Dear Referee 1,

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.

This study evaluates the impact of mean flow on the evolution of self-aggregated convection through the imposed wind in surface fluxes. Due to the enhanced surface momentum fluxes, the convection eventually becomes quasi-stationary against the mean wind. The authors further point out that WISHE effects have a relatively small role for the convection to be quasi-stationary. The paper is compact and well written and deserved to be published after addressing the following comments.

Major comments:
1. My major concern is on the experiment design. I think the authors need to emphasize the difference between imposing a mean flow in the cloud-resolving model (i.e., nudge domain averaged horizontal wind in the model) and putting the model domain on a conveyor belt by adding wind only to surface fluxes. The approach proposed in this study potentially eliminates important processes, including the build-up of near-surface wind shear, the interactions between the mean flow and the cold pools, and the importance of boundary layer processes to the aggregation.

We think that a nudging approach that the reviewer suggests would show a different response in the quasi-equilibrium stage in the way that the background wind is maintained, although we expect the response in the transient phase to be similar between the approach we have taken and the nudging approach.

In the initial stage our approach allows for boundary layer processes like near-surface wind shear (which can still be seen in the quasi-equilibrium stage in Fig. 5 in the submitted manuscript) and the interaction between the mean flow and cold pools. The equilibrium response of the system, when the whole atmospheric column and with it the convective cluster becomes stagnant compared to the surface, i.e. Uabs=0 m/s, is equal to an RCE without a mean flow. Even then we still see local near-surface wind shear and cold pool dynamics but without a mean flow. In response to this comment and also the other reviewer's main comment, we

a) describe the setup more clearly including its implications for boundary layer processes (line 65-68 in the revised manuscript):

    "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.)"

b) more clearly communicate the "triviality" of the equilibrium response and focus more on the physically more interesting transient response and the UB2_unius experiment (line 68-70, 156-158):

> "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."
>
> "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."

2. The discussion focus on the surface fluxes, but the convection structure change from UB0 to UB4 is not discussed. The only clue given in this manuscript is the cloud top height in Fig.5. Based on my eye measurement, the horizontal scale of the convection in UB0 is around 100 km. But the scale shrinks to 50 km in UB2 and maybe 20 km in UB4 at the end of the simulation. It raises an interesting question: Can we still call convection in UB2 and UB4 aggregated convection (i.e., convection sustained by its circulation). The change in convective structure might also explain why further increase the wind speed, the convection disaggregated.

We appreciate that the reviewer mentions this point. We have included a new figure 1 in the revised manuscript to display the horizontal structure of the convective cluster on the last day of the simulation period:

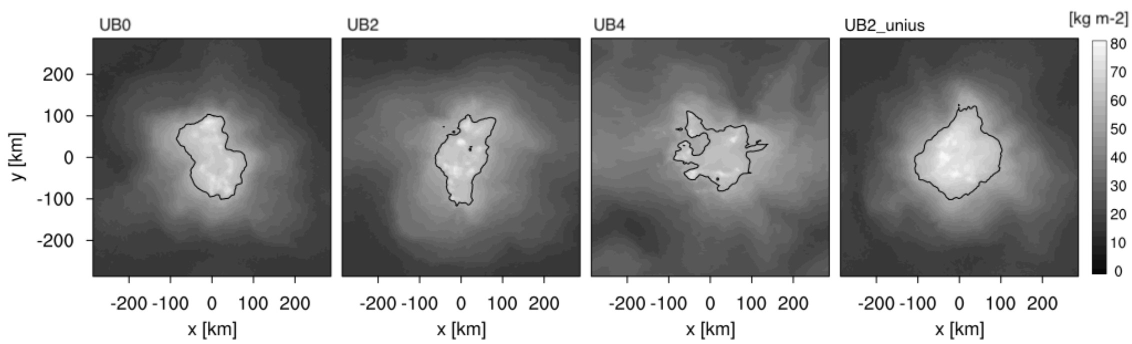

Figure 1 Daily average precipitable water on day 19. Black contours indicate where precipitable water is equal to 58 kg/m2

