Review of "Influence of the representation of convection on the mid-Holocene West African Monsoon" by Leonore Jungandreas et al.

Reviewer: Aiko Voigt

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.

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?

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?

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.

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.

L125: I find the wording of "per latitude" unnecessary or confusing. The units of precip are mm/day and not mm/day/latitude.

Fig. 3, caption: Domain a should probably read WAM domain.

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?

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.

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.

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.

Typos:

L8: remove "," after precipitation

L9: I think the line break should be removed

L68: hasalready

L133: the the

L254: another->other

L327: The the

L342: Add "," after characteristics and after events (in line 343)