Reply to the reviewers' comments

We would like to thank Roger Smith, Mike Montgomery, and Reviewer 2 for carefully reading the manuscript and their suggestions to improve it. We have addressed all points below.

Reviewer 1

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.

We thank the reviewers for their interest in the research and for their helpful suggestions to improve the work. In revising the manuscript, we have attempted to address the issues raised and feel that as a result there is a much clearer message in the paper. We will respond to the individual comments below.

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?

We think this may just require some clearer explanation. By 'balanced effect' of VHT we were referring to the induced secondary circulation caused by the azimuthally averaged heating and AAM of these structures.

We have been able to show that the weakening of the pressure gradient force within the eyewall is due to a rise in the pressure which lowers the radial pressure gradient (Fig. R1a,b). An increase in pressure may be due to mass convergence or a weakening in the ascent, both explanations are plausible, but we haven't been able to determine which is the main cause as the pressure budget does not balance well (Fig. R1c,d). The decrease in the pressure gradient force is accompanied by a decrease in the tangential wind and therefore a decrease in the centrifugal and Coriolis force. As a result, approximate gradient wind balance, above the boundary layer, is maintained. This change in the pressure gradient force is instantaneously transferred into the boundary layer, however, the tangential winds do not decrease and so the centrifugal and Coriolis force remain high, hence the agradient wind within the boundary layer increases. We have added this explanation to the first section in the unbalanced dynamics part of the manuscript (Section 4.4).

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

This expression tries to convey that diabatic heating is required to maintain the ring-like PV structure (since the ring is barotropically unstable), and that in the absence of diabatic heating, the most stable state is a PV monopole. So, a weakening of the diabatic heating seems to cause a tendency towards a more barotropically stable monopole-like PV state. We have revised the text in the discussion describing this process to make our meaning clear.

(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 BL outflow jet is causing weakening at the radius of the eyewall while outside the eyewall spin up is occurring due to the unbalanced spin up mechanism. Re-formation of the eyewall at a larger radius can be explained by spin-up occurring outside of the eyewall and weakening within the eyewall. The caption in the cartoon in Fig. 19 in the manuscript has been changed accordingly.

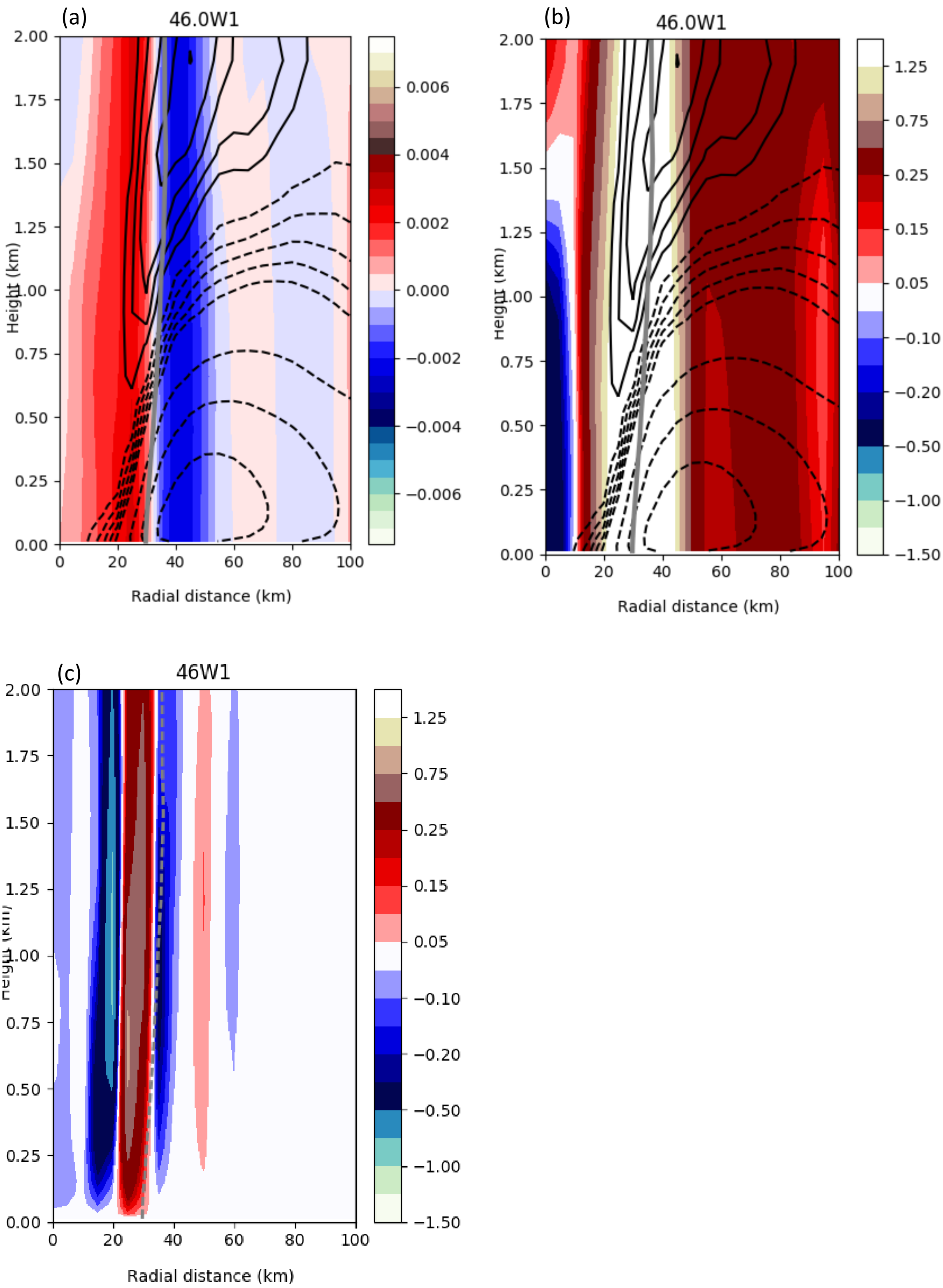

Figure R1: (a) Pressure gradient force tendency (m s^{-2} h^{-1}, shading) and black contours show the radial wind in 1 m s^{-1} intervals. (b) Coloured contours show pressure tendency (hPa h^{-1}) and black contours show radial wind in 1 m s^{-1} intervals. (c) The right-hand side of the pressure 'budget' (integrated horizontal divergence + vertical velocity + integrated mass advection in units of hPa h^{-1}.)

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

Thank you for pointing out the Smith et al. (2021) paper, which we had not seen before. There are indeed some striking similarities here. Our work may help tie together the boundary layer processes described in Smith et al. (2021) and previous work on vacillation cycles and 'fake SEF' in failed eyewall replacement cycles which also describes a situation where a rainband has a disruptive effect on the eyewall, but a secondary eyewall replacement does not occur. A future paper will look at some key differences between the proposed mechanism and eyewall replacement cycles. In the manuscript we added a reference to Smith et al. (2021) and included discussion of how their results fit with our findings.

