Reply to the reviewers' comments

We would like to thank Roger Smith and Mike Montgomery for carefully reading the manuscript and their suggestions to improve it. We have addressed all points below.

The manuscript has undergone substantial corrections particularly in the discussion and interpretation of the results. In particular we have clarified our results in a way that should be more intuitive to the reader and have been more careful to not over ascribe cause and effect between separate elements of the narrative.

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".**

   We found that the increase/decrease in the agradient wind during the weakening/strengthening phases does not depend on the sign of the agradient wind. This is shown in Figure 14a where the sharp increases or decreases are well aligned in the whole boundary layer regardless of whether the wind is subgradient (blue line) or supergradient (yellow line). The intensity fluctuations are defined with respect to the 10-m total wind speed, minimum sea level pressure and radius of 10-m total wind speed. These three parameters (and others) are shown in Fig. 4. We also no longer attribute cause and effect in the manner described.

2. **Incidentally, at line 6, what is the difference between an "isolated local" region and an "isolated" region?**

   We intended to mean there is a separation between these two regions. We have made this clearer in the text and describe these structures as 'isolated regions of high relative vorticity and vertical velocity'.

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 symmetric convection playing a key role in the fluctuations. So what are these key novel results?**

   The intensity fluctuations are caused by the growth of wave-2 convective modes (during the strengthening phases) as a result of barotropic instability, that eventually destabilize the eyewall through balanced and unbalanced processes. We have made changes in the text to more explicitly highlight the role of barotropic instability in the intensity fluctuations. In particular, details of the relationship between barotropic instability and the fluctuations are summarised in lines 783 to 809.

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?**

   The convection is generated stochastically but is increasingly more likely towards the end of a strengthening phase due to the increase in wave-2 barotropic instability. We have made this clearer in the text, in particular in lines 783 to 809.

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)?**

   We define the weakening periods to be when the surface 10-m wind speed is decreasing or stable and the mean sea level pressure is increasing. This definition is merely used to define the start and end of the weakening and strengthening periods. The azimuthally averaged tangential wind above the boundary layer at 1500 m shows even bigger fluctuations which can be seen in Fig. 4. This is also explained in the summary section in lines 779 to 782.

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.**

   We are referring to the height dependent maximum azimuthally averaged tangential wind. Throughout the text it has been made clearer when we are referring to maximum azimuthally averaged tangential wind or the maximum radius of azimuthal tangential wind. In addition, we have been clearer about the definition of the fluctuations (which is based on changes in 10m wind speed) and large changes that occur during these fluctuations, in particular just above the boundary layer in the azimuthally averaged 1500m tangential wind speed.

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.**

   The main point we want to make here is that the mechanism proposed in Nguyen et al. (2011) where PV is mixed into the eye during the symmetric to asymmetric transition and causes a subsequent drop in pressure during the asymmetric phase is not happening in our study. We have restructured the text slightly to explain this point in more detail. In particular, we discuss the relevance of pressure drop in the context of Nguyen et al. (2011) in lines 651 to 664.

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?**

   “More monopolar” refers to the radial distribution of PV as measured by the metric in Fig. 7a. Specifically, “more monopolar” means a higher PV in the centre of the storm relative to the PV at the RMW at that height (in this case 1500m). However we no longer use this terminology given that a true monopole does not form, instead we refer to the distribution being less ‘ring-like’. Elongated refers to the shape of the PV ring prior which is more elliptical in the strengthening phases (Fig. 7c). The key point being made here is that there is a change in both the azimuthal *and* radial structure of the PV. In previous studies the reduction in azimuthal symmetry was correlated to a reduction in radial symmetry; this is shown not to be the case in our study. No other study on vacillation cycles shows this anticorrelation, they either show a correlation or do not measure both radial and azimuthal distributions. Within the text we have also made the points clearer by referencing to appropriate figures when describing aspects of the storm structure such as PV ring eccentricity.

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?**

   The barotropic instability may be implied from the horizontal distribution of the PV. A change in sign of the radial PV gradient implies a barotropically unstable state. By comparing, for example Fig. 6a to Fig. 6c, we can see that the initially barotropically unstable state, above the boundary layer becomes barotropically stable with no longer any change in sign in the radial PV gradient. So, to answer your 2nd question, this change in barotropic stability is linked to the radial change in the PV structure. In the absence of diabatic heating, since the PV distribution satisfies the Charney-stern criteria for barotropic instability, the ring structure will be mixed out over time. A breakdown, of sorts, does occur in the weakening phase, so the point being made is that the diabatic heating is no longer sufficient to maintain the barotropically unstable state which causes a reversion to a more monopolar PV distribution. We know from trajectory calculations (see Fig. 8) there is transport of PV inwards which is then mixed out in the eye, which happens at the end of the strengthening phase when instability is at its highest. We have made this clearer in the text the relevance of the changing PV structure is explained more clearly in the context of the ongoing weakening of the TC during the middle of the weakening phase

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?**

As in Nguyen et al. (2011) a combined convective, barotropic instability is being proposed and in a similar way the increase in the barotropic instability promotes the growth of these structures which, for clarity, we are now calling 'isolated regions of rotating deep convection'. As the isolated regions of rotating deep convection' grow, convection being preferentially stronger in these regions is what leads to a more asymmetric looking eyewall. In the absence of the isolated regions of rotating deep convection the convection is more uniform. We have made this clearer in the text.

