E. Nicholls

Summary

The authors further examine the evolution of TCs in response to the environmental vertical wind shear and test their previously developed framework of thermodynamic modification of the inflow layer using idealized simulations. They have designed five more numerical experiments beyond their previous work with modifications of the surface exchange coefficients and inclusion of ice microphysics in their new experiments. They compared detailed vortex structure and inflow layer structure between the new experiments and previous ones in response to the intensity change. They concluded that their proposed framework is a robust description of intensity modification by the wind shear.

This is a well-written paper. The manuscript represents a substantial contribution to the scientific progress within the scope of Atmospheric Chemistry and Physics. The scientific methods and assumptions are valid and clearly outlined.

Recommendation: The manuscript will likely be suitable for publication following a minor to moderate revision that responds to all of the matters listed below. I am willing to review a revised version of the article if needed.

Relatively major comments:

1. In the idealized experiments in Riemer et al. (2010), the shear direction is in the same direction as the storm motion. Is this setup the same for the new experiments added in this paper? In real hurricanes, there is generally an angle ($\sim 60 - 90$ degree) between the shear and storm motion direction as shown by previous studies by John Molinari and Gary Barnes. The authors' new framework will be more convinced if additional numerical experiment can be done with the wind shear direction different from the storm motion. One more set of experiment should be enough to answer this question.
2. The vertical shear profile the authors' used is a cosine shape shear. Recent studies by Yuqing Wang have shown that the simulated TC structure is sensitivity to the shapes of the vertical shear profiles. The authors my need to test the effect of different shapes of shear on the robustness of their framework. At least, they should mention this effect.
3. The authors only showed θ_e and θ_e depression fields when presenting their most important results to confirm the thermodynamic modification by wind shear. It will be complete to add plots of temperature and humidity. Is the θ_e depression caused by temperature or humidity modification by the wind shear? How do the convective downdrafts influence the temperature or humidity? This type of discussion will clearly connect the authors' framework with surface flux transfer processes.

4. It is a really nice idea to calculate the timescale for vortex spindown in section 4.2. Equation (6) includes an important parameter h . The authors should specify how h is defined. Recent study by Jun Zhang et al. (2011MWR) showed that boundary layer height in hurricanes can be defined differently. Different definitions of boundary layer top would give different height scales that affect timescale calculation in Eq. (6).

5. The authors claimed that the decrease of intensity is mainly due to the decrease of low-level θ_e after shear is introduced. In Figs. 7 and 8, it is shown that the decrease of θ_e is only in a relatively small area compared to the whole inner core region. I am wondering if this small area low θ_e air is able to shut down the convection. It is likely the integrated downward θ_e flux is an important parameter to look at as well.

6. Is the depression of inflow layer θ_e (in Figs. 7 and 8) sensitive to the time window you chose in the analyses? How about the θ_e depression calculated from 1 to 3 h after the shear is introduced or time window between 4 and 7 h? When did the θ_e depression start to happen? Since θ_e in the boundary layer varies with intensity, it would be interesting to scale the θ_e change by the intensity change.

Minor comments:

1. Fig. 10 caption, should be CBLAST54/CBLAST68?

2. First line in section 3.7, low - θ_e ?

3. Eq. 4. The definition of the θ_e flux needs to be defined more clearly. The authors should distinguish this flux with the standard turbulence flux.

4. Since the model the authors used is not coupled with ocean model, ocean feedback to the asymmetry of surface fluxes may influence the authors' result. It is worthwhile to mention the limitation of the authors' results with lack of ocean response induced sea surface temperature cooling effect.

5. The authors should also mention the storm motion effect may contribute to the boundary layer thermodynamics asymmetry.