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Comment on wcd-2021-81

Anonymous Referee #2

Referee comment on "Intensity fluctuations in Hurricane Irma (2017) during a period of rapid intensification" by William Torgerson et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-81-RC2, 2022

This paper examines the relationship between changes in the intensity of Hurricane Irma, particularly during a 2-day period of rapid intensification, and a range of inner-core processes. The calculations and deductions are based on an ensemble of runs with the UK Met Office Unified Model, although most of the analysis is focused on the most realistic member of the ensemble.

General Comments

My main criticism is that there is a great deal of detail presented, and that this level of detail is difficult at times to follow, making the paper hard work. It's not always easy to see the point of some of the details and the relevance to the narrative. Of course, the scientific story is complicated and, in my view, no paper on the topic has really nailed it yet. Nonetheless, the paper makes a strong contribution to the general topic of rapid intensification and documents some of the inner-core processes that lead to fluctuations in the rate of intensification. I recommend that it be published after major revision.

Specific Comments

L 53 - 56. Some of the references are a bit misleading. For example, the results attributed to Hankinson et al. (2014) and Rief et al. (2014) should be attributed to Nguyen et al. (2011) as the results appeared first in the original paper. The main contribution from Hankinson et al. was to extend the results of Nguyen et al. to an ensemble, and the main contribution from Rief et al. was to examine the robustness of the results to using a different non hydrostatic model (WRF).

L 95. "… and did not intensify due to less favourable environmental conditions." Be more explicit. What was it about the environment that prevented intensification?

L 98. " … with sufficient mid-level tropospheric moisture for intensification …". How much is sufficient? "… high sea surface temperatures …". Be explicit: what was the SST?

L 104. "Despite favourable conditions …". What exactly was it about the environment that made the conditions favourable?

L 185. How well does the PV budget close?

L 291-307. The advective part of the PV change is discussed and plotted in Fig. 8. What about the physics part? How large is it? What's its structure and evolution? What part does it play in the story?

L 331. I don't really follow this argument. The barotropic conversion rate becomes more positive (less negative) at the onset of the weakening phase, which means that barotropic processes are increasing the mean state. Wouldn't we expect that to correspond with an intensification of the vortex?

L 339 - 340. ".. . with significant vertical depth albeit with lower values in these quantities." Be more explicit. How deep are the clouds and how strong are the updrafts compared to Smith and Eastin's definition?

L 394. How is weak and strong VHT activity defined?

Figure 10. Consider W1 (row 1). The tangential wind in the eye wall is decreasing everywhere. In the boundary layer, the mean contribution is strongly positive and the contribution from friction is large and positive (from the figure caption). The contribution from the eddy terms is negative, but smaller than the mean term, and even smaller than the sum of the mean and friction terms. How then is the tangential wind tendency negative in the boundary layer? The same goes for row 1 of Fig. 11.

L 402-407. As noted, the position of the VHT relative to the position of the eye wall is important as it effects whether the VHT spins up or spins down the vortex. The authors have done some work on this but not shown it in the paper. The relationship between the positions of the VHT and eye wall seems to me to be important and I think that the authors should expand on this point. In fact, it's central to the schematic later in the

paper.

L 448. "… inner rainbands which de-localized …". I don't know what this means.

L 476. ")". There's only a closing bracket.

L 536. The introduction of the schematic is a a bit abrupt. There's no statement telling the reader the you're synthesising the results in a schematic. What's more Fig. 18 is reference before Figs. 16 and 17.

L 536. Should be "(Fig. 18 a, **b**)"?

L 538. "… rainbands are not associated with the convective generation of PV outside the eyewall …". Do you really know this? You've only told us about the advective changes. Have you calculated the diabatic change in the PV from Eq. 1? What does it look like?

L 544 and Fig. 18b. You can't really say that the convergence lowers the pressure gradient force, can you? Isn't the wind field responding to changes in the gradient of the pressure field?

Figure 18d. How exactly does the lack of diabatic heating cause PV mixing? Nguyen et al. (2011) that the instability on the PV ring is a combination of barotropic and convective instability (as pointed out in L 567-568). In other words, the instability depends (in part) on the diabatic heating.

L 552. Should be "(Fig. 18 **c**)"?

L 552. Why should the symmetric structure be maintained initially?

L 553. Why do the conditions for VHT-like structures become increasingly better? This is an important point that hasn't really been addressed.

L 568. "… VHT-like structures … seem to be a cause of the instability …". I don't see how you can conclude this if the instability is a combined barotropic-convective instability.

4.5 Discussion. Kosin and Eastin (2001), perhaps the most important paper on the topic, has been left out of the discussion. I think it has to be included. In the terminology of Kosin and Eastin: regime 1 = ring structure —> relative intensification; and regime 2 = monopole —> relative weakening. Kosin and Eastin discuss how the moisture and equivalent potential temperature changes with the fluctuations. This has implications for the formation of convection and VHTs. How do their observations fit with the schematic?

L 666. Should be “fv_g” not “fr”.