This paper examined SEF events simulated in a model ensemble and compared it to intensity fluctuation during RI. While the overall quality of the writing is commendable, there are some notable concerns related to the choice of comparison, missing components, and the comprehensiveness of the SEF theories under examination. Given the amount of effort needed to resolve these issues, I recommend rejection until the authors solve these fundamental issues.

Major issues:

1. **Motivation for comparing SEF with intensity fluctuation during RI**

The first major issue of this study is the authors' choice and lack of clear motivation to compare the SEF which occurs *after RI*, with the intensity fluctuation that occurs *during the RI period* (i.e., section 3.3.1 to 3.3.4).

These two types of events occur at different stages of the TC lifecycle (*during* versus *after* RI). The simulated SEF events occur near T+77h, with T here denoting the model initialization time at 05 Sept 1200UTC, which implies that the SEF roughly occurs near 08 Sept 1700UTC. On the other hand, the first episodes of simulated intensity fluctuation occur near T+44, with T here representing the model initialization at 03 Sept 0000UTC, which implies that the intensity fluctuation occurs near 04 Sept 200UTC. This means that these two events have a time difference of almost 3.5-4 days (about 93 hours). Although the authors did not show intensity time series in this manuscript, from their recently published Torgerson et al. 2023 and it is shown that the corresponding intensities of these two types of events are significantly different (i.e., 950 hPa for intensity fluctuation and near 910 hPa for SEF). This important information is *not* mentioned anywhere in this manuscript.

Because of the large difference in time and intensity when the two types of events occur, many aspects of the storm are drastically different, such as inner-core wind structure, inertial and symmetric stabilities, relative humidity distribution, and very importantly the environment in which the storm was embedded (see my comment 4 below). It is only until very late in the manuscript that the authors showed the θ_e cross sections in Fig. 13, which gives the readers some senses of how different the TC vortex structure is between those two events. As in Fig. 13a-d, after RI (when SEF occurs), the θ_e at the TC inner core is significantly higher, with contours that are more vertically oriented. This θ_e structure indicates a more symmetrically neutral eyewall structure and must be associated with a significantly more intense tangential wind structure and inertial stability (which is not shown!), as well as less tendency for the inner eyewall to further intensify. In contrast, during RI, the TC intensity must still be below its potential maximum intensity and the primary eyewall has more conditional/symmetric instability. As shown in Fig. 13e-h, the eyewall θ_e during the early intensity fluctuation has a clear inward bending structure and decreases vertically near z = 2 - 5 km, indicating the eyewall has a greater conditional/symmetric instability and thus greater tendency to further intensify. However, the TC intensity and basic state vortex structure are not mentioned or examined (not in the discussion of Fig. 13, nor presented in any of the earlier figures), nor considered to be relevant factors contributing to the distinct behaviors in the intensity fluctuation.

Fundamentally, given the large differences in the basic-state vortex structure and intensity, *why should we expect that the TC would undergo a similar evolution? In other words, what makes the authors think that these two types of events are comparable to one another?* To be honest, the entire section 3.3 reads like it is comparing apples with oranges and *then concludes that apples and oranges are different*. For example, near L254-268 where the authors discussed Fig. 9, they compared the low-level PV evolutions between SEF and intensity fluctuation. The main findings (summarized below) are that

- The increase in azimuthal symmetry in the intensity fluctuation was attributed to the weakening of wave–2 inner rainband structures, but not present in the ERC.
- The changes in azimuthal PV symmetry during the ERC are smaller than the intensity fluctuations.
- The maximum standard deviation of PV does not change much during ERC, whereas for the intensity fluctuation, there is a rapid decrease in the standard deviation of PV during a weakening phase and an increase in the standard deviation of PV during a strengthening phase.

But what are the respective implications of these three findings? Given that in the SEF case, we know that there is an outer eyewall forming, which then undergoes ERC, whereas in the intensity fluctuation, there is no outer eyewall formation, all we can conclude here is that they evolve differently because they are fundamentally different processes that occur in different background vortex state. In fact, the authors also did not provide any further discussion about the implications of these differences.

