

SOIL Discuss., referee comment RC1  
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## **Comment on soil-2021-45**

Anonymous Referee #1

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Referee comment on "Biochar alters hydraulic conductivity and impacts nutrient leaching in two agricultural soils" by Danielle L. Gelardi et al., SOIL Discuss.,  
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The authors contribute with their study to an ongoing and substantial discussion of the effect of biochar on the hydraulic properties of soils and the potential of biochar to bind and retain nitrate and ammonium in soils. While this is an important discussion for the application of biochar in agricultural soils, the submitted manuscript is not well structured and, much more importantly, it is not clearly providing a novel approach or understanding for the ongoing scientific discussion. Furthermore, the manuscript is not transparent to follow the methodological approach. It is not clear why the column retention experiment was only performed for the HSL and the described effect of additional nitrate leaching with biochar is not supported by shown data. The fairly short discussion is by far not complete. Many aspects contradicting the here reported findings are not considered (please see specific comments). This results also in a lack of new mechanistic understanding and the link to the agricultural soils. For example, the authors are not considering the effect of the two agricultural soils on the nutrient mobility or bring their findings in context of potential field applications.

I highly recommend the authors to consider a critical discussion of their findings, developing supported mechanistic understanding from these experiments, improve the transparency of the experimental approach and improve the overall manuscript structure. Given these aspects, I decided to reject the current manuscript for publication in SOIL.

Specific comments:

Abstract and introduction:

Line 9-10: specify "saturated hydraulic conductivity ( $K_{sat}$ )"

Line 44-46: Provide reference for this statement

Line 65: What is the mechanism for the high values found in Yin et al (2018). Please provide more details

Line 96-102: Too detailed method description for an introduction. Please shorten to avoid repetition.

Material and Methods:

Line 106-108: What are the production durations of the selected chars

Line 107-108: Please provide details on the char with inoculated microbial formula

Line 113: What is the duration of the individual temperature steps?

Line: 122: The authors can avoid to mention a private company because they followed the standardized protocol

Line 123: Provide ISO number here

Line 124-128: Not clear if the authors used finally the DRIFT or FTIR. Please clarify.

Line: 137-138: Not clear to what field trials they authors are referring here. Were the soils taken from long-term field trials locations?

Line 147: What was the core volume?

Line 165: The authors should include more information about the tested models in the supplement. Which fitting parameter were considered to evaluate the goodness of fit and avoid over parameterization (e.g. AICc)

Line 176-177: Was the soil and biochar homogeneous mixed? How was this ensured?

Line 177: What would be the typical application rate on the agricultural soils used in this study?

Line 181: Why did the authors not include both soils here? Please provide a clear argument since this is substantial for the whole discussion of the manuscript.

Results: Large parts of the result section describes the biochar and soil. The author should consider to include the characterizations in the material and method section. The result section should focus on the actual findings regarding the sorption and K<sub>sat</sub> effect of the biochar on the soils.

Line 196: Please specify "carbon, hydrogen contents and leachable DOC"

Line 196-197: Please avoid interpretation of the data and comparison to the litterateur in the result section. This is part of the discussion.

Line 203-204: This aspect should be considered in the discussion and clearly mentioned in the material and methods. The oxidation state of the biochar will also influence the surface reactivity, which may, in fact, explain the here observed findings.

Line 211: include "1410 and 1418 cm<sup>-1</sup>"

Line 213-215: As mentioned above these differences in biochar production should be clearly presented in the material and method section and also critically discussed in the discussion

Line 237: Soil texture expressed as mass per mass (g/g) is a content and not a concentration. Furthermore, avoid digits for these values.

Table 3: Correct the number of digits for texture. Also, pH is commonly measured with one digit precision.

Section 3.3: Provide the data for the nitrate leaching. What is the order of magnitude of the nitrate release? This data needs to be shown.

Figure 2: Please show the fitted isotherms

Line 268-369: Please specify this statement and clearly indicate to which the p values correspond to.

Line: 285: "HSL at pore volume 14.3" corresponds this to the controls?

Discussion: This discussion is not complete and is not discussion available contradicting literature. A few suggestions can be found below. However, I recommend an extensive literature review to develop a structured and complete discussion.

