Review of the revised 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

Our summary of the first review of this manuscript read: “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.” Unfortunately, this summary remains valid.

In our first review, we made a few suggestions for a way forward and the authors have made a commendable attempt to explore these suggestions. The authors have set themselves a tough problem to research, but reading the discussion section 4.5 and the summary and conclusions in section 6, it is not clear to us that they have made much headway. In fact, we really struggled to understand what they think is going on and to make matters worse, they seem to have changed their own view in going from the abstract to the conclusions. We have tried to articulate our concerns in detail in the hope that it will help the authors to rethink and revise what they have written to make the arguments clearer.

Review signed: Roger Smith and Michael Montgomery

Comments and questions

1. In the abstract, it is stated that “The boundary layer was found to play an important role in the cause of the intensity fluctuations with an increase in the agradient wind within the boundary layer *causing* (our emphasis) a spin-down just above the boundary layer during the weakening phases whereas during the strengthening phases the agradient wind reduces.” For a start, doesn’t this depend on whether the agradient wind is positive or negative? And what metric is being used to charaterize the “intensity fluctuations”? Are you talking about the maximum wind speed anywhere or the maximum 10 m wind that forecasters use? In the summary (section 6), you say that your study “ ... emphasises the role of the inner rainbands in *causing* (our emphasis) weakening periods.” Finally, the last part of the quoted sentence would be improved by using “weakens” instead of "reduces".
2. Incidentally, at line 6, what is the difference between an “isolated local” region and an “isolated” region?

Before reading the main body of the manuscript, we studied the Summary and Conclusions, hoping to gain an overview of the new insights emerging from the manuscript, but we were most disappointed. Almost every sentence raised scientific questions and by the end of the section we were no wiser. We strongly recommend that the authors go through this section in detail and revise accordingly to address our questions enumerated below.

3. At lines 716-718 we are told that "Key and novel results include the finding that intensity fluctuations are related to convective and barotropic structural changes with the asymmetric convection playing a key role in the fluctuations. So what are these key novel results?

4. In the next sentence we are told that "Both unbalanced and balanced intensification processes were important with the balanced effect of inner rainband convection *leading to* (our emphasis) an unbalanced boundary layer response which, in turn, caused a spin-down during weakening phases." But where did the inner rain band convection come from in the first place?

5. At lines 721-722 we are told that: "In Hurricane Irma, during the second period of rapid intensification, intensity fluctuations occurred, defined as short term intensification and weakening periods". But how are these intensity fluctuations characterized? By the maximum tangential wind speed or the maximum total wind speed at 10 m (the forecaster definition)?

6. In the next sentence we are told that: "The tangential wind, *at all levels (our emphasis),* increased more during strengthening phase than it decreased during weakening phase so the fluctuations do not prevent the storm from rapidly intensifying." Does at all levels mean everywhere in the flow? See last point.

7. In the next sentence we are told that: "During the weakening phase the mean sea level pressure rose nearly concurrently with the weakening of the tangential wind which was the opposite of e.g. Nguyen et al. (2011) where the weakening of the tangential wind was accompanied by a mean sea level pressure drop." Why is this information provided at this point and why is it a key finding? Key findings should consist of explanations.

8. The next key finding, at lines 727-729 is: "During strengthening phases the PV distribution was an elongated ring which became more azimuthally symmetric and monopole-like during weakening phases. This contradicts previous studies (e.g. Nguyen et al., 2011) which show an association between azimuthal symmetry and a ring-like radial state and use the terms interchangeably." It seems reasonable to ask what is meant by an elongated ring becomong more monopole-like means? Is it just that that the hole in the ring became smaller? The second sentence about the contradiction with previous studies is unclear, but if it qualifies as a key finding, it requires an explanation. And how many other studies does the finding contradict?

9. The next key finding, at lines 730-733 is: "The change in PV structure is thought to be linked to a build up of barotropic and convective instability during the strengthening phases. During the start of the next weakening phase a breakdown and reorganisation of the eyewall occurs as the diabatic heating is no longer strong enough to maintain the barotropically unstable state. This leads to PV being transported towards the eye and to a rapid increase in barotropic stability." First of all, "is thought to be linked to" is not a very strong statement. What is the basis for this thought? Second, what change in PV structure are you talking about here? In what way does diabatic heating maintain the barotropically unstable state and what is the relevance of the barotropically unstable state? Is there lateral PV mixing going on here as in Schubert et al (1999)? Note that mixing and instability are not synonymous. You say that "This leads to PV being transported … ", but to what, precisely, does "This" refer?