Similar issues go with the subsequent sections. For instance, in L280-283 describing intensity fluctuation,

"*However, unlike in the case of the eyewall replacement cycle, there is no updraught (associated with outer rainbands) outside of the eyewall and above the boundary layer that moves radially inwards over time before merging with the boundary layer updraught associated with the newly forming secondary eyewall.*"

This is essentially saying that the intensity fluctuation is different from SEF because there is no mechanism to form a new eyewall. But, *do we need to compare SEF with intensity fluctuation in order to learn about the role of the developing rainband in the SEF process?* The answer is no because the importance of outer rainband development has been identified in many other studies (see my major comment 3), all of which did not compare SEF with intensity evolution to reach this conclusion. *So, what are we expecting to learn by comparing SEF with intensity fluctuation?*

Most of the discussion in sections 3.3.1 to 3.3.4 simply highlights the *intrinsic differences* between the SEF and intensity fluctuation, all of which are not surprising given they occur in substantially different intensities and vortex states and are undergoing distinct evolutions. The fundamental issues that need to be addressed here are *what are the motivations to do these comparisons*? *What are the justifications to convince the readers that these two types of events are comparable to one another, so that it is possible to identify a nontrivial cause leading to the observed differences?* And finally, *what meaningful conclusions can we draw based on the observed differences?*

2. Confusing time referencing

As I mentioned in major comment 1, it takes the reader quite some effort to compare different figures in the manuscript to realize the exact time difference between these events. Many of the Hovmoller diagrams start with some positive hours, such as 70 h in Fig. 2, 3, and 60 in Fig. 6. I presume for SEF-focused figures, these times are counted from the *initialization time at 05 Sept 1200UTC*. On the other hand, for intensity fluctuation-focused figures, such as Fig. 8b, 9e-h, 10a-h, and 11a-h, the times T+xh are counted from *the initialization time at 03 Sept 0000UTC*. This time labeling scheme is misleading, because, for example, it gives the impression that these two events are about 33 hours apart (comparing Figs 9a-d and 9e-h). However, because of the initialization time difference, there is an additional 60 hours of time difference (i.e. time difference between 03 Sept 0000UTC and 05 Sept 1200UTC), making the total time difference of 93 hours time difference, which is almost 4 days. This approach of time referencing hides the large time difference between the two events, which is inappropriate. Therefore, the authors need to come up with a new time referencing approach that accurately describes the actual time difference between the two types of events. One possible approach is to use different labels in all figures that currently use the label T, such as T_{0309_00Z} and T_{0509_12Z} to represent the two initialization times.

3. SEF mechanism related to outer rainband dynamics

This study only examined two VRW-related hypotheses (i.e., VRW stagnation radius and filamentation) and boundary layer unbalanced processes. However, there is a large body of studies emphasizing the rainband-driven SEF mechanism. Here is listed just a small portion of them: Qiu and Tan 2013; Li et al. 2014; Zhu and Zhu 2014; Zhu et al. 2015; Tang et al. 2017; Chen 2018; Chen et al. 2018; Yu et al 2021a,b, 2022. The only study that the authors cited in the introduction is Didlake et al. 2018. But rainband-related SEF hypothesis was proposed as early as Didlake and Houze 2013 and has since been further developed by many subsequent modeling and observational studies.

For instance, Qiu and Tan (2013) examined the connection between unbalanced boundary layer response to asymmetric inflow induced by the outer rainband. This asymmetric inflow is also similarly identified in observation (Didlake and Houze 2013; Didlake et al. 2018) and many modeling studies (Dai et al. 2017; Zhang et al. 2017; Chen 2018; Chen et al. 2018; Yu et al. 2021a,b and 2022), and is referred to as “mesoscale descending inflow” (Didlake and Houze 2013; Didlake et al. 2018; Yu et al. 2021a and 2022). Yu et al. 2021a and 2022 demonstrated the important role of a mesoscale descending inflow within the stratiform precipitation region in initiating the broad-scale wind field acceleration and boundary layer cold pool dynamics in sustaining convective updraft at the inner edge of the descending inflow. From observation, it has even been demonstrated that 79% of observed SEF events have a stationary rainband complex within 6 hours of the SEF development (Vaughan et al. 2020). Given all these important rainband-focused SEF research and the apparent importance of rainband processes in the present case, I am surprised that the author neglected this large body of literature and only focused on examining the few hypotheses that do not emphasize rainband dynamics.

