Dear Referee 2,

we thank the reviewer for the valuable comments and suggestions. We have carefully taken into account the comments and have reflected most of the suggestions in the revised manuscript. Here we repeat your comments in bold and write our responses in normal font.

General Comments: This work uses the framework of a radiative convective equilibrium (RCE) experiment to investigate the effect of the wind-induced surface heat exchange (WISHE) mechanism on the moving speed of the active region of convection. By this unique approach, the authors found that the effect of WISHE to slow down the movement is very weak. Specifically, the quantification that the deceleration effect of WISHE is about 5% is valuable. The idea of the Galilean transformation to avoid difficulty in the discretization of the nonlinear term is interesting. Unfortunately, however, I cannot help recommending REJECTION of this work due to the inappropriate experimental configuration at this time. I am wondering if the authors really intended to include the momentum exchange between the surface and the atmosphere. As shown in this work, the surface drag acts to force the atmosphere to move with the ground, and the distinction between the transient response and the quasi-steady response becomes unclear. In my understanding, there is no positive reason to analyze the transient response. It seems that the authors already noticed this problem, and thus, they had performed the UB2_unius experiment. Would it be so difficult to include influence of the mean wind only in the surface enthalpy equation, but not in the surface momentum equation? One way may be to use two independent u_h values: one is the same as eq. (2) and the other is without u_b. If the coding is complicating and/or the computational cost is huge, analyzing the transition stage of the system may be acceptable. I think, however, that this is not the case. It would be desirable to revise the experimental settings and redo the whole experiments. After that improvements, the authors' arguments will be clearer, and it will make an essential contribution to the area of the RCE research.

We agree with the reviewer in that it is interesting to perform an experiment that includes the influence of the mean wind only in the surface enthalpy equation, but not in the surface momentum equation. The reviewer is quite correct in his/her suggestion to decouple the momentum fluxes, but (and for the reasons the reviewer stated) we had already done this (UB_unius in the submitted manuscript). However, and in retrospect, this contribution was not presented in a particularly transparent manner. In the revised manuscript (line 65-70), we have substantially rewritten the manuscript to bring this issue more to the forefront.

> "In the long run with a mean flow the surface transports its signal through the atmosphere, until the whole column is in balance again and stagnant compared to the surface. (Note that this equilibrium response is different from the equilibrium response of a nudging approach, where a background flow is maintained. For the transient response we expect a similar behavior of both approaches.) For the mechanism denial experiment a mean flow over the surface is maintained by including the influence of the mean wind only in the surface enthalpy equation but not in the surface momentum equation."

We do think that in addition to the UB2_unius experiment it is not unreasonable to also learn from the transient response of UB2 and UB4, which include the influence of the momentum equation. The time scale of convective adjustment is in the order of hours, and in the simulations the convective cluster continues to propagate for several days during the transient phase. This time scale is a lot larger than that for the convective adjustment. We have included this in line 156-158 in the revised manuscript:

> "The temporal evolution of the propagation speed demonstrates that the spin-down of the propagation speed occurs over a week whose time scale is longer than the convective adjustment time scale, which is in the order of hours, and the convective cluster settles around two weeks after it begins to propagate."

Furthermore, we think it is informative to analyze the transient response is to better understand the role of the asymmetry of the surface momentum flux in slowing down the propagation of the convective cluster in our simulations. For these reasons, we think it is worth investigating both types of simulations in the manuscript.

Other Comments: 1. Abstract: "phenomenon found in. . ." may be replaced with "phenomenon seen in. . ."

We have replaced 'found' with 'seen' on line 1 in the revised manuscript:

> "Convective self-aggregation is an atmospheric phenomenon seen in numerical simulations in a radiative convective equilibrium framework of which configuration captures the main characteristics of the real-world convection in the deep tropics."

2. It is desirable to review more specifically about the resemblance between the self-aggregation in RCE and the MJO. Since the pure RCE lacks the vertical wind shear, the resemblance between the MJO and the RCE is questionable.

We do not intend to look at the MJO directly but see our study as a step to better understand how asymmetries in surface fluxes affect aggregation. One of the processes that might be important for the MJO is WISHE. But, if WISHE is important for the MJO, it is so in a different way than how we think about it in this study.

We have clarified this in the revised manuscript in line 32-33:

> " As a step, we focus on how asymmetries in the surface flux, in response to a mean flow, affect the propagation of a convective cluster in RCE."

3. Note the number of vertical levels in section 2.

The number of vertical levels has been added in line 54-55 in the revised manuscript:

> "The 63 vertical grid levels are stretched, starting from a grid spacing of 75 m at the first model level up to 1367 m near the model top."