10. The next key finding, at lines 734-735 is: "The increase in barotropic stability during the weakening phases makes the formation of the VHT-like structures less likely. As a result the eyewall becomes more azimuthally symmetric." The reader might ask, why these statements are true. What does the liklihood of "VHT-like structures" have to do with barotropic stability? Why would barotropic instability be favourable to VHTs? Wouldn't VHTs be more related to convective instability? And why would the reduced likelihood result in a more symmetric eyewall?

11. The next key finding, at lines 736-739 is: “During strengthening phases, the diabatic heating distribution had a smaller radial spread 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 spread than the diabatic heating during the strengthening phases. The change in diabatic heating during the weakening phase was linked to convection becoming weaker and the eyewall thicker.” Was *all* the heating outside the RMW during weakening phases? Also, why did the convection become become weaker and what are the consequences of having a thicker eyewall?

12. The next key finding, at lines 740-743 is: “The change in heating structure at the start of the weakening phase, associated with VHT–like structures forming just outside the eyewall near the inner rainbands caused the strengthening of the outflow jet above the boundary layer both directly through the induced balanced circulation and by depriving the eyewall of heat and moisture, weakening the eyewall convection and further reducing the ability of the eyewall to ventilate the mass inflow from the boundary layer.” This whole statement is somewhat indigestible, but it raises some questions. First, why are the VHT structures forming just outside the eyewall? And how do you know that these “caused the strengthening of the outflow jet above the boundary layer”? Are you arguing that this is an enhanced “suction effect” associated with the VHTs? Also, how do you know that the VHTs deprived the eyewall of heat and moisture? Doesn’t the weakening of the eyewall convection depend in part on the degree of convective instability?

13. The penultimate key finding, at lines 744-749 is: “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. During the weakening phase as the VHT–like structures became less common, this lack of contribution to the tangential wind above the boundary layer likely led to further weakening. Vertical advection of absolute angular momentum contributes positively to intensification above the boundary layer. In the boundary layer the radial advection of mean absolute angular momentum contributes positively towards intensification.” The first question is, in the first sentence, are you talking about the *vertical* eddy advection of angular momentum? To what “further weakening” does the lack of contribution to the tangential wind above the boundary layer lead to? What is weakening? In the penultimate sentence, what, precisely, does intensification refer to? The tangential wind speed? Regarding the last sentence, wouldn’t it be most surprising if the radial advection of mean absolute angular momentum did not contribute positively towards intensification in the boundary layer, assuming of course that intensification refers to a spin up of the tangential wind speed?

14. The final key finding, at lines 744-749 is: “Unbalanced dynamics were shown to play a role in the intensity fluctuations. During the weakening phases an unbalanced supergradient tangential flow within the boundary layer, which could not be adequately ventilated by the eyewall convection, produced an outflow jet, above the boundary layer, which acted to spin-down the flow above the boundary layer by transferring low angular momentum from the eye outwards.” We do not understand the second sentence. First it is unclear to what “which” refers to in each case. You seem to be talking about the ventilation of a tangential component of the flow, but the immediate idea of ventilation refers to the radial inflow of mass. Why would the vertical advection of a supergradient tangential flow lead to spin down aloft?

Other issues

Having been unable to find much to latch on to from the Summary and Conclusions section, we were getting rather burned out and it is possible that other readers would have a similar problem. However, we did make an effort to understand the cartoon in Fig. 19 that the authors developed to summarize the processes responsible for the intensity fluctuations in their study. As it did in our first review, the cartoon still raises a number of questions:

15. One very basic question is how the authors envisage VHTs differ from ordinary eyewall convection? Panel (a) of the cartoon highlights such a difference. In panel (b) it is indicated that these VHTs help strengthen winds above the boundary layer through radial eddy advection of absolute angular momentum, but so would any convection beyond the eyewall. Why are the two VHTs so special?

16. In panel (c), why are the VHTs extending radially outwards in an upstream direction compared with panel (a)?

17. In panel (d), in what way does convergence enhance convection? What aspect of convection is enhanced and why? Does convergence enhance the buoyancy within convection? Why are there reduced tangential winds above the boundary layer when, as indicated, the radial inflow above the boundary layer is enhanced? Wouldn't this inflow lead to enhanced spin up and therefore a larger inward pressure gradient force (assuming approximate gradient wind balance)?

18. Panel (e) suggests a broadening of the eyewall. If that is the case, wouldn't this thickening decrease rather than increase the potential for barotropic instability?

19. Panel (f): according to balance theory, the strength of the overturning circulation depends on the spatial gradients of diabatic heating rate and not on the heating rate, itself. Therefore invoking the diabatic heating as "insufficient to maintain a ring-like PV structure" is obscure. For the same reason, one cannot argue that "reduced diabatic heating means eyewall is less able to ventilate BL mass influx."

20. A more general comment on the cartoon is that it does not appear to connect with the metric you use to characterize strengthening or weakening? See point 5 above. Further, is the cartoon consistent with the description in the Abstract?