Response to the review by Aiko Voigt on the manuscript cp-2020-162 "Influence of the representation of convection on the mid-Holocene West African Monsoon" by Leonore Jungandreas et al.

We thank the reviewer for his effort in carefully reading and commenting on our manuscript. In the following, we reply to his comments point by point.

Review*: Jungandreas et al. address the long-standing challenge of capturing the northward extension of the West-African monsoon during the mid-Holocene in climate models. The rainfall extension is indicated by climate proxies, but coarse resolution models with parametrized convection consistently have failed to capture it. One suspected reason is the misrepresentation of convection in such coarse models; i.e., it has been hypothesized that representing convection explicitly by going to storm-resolving resolutions might "solve" this problem. In this paper, the authors show that this is not the case, at least not in the ICON-NWP model in limited-area setup used here. Quite the contrary, they find that a low-resolution version of the model with parametrized convection exhibits a more northward precipitation extension than the fine-resolution version with explicit convection. This is an interesting and intriguing result, based on which I strongly support the publication of the paper in Climate of the Past. Another interesting finding is that the "failure" of the fine-resolution model version can be ascribed to the inability of the soils to hold the large amount of rainfall generated, leading to strong runoff, relatively drier soils, and hence less precipitation.*

The paper is well written and clearly structured - this is much appreciated. A potential shortcoming of the paper is that some of the analysis could go into more detail, and it would seem they could do so with relatively little additional work. I give a few examples below. At the same time, I feel the results as they stand are sufficiently interesting, and so these examples are suggestions that the authors might or might not want to follow.

L8ff: I find the abstract to not be completely consistent. It starts with saying that the 5km-E version has a more realistic spatial distribution and intensity of precipitation, and then argues that the 40km-P version performs consistently better. I understand the point regarding the precipitation intensity, but not the point about the spatial distribution.

Reply: With the more realistic spatial distribution in the 5km-E simulations we refer to the occurrence of more local (but strong) precipitation events. In the 40km-P simulation we notice that there is almost no grid cell that receive any precipitation. This gives a spatial precipitation pattern of widespread (at least) light precipitation. In the 5km-E simulation it occurs much more often that grid cells receive no rain. This gives a spatial pattern that is more locally confined and not so extended as in the 40km-P simulation. We assume the fact that it is not always and everywhere drizzling in the 5km-E compared to the 40km-P simulation to be more realistic. The behavior of parameterized convection schemes to produce too often too light precipitation is consistent with several other studies (see introduction). We will clarify these points in a revised version of our manuscript.

Review: *L65ff, Sect. 2.1: It would be nice to have a little more background on the simulation setup. E.g., what is the update frequency of the lateral boundary data?*

Reply: In L92 we describe that lateral boundary conditions are updated every 6 hours. These lateral boundary conditions are also obtained from variable fields from the MPI-ESM Holocene simulations (L72-73).

Review: *L65ff, Sect. 2.1: I would also be interested in seeing how the ICON-NWP runs compare to the precipitation from the global MPI-ESM model. E.g., is 40km-P also better than MPI-ESM?*

Reply: A direct comparison between the MPI-ESM Holocene simulation and the 40km-P simulation is not possible at this stage of our study. The MPI-ESM uses a dynamic land-vegetation scheme (JSBACH), and therefore, it simulates an extended greening of the Holocene Sahara. In contrast, we have prescribed present-day conditions in the Sahara for the nested regional climate simulations. We did this as a first step – like it was done in the Paleo Modeling Intercomparison Project (PMIP) Phase 1. Hence, comparing the 40km-P simulation to the global MPI-ESM model would be an unfair comparison.
In a second set of simulations, we have prescribed a green Sahara, consistently with the MPI-ESM-Simulations, and we have done additional sensitivity studies using different soil moisture configuration to see whether the results obtained in the current study are affected by the land boundary conditions. Currently we analyze these data. We have found little change so far. Hence our results of the current study are qualitatively robust. A paper is in preparation in which we will certainly take up your suggestion.

Review: *L94ff: I would like to see a bit more justification for the chosen years, especially since later only one of the years is studied in more detail. E.g., a figure would help to make the arguments more explicit.*

Reply: After the first 15 years of the spinup simulation we choose two years: 1. based on the JJASO mean precipitation rate over land points over north Africa (37°W-52°E, 0°N-40°N) and 2. based on the northward propagation of precipitation. We looked for a combination of relatively high (weak) mean JJASO precipitation rate and strong (weak) northward extension for the strong monsoon year (weak monsoon year). We will add a figure and a short explanation in the revised manuscript.

Review: *L105: How is the diurnal cycle modified? And why is the 5km-E version also affected by this change (Fig. 2b)? It then seems the change cannot be a tuning parameter of the convection scheme.*

Reply: Peter Bechtold developed a new CAPE-based closure for the convection scheme, which is described in Bechtold et al. (2014). The new closure is not only based on CAPE but also takes into account boundary layer forcing. The boundary layer forcing is included via a boundary layer time scale that acts to delay the development of deep convection. Depending on the chosen time scale, the convection can be more or less delayed. If the boundary layer time scale is set to the deep convective adjustment time scale, then the boundary layer forcing is not taken into account, which leads to a more rapid development of convection with a midday peak.

The 5km-E version can be slightly affected because of the nesting setup we use. The 40km domain is the parent domain of the simulation. It drives (via boundary and initial conditions) the 20km domain, the 20km domain drives the 10km domain and the 10km finally drives the 5km domain. Therefore, modifications in the 40km domain can yield small variations in the 5km domain. We will add this information in a revised version of our manuscript.

Review: *L125: I find the wording of "per latitude" unnecessary or confusing. The units of precip are mm/day and not mm/day/latitude.*
Reply: will be corrected

Review: *Fig. 3, caption: Domain a should probably read WAM domain.*
Reply: It should read WA-Domain what stands for “West Africa”. We will spell out the acronym. We also clarify this in Fig. 1.

Review: *L188: I assume the local drying refers to the runoff described later. Maybe this can be hinted at already here so as to help orient the reader?*

Reply: Yes, the local drying refers to the drying due to the high runoff. We will make this clearer in the revised version.

Review: *Fig. 8: In addition to the maps it would be nice if you could calculate the moisture flux into/out of the WAM domain. It's a bit hard to see from the maps.*

Reply: Yes, we will calculated the domain mean moisture flux over the WA-Domain and add the num ber in the text in the revised version.

Review: *L249: I suggest you include a sentence of the end of this section that I assume should say that there is more moisture advection in the 5km run, so this cannot explain the drier atmosphere.*

Reply: Will be done. The maps and the calculation suggest that the moisture advection into the WA-domain is stronger in the 40km-P simulation than in the 5km-E simulation. This again confirms that the precipitation in the 40km-P simulation is higher than in the 5km-E simulation. Only in the Gulf of Guinea, the moisture transport is stronger in the 5km-E simulation. This suggests that the moisture advection from the Gulf of Guinea in the 5km-E simulation is either not large enough to overcompensate the drying induced by the runoff or/and the moisture is not transported sufficiently inland.

Review: *Data statement: I would like to see a proper data statement. Can the simulations be made public? I found the analysis scripts and the runs scripts in the linked data file. That should be described in more detail.*

Reply: The MPI good scientific practice only includes primary data. Primary data includes the model code and the needed input data files to re-run the simulations. The full simulations are not included. However all data are available upon request.

All typos will be corrected in the revised version of the manuscript.