Response to comments of Referee #2

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

The novel contribution of this paper is to use the diagnostic framework of Emanuel (2019) to help understand the physical reasons for the differences among a set of 4 experiments using an aquachannel model with prescribed, zonally symmetric sea surface temperature. The 4 experiments differ only in whether or not they use a parameterization of deep convection or one of two parameterizations of shallow convection. The horizontal grid spacing is 13 km, so deep convection is poorly resolved and shallow convection is not really resolved at all. Even so, understanding why the results differ, even if the results are seriously compromised relative to nature, is a step forward, so I am in favor of seeing some version of the paper published with this strong caveat.

As both reviewers pointed out the caveat regarding the model resolution, we plan to include 5-km aquachannel simulations with explicit and parameterized deep and shallow convection. The configuration of them corresponds to E13 and P13, except for horizontal resolution. The high-res simulations show that mean tropical rainfall depends on resolution (Fig. 1). We apply the ITCZ diagnostics presented in the submitted manuscript to investigate what processes are responsible for the sensitivities. There are some changes due to the resolution dependency, such that the vertical difference in moist static energy (h_b-h_m) becomes important for rainfall differences while surface enthalpy fluxes still play a crucial role. We plan to include the 5-km simulations and results in the revised manuscript by describing sensitivities of mean tropical rainfall to convective treatment and model resolution.
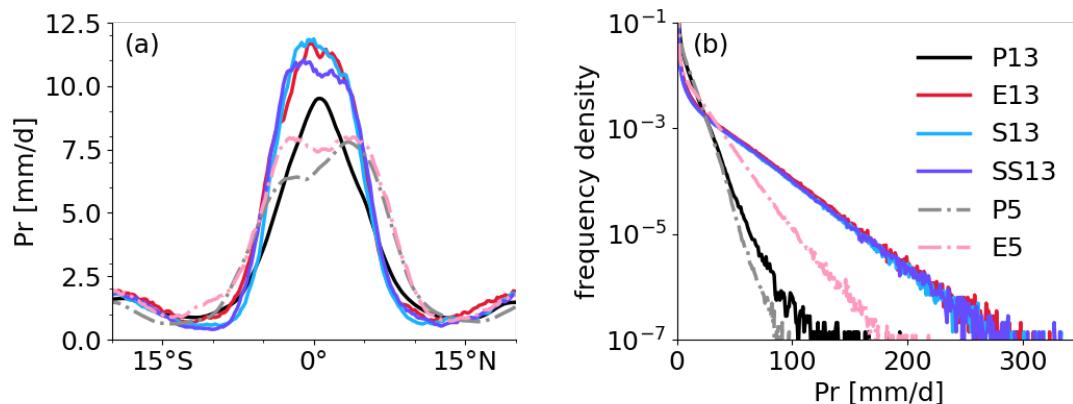

Figure 1. Distributions of (a) time and zonal mean of precipitation rate and (b) precipitation intensity between 20°N/S. Precipitation fields are coarsened on a 0.2° lat-lon grid using a conservative remapping. P5 is the 5-km aquachannel simulation with parameterized deep and shallow convection and E5 is the one with explicit deep and shallow convection.

The Emanuel framework consists of diagnostic equations for cumulus updraft mass flux, large-scale vertical motion, and a single predictive equation for the mass-weighted vertically integral of the moist static energy. In the original paper, it was used as a tool for very basic understanding of tropical circulations. Here it is being used instead to help diagnose and understand complex simulations, albeit in a simple aquachannel framework with steady, zonally symmetric SSTs.

Of the three equations in the original framework, the current authors use only one. It would be useful if they could explain why they chose only a single diagnostic. The most important criticism I have is that it is not made clear what is being specified and what is being calculated from this framework. I gather from a mediated, anonymous exchange with the authors that the models' precipitation, surface heat fluxes, radiative cooling, dry static stability, and difference between boundary layer and lower tropospheric moist static energy are being fed into the framework, and precipitation efficiency and updraft mass flux are being diagnosed. Whatever the case, the inputs and output(s) must be clearly stated. The sentence "In our diagnostics, M_u and $epsilon_p$ are not obtained directly from vertical motion but indirectly using other consistent quantities" is far too vague. Perhaps just state that these quantities are diagnosed using (1) and (2) with inputs from the simulations.

Amongst the three equations in Emanuel (2019), we only picked the formulation of M_u because this variable is directly related to precipitation through Eq. 2 in the submitted manuscript.

We thank the reviewer for spending time sorting out which variables are diagnosed through the framework and which ones are computed from model output. We will elaborate on this, clearly explaining why we chose the formulation of M_u and what quantities are diagnosed in Sect. 4 where we describe the ITCZ diagnostics in the revised manuscript.