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.

We agree that the sentence could have been formulated better and have changed the wording. You are correct, intensity fluctuations don't cause weakening and intensification but are defined as periods of weakening. In other words, in intensity fluctuations occurring during RI, the "weakening periods" were characterised by an increase or the stagnation of the pressure and a decrease in the tangential wind. The point we want to make here is that the fluctuations do not stop the overall intensifying (RI) trajectory of the storm. In other words, the reduction of the tangential wind speed during a weakening period is always smaller than the increase in the tangential wind speed during the next strengthening period.

(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.

The main point here is that azimuthal symmetry is anti-correlated with how ring-like the PV distribution is, which is the *opposite* of what previous papers found. We have made that clearer in the text.

(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?

When calculating the potential temperature budget, the term that contributed the most was the ‘cloud rebalancing’ parametrization (latent heat release from cloud formation), so the weakening of the eyewall, in terms of decrease in tangential wind, can be attributed to less latent heat release in the weakening phases since a decrease in the strength of the convection prevents the eyewall from being able to ventilate as much of the mass flux from the boundary layer. By smaller radial extent we do mean the thickness. If you take an arbitrary medium heating contour like 20 K h^{-1}, in the weakening phases it would have a greater radial extent, but a higher contour such as 50 K h^{-1} might be completely absent unlike in the strengthening phases where the heating column is stronger and narrower. We have reworded this point to make it clearer what we are trying to say.

(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).

We grouped the radial advection and vertical advection terms together. However, above the boundary layer radial advection of eddy angular momentum is largely responsible for the spin up. We agree that the VHT-like structures do project onto the main field, but we viewed the effect of the VHT-like structures on the eddy fields to be the most relevant to the mechanism of the intensity fluctuations. We did also plot the mean fields which show that mean advection of absolute angular momentum is responsible for spin up within the boundary layer. We have reworded this point in the paper.

(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?

We did not mention a supergradient wind above the boundary layer, instead we refer to a supergradient wind *within* the boundary layer that provokes an outflow response at the top of the boundary layer in order to return to gradient balance in the free vortex above. We have reworded this point in the paper.

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.

Adding the ventilation diagnostic from Smith et al. (2021) is a very good suggestion as it provides a useful means of understanding the dynamics at a deeper level. We have added a figure for ventilation diagnostic to the manuscript (Fig. 16) along with a relevant subsubsection explaining its significance and compared the results to Smith et al. (2021). The figure is also included in this reply as Fig. R2 and shows how the ventilation index and the related outflow jet vary throughout the simulation. We see that during the weakening phases the ventilation index becomes more negative which is associated with the reduced convection in the eyewall region increasingly more unable to ventilate the boundary layer inflow. The decreasing ventilation index is also associated with a concomitant enhancement of the boundary layer outflow jet which leads to further weakening of the eyewall convection, and in turn a further decrease in the ventilation diagnostic.

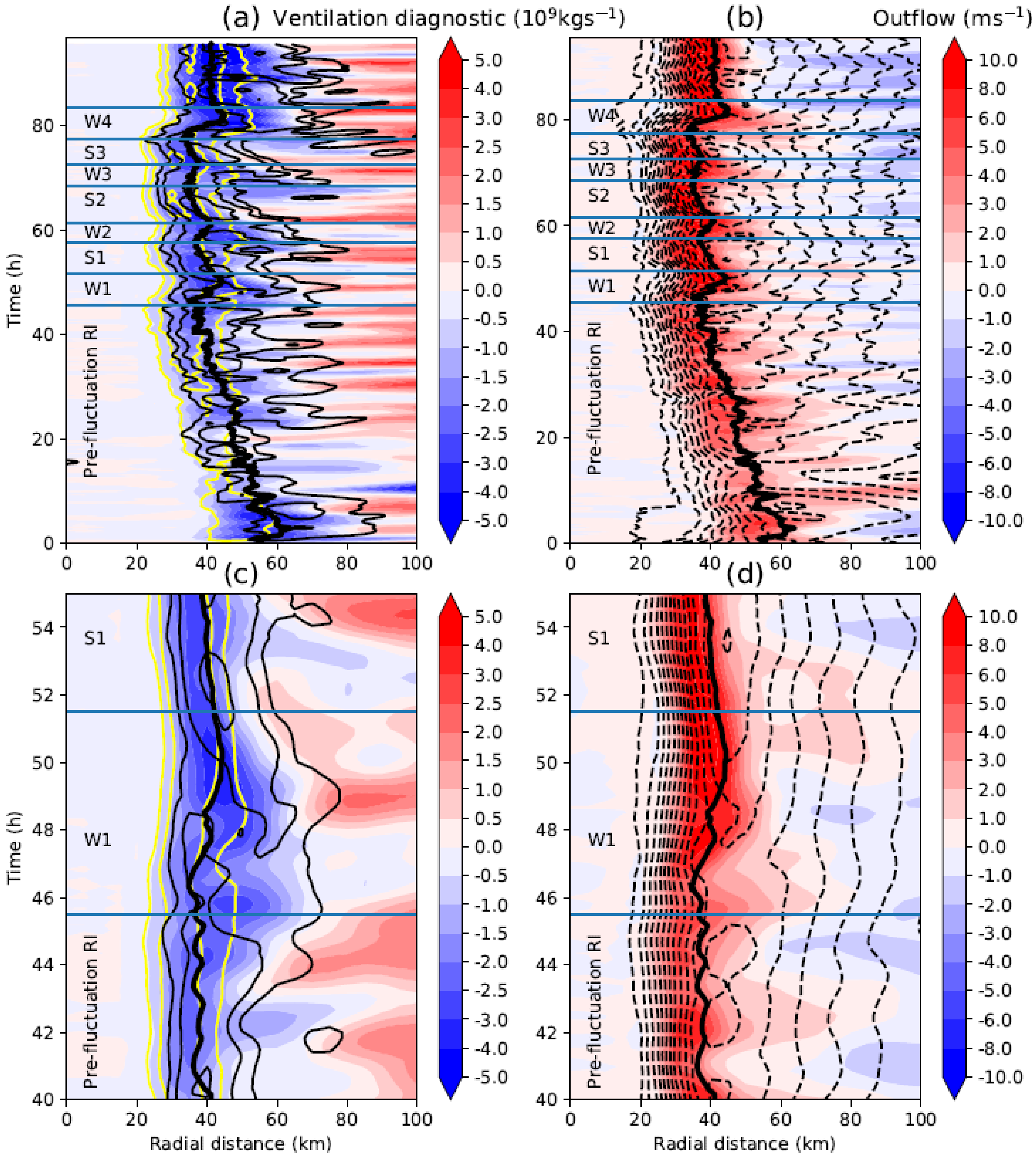

Figure R2: (a) Coloured contours show ventilation diagnostic index 10^9 kg s^{-1} with the azimuthally averaged radially integrated mass flux taken between a height of 6 and 1 km as a function of integration radius. Black contours show vertical velocity in 1 m s^{-1} intervals and the 0.5 m s^{-1} contour for 6 km height while the yellow contours show the same for 1 km height. (b) Coloured contours show the azimuthally averaged radial wind at 1532 m height (just above the boundary layer, m s^{-1}) while black contours show the azimuthally averaged surface radial wind in 2 m s^{-1} intervals (dashed contours indicate negative radial wind or inflow). (c) and (d) show zoomed in versions of (a) and (b) respectively highlighting the times around W1. The RMW for the maximum azimuthally averaged tangential wind at 1532 m height is indicated by the thick black line in all subplots.

