

The article presents a sensitivity analysis to soil hydraulic and thermal properties carried out with the Earth System Model (MPI-ESM). These soil properties are modified to mimic effects of biochar addition over agricultural lands. The manuscript is generally well presented and the topic could be interesting – even in the case of “negative results”, i.e., biochar addition effects on climate are negligible; however, I have a number of comments that I think needs to be resolved before considering the manuscript further.

R: thank you for the positive assessment of this study

1) The effect of biochar is considered only in terms of increment in SOC and hydraulic and thermal properties are modified accordingly using PTFs. This is a limitation already mentioned by the authors (LL 379-382). As largely the manuscript is a sensitivity analysis to soil properties, I wonder if some other modifications induced by biochar as changes in pore size distribution or saturated hydraulic conductivity could be somehow imposed from literature, rather than passing through PTFs to represent a more realistic change of soil properties unrelated to SOC. For instance, in Table 1 saturated hydraulic conductivity somehow decreases in the various model scenarios while biochar addition literature suggests an increase.

R: Thanks for the comment. We do realize there is a pore size distribution change or soil structure change due to the biochar addition, as we discussed in section 4.1. However, we are not aware of any literature that draws a numerical relationship between biochar addition and soil pore size change. What we try to do in this study is to force the biochar dosage to an unrealistic high amount to somehow induce a higher change in soil properties, but as you noticed in Table 1, the saturated hydraulic conductivity change can not be reproduced with our approach but needs a direct modification of soil structure. However, we do lack a knowledge understanding to implement such changes.

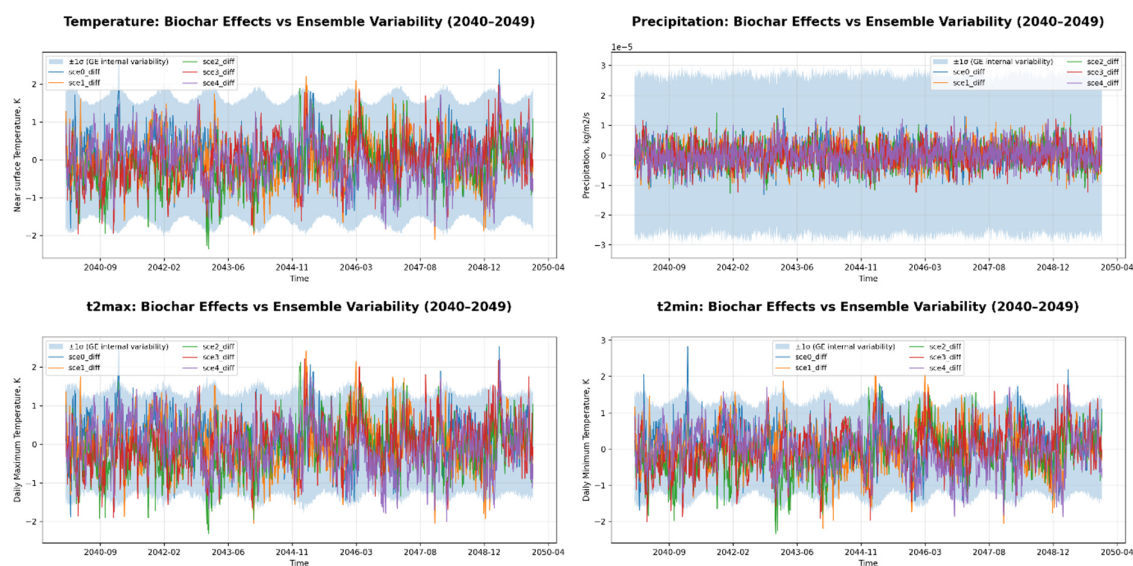
2) In the numerical experiment biochar are added to all agricultural regions with various applications rate 5 to 150 ton/ha. Current biochar production capacity, according to my estimate (please double check), would be just sufficient for 700 km² to 24 km² of land at these application rates, vastly different than the size of global agricultural land. In other words, while the manuscript is a nice numerical experiment, it is very unlikely a realistic scenario, even in a distant future. This should be at the very least discussed.

R: thanks for pointing this out, we will double check the total biochar addition of each scenario and include it in the revision. The main reason that we want to push the addition to a high end is to induce higher changes in soil properties that are probably underestimated by the selected PTFs.

3) Even more important, most of the reported differences have a relatively small magnitude. Now it depends exactly on how the experiment was implemented but, somehow by perturbing the soil properties, one perturbs the initial conditions of the ESM simulations, which means that the base simulations and various scenarios do not differ uniquely because of soil properties but also because of internal climate variability (two different stochastic trajectories of climate). For air temperature this might not be a big problem, but usually, internal climate variability can create differences (even over 40 year averages) for precipitation statistics and extremes in general that can be of the magnitude of reported changes or larger, thus I wonder how many of these changes are indeed a clear signature of soil properties rather than simply internal climate variability. Without a quantification of stochastic uncertainty, results as table 2 are not really useful. If the authors have imposed specific controls to avoid effects of internal climate variability, it needs to be explained, but I doubt this is the case. If changes in climate

induced by soil properties are smaller than internal climate variability, this is still a result worth publishing in my view, but the narrative of the manuscript should change.