Line 315: It is mentioned already that this char might be oxidized, the authors should clearly indicate this in the sections before. This initial "bias" effect needs more critical discussion here. The whole paragraph provides no mechanistic discussion. It is just comparing the findings with the literature. Please improve the discussion here and connect the different sorption capacities with the properties of the chars.

Line 318-320: Figure 2 shows actually no clear differences between SW800 and the other chars. What is the explanation? In fact, only AS800 shows the previously mentioned large binding capacities of ammonium.

Line 324-329: The whole paragraph misses to bring the findings of this study in context of studies with contradicting results which is actually in some of the already cited papers. But there is certainly more literature on this effect and higher nitrate binding capacities are reported. Only Zhang et al (2020) is cited here to support the findings of this study, which

is by far not complete. Here are a few suggestions also providing contradictory findings (and literature within):

Kameyama, K., Miyamoto, T., Iwata, Y., and Shiono, T.: Influences of feedstock and pyrolysis temperature on the nitrate adsorption of biochar, *Soil Science and Plant Nutrition*, 62, 180–184, <https://doi.org/10.1080/00380768.2015.1136553>, 2016.

Cao, H., Ning, L., Xun, M., Feng, F., Li, P., Yue, S., Song, J., Zhang, W., and Yang, H.: Biochar can increase nitrogen use efficiency of *Malus hupehensis* by modulating nitrate reduction of soil and root, *Applied Soil Ecology*, 135, 25–32, <https://doi.org/10.1016/j.apsoil.2018.11.002>, 2019.

Yang, J., Li, H., Zhang, D., Wu, M., and Pan, B.: Limited role of biochars in nitrogen fixation through nitrate adsorption, *Science of The Total Environment*, 592, 758–765, <https://doi.org/10.1016/j.scitotenv.2016.10.182>, 2017.

Aghoghovwia, M. P., Hardie, A. G., and Rozanov, A. B.: Characterisation, adsorption and desorption of ammonium and nitrate of biochar derived from different feedstocks, *Environmental Technology*, 1–14, <https://doi.org/10.1080/09593330.2020.1804466>, 2020.

Hagemann, N., Kammann, C. I., Schmidt, H.-P., Kappler, A., and Behrens, S.: Nitrate capture and slow release in biochar amended compost and soil, *PLoS ONE*, 12, e0171214, <https://doi.org/10.1371/journal.pone.0171214>, 2017.

Section 4.2: Similar to the paragraph before, this section misses a critical discussion of the findings. The authors need to include a more mechanistic explanation of the ammonium and nitrate retention in soils. Actually, the soil effect (e.g. texture and pH) is not included at all. All these observations are also based on the experiment of the HSL. This need to be critically discussed. The effect may change drastically with different soils. Please follow also here the above mentioned literature, which is only a short list of literature on this topic.

The authors also miss to bring their findings in context of the applicability under field conditions and unsaturated soil conditions.

Section 4.3: This section also misses some aspects which need to be discussed in this context. Only one application rate of biochar was used, it is not discussed if this rate is representative for these soils and its agricultural use. Furthermore, it is known that also the application rate and particle size has an effect on the  $K_{sat}$  depending in the soil

texture as discussed in the below listed literature.

Obia, A., Mulder, J., Hale, S. E., Nurida, N. L., and Cornelissen, G.: The potential of biochar in improving drainage, aeration and maize yields in heavy clay soils, PLoS ONE, 13, e0196794, <https://doi.org/10.1371/journal.pone.0196794>, 2018.

Herath, H. M. S. K., Camps-Arbestain, M., and Hedley, M.: Effect of biochar on soil physical properties in two contrasting soils: An Alfisol and an Andisol, 209–210, 188–197, <https://doi.org/10.1016/j.geoderma.2013.06.016>, 2013.

Barnes, R. T., Gallagher, M. E., Masiello, C. A., Liu, Z., and Dugan, B.: Biochar-induced changes in soil hydraulic conductivity and dissolved nutrient fluxes constrained by laboratory experiments, 9, <https://doi.org/10.1371/journal.pone.0108340>, 2014.

Line 353-354: What was the relative particle size distribution. These characteristics are not presented.

Line 354-355: How can the authors provide prove of this statement?

Line 374-376: This has not been discussed so far. But the field applications of this experiment need to be included in the critical discussion. The intention of this study was, according to the title, to consider agricultural soils. Furthermore, how can the authors draw a conclusion for flooded agricultural systems when they did not include soils from such systems?