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?

We prefer PV as a diagnostic tool due to its link to barotropic stability, but relative vorticity seems to be most commonly used in literature on VHTs. That is why we use relative vorticity when talking about VHT-like structures.

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.

In line 25 in the literature review we are referring to a very simplistic hypothetical case. We made this clearer in the revised version of the manuscript. Where we plot height dependent RMW in the results section we have made clear at which height this is calculated.

3. Figure 4: Must be max tangential wind speed. What is the RMW of vmax?

The RMW is the radius at which the azimuthally-averaged tangential wind at the specified height is maximum. Where 'surface (10m) maximum total wind speed' is used, it refers to the maximum total wind speed with no azimuthal averaging. We have updated the text and figure caption accordingly.

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?

PV gives insight into how the barotropic structure and instability changes between the weakening and strengthening phases. In addition, in previous research on vacillation cycles PV was used as a key metric to distinguish between 'asymmetric' and 'symmetric' states which have some similarities to our weakening and strengthening phases. Looking at how the PV distribution changes will allow for a better comparison between our fluctuations and the described vacillation cycles and help determine whether they are equivalent phenomena.

We added a sentence at the start of the PV paragraph which reads as: "Previous studies on vacillation cycles have used PV as a metric to show the structural changes of the vortex during the weakening and strengthening phases. Van Sang et al. (2008) described how, a barotropically unstable ring–like PV state would break down into isolated inward moving PV anomalies. To determine whether the intensity fluctuations are similar to these vacillation cycles it is helpful to examine this PV structure."

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?

We have added the definition of the barotropic conversion rate along with some references to its use in vacillation cycle papers. We agree it does seem counter intuitive that tangential windspeed decreases, although this is only one term of the eddy kinetic energy budget (Hankinson et al., 2014):

$$\frac{\partial K'}{\partial t} = ERF + EVF + CONV - BARO + Fric_{K'}$$

We have explicitly added the equation into the paper and explained how the barotropic conversion index can be negative throughout the simulation while the TC is still able to intensify. The variable functions as a useful proxy for the barotropic instability which is important in determining the structure of the tropical cyclone.

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?

Adding a motivation for the VHT analysis is a good suggestion. VHTs are important because they are likely to play an important role in the transition from a strengthening phase to a weakening phase. In addition to the fact that VHT-like structures appear more often and are stronger during the strengthening phase, they are also hypothesised to be an artefact of high convective and barotropic instability and the balanced response of these structures in terms of the inflow they induce at higher radii leads to convergence which we think plays a role in the transition to the weakening phase.

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).

The PV-ring state is always barotropically unstable due to the presence of a sign change in the PV gradient. The key processes are that diabatic heating weakens and therefore the TC cannot remain in the barotropically unstable state. Consequently, PV is transported inwards towards the eye at the start of the weakening phase and the PV structure becomes more monopole-like. In addition, the VHTs are at their strongest just prior to the start of the weakening phase. We have added more details at the end of the tangential wind budget discussion (section 4.2.3 of the revised manuscript) with reference to the VHT-like structures emerging as a result of the barotropic instability.

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?

You are right, balanced dynamics are indeed being invoked. Additional work (not shown in the paper) was undertaken to provide robust evidence for this balanced induced circulation above the boundary layer including the running of the idealized balanced model as used in Smith et al. (2015) to replicate some of the features in the secondary circulation. We have added more explanations in lines 434-436 of the revised manuscript.

9. L412-415: What is the significance of the information provided here? Why is the lowest heating rate maximum at such a low level?

The diabatic heating distributions (and their radial and vertical gradients) are important to understand the balanced circulations above the boundary layer. In addition, weakening diabatic heating in the eyewall is relevant in terms of the TC's inability to maintain the unstable barotropic structure and inability to ventilate mass from the boundary layer. The important information is the differences in the heating distributions between the weakening and strengthening phases. We have added information about potential temperature budgets in line 437 - 439

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?

The larger radial extent of the diabatic heating distribution is the most substantive difference between weakening and strengthening phases in terms of diabatic heating distributions. The larger radial extent in the weakening phase is important at developing a secondary circulation at the higher RMW, sowing the seeds for the RMW increase. We have added some points in the manuscript in lines 444-445 to make this clear.

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?

This is a good point. Instances of 'diabatic heating' should be replaced with *r* or *z* gradients of diabatic heating. Fortunately, the argument and conclusion would remain unchanged. We have added some sentences to emphasise that it is the radial and vertical gradients of the diabatic heating that are important.

12. L457-459: This sentence is indigestible.

You are right and we have reworded the sentence. We were trying to say that: (i) VHT structures outside the original eyewall spread out azimuthally to form more azimuthally symmetric convection and (ii) this convection becomes dominant at the slightly larger RMW.

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 mis-leading as the arguments refer to radial displacements of air parcels.

Yes, this is true because the AAM is lower the closer to the centre you get. It isn't true that the gain through rapid reduction in *r* needs to completely offset the AAM loss, it just needs to offset it enough such that the AAM decreases less rapidly than the AAM contours. This inaccuracy has been corrected.

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”?

The RMW defined as the radial location of the maximum in the azimuthally-averaged tangential wind varies with height and time unlike the fixed radius at 35 km also shown in panel a of Figure 14. This fixed radius was chosen as it approximately corresponds to where the eyewall is, although the eyewall’s radius is not constant (and nor is the RMW) hence why the panels b-d of the same figure show the values at the RMW. We chose to show plots of both a fixed radius and an RMW dependent radius to separate out effects in the supergradient wind that can be explained solely by the expansion of the RMW. We have changed our argument in the manuscript and tried to make our points clearer. The argument goes as follows: (1) The tangential wind weakens above the boundary layer in the eyewall region due to an increase in inner rainband convection enhancing the outflow jet and advecting low absolute angular momentum air outwards. (2) The reduction in the tangential wind above the boundary layer is accompanied by a reduction in the pressure gradient force such that gradient wind balance is maintained. (3) The reduction in PGF above the boundary layer is instantaneously transferred into the boundary layer leading to an increase in the agradient wind, i.e the wind becomes more supergradient within the boundary layer and ascent is therefore encouraged at a higher radius.

