Response to comments of Referee #3

We thank the reviewer for the thoughtful comments on our manuscript. We have carefully considered each of the comments. The reviewer's comments are repeated in normal font and our responses are followed in blue.

The revised manuscript has mostly addressed the concerns from the previous reviewers. The main issue in the last review round was the resolution (13km) being too coarse to resolve convection. In the revised manuscript, the authors have included two higher-res experiments (5km, P5 and E5). It is nice that in the conclusion section, the authors show a schematic and summarise the importance of different processes in each configure. Before the final decision, I still have some concerns (mostly minor).

For the schematic plot, discussions about the first two panels (dependence on convective treatment at 13km and dependence on resolution with the same convective treatment) are quite clear, but the discussion about the dependence on convective treatment at 5km is bit too short and general. The authors mention that 5km is more complex than 13 km. Why? I would like to see more about this. For instance, what is consistent and what is different?

Thank you for pointing this out. We have elaborated the description of the dependence on convective treatment at 5km in the conclusions.

L702-712: "The sensitivity of rainfall to convective treatment at 5km exhibits some differences from that at 13km. As for the other cases, explicit convection produces more rainfall than parameterized convection. The difference in rainfall is associated primarily with differences in convective updraft mass flux related to BLQE, while precipitation efficiency remains largely unaffected by convective treatment. At 5km, however, the differences in surface enthalpy fluxes are not due to differences in surface wind speed, which is relatively similar in the two runs, although the Hadley circulation is stronger in E5 than in P5. Instead, a larger moisture contrast between the ocean surface and the air in E5 enhances surface enthalpy fluxes and convective mass flux. Furthermore, the effects of stronger convective mass flux on rainfall is partially offset by relatively low column averaged humidity, of which the differences in moisture in the lower troposphere contribute the most. Thus, BLQE is still key to understand the dependence on convective treatment at 5km, yet the balance is achieved by thermodynamics within and above the BL, while dynamic fields are less involved."

Another concern from the previous reviewer is that this is a diagnostic framework which does not help us understand causality. The authors have clarified in the reply that they cannot disentangle what drives what and addressing 'links' is not their main focus. This is a fair reply. However, in the manuscript, the authors should be careful when describing the relationships. Because in the end, there may be explanation for causality in both directions.

Below is an example:
-Line/section 700, last two lines, 'the vertical contrast in MSE becomes larger with increasing resolution….' I am a bit confused by this sentence. How would large MSE difference broaden ITCZ? The authors seem to argue drier free-troposphere would increase more import of low MSE air into the boundary layer, which then would increase surface fluxes. However, based on EQ 1 and figure 4, wouldn't that be the opposite? More low MSE air into the boundary layer would reduce MSE in the PBL. This would narrow ITCZ.

We admit that this is a little misleading and that we somewhat overemphasized the "broadening" of the ITCZ. As described in Sect. 3.1, the broader ITCZ, which especially extends further into the northern hemisphere, seems to be associated with an asymmetry in the initialization already. E5 and P5 are both initialized with the output from P13, which has the MJO-like disturbance for its last simulation days. This disturbance is accompanied by enhanced rainfall in the northern hemisphere, creating an asymmetry in rainfall pattern. This asymmetry in P13 is carried on for P5 somehow more clearly than for E5, in which we still see a hint of an asymmetry, e.g., slightly increased rainfall in 7-10°N. In the revised manuscript we have played down it and have focused on the reduced rainfall in the ITCZ due to horizontal resolution.

L12-13:: "Increasing horizontal resolution substantially reduces the rainfall maximum in the ITCZ, while the strength of the Hadley circulation changes only marginally."

L617: "Decreased M_u with increasing resolution is associated with the combined effects of increased h_b-h_m and suppressed F_h, which is shaped by $\overline{U_h}$ through the large-scale circulation."

L679: "These changes are more pronounced at 13km than at 5km, which shows reduced rainfall in the ITCZ."

L696-700: "Additionally, the vertical contrast in moist static energy in the ITCZ becomes larger with increasing resolution. The combination of suppressed surface enthalpy fluxes and increased vertical contrast in moist static energy are associated with reduced convective mass flux, while precipitation efficiency changes little due to increasing resolution. This underlines the importance of BLQE to understand rainfall difference due to changing resolution."

I am surprised that precipitation efficiency does not change much across these different setups, given the vast differences in the convective treatment. I wonder if the authors have some ideas about that.

We were surprised about this as well but don't have an obvious explanation for it. We planned to look into this point from a different angle by calculating condensation rate tendency from the microphysics scheme, but it would be inconsistent to compare that between parameterized and explicit convection. Our ITCZ diagnostic can only tell precipitation efficiency averaged over time and longitude. The averaging collects different precipitation efficiencies from individual precipitating clouds. Certainly, these clouds would have a wide range of values in precipitation efficiency, but the mean precipitation efficiency is quite similar between the simulations. We

can speculate that the convection scheme in the model is somehow tuned to produce an precipitation efficiency close the explicit treatment. We leave a deeper analysis for future research.

For the analyses in this study across different resolutions, were the data with different resolutions regridded to the same grid?
Yes, we remapped model grids to 0.2° lat-lon. This is described in L197.

Section 2 'Aqauchannel' —> Aquachannel
We have corrected the typo in the revised version.