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?**

Not all the heating, but during the weakening phases the majority of the heating is, particularly during the middle of the weakening phase. For example, compare Fig. 12a with Fig. 12e. The weaker convection, from an azimuthally averaged perspective is linked to the convection from the VHTs that formed during the strengthening phase merging with the eyewall convection. One consequence of a thicker eyewall is a reduced radial gradient of heating, which has an effect on the balanced response. We have made this clearer in the text.

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?**

The isolated regions of rotating deep convection' do form in the eyewall, but on the major axis of the ellipse so from an azimuthally average perspective they are further out (see Fig. 13a for example which shows the location of two isolated regions of rotating deep convection' at either end of the eyewall along the major axis). We have experimented with the balanced model and have been able to show that this distribution of diabatic heating does lead to enhanced outflow and, yes, we do posit a suction effect associated with the isolated regions of rotating deep convection'. We have been able to plot equivalent potential temperature to show that, during the weakening phase the region outside of the eyewall is a moister, warmer environment. We have made this clearer in the text.

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?**

When we refer to 'eddy advection of angular momentum' we have changed this to eddy radial vorticity flux which is the third term in our equation 4. The eddy radial vorticity flux contributes more positively to the overall eddy advection term, and overall, the eddy advection is also a positive contributor to the tangential wind especially in the strengthening phases. In the weakening phases the vertical advection terms of equation 4, above the boundary layer, reduces which also contributes to the weakening, so the further weakening is describing the less intuitive eddy radial vorticity flux. 'Weakening and intensification' in this context refers to decreases and increases of the azimuthally averaged tangential wind around the RMW respectively. We agree the last sentence is confirming our intuition, the main novel point being made here is really that the eddies are contributing to the continued intensification of the TC above the boundary layer in the strengthening phases through eddy radial vorticity flux. We have made this clearer in the text.

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 spindown 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?**

We agree this is unclear, in terms of adequate ventilation we are referring to the strong mass influx in the boundary layer not the primary circulation as this sentence appears to read. This is a grammatical mistake rather than a misunderstanding of the concepts on our part. There are three reasons for the increased outflow (i) balanced effects above the BL which we described in a previous response, (ii) outward acting force as a result of the tangential flow becoming supergradient, and (ii) inability for convection to ventilate the frictionally induced mass convergence within the boundary layer. We have made this clearer in the text.

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?**

Symmetrical convection beyond the eyewall might yield similar results though contributions to the mean advection of AAM but there is a mechanism for how this asymmetric convection might develop and then its impact on the TC eyewall. We updated the schematic and the text to make both clearer.

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

It is hypothesised that the isolated regions of rotating deep convection'that form within the eyewall are convectively coupled to outward propagating vortex Rossby waves that move upwind relative to the tangential flow. We updated the schematic and the text to make both clearer.

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)?**

Convergence increases outside of the eyewall during the start of the weakening phases. In cases where there is a conditionally unstable environment, convergence provides a lifting mechanism to initiate convection. Regarding reduced tangential winds, we acknowledge that this is unclear, tangential winds can increase at radii where the inflow is present. The mention of 'reduction of tangential winds' refers to the radial location around the eyewall where tangential wind above the boundary layer starts to drop at the start of the weakening phase. We updated the schematic and the text to make both clearer.

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?**

Yes it does, we have perhaps been unclear by referring to barotropic *stability* instead of instability which we have said increases. We updated the schematic and the text to make both clearer.

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."**

Apologies, this is clumsy terminology on our part. In the PV tendency equation, the term we are referring to is the spatial gradient of diabatic heating projected onto the vertical component of absolute vorticity. Many studies crudely refer to this as simply 'diabatic heating' (e.g. Wang et al 2009). Further barotropic experiments in the absence of diabatic heating (and where the spatial gradient of DH will also be zero) show the ring structure is unstable and degrades into a monopole. It would be more rigorous for us to say, which is also true, that a high spatial gradient of diabatic heating is necessary to generate the PV necessary to maintain the hollow structure. This is what we have found, with the spatial gradient of diabatic heating being high prior to the start of the weakening phase.

Similarly, when we refer to greater values of diabatic heating in the context of ventilating mass flux in the boundary layer what we should say is greater radial gradients of diabatic heating.

We updated the schematic and the text to make both clearer.

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?

Our definitions of 'weakening' and 'strengthening' phases revolve around surface changes in MSLP, RMW and total wind speed partly because these are more easily compared with the observations. However, we only use these quantities to define the start and end of weakening and strengthening phases. In fact, there are more dramatic structural changes that occur at the top of the boundary layer than at the surface which is why we concentrate, throughout, on the 1500-m level.