15. L485ff: The arguments here seem to be pure speculation and I am totally confused.

We have added some clarification to the paper. The positive radial gradient of the diabatic heating associated with rainband convection is thought to accelerate the outflow, above the boundary layer and radially inside the rainband. That is to say, the rainband induces a balanced outward secondary circulation in the same region as the outflow jet above the boundary layer, and therefore enhances this outflow jet.

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.

This is a good suggestion. We have added a figure below (Fig R4b) showing the vertical advection of the tangential wind in the supergradient boundary layer and have discussed the results in relation to Schmidt and Smith (2016) in the 3rd paragraph of the Unbalanced dynamics section of the revised manuscript. In the eyewall region the supergradient winds which are strongest in the middle of the boundary layer (at around 700m) are advected upwards. Above the boundary layer the radial advection of lower absolute angular momentum air approximately cancels out the vertical advection from the boundary layer and is close to gradient wind balance. During the weakening phases the wind becomes more supergradient in the boundary layer (see also Figure 14 in the revised manuscript). However, the vertical transfer of this supergradient wind only happens effectively outside the RMW at the top of the boundary layer. Hence, during the weakening phase the tangential wind is able to spin up outside the RMW but weakening still occurs inside.

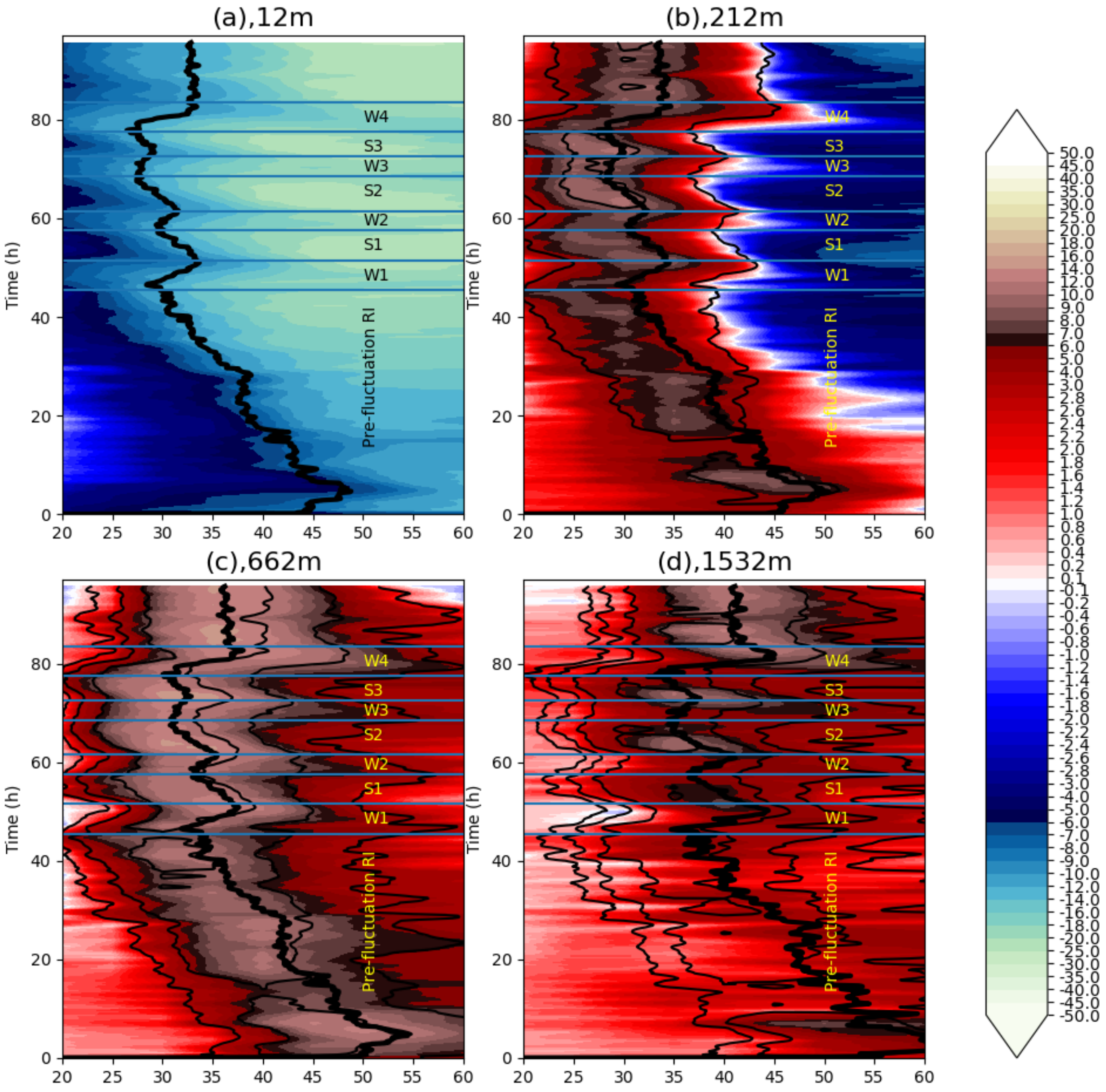

Figure R3: Coloured contours show the strength of the agradient wind (m s^{-1}) at various levels throughout the boundary layer. Line contours show the vertical velocity. The RMW is indicated by the thick black line.

