Dear Editor,

we would like to express our sincere thanks for your careful handling of our manuscript. We carefully considered your thoughtful recommendations to revise the manuscript. In the revised version of the manuscript, we now discuss the limitations of our setup in detail and we made an effort to clearly communicate the gap between our simplified setup and real-world tropical convection. Please find the details of our modifications to the manuscript along with our response to reviewer 2.

Sincerely

Dear Referee 2,

we thank the reviewer for the comments and suggestions. Here we repeat your comments in bold and write our response in normal font.

I was disappointed when I read the revised manuscript and the authors response because it was obvious that the authors had not considered and responded to my comments seriously. For that reason, I considered recommending REJECTION again. As I said before, there are two major problems. One is that the emphasis on the relationship between this study and MJO is quite misleading. It is permissible to speak roughly about the implication of RCE on MJO. But WISHE is a controversial part of the MJO mechanism, and we should refrain from rashly confirming or denying its importance through studies that are not directly related to. The spatial scale of the organized system is about O(100km) in this study, and only the influence of WISHE on the mesoscale systems, which are in the subscale of the MJO, can be discussed.

We would like to assure the reviewer that we took their comments seriously and are sorry if we failed to communicate that. We agree that the gap between this study and MJO was not communicated clearly in the manuscript. The potential relation between self-aggregation and the MJO (as described in the previous manuscript) was the motivation for our study, and we believe how this line of thinking led us to study a much simpler problem is important to acknowledge. However, we agree that it would be inappropriate to give the impression that our results end up being informative of our understanding of the MJO. Indeed, difficulties associated with our framework highlighted deficiencies in our understanding on a more basic level, in terms of attempting to decouple thermodynamic fluxes from those that alter the mean flow. In the revised manuscript we made an effort to avoid any ambiguity and clarified the gap between the motivation and focus of our study (line 12-14, 24-25, 34-35, 252-255).

> "In this manuscript we explore the simplest possible configuration that allows the interaction of a convective cluster with a mean flow. This is motivated by a desire to better understand processes influencing the propagation of organized deep convection in the tropics."
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> "This leads us to the more basic question of how convective self-aggregation responds to the imposition of a mean flow."
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> "This line of thinking leads us to attempt to study a much simpler problem, which is how convective-self aggregation responds to the imposition of a background mean flow."
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> "While the problem we study is probably too simple to meaningfully inform our understanding of much more complex and larger scale processes like the MJO, it does highlight how a consideration of surface thermodynamic fluxes alone has only a small influence on the propagation of the convective cluster, and how considering these fluxes in isolation of the associated fluxes of momentum, distorts our understanding of the response to the asymmetry imposed by the mean winds."

The other is that I am not convinced with the merit to analyze the transient response of the RCE system. As we see in Fig. 6b and c, the organized system moves in the x-direction of the reference coordinate, and this ruins the interesting Galilean transformation approach (to avoid the numerical difficulty in the advection term). As it is suggested by the different evolution of UB2 and UB4 in Fig. 2 (above), the "transient" responses (day0-5) of UB2 and UB4 may not be the same. Uabs of UB4 decreases nearly exponentially, but Uabs of UB2 is nearly constant during day0-5. In addition, it is clear from Fig. 2b (below) that UB4 is still in the transient phase for day15-20.

We are aware of the limitation of our simulation setup and attempt to make this clear for the reader. Nevertheless, we think our results are worth an interpretation and are willing to share what we have learned throughout the study. We have included this in the experiment setup and conclusion section (line 91-95, 260-264, 268-270).

> "In retrospect, this modification ends up being effective only to a limited extent, as the advantage of a Galilean transformation to avoid numerical errors from advection is lost when the convective cluster start to move through the grid boxes. For futures studies that aim to study the interaction of convective self-aggregation with a mean flow, mechanisms for maintaining the mean flow must be included (e.g. a nudging of a large-scale flow), which couples the thermodynamic questions we had wished to study to dynamical ones."
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> "A Galilean transformation can have the advantage of avoiding numerical artifacts of advection. The benefit of the approach, however, ends up being true only to a limited extent, as the convective system start to propagate through the model grid in our study. Nevertheless, the simulations show that the convective system maintains its thermodynamic structure until the end of the simulation period when $\mathrm{u_b} \leq 4\ \mathrm{ms^{-1}}$. For future studies, we recommend considering the momentum flux response to a large-scale motion by including a physical mechanism for maintaining a mean flow.
>
> The simplicity of our framework and the difficulties encounter in the setup of the simulations prevent direct inferences from our study for real-world propagating deep convection, let alone the MJO."
>
> "Nonetheless, the basic questions it highlights — such as the role of surface momentum fluxes in WISHE-like mechanisms — are likely to be fruitful avenues to explore when pursuing understanding of more complex phenomena."