Here, I want to clarify that I do agree that the unbalanced boundary layer process is an important part of the SEF mechanism. However, it should be noted that the unbalanced boundary layer process argument does not emphasize the importance of the rainband process *as a precursor of SEF*. Given that in the present case, the rainband development precedes the onset of SEF (or as the precursor of SEF), it is unreasonable not to examine whether the simulated SEF event shares similarities with the hypotheses and findings from the other rainband-related SEF studies. Therefore, the entire section 3.2 seems to be incomplete to me.

4. Environmental wind shear is the largest organizing factor of the TC rainband development

Given the importance of rainband development in the simulated SEF case, it is important to show the environmental controlling factors related to rainband development. Specifically, it is well known that the largest organizing factor of rainbands is environmental shear. However, this study mentions nothing at all about the role of environmental shear. How does the environmental shear vary over time, particularly during the two periods of intensity fluctuation events and SEF? Are there any differences in shear magnitude and direction in shear during these two types of events? Based on a quick search of relevant publications, Fischer et al. 2020 showed that the 850-200 shear increases to about 5 ms^{-1} between 07-08 Sept during the SEF event, while near 04-05 Sept the VWS was only about 2-3 ms^{-1} (see their Fig. 3a). This increase in VWS is likely an important environmental controlling factor that contributes to the development of outer rainband in the SEF event (both actual or simulated). This also explains why the rainband during RI is much less developed due to weak shear in the environment. Again, this important piece is entirely missing in this study.

5. Emergence mechanism of supergradient wind

While I agree that the unbalanced boundary layer process is an important part of the SEF mechanism, one major issue of the argument is that the boundary layer spin-up mechanism does not explain what exactly drives the emergence of supergradient wind. As stated in L388, the authors do not seem to delve into the detailed analysis to determine what causes the enhanced inflow, but merely point to the positive feedback between rainband and boundary layer processes, i.e., expanded tangential wind field, enhanced boundary layer inflow and the subsequent emergence of supergradient wind and updraft? I would say this feedback mechanism is not new. If the authors are reluctant to determine the cause of enhanced boundary layer inflow, then what is exactly new here?

6. Other causes of intensity fluctuation

For pre-intensifying TC in shear, intensity fluctuation is often caused by the inward intrusion of low-θ_e air into TC's inner core, a process known as ventilation. This ventilation is not the same as the mass ventilation out of the boundary layer (diagnosed in section 3.3.3) that relates to the deepening of surface pressure. Low-θ_e ventilation is a well-known factor that can cause the weakening of TC (Tang and Emanuel, 2010, 2012a,b) or lead to delay of intensification, specifically for TCs in shear that are before reaching its mature state. Specifically, it is known that there exist several pathways of ventilation, namely the mid-level radial ventilation and the low-level downdraft ventilation. Alland et al. 2020a,b showed a nice summary of these pathways. A recently published study (Yu et al. 2023) also show how low-θ_e ventilation into TC inner core can cause substantial weakening of TC intensity. These possible mechanisms of intensity weakening are not examined or mentioned in the manuscript.

<u>Overall suggestions to resolve these major issues and improve the manuscript:</u>

Based on the major issue discussed above, I strongly suggest the authors rethink the necessity of comparing SEF events with intensity fluctuation that occurs in a much weaker intensity state. At this point, this manuscript is structured in a way that the comparison is not necessary. If this paper aims to focus on examining the SEF process, I strongly suggest the author look into the large body of rainband-related SEF studies and delve into the rainband structure to examine the linkage and feedback between TC rainband and unbalanced boundary layer processes. If this paper aims to focus on intensity fluctuation, then I also agree with Dr. John Methven's comments about the need to distinguish the current paper from the authors' recently published 2023 paper.