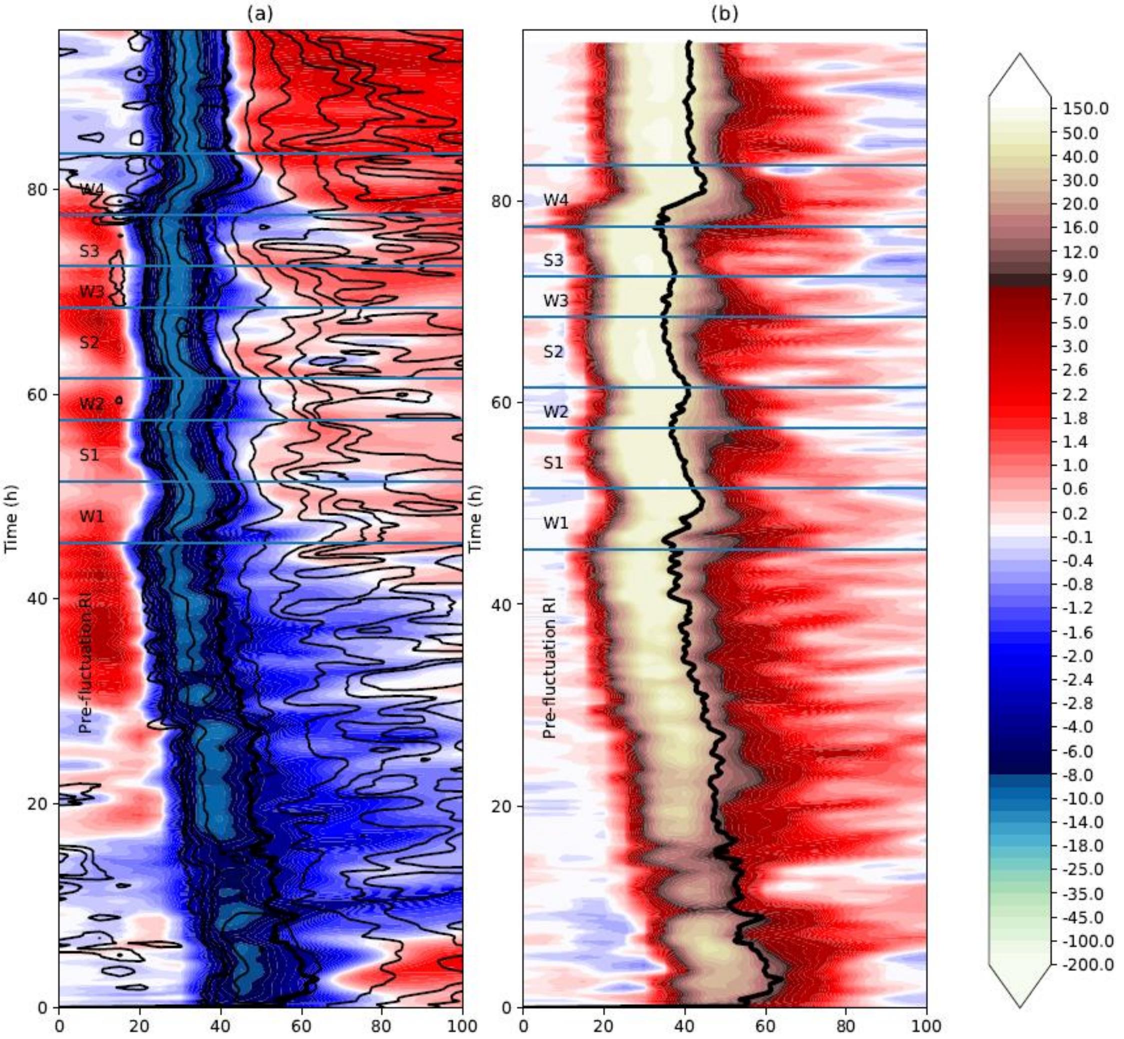

Figure R4: (a) Coloured contours show the difference in the tangential wind (m s^{-1}) between the top of the boundary layer (1532 m) and the middle of the boundary layer (667 m). Line contours show the vertical velocity (m s^{-1}). (b) Coloured contours show the magnitude of the contribution to the tangential wind of the vertical advection of absolute angular momentum (m s^{-1} h^{-1}) at the 1532 m level. The RMW is indicated by the thick black line.

In addition, to the overall trend of the agradient wind throughout the simulation we can also see that the tangential wind at the top of the boundary layer is typically at least 5 m s^{-1} weaker than at the middle of the boundary layer in the eyewall. At the start of, and a couple of hours prior to, the weakening phase outside the RMW the tangential wind at the top of the boundary layer becomes even weaker while inside the RMW the tangential wind may become stronger.

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).

We have included a figure in the redraft showing the ventilation diagnostic to provide a more complete picture as in Smith et al. 2021. We do believe the findings in our paper have some similarities to that of Smith et al., 2021 but with some key differences. Unlike Smith et al., 2021 the eyewall is always unable to completely ventilate the mass inflowing from the boundary layer and, as such, always has an outflow jet above the boundary layer. We do find that at the radius where the strongest inner rainband convection (and VHT-like structures) occur the convection is still insufficient to ventilate the incoming mass just prior to the weakening phases which leads to an outflow occurring above the boundary layer at radii greater than this radius of strongest convection, outside the eyewall. Also, we find that during the course of the weakening phase the ventilation diagnostic index, in the eyewall, becomes more negative as the convection within the eyewall weakens, which further increases the boundary layer outflow and provides an increasingly favourable environment for convection at a greater radius.

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).

We have calculated the ventilation index as suggested and included a discussion of this index in ll. 542-566 of the revised manuscript. Please also see the reply to the previous point.

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.

We addressed this issue when editing the manuscript in regard to the previous two points.

Reviewer 2

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.

Thank you for your overall positive view on the paper. We have tried to "declutter' the paper to make it easier to read and not to distract the reader from the main message.

Specific Comments:

L 53 - 56. Some of the references are a bit misleading. For example, the results attributed to Hankinson et al. (2014) and Reif 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 Reif et al. was to examine the robustness of the results to using a different non hydrostatic model (WRF).

That is a good point. We have fixed that.

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

During this period the SSTs were marginal (around 26-27°C) and dry air was present to the northwest of the storm centre (Saharan air mass). We have added that information to the manuscript.

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

During this period 500-700 hPa layer relative humidity has increased to around 55% around the TC as the TC had moved to the south of the dry airmass. SSTs had increased more markedly to 28-28.5°C and are at this point continuing to increase. We have updated the manuscript to include these details.

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

Low vertical wind shear, high SSTs and adequate mid-level moisture (>50% 500-700 hPa relative humidity). We have updated the manuscript to make this clear.

L 185. How well does the PV budget close?

The Lagrangian form of the PV budget closes almost perfectly. The initial advected PV tracer plus the total PV tracer plus the dynamics tracers (first two terms in equation 1) almost exactly equal the LHS with the value of epsilon being extremely small. This budget is shown in Fig. R5 with the residual term being an order of magnitude smaller than the physics term. We have not included the figure in the manuscript but do now mention the small size of the residual term.

