

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2021-259

Tobias Karl David Weber (Referee)

Referee comment on "Peat macropore networks – new insights into episodic and hotspot methane emission" by Petri Kiuru et al., Biogeosciences Discuss.,
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The manuscript is a carefully detailed and well described study on pore network analyses of peat soils with a depth differentiated view. The aim is to use results of pore network analyses to help explain methane production potential in these environments. The level of language is very high, the text flow is very good, and it was a great pleasure to review. Although I am not that familiar with micro CT, image analyses and pore network simulations, the results are feasible, while not super surprising, as would be expected from the literature. Still, the manuscript is novel in its approach and comparisons.

I was left a little bewildered, what had happened to the discussion of methane (L371-383 gives some general statements on the importance and general conceptual discussion), in particular, since it was not explicitly modelled nor were the results contrasted against methane flux measurements from the field.

My recommendation is that, now with some distance, the authors re-assess the paper and whether or not the self set aims are fully fulfilled. Perhaps it might be a good idea to focus a little more on the strong technical part of the manuscript and limit the study a little more to the descriptive nature, and then discuss the methane production and diffusion from this vantage points in a more speculative manner. After all, the emerging methane emissions are mostly explained by a conceptual model.

Major concern

What does this study have to do with methane emissions? This remained unclear to me.

Either, the scope of the study should be a more quantitative

Minor concerns

L5 How can the **formation** of anaerobic pockets be conceptualized in a pore network approach? This is left unaddressed.

L42-43 Here peat specific citations should be made (e.g. Hayward and Clymo (1982), Weber et al., 2017), after all, they suggest a pore size distribution.

L53 like previous comments.

L 90 ff section 2.2: was the VGM model fitted to the averages of the replicates from each depth, respectively? Just a minor information to add.

L94 Sentence not needed, but not harmful, either.

L97 in -> at

L105 This is a rather bold statement. If samples were not saturated under CO₂ environment of vacuum, my impression is this is not actually correct. Please do not use this assumption.

L118 quantify the sample: What is meant by this? Please specify.

L122 is resolution meant, here?

L122 if this is the resolution, then the real micropores <10micron are not resolved. Thus, much of the area where anaerobes may continue to exist is not covered. Please address this limitation in the discussion. This, alongside the problem of dissecting organic from water.

L191 The resulting image

L198ff you state you exclude the effect of shrinkage (I think you should neglect it in this analyses), but I am not convinced this method does what you say it does. Perhaps elucidate a little more.

L213 relatively? to what?

Figure 3: Are the results shown for 3 samples, only, are there replicate samples included in the data? Potentially, including uncertainties in x might help explain the deviation from the 1:1 line (Table 1). Also, I expect that since the air filled porosity = 0 at saturation assumption is not warranted, the data points would be shifted to the left in x . This could be explored a little more. Also, what would the intercept in Figure 3 represent?

L256: "rather coherent": what does this mean, specifically? Contrary to this statement, in Figure 4 I see quite some systematic deviation.

Figure 4: exhibits bimodality in the retention curve (i.e. some macroporosity close to saturation) due to sharper drop around 1 cm pressure head. This is visible at the top, and not at the bottom as expected from pedogenesis, again Weber et al. 2017.

Figure 4: It appears that a normalization was done onto one air filled porosity. correct? If so, please specify.

Figure 4: why are these simulations not carried out until a pressure head of -100cm? (Same in Figure 5).

L 290: This pattern can, again, be observed in Weber et al. (2017), but also in other sources in the literature. I suggest adding citations, here.

L384-385 repetition of introduction

L393: a bit of a leap of faith or perhaps a rather general statement: the simulation provide support for the conceptual understanding to be ok. I think these hard earned results should perhaps be discussed a little more in acknowledgement to this (although I detect absolutely no oversell, here).

L425: I see the potential to discuss the obtained pore space characterizing numbers in the light of other peoples results re: methane transport, or any other gas transport in particular. If this does not exist, I suggest to scope this section a little more carefully.

L448 mark you, many of the dead end pores might not have been resolved due to the micro CT technique. Perhaps this should be contextualize, too.

L486: How can a relative quantitative measure (anisotropy) determine the efficiency (a qualitative) absolute