Minor comments:

Title: The title is incorrect. The eyewall replacement cycle does not happen *during* a period of rapid intensification period, but *after* the rapid intensification.

L11: Nascent TC is susceptible to environmental shear, which could cause a substantial delay in intensification. During this delay, intensity can remain nearly steady as the TC vortex undergoes precession. So the statement that the intensity of newly formed TC typically increases over time is incorrect.

L38: outflow ~~jet~~. Jet usually is used to describe localized enhanced wind fields. For TC vortex, the strongest wind is mostly likely along the tangential direction, e.g., supergradient jet.

L41-46: This paragraph aims to provide the motivation, but is weak. Only stating that this study aims to compare these two events is not sufficient. As explained by my major comment 1, the background state TC vortex is drastically different, making a direct comparison of these two events nearly impossible to extract meaningful conclusions, other than saying that they are intrinsically different processes that evolve entirely differently. This paper needs to substantially strengthen the motivations by providing stronger reasoning, such as explaining why the authors think that these two processes are comparable. Why is it necessary to compare these two processes?

Introduction: Overall, the discussion about rainband dynamics in the introduction is not enough. There is only one sentence in L25-27 citing Didlake et al 2018. Given that rainband dynamics plays a clear role in the simulated SEF event, the authors need to provide a brief survey about rainband dynamics in a sheared environment to allow readers background information about the role of rainbands in the SEF process, a summary of what previous rainband-related SEF studies had learned (see major comment 3), and what still remains unclear.

L60: that's brightness is inversely correlated to → which has brightness inversely correlated to

L99: The SEF event happened earlier in reality → The *observed* SEF happened earlier in reality.

Section 3.1: If this study aims to compare the SEF with intensity fluctuation, this section should not only focus on the SEF event but also provide basic information about the vortex structure during intensity fluctuation. Specifically, a comprehensive comparison of the vortex structure (e.g., tangential wind, moisture) when these two events happen is also necessary to show readers the basic vortex structures when the two events happen and how comparable they are. Also, discussions about the difference in intensity (showing intensity time series) and environmental wind shear (also time series) are necessary.

Section 3.2: This list of SEF mechanisms is not comprehensive. SEF-related mechanisms also need to be examined. See major comment 3.

L259-260: A theory of barotropic instability across the moat in the ERC process (Lai et al. 2019; Lai et al 2021a,b) has been proposed in recent years that explain the elliptic shape of the PV structure during ERC, as well as the rapid decay of the inner eyewall, which are clearly relevant to this study.

Figures: Many of the plan-view figures do not have spatial markers in the x and y axes. Even though the authors added circles representing 25 and 50 km radii, but showing markers in the axes can let the readers see the exact size more clearly. Also, the black circles overlap with the black contours, making them difficult to see (e.g, Fig. 9). Also, in Fig. 1, there is no circle nor markers in the axes, which are absolutely needed. How can we tell if the simulated TC size is realistic compare to the observation?

References mentioned:

Alland, J. J., B. H. Tang, K. L. Corbosiero, and G. H. Bryan, 2021a: Combined effects of midlevel dry air and vertical wind shear on tropical cyclone development. Part I: Downdraft ventilation. J. Atmos. Sci., 78, 763–782, https://doi.org/10.1175/JAS-D-20-0054.1.

Alland, J. J., B. H. Tang, K. L. Corbosiero, and G. H. Bryan, 2021b: Combined effects of midlevel dry air and vertical wind shear on tropical cyclone development. Part II: Radial ventilation. J. Atmos. Sci., 78, 783–796,https://doi.org/10.1175/JAS-D-20-0055.1.

Chen, G., 2018: Secondary eyewall formation and concentric eyewall replacement in association with increased low-level innercore diabatic cooling. J. Atmos. Sci., 75, 2659–2685, https://doi.org/10.1175/JAS-D-17-0207.1.

Chen, G., C. C. Wu, and Y. H. Huang, 2018: The role of near-core convective and stratiform heating/cooling in tropical cyclone structure and intensity. J. Atmos. Sci., 75, 297–326, https://doi.org/10.1175/JAS-D-17-0122.1.