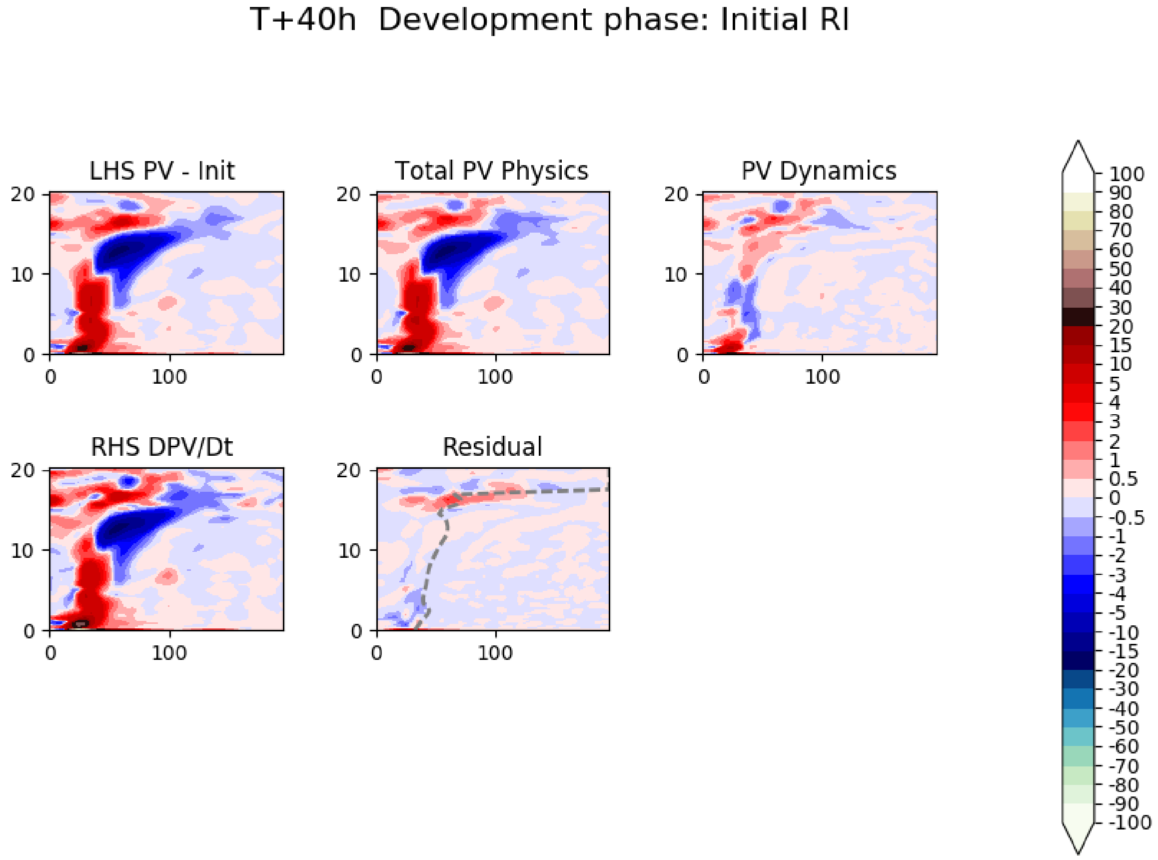

Figure R5: Lagrangian PV budget (PVU units) showing (a) the initial advection tracer subtracted from the diagnosed PV (Left hand side of Equation (1) in the revised manuscript), (b) the total physics tracer (first term in the right hand side of equation (1) in the revised manuscript), (c) the dynamic correction term (second term in the right hand side of equation (1) in the revised manuscript) and (d) the residual which is equivalent to ε in equation (1) in the revised paper.

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?

The physics part is large (of comparable magnitude and opposite sign within the eyewall) and is dominated by the cloud rebalancing component (latent heat from cloud formation) which is shown in Figure R6 with the cloud rebalancing tracer and microphysics tracer responsible for most of the PV increases in the eyewall region. The main role of the physics part is maintenance of the PV ring within the eyewall to counterbalance loss through vertical advection. We have added a footnote to explain this in the paper.

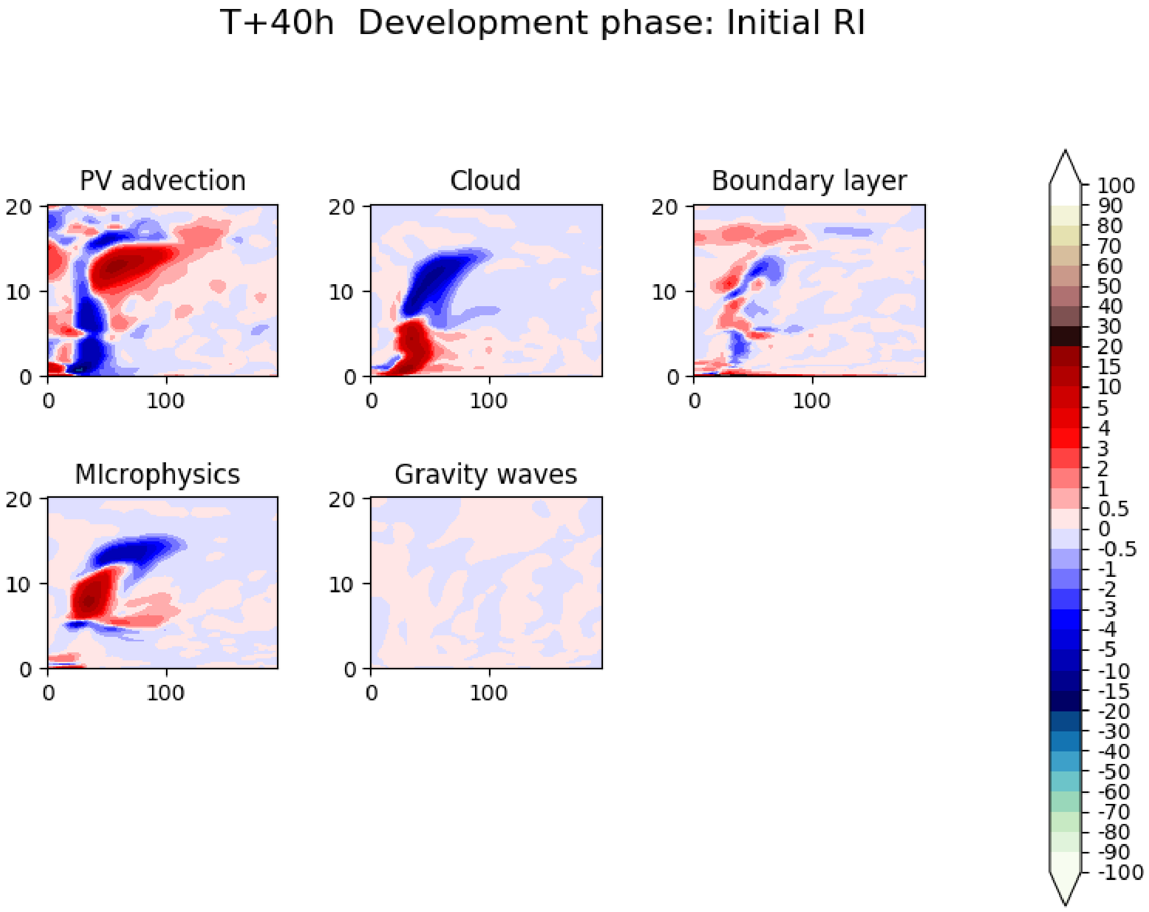

Figure R6: Lagrangian PV budget (PVU units) showing (a) the advection calculated by taking the initial advection tracer subtracted from the diagnosed PV from the previous hour, (b) the cloud rebalancing tracer, (c) boundary layer tracer, (d) the microphysics tracer, and (e) the gravity wave tracer.