> "Figure 1 illustrates the daily average spatial pattern of the convective cluster on the last day in the experiments. All simulations show that the quasi-circular pattern of the convective cluster lasts until the end of the simulation period, and the horizontal scale of the cluster size is comparable among all simulations, although the spatial variability of precipitable water is weak for UB4 compared to the other experiments. The standard deviation of the daily average precipitable water on the last simulation day is 14.2, 12.1 and 10.4 kg/m2 for UB0, UB2 and UB4, respectively. This standard deviation varies in time and, e.g., is as low as 10.9 kg/m2 on day 6 in the control case UB0. The domain mean precipitable water on the last day increases with increasing Ub, having the daily mean value of 26.5, 30.4 and 34.3 kg/m2 for UB0, UB2 and UB4. The larger domain mean precipitable water with increasing Ub might be associated with our simulation setup of a propagating cluster in double periodic boundary condition which results in nine full transits through the domain in case of UB4

> (Matheou et al., 2011). Despite this artifact, the convective cluster remains organized over the simulation period in all experiments and we expect this difference to play a minor role in the following analysis."

We do not find the horizontal scale shrinks with increasing Uabs. In the Hovmöller diagram of the cloud top height one can easily be misled when estimating the horizontal scale because of the slanted pattern. In a new Fig. 1 in the revised manuscript, you can see the structure of the convective cluster for all experiments. In terms of daily average precipitable water convection is still aggregated for UB2 and UB4, although the spatial variability of precipitable water for UB4 is relatively weak compared to UB0, UB2 and UB2_unius. We performed UB4 for another 10 days, i.e., up to day 30, to confirm that self-aggregation maintains, and the convection is indeed aggregated until day 30. In contrast, a simulation with imposed mean wind of 5m/s, which is not discussed in the manuscript, begins to disaggregate convection around 5 days after imposing the mean wind.

Minor comments:

In Fig. 1 The Uabs evolution for UB0, UB2, and UB4 are quite different during t=0 to t=5 days. The UB0, which should be very close to no imposed wind, has no fluctuation within first 5 days but fluctuate strongly afterward. This is similar to UB2 but with higher Uabs; on the other hand, the Uabs in UB4 decreases linearly within the first 5 days suggest the active WISHE feedbacks proposed by the authors. Do you have any explanation for this?

When applying a broader running average (e.g., 72 hours) than what we used in Fig.2 in revised the manuscript (24 hours), UB2 and UB4 show a near-linear decrease of Uabs in the first 5 days. UB0 is the simulation with no imposed wind as a reference. The drop of Uabs for UB2 is associated with the fluctuation of the cluster shape. The cluster experiences expansion and contraction in all direction, but not necessarily uniformly in all directions at the same time. For example, when the cluster expands in y-direction, the PW-weighted center moves slightly away from the previous time step (in terms of x-direction as we present the propagation speed in x-direction in which we impose the mean flow) and when it contracts back, the PW-weighted center moves farther away from the previous time step. We have given more detail about the Uabs fluctuation (line 149-152 in the revised manuscript):

> "Since the cluster is formed by a group of individual convective cells, the shape of cluster is not firmly fixed. The cluster expands and contracts in time (though not necessarily in all directions at the same time, see the daily PW for UB2 in Fig. 1) and sometimes smaller convective cells emerge outside the main cluster (see the cloud top height for UB0 in Fig. 6)."

Fig. 2 Quantities are averaged over 5 days and 10 km. What does 10 km mean?

To get a smoother signal, we averaged quantities over 10 km in r-direction, not applying a running average. This detail has been updated in Fig. 3 and Fig. 4 in the revised manuscript.

Line 48 a grid spacing of 75m at the first model level. Add up to XXX m near the model top.

This has been included in line 53-54 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.”

Line 89 day 22 doesn’t make sense here. Do you mean 22 days after restart or 2 days after restart?
Line 91 same problem as line 89.

We have clarified this (Sect. 2.2 in the revised manuscript). To check whether or not the disabled dynamic feedback affects the aggregation, we restart a simulation with suppressed dynamic feedback from day 22 of the analyzed simulations equivalent to -4 day of the spin-up simulation without a mean flow. This is included this in line 115-117 in the revised manuscript:

> “For the remainder of the study we refer to the simulation day, where we begin to impose the background wind, as day 0 (day 26 above). For example, the time when we restart the denial experiment without mean wind (day 22) would be equivalent to day −4, and the time when the mean wind is introduced to be imposed to the denial experiment (day 26) is day 0 from now on.”

Fig 2 and others, it’s better to put figure legend to all the figures.

Figure legend has been added to Fig. 3, Fig. 4 and Fig. 5 in the revised manuscript.

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."