Dai, Y., S. J. Majumdar, and D. S. Nolan, 2017: Secondary eyewall formation in tropical cyclones by outflow–jet interaction. J. Atmos. Sci., 74, 1941–1958, https://doi.org/10.1175/JAS-D-16-0322.1.

Didlake, A. C., and R. A. Houze Jr., 2013: Dynamics of the stratiform sector of a tropical cyclone rainband. J. Atmos. Sci., 70, 1891–1911, https://doi.org/10.1175/JAS-D-12-0245.1.

Fischer, M. S., R. F. Rogers, and P. D. Reasor, 2020: The rapid intensification and eyewall replacement cycles of Hurricane Irma (2017). Mon. Wea. Rev., 148, 981–1004, https://doi.org/10.1175/MWR-D-19-0185.1.

Li, Q., Y. Wang, and Y. Duan, 2014: Effects of diabatic heating and cooling in the rapid filamentation zone on structure and intensity of a simulated tropical cyclone. J. Atmos. Sci., 71, 3144–3163, https://doi.org/10.1175/JAS-D-13-0312.1.

Qiu, X., and Z. M. Tan, 2013: The roles of asymmetric inflow forcing induced by outer rainbands in tropical cyclone secondary eyewall formation. J. Atmos. Sci., 70, 953–974, https://doi.org/10.1175/JAS-D-12-084.1.

Tang,X., Z. Tan, J. Fang,Y.Q. Sun, and F.Zhang, 2017: Impacts of the diurnal radiation cycle on secondary eyewall formation. J. Atmos. Sci., 74, 3079–3098, https://doi.org/10.1175/JAS-D-17-0020.1.

Tang, B., and K. Emanuel, 2010: Midlevel ventilation's constraint on tropical cyclone intensity. J. Atmos. Sci., 67, 1817–1830, https://doi.org/10.1175/2010JAS3318.1.

Tang, B., and K. Emanuel, 2012a: Sensitivity of tropical cyclone intensity to ventilation in an axisymmetric model. J. Atmos. Sci., 69, 2394–2413, https://doi.org/10.1175/JAS-D-11-0232.1.

Tang, B., and K. Emanuel, 2012b: A ventilation index for tropical cyclones. Bull. Amer. Meteor. Soc., 93, 1901–1912, https://doi.org/10.1175/BAMS-D-11-00165.1.

Vaughan, A., Walsh, K. J. E., &Kepert, D. J. (2020), The stationarybanding complex and secondaryeyewall formation in tropical cyclones. Journal of Geophysical Research:Atmospheres,125, e2019JD031515.https://doi.org/10.1029/2019JD031515

Yu, C.-L., A. C., Didlake Jr., and F. Zhang, 2021a: Asymmetric rainband processes leading to secondary eyewall formation in a model simulation of Hurricane Matthew (2016). J. Atmos. Sci. 78, 29-49.

Yu, C.-L., A. C., Didlake Jr., F. Zhang, and J. D., Kepert, 2021b: Investigating axisymmetric and asymmetric signals of secondary eyewall formation using observations-based modeling of the tropical cyclone boundary layer. Journal of Geophysical Research: Atmospheres. 126. 10.1029/2020JD034027.

Yu, C.-L., A. C., Didlake Jr., and F. Zhang, 2022: Updraft Maintenance and Axisymmetrization during Secondary Eyewall Formation in a Model Simulation of Hurricane Matthew (2016). J. Atmos. Sci. 79, 1105-1125.

Zhu, Z., and P. Zhu, 2014: The role of outer rainband convection in governing the eyewall replacement cycle in numerical simulations of tropical cyclones. J. Geophys. Res. Atmos., 119, 8049–8072, https://doi.org/10.1002/2014JD021899.

Zhu, P., and Coauthors, 2015: Impact of subgrid-scale processes on eyewall replacement cycle of tropical cyclones in HWRF system. Geophys. Res. Lett., 42, 10 027–10 036, https://doi.org/ 10.1002/2015GL066436.