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?

The total kinetic energy in the mean state is also determined by other terms such as the mean and radial advection which contribute more to the total kinetic energy budget as in Hankinson et al. (2014). The main point that should be taken away from this discussion is that when the weakening phase starts, the barotropic instability has started to decline (become less negative). The emphasis should be on the increase in the magnitude of the barotropic instability towards the end of the strengthening phases, i.e. during the course of a strengthening phase the barotropic instability increases (barotropic conversion rate becomes more negative) until reaching a maximum prior to the weakening phase where it suddenly decreases and (barotropic conversion rate becomes more positive) less energy is now transferred from the mean to eddy state. An explanation has been added to the previous paragraph of the revised paper.

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?

In Smith and Eastin the definition of a vortical hot tower requires perturbation vertical velocities greater than 5 m s^{-1} and cloud depths greater than 6 km along with perturbation relative vorticity above 10^{-3} s^{-1}. It is common to see perturbation vertical velocities in the 3-5 m s^{-1} range and sometimes exceeding the 5 m s^{-1} requirement and perturbation relative vorticities above 10^{-3} s^{-1}. However, strict implementation of the algorithm requires that this be maintained over a depth of at least 6 km for the perturbation vertical velocity requirement and 3 km for the relative vorticity requirement, which never occurs in our simulations. In addition, these requirements must be met for the same latitude and longitude point, so a VHT-like structure that is sheared will also not meet the requirement. The algorithm was likely designed for a weaker storm (less than hurricane strength) prior to rapid intensification where VHTs are much stronger and deeper relative to the storm scale circulation. The strict definition has been added to the manuscript and compared to the less strict usage adopted here.

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

Where strong VHT exists, there will be perturbation relative vorticities above 10^{-3} s^{-1} and perturbation vertical velocities above 2 m s^{-1} and either the perturbation relative vorticity or vertical velocity is maintained over a large depth (visible at 2532 m, 4963 m and 9934 m) for the same VHT. We have added this information to the manuscript.

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.

This is a typo in the caption. The friction term is always large and negative, not positive. We have corrected that in the manuscript.

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.

Thank you for the suggestion. We have looked into it, and a key point we wish to emphasise which can be seen in Figures 10, 11 and 18 of the revised manuscript is that the VHT-like structures contribute to the increase in the strength of the tangential wind, in the eyewall above the boundary layer. The contribution to the strength of the tangential wind, above the boundary layer, is especially strong when the radial location of the VHT-like structures are near the RMW (see for example Fig. R8c,h where there is significant eddy contribution to the tangential wind just inside the RMW when the VHT-like structures are near the RMW). Prior to the weakening phases some differences are apparent, firstly the lifespan of the VHT-like structures are longer (compare rows 3, 4 with rows 1, 2 in Fig. XX) and the radial location of the structures are not limited to near the RMW. In the case of Fig. R8m,r there is distinct ascent (associated with these structures) both outside and inside the RMW. It is apparent that strong VHT-like activity, especially where these structures are present away from the RMW, are not beneficial to the TC in spinning up the tangential wind above the boundary layer just inside the RMW.

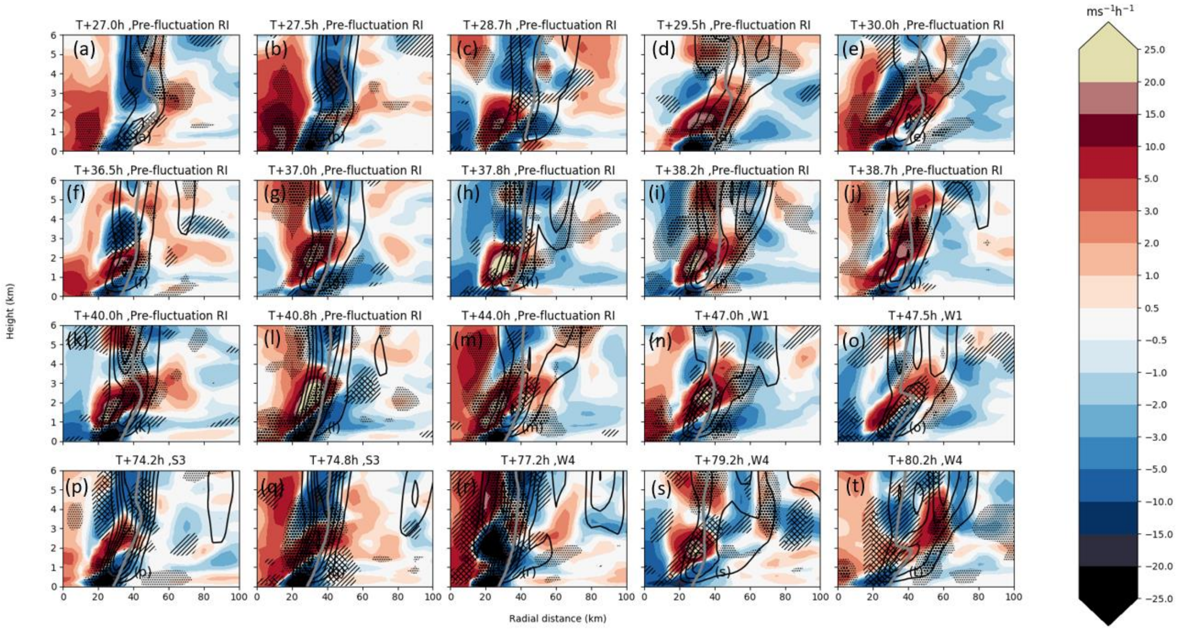

Figure. R7: Coloured contours show the eddy contribution to the tangential wind in ms^{-1}. Line contours show the azimuthally averaged vertical velocity in 0.2 ms^{-1} intervals. The tendency in tangential wind is also shown as small dots showing +2 ms^{-1} h^{-1} large dots showing +4 ms^{-1}h^{-1}line hatches showing -2 ms^{-1} h^{-1} and cross hatches showing -4 ms^{-1}h^{-1} Each row represents a period where strong VHTs occurred with row a-j showing examples of where strong VHT activity did not subsequently lead to a weakening phase. Panels k-t show VHT activity prior to and during the start of W1 and W4 respectively. Columns show selected times with (a,f,k,p) representing the start of the period with strong VHT activity, (c,h,m,r) when the strongest activity occurred and (e,j,o,t) after the VHT activity. Other panels represent intermediate periods.

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

Initially the strongest convection associated with the inner rainbands is confined, largely, to the intersection between the rainbands and the eyewall. During the weakening phase this convection spreads out azimuthally covering an increasingly greater proportion of the eyewall until the localized structures have completely lost their individuality and the eyewall is essentially homogenous azimuthally. We have rephrased and clarified this sentence.

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

Changed.

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

This is a good point. We reference the schematic later in the revised manuscript and introduce it more to make it easier for the reader to follow.

