Review of “Effect of nitrogen limitation and soil processes on mid–Holocene greening of the Sahara” by Lee et al.

In this work Lee et al. present a range of numerical modelling results aimed at investigating the effects of nitrogen limitation and soil processes on the vegetation expansion in North Africa during the mid-Holocene. This is an important question as the authors demonstrate they would have contributed significantly to the greening of the Sahara.

The authors used an advanced ESM, while the model configuration may have some flaws. Multiple global simulations of 8000 years ago were performed with different model set-up targeting different combinations of soil carbon, nitrogen, and lake area, etc. Some simulations require additional model input such as the mid-Holocene soil carbon and nitrogen in North Africa, and the authors have made plausible assumptions.

The results highlight the albedo- and evapotranspiration-precipitation feedbacks related to soil biophysical properties and soil nitrogen limitation, and they can potentially explain why current ESMs fail to realistically simulate a Green Sahara.

I think this is an interesting study and it is easy to understand its scientific message. However, I suggest some points, mostly regarding the methodologies, be addressed or be clarified before it is considered for publication, as follows.

1. I feel the MH period that is used in this study (8 ka) should be more clearly stated in the title or abstract. Since 8 ka is a less common model setup compared to 6 ka, some limited efforts would be beneficial to the modelling community to quantify the differences in climate/ecosystem response between 8 ka and 6 ka.

> Reply: We selected 8ka because previous studies reported that Sahara was greener in this period than 6ka (Jolly et al., 1998; Groner et al., 2018) as we already mentioned it in 2.2. We revised the abstract to clarify this selection of modeling period.

2. The term “Earth system model” (widely used in the text) is confusing. As I read in Sec 2.2, there is neither description on the ocean/sea-ice model component that was employed, nor how the ocean state was prescribed. The authors might have used a model configuration of only dynamic atmosphere and land surface, which can cause serious caveats. Ocean dynamics plays an important role in amplifying the orbitally induced strengthening of WAM (e.g. Braconnot et al. 1999).

> Reply: As far as we understand, Earth system model (ESM) indicates as global climate models that incorporate global carbon cycle into the model. In this sense, CESM can be called earth system model and full name of CESM is the community ESM.

Please consider that we simulated the green Sahara (GS) using one of the ESMs with the pre-industrial ocean data with the nitrogen parameterizion. We fully agree that ocean dynamics are important in correct simulation of the GS and we already cite Branconnot et al. (1999) in our manuscript. However, please consider that our focus is to clarify the effect of nitrogen limitation and soil processes on the green Sahara simulation rather than exact reconstruction of the GS. We revised our manuscript for better readability of this issue.

3. The model spatial resolution (4 deg in N-S direction) is very coarse. For a range of the whole simulations, the vegetation extent only shifts a few gridcells (e.g. only one gridcell from extreme cases of 0K to 8KCNS, no shift from 0K to 8K or from 8KCN to 8KCNS). With such a coarse resolution, is the division of three boxes (the overlap takes up half of the box) necessary? Is it feasible that the authors repeat some representative simulations (e.g. 8K and 8KCNS) with higher resolution? Also, how can the authors estimate 2 deg shift (Line 192) from 4 deg model output?

> Reply: We fully understand this concern on the resolution. We think that benefits from high resolution simulation is not much than the current coarse simulation because we clarified the underlying physical mechanism to regulate the GS by nitrogen limitations and soil physical properties. We expect to get the same results in the higher resolution simulations because the same underlying processes should be even in the high resolution simulations, which is important contribution of our study. Please also consider that our computing power is not enough to catch up much higher resolution simulations of the CESM.

4. It is quite puzzling as the authors reported that the model does not simulate increased vegetation fraction or GPP even with increased precipitation in 8K (Fig. 4). Are the vegetation fraction, GPP and dominant land cover type annual mean or JJAS mean (not clearly stated for soil water, albedo, etc.)? Could there be a mismatch? Could it be related to the coarse spatial resolution? It is also not easy to understand when the dominant land cover type is unchanged as bare soil, the vegetation fraction and GPP changes over these gridcells can be quite large (e.g. Fig. 4 8K vs. 8KCN).

>> Reply: Please make sure that vegetation fraction does not increase in 8K because nitrogen limitation does not make substantial GPP to make extensive vegetation in this region, which is discussed in our manuscript. Also consider that in the 8K simulation, there is slight increase in vegetation in the area where GPP increases as the reviewer pointed out. However, such increases are relatively small and so

dominant land cover is still bare soil. This is not mismatch with any other related variables and please consider that our contributions are to find physical mechanism to make such simulation results based on code analysis and sensitivity experiments in our manuscript. This is why we believe that this cannot be explained by the model resolution because we explain physical mechanism on such unchanged vegetation fraction despite increases in precipitation in the 8K. We revised our manuscript to incorporate the comments for better readability.

5. It can be more informative for earth system modelers if the authors can do an estimate for the relative contribution from each process to the greening of North Africa. It should not be difficult if the authors create an index to quantify the vegetation condition within a box and assume all the factors (MH boundary conditions, soil texture, nitrogen limitation) can be added up linearly.

>> Reply: We revised our manuscript to incorporate the reviewer's comment by adding relative contributions of soil nitrogen, soil type, and orbital change to the greening of the Sahara in the SS region to section 3.3.

Technical:

L32 platform -> example

>> Reply: As the reviewer suggested, we changed the word.

L33 respects -> aspects

>> Reply: As the reviewer suggested, we changed the word.

L101 iss -> is

>> Reply: We corrected it. Thank you.

Fig. 8f Please explain more on the sand fraction used here.

>> Reply: Soil type is decided as the relative percentages of sand, silt, and clay in soil and soil physical parameters are assigned based on the soil type (Hurrell et al., 2013). We revised our texts to clarify the meaning of sand fraction in the figure.