Review of the manuscript entitled: "Intensity fluctuations in Hurricane Irma (2017) during a period of rapid intensification", by William Torgerson, Juliane Schwendike, Andrew Ross and Chris J. Short, submitted to *Weather and Climate Dynamics Discussions*.

Recommendation: Major revision

Summary

This paper presents an analysis of intensity fluctuations observed during a period of rapid intensification of Hurricane Irma (2017) using an ensemble of Met Office Unified Model convection-permitting forecasts. It is shown that intensity fluctuations consist of alternating weakening and strengthening phases and that during the weakening phases, the tropical cyclone temporarily paused its intensification. Reasons for the intensity fluctuations are explored.

While this study is a commendable attempt to provide dynamical interpretations of the storm behaviour, in our view it falls short of providing a clear understanding of the phenomena described. Moreover, the summary cartoon devised to underpin the explanations raises a number of questions as highlighted below.

The authors have tried to identify pieces of the "intensity-fluctuation puzzle", but have not yet provided a convincing link between the various pieces. We have some suggestions below for a way forward and would encourage the authors to consider these suggestions.

Major comments

1. The cartoon presented in Fig. 18 raises a number of questions:

   (a) The text in panel (b) reads: "Balanced effect of VHT in inner rainband creates convergence just above the boundary layer lowering the pressure gradient force and increasing the agradient wind." It is difficult to follow this reasoning. What is the "balanced effect of VHT"? And how does convergence lower the pressure gradient force? Are you talking about the radial pressure gradient?

   (b) In panel (d), how does the "lack of diabatic heating" *cause* "PV to be mixed inside the eye"?

   (c) In panel (e), why does the eyewall reform at a larger radius? This step would seem to be an important one in the whole process.

   The cartoon is rather suggestive of a phenomenon recently articulated by Smith et al. (2021) that the authors may be unaware of.

2. The key findings of the study are enumerated in the summary and conclusions section, but the take home message from these findings are somewhat thin. Let me discuss these in turn:

   (a) "In Hurricane Irma, during the second period of rapid intensification, intensity fluctuations occurred, which caused short term intensification and weakening periods, although overall the storm continued to intensify."

   This would seem to be a tautology: "intensity fluctuations" causing "short term intensification and weakening periods". This doesn't say very much of substance.

(b) “During strengthening phases the PV distribution was an elongated ring which became more azimuthally symmetric and monopole-like during weakening phases. Note that the azimuthal symmetry is independent of the radial PV distribution and the ring-like PV states (strengthening phases) were associated with less azimuthally symmetric distributions.”

The first sentence is merely a description of the simulation. It is not clear to us why is this information is being provided and in what way it provides new understanding. The second sentence is simply mysterious.

(c) “During strengthening phases, the diabatic heating distribution had a smaller radial extent and a stronger heating maximum which is located within the RMW. During weakening phases the heating was outside the RMW and had a greater radial extent than the diabatic heating during the strengthening phases.”

The same remark can be made as that in (b). Why is this finding thought to be worthy of mention? In particular, what is the explanation for these changes in diabatic heating rate. In fact, what is meant by “smaller radial extent”. Do you mean radial thickness? And smaller than what?

(d) “VHT-like structures were stronger and more common during strengthening phases than weakening phases and contributed positively to intensification through eddy advection of angular momentum.”

Are you talking about radial advection or vertical advection of angular momentum? Wouldn’t one be surprised if this were not the case? See e.g. Nguyen et al. (2008). However, note that the localized VHT structures project also on to the mean fields as well as the eddy fields (see e.g. Persing et al., 2013).

(e) “Unbalanced dynamics were shown to play a role in the intensity fluctuations. During the weakening phases an unbalanced supergradient tangential flow produced an outflow jet which acted to spin-down the flow above the boundary layer by transferring low angular momentum from the eye outwards.”

This may be the case, but how do the authors account for the unbalanced supergradient tangential flow above the boundary layer in the first place?

It would be interesting to know how the diabatic heating distribution relates to the ventilation diagnostic introduced in the paper by Smith et al. (2021), which is seen as a measure of the ability of deep convection to evacuate mass at the rate it is converging in the boundary layer. In fact, this ventilation diagnostic may provide a useful link to relate the various quantities investigated in this paper.