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

Changed.

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?

The other terms in the PV budget were calculated, and there was no change in the dominance of any particular budget term (cloud rebalancing was always the dominant physics term). During the weakening phase there was less PV generation within the eyewall but there was no indication of any increase in PV generation outside of the eyewall associated with convection and no secondary PV rings as in the case of an eyewall replacement cycle. We have added an explanation.

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?

This sentence was unclear and we have reworded it to avoid confusion. The reduction in PGF is accompanied by a weakening of the tangential wind above the boundary layer such that gradient wind balance is maintained. In terms of the initial cause of the reduction in the tangential wind above the boundary layer, this is likely related to the inner rainband convection which has two effects; firstly by directly depriving the eyewall of moisture diabatic heating in the eyewall may be weakened (leading to reduced convection, a less well ventilated eyewall, an enhanced boundary layer outflow and spin down), but also by creating a region of convergence near this inner rainband from the induced balanced circulation which promotes convection near the rainband rather that at the eyewall. We have clarified this explanation in lines 618 – 624 within the discussion section.

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.

The diabatic heating is necessary to maintain the barotropically unstable ring structure. Without the diabatic heating, the storm reverts to the more barotropically stable monopolar state and PV is mixed into the eye. We have made this clearer in the text.

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

Changed.

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

Initially at the start of any strengthening phase inner rainband activity is weaker, and there is no or weak VHT-like activity. Throughout the strengthening phase these structures become more common and stronger and contribute to the less azimuthally symmetric structure. Initially though there has been no time for them to form, and with a greater amount of time passed the probability of a particularly strong VHT-like structure having developed increases. We have added a clarifying sentence.

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

The eyewall convection becomes stronger throughout the strengthening phases, and the water vapour mixing ratio increases. Although convection is random and stochastic, as time passes and the region of the eyewall moistens any burst of convection is likely to be stronger, and hence also has a greater probability of being disruptive to the eyewall. We have added a discussion of this point to the manuscript.

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.

We have changed the language so it sounds less speculative and added some additional clarification.

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?

This is a good point. We have referenced the study by Kosin and Eastin (2001) now and have expanded the discussion of the results to compare with their results. In particular, we think their regimes 1 and 2 are similar to our strengthening phases and weakening phases, respectively. Fig. R8 shows the azimuthally-averaged equivalent potential temperature, is increasing in the eye, consistent with their hypothesis of eyewall to eye mixing and Figure 8 in the revised manuscript. which shows the gain in PV within the eye during the W1 weakening phase is due to the large scale transport of PV. Trajectories also show that during the course of W1 a weak inward and upward airflow develops within the eye as PV is transported towards the centre. We have not included Fig.R8 in the revised manuscript as it does not add further insight to the figures already presented.

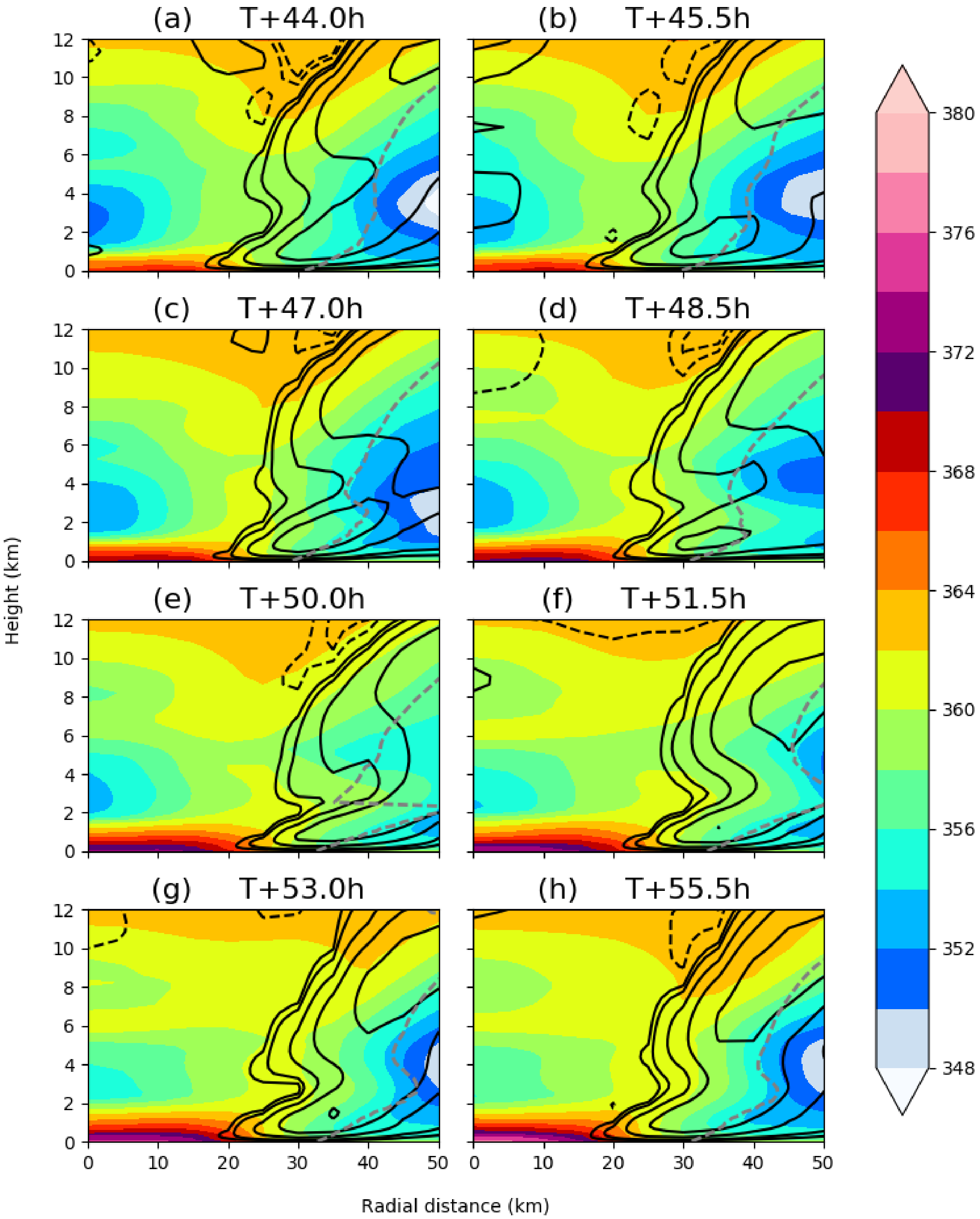

Figure. R8: Azimuthally averaged equivalent potential temperature (K, shaded) and vertical velocity (0.1,0.2, 0.5, 2, 5, 10 ms^{-1} positive (solid) and negative (dashed) black contours) for various times prior, during, and after the W1 weakening phase.