To avoid overloading the figures, we display simulations with $\mathrm{u_b}$ of $0, 2$ and $4\ \mathrm{ms^{-1}}$ in the manuscript, although we have run several simulations with different wind speeds of $\mathrm{u_b}$ and with ensemble perturbation (figure 1 in this document). There is some variability in the estimate of $\mathrm{u_{abs}}$ but this set of simulations confirms that the decrease in $\mathrm{u_{abs}}$ in the first days is systematic with increasing $\mathrm{u_b}$. We have seen the systematic change in the θ_e flux at the surface and F_m among the simulations, including Ub1, UB3 and ensemble runs as in Fig 3 in the submitted manuscript. We see the response to the imposed wind is consistent in the transient phase when ub $\leq 4\ \mathrm{ms}^{-1}$. We additionally ran UB4 until day 30 which confirms that the simulation also reaches a quasi-equilibrium state. $\mathrm{u_{abs}}$ for UB4 fluctuates around zero in the additional simulation days (day 20-30). The estimated propagation speed for UB4 (day 15-19) is $0.29\ \mathrm{ms}^{-1}$ with higher fluctuations around zero compared to UB0 and UB2. Since $\mathrm{u_{abs}}$converges to zero from day 15, we did not extend the simulation period for the other experiments. Thus, in the manuscript we decided to present the results using the period up to day 20 but this information has been added in the revised manuscript (line 155-158):

> "Additional simulations with $\mathrm{u_b}$ of 1 and $3\ \mathrm{ms}^{-1}$show agreement in that the propagation speed decreases in the first few days and eventually the propagation speed converges to zero (not shown). Additional simulation days for UB4 (until day 30) corroborate that UB4 reaches a quasi-equilibrium state (not shown)."

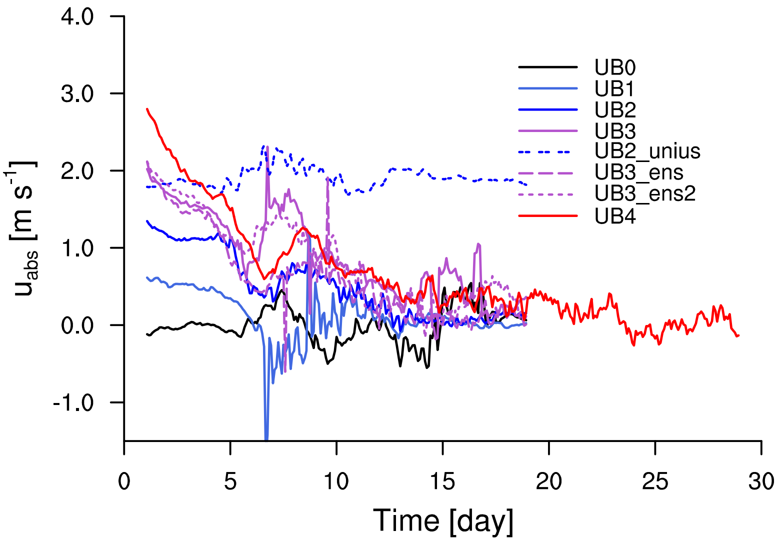

Figure 1 Temporal evolution of u_{abs} in the x-direction as in Fig 2 in the revised manuscript. UB1 and UB3 represent the simulations with an imposed wind flow of 1 and $3\ ms^{-1}$, respectively. UB3_ens and UB3_ens2 are the ensemble runs for UB3 where we slightly changed the maximum time step to 14 and $13s$, respectively. (The maximum time step is set to $15s$ for all simulations in the revised manuscript.)

So, I cannot recommend publication of this manuscript in its current form, but obviously, that is just my opinion. I think that addressing the second point is particularly difficult because they have to redo all of the experiment and reinterpret it. So, at the very least, I would like to request that they rewrite Introduction and stop trying to force this study to be meaningful in relation to the MJO.

We have revised the introduction to clearly communicate the gap between our study and the MJO (please see our answer to the first comment). The resemblance of the MJO and self-aggregation found in the previous studies, however, was the sincere motivation of our study and acknowledging this seems fair (as discussed earlier). The simplified framework and the difficulties encounter in the setup of the simulations make the results less relevant to the MJO than we had originally hoped. To account for this, we now critically discuss the limitations of the setup in Section 2.1 and the Conclusion (please see our answer to the second comment). Also, we have adapted the abstract to clarify the objective of the study in the beginning (line 3-4):

> "We impose a background mean wind flow on convection-permitting simulations through the surface flux calculation in an effort to understand how the asymmetry imposed by a mean wind influences the propagation of convection."