Specific comments

1. L4-7: It is unclear why potential vorticity and relative vorticity are invoked in the same sentence. Why not stick with one or the other? Is there any special reason to prefer potential vorticity over relative vorticity?
2. The acronym RMW is defined at line 25, but no height is specified. Is it the absolute maximum? It is relevant to know this height in Fig. 14, for example.
3. Figure 4: Must be max tangential wind speed. What is the RMW of vmax?

4. L257-326: Why are the PV fields being shown? Since PV is not conserved and you are not inverting it, its use needs to be explained. What is the significance of the structural changes of PV? In fact, how is it defined?
5. L327-335: The definition of the barotropic conversion rate should be spelled out in the text. Even so, the energy transfer is from the mean flow to the eddies, which would suggest that the maximum tangential wind speed is decreasing with time, which is apparently not the case. How important are the energy fluxes through the boundaries of the integration domain?
6. L343-369. The section on VHTs is purely descriptive and contains a lot of detail. The problem for us is, it is unclear where this section is leading. It would seem desirable to include a few sentences to guide the reader into where it is leading. For example, why are the VHTs important?
7. L405-407: This sentence seems to put the cart before the horse as it invokes the VHT structures to be the primary cause of the stirring in the eye. However, the stirring is presumably a result of barotropic instability, with the understanding that the convection is enhancing the growth rates of the unstable modes (see Nguyen et al. 2011).
8. L409-410: The authors should explain here why they are showing the distribution of diabatic heating rate. Are they invoking balance dynamics (following Shapiro and Willoughby, JAS, 1982) to infer changes in the mean secondary circulation that might ensue?
9. L412-415: What is the significance of the information provided here? Why is the lowest heating rate maximum at such a low level?
10. L416-417: Why is this information provided? Why is the greater radial extent of the heating rate worthy of note? What are the implications?
11. Section 4.3: the questions in (10) apply to much of this section. Shapiro and Willoughby op. cit. show that the balanced secondary circulation depends on the radial and vertical gradients of diabatic heating rate and not on the diabatic heating rate, itself. But what does the heating rate by itself tell us?
12. L457-459: This sentence is indigestible.
13. L463-464: This sentence is misleading since, from an axisymmetric perspective, converging air parcels in the vortex boundary layer are always losing AAM. The word "initially" is misleading as the arguments refer to radial displacements of air parcels.
14. L471ff: Figure 14 is intriguing and potentially important, but raises a number of questions. First, which RMW is being referred to here? And how does it compare with the radius of 35 km? Why would "a decrease in the agradient force per unit mass" *cause* "an increase in the agradient wind"? How does "the appearance of a convergence zone above the boundary layer" cause "a decrease in PGF"? How is the "balanced inflow" calculated, or is it speculation that the inflow is balanced? How does "rainband convection" enhance "the balanced inflow"?
15. L485ff: The arguments here seem to be pure speculation and I am totally confused.
16. First of all, why not explain the trends in this figure before trying to explain the wiggles? Second, in explaining both the trends and the wiggles, it would seem to be necessary to show the vertical advection of the supergradient winds from the boundary layer into the eyewall (Schmidt and Smith 2016, Montgomery and Smith 2017, see also Smith et al. 2020). It would appear that the results you show are affirmation of the eyewall spin up articulated in the latter papers. This would seem worthy of mentioning.

Section 4.4.2: To consider the tangential wind budget alone is not sufficient "to understand how the boundary layer and outflow jet change and lead to a spin–down above the boundary layer".

This budget cannot explain "changes in the secondary circulation and what drives these changes" as this requires consideration of the radial and vertical components of the momentum equation as well (Smith and Montgomery 2015). The primary reason that air flows outwards above the boundary layer is that inner-core deep convection is collectively too weak to ventilate mass at the rate that mass is being funnelled to the base of the eyewall by the boundary layer (Kilroy et al., 2016, Smith and Wang 2018, Montgomery et al., 2020, Smith et al., 2021).

L515: For reasons discussed above, "the intensity fluctuations in Hurricane Irma" cannot "be understood in terms of unbalanced boundary layer dynamics" without considering the changes in deep convective mass flux. One solution to this issue might be to investigate the ventilation diagnostic used by Smith et al. (2021).

L588: The inability of the boundary layer updraft to properly couple with a potential secondary updraft above is precisely what the last two points are trying to convey.

Signed: Roger Smith and Michael Montgomery

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