One clear difference among the simulations is that the parameterization of deep convection tends to weaken the Hadley circulation. The diagnostic framework does not really help us understand why. Since the output is precipitation efficiency and mass flux and everything else is fed in from the simulations themselves, one would suppose that the focus would be in the predicted quantities. To imply that the Hadley circulations in the simulations with no parameterization of deep convection are stronger because the wind-driven fluxes are stronger seems tautological. When the stronger fluxes are fed into the framework, it dutifully diagnoses a stronger convective mass flux in the ITCZ; not sure what we have learned. I think the authors are up against the age-old problem of inferring causality in a steady system. One might also point out that the specification of SST means that surface energy balance is not enforced; if a slab ocean were coupled it would not be able to sustain the large differences in turbulent heat fluxes observed among the experiments.

We would like to highlight that our focus is on mean rainfall and we mainly address "links" between processes important for rainfall. The links do not mean a unidirectional but multidirectional interaction. This means that we cannot disentangle if the increased rainfall

with explicit deep convection drives the stronger large-scale circulation, which can lead to enhanced surface fluxes, or if the stronger large-scale circulation results in the increased rainfall. What we identify is that the differences in rainfall and large-scale circulation are strongly coupled through boundary-layer quasi-equilibrium. The interesting point that the reviewer raised regarding the slab ocean model makes us wonder if the link between rainfall and large-scale circulation would get weaker, if we included atmosphere-ocean effects. We presume that the link may get weaker but cannot give a definite answer. We, however, believe that in this case the application of the ITCZ diagnostics presented in the submitted manuscript would certainly help find what processes are important! In the revised manuscript, we will clarify what problem we are addressing and will discuss about the limitation of the simulation setup associated with the prescribed SSTs.

One result that is fascinating is the constancy, across experiments, of the precipitation efficiency in the ITCZ region. It would be great if the authors could address this result.

We will describe and discuss precipitation efficiency in more detail in Sect. 5.5 in the revised manuscript.

Another improvement that very much help with the understanding of the diagnostic is to plot, either as part of Figure 4 or as a separate figure, the actual terms in (1); namely, the ratio of the surface heat flux to the moist static energy difference, and the ratio of the radiative cooling to the dry static stability.

We will add an additional plot to show the latitudinal distributions of the terms, $F_h/(h_b-h_m)$ and Q/S in the revised manuscript.

One other question I have is why the authors chose the diagnostic equation for the cumulus updraft mass flux rather than the one for the large-scale vertical velocity. Is it because the latter is difficult to sample in the simulations? More difficult than sampling rainfall?

This is again because M_u is directly linked to rainfall through Eq. 2. We will clarify this in the revised manuscript.

A few specific points:

Figure 1: As the authors note, the model does not seem to have settled down into a steady state by the end of the integration. It might be worth it to extend one of the 4 simulations beyond this ending time.

That is a good idea. Unfortunately, the project this work is embedded in is now coming to an end and we need to wrap up. In addition, the same aquachannel simulations have been used for a companion project for data assimilation (DA). As understanding meteorological information in the same simulations is critical for the DA project, we focus on the 40-day period. We will keep this aspect in mind for future planning.

Line 199: "We speculate that extreme rainfalls…."

This will be changed in the revised version.

Line 210-211: It is not necessarily true that the steady state must be equatorially symmetric. There can be spontaneous symmetry breaking.

We agree with that. We will address this point in the revised version

Equation 2: I would have thought that the water vapor concentration that appears here should be evaluated at cloud base rather that taking a vertical average.

We will check if the results would change using the water vapor concentration at cloud base and discuss this in a revised version. Given the fact that profiles of moist static energy differ mostly in the lower troposphere, implying differences in humidity, we expect the results not to change much.

Section 5.1.1: I understand the breakdown between wind and delta enthalpy, but why is it important to distinguish sensible from latent fluxes here?

Our intention is to show important contributing factors to mean F_h, the sum of surface sensible and latent heat fluxes. The surface sensible and latent fluxes share surface wind speed, but thermodynamic variables (Δq and ΔT) are different. So, it is interesting to examine if the thermodynamic conditions also differ and whether it is mainly due to temperature or moisture. For example, convective downdrafts transport colder and moister air into the boundary layer, which can lead to substantial influences of thermodynamics on surface fluxes. It is not our case, but it may be if one uses a slab ocean model (Tompkins and Semie, 2021), as the reviewer mentioned. Therefore, it is worth exploring and demonstrating the different thermodynamic influences separately. In the revised manuscript, we will clarify why we distinguish surface latent and sensible heat fluxes in the context of Sect. 5.1.1.

Line 633-634: If radiative cooling is shut off, there can be no latent heating that, over the whole domain, must balance the cooling. The system would shut down.

What we mean is a change or perturbation of radiative cooling. It does not mean that there is no radiative cooling at all. We will clarify this in the revised manuscript.

Reference
Tompkins, A. M., & Semie, A. G. (2021). Impact of a mixed ocean layer and the diurnal cycle on convective aggregation. *Journal of Advances in Modeling Earth Systems*, 13, e2020MS002186. https://doi.org/10.1029/2020MS002186