EGUsphere, referee comment RC1
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Comment on egusphere-2022-890

Anonymous Referee #1

Referee comment on "How does the explicit treatment of convection alter the precipitation–soil hydrology interaction in the mid-Holocene African humid period?" by Leonore Jungandreas et al., EGUsphere,
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Jungandreas et al. investigate the influence of resolved deep convection on African monsoon rainfall and associated land-atmosphere feedbacks during the mid-Holocene in the ICON-NWP model. They compare simulations with parameterized and with resolved deep convection for a prescribed present day vegetation cover and for a more realistic prescribed mid-Holocene vegetation cover. They find a substantial northward shift of the West African monsoon regardless of the representation of convection for the mid-Holocene vegetation cover. However, the changes are larger in the simulation with parameterized convection, which is consistent with the results of an earlier paper by the authors. They argue that these differences are related to soil moisture feedbacks, which are strongly controlled by runoff. Additional simulations with a constant soil moisture field reduce the differences in the precipitation response to the prescribed mid-Holocene vegetation cover. This is a very interesting finding, pointing out the importance of the adequate representation of land-atmosphere interactions.

The manuscript is well written and clearly structured. It provides important insights for the paleo climate and the climate modeling community. I found no major issues and think it can be published after clarifying the minor comments outlined below.

1.) Throughout the text the authors refer to the period as “mid-Holocene” but in the title it says “Holocene” which of course is not wrong, but maybe a bit inconsistent.

2.) The manuscript starts with the term “storm-resolving” simulations in the second line of the abstract, which means resolved deep convection on the kilometer scale. However, the authors refer to it simply as “explicitly resolved convection” throughout the text, before calling it “deep-convection” in the last sentence of the conclusion. The terminology could be clarified in the beginning.

3.) The authors refer to the simulations with present day vegetation cover as “Desert Sahara”. The word “Sahara” is derived from the Arabic word for desert, making the naming “Desert Deserts”. A more applicable nomenclature without changing the abbreviation “DS” could be “Dry Sahara”.

4.) In line 102 the authors note: “We select two years after this 15-year soil moisture-spinup phase and start our nesting experiments for the boreal summer monsoon season.” I would like to know why these two years were chosen and which of them was/were used in the analysis?

5.) I would be interested to know whether the precipitation in the GS and GS-cSM simulations is sufficient to sustain the prescribed vegetation cover.

6.) In Figure 10 a) there seems to be a “separation” of 40 km-P data-points at about 10 % runoff and between 2 and 4 mm/day precipitation. This “separation” is also visible in Fig. 10 b) at about 10 % runoff with a wide range of corresponding soil moisture changes. I would be interested in an explanation for these results. Are these data- points related to a specific time during the JAS season (and thereby maybe also a region)? This is particularly interesting to me because, if these data-points are neglected, the precipitation (mm/day) to runoff (%) relationship appears to be similar for the 40 km-P and 5 km-E simulations in the overlapping range of 2.5 to 4.5 mm/day, i.e. all data-points roughly follow a linear trend.

7.) In some Figures the dpi seems to be too low (e.g. Fig. 3 or 5).

Formalities and Typos:

- Figure 1 and 8: Change “sahel” and “sahara” to “Sahel” and “Sahara”.
- Figure 11 and 12: The meaning of the black dashed lines is not described in the caption.
- Line 34: Change “feed back” to “feedback”.
- Line 246: Change “becomes” to “becoming”.
- Line 282: Change “daily-change” to “daily change”.