R: thanks for pointing this out. We have downloaded the grand ensemble (GE) simulation of MPI-ESM LR 1.2 from Maher et al. 2019 and compared its internal variability of daily temperature (tas), precipitation (pr), maximum and minimum temperature (tasmx, tasmin) with our biochar addition effect. The global effect of biochar addition on temperature is always within the internal variability of the GE, while effect over land area is not smaller than the GE internal variability, indicating the possible local impact. However for precipitation, the biochar effect is smaller than the IA of GE. We will include this part in the revision.



4) Finally, the mechanistic explanation provided by the authors, i.e., redistribution of sensible heat and modulation of atmospheric boundary layer dynamics, increases in cloud cover and surface humidity and reduction in outgoing longwave radiation are not entirely explained. First of all, if latent heat does not change, sensible heat changes do require a change in net radiation which is not fully explained or justified. Second, if latent heat is not changing what is causing the increase in near surface humidity. The authors need to dig deeper into the mechanisms causing the reported changes.

R: We did not intend to suggest that latent heat flux does not change at all. Rather, our results indicate that latent heat flux does not increase consistently across all biochar addition scenarios, contrary to our initial expectation. In contrast, the responses of near-surface relative humidity and sensible heat flux are more coherent across scenarios. We also note that none of the examined variables exhibits a clear dose-dependent response, which likely reflects the dominance of internal model variability relative to the comparatively small magnitude of biochar-induced soil property changes. We will clarify it in the revision.

Minor comments

LL 77. Please note that 200 km is an extremely coarse resolution, and unlikely to be able to properly represent effects of soil properties on hydrological dynamics, which are likely to manifest more in models able to solve for lateral water redistribution and groundwater dynamics.

R: thanks for the explanation.

LL 85. Based on subsequent parts of the manuscript and especially caption of Fig. 3 where it is finally clear from the variable names, these should be water content at field capacity, water content at saturation, water content at wilting point. “Field saturation” by itself does not mean anything. It is also unclear why initial and maximum moisture content are treated as parameter and especially how maximum moisture content is different from “saturated water content”, which would be the correct name.

R: thanks for the comment, we will revise the name in the revision. The maximum moisture content and saturated water content are basically the same thing but in different unit, the first one is in the unit of m, and the latter one m/m.

LL 89. I would suggest mentioning already which PTF is used.

R: thanks, we will include it in the revision

LL 101. Which future emission scenario was used?

R: ssp245, thanks for reminding, we will add it in the revision

LL 113. What is the total area in km² upon which biochar application was simulated?

R: The way we apply the biochar addition, is to multiply the application rate with the agricultural fractions from the ESA CCI dataset, therefore it is not only the total area but also the agricultural fractions that matters. This is also related to the (1) major comment you addressed.

LL 127-135. Please make clear, what are the inputs/outputs that are used in each PFTs. This part is confusing. Field saturation should be “saturated water content”, and field capacity, “water content at field capacity”; the same for water content at wilting point. Which water potential values were selected to determine water content at field capacity (-33kPa?) and water content at wilting point (-1500kPa?). This should be clarified. Saxton and Rawls pedotransfer functions do not compute a residual water content. The water potential value associated with the residual water content needs to be specified somehow. Please note that wilting point will depend on the specific vegetation type – not only on the soil, it would be better to define the residual water content, taking some very low water potential (i.e., -10MPa), but of course it depends on the detail of MPI-ESM implementation. It would be better to explain this part properly.

R: thanks for explanation. We followed the modified implementation of Saxton and Rawls pedotransfer functions in the QUINCY model, and did not fully reproduce the original function. We will clarify it in the revision

LL 135-136. This part is not well explained, how exactly these ratios were used?

R: we use the ratios calculated from Saxton and Rawls to estimate the FC and WP from the CLM5 functions, as CLM5 functions don't explicitly calculate FC and WP but MPI-ESM needs FC and WP.

LL 153. Water content is a prognostic variable – not a parameter or a property that can be derived from SoilGrids.

R: In the current MPI-ESM version we are using, heat capacity is a value reading from a forcing map. Our modification is to calculate a new heat capacity map using the CLM5 functions with soil properties from SoilGrids, in the CLM5 function for soil heat capacity, we need water content in the calculation, therefore we use the Soilgrids water content value in the calculation for consistency.

LL 205. Why Ksat is decreasing? More SOC should create more and larger pores and a more hydraulically conductive soil overall.

R: this probably is caused by the value of saturated hydraulic conductivity for organic matter in Lawrence and Slater 2008, in which they assume a low value of 0.0001 m/s instead of 0.00028 m/s due to numerical stability issue in their model. We will include this in the discussion.

LL 250-260. The authors use throughout the manuscript variable names equal to the model definition of these variables, for instance, ahfs for sensible heat, I would suggest using in the figures and text the more common nomenclature and symbols, i.e., H for sensible heat, λE for latent heat, etc., it will make the reading more intuitive.

R: thanks, we will modify the figures and tables in the revision

LL 340-342 and 360-362. It is not very clear what these percentages represent.

R: The sum of all the percentages are 100%, referring to the total response to biochar addition. The percentage of each continent or region, indicates how much it contributes to the total response (of land area or a certain continent).

LL366. I am not sure SOC encompasses all the first order effects of biochar addition in changing the soil properties. I would rather argue that even if biochar addition will not modify SOC at all, soil hydraulic and thermal properties would still be considerably influenced, so not all the first order effects are captured.

R: thank you for pointing it out, it was a bad choice of wording. What we really want to argue here is the strong correlation between biochar addition and soil property changes in PTFs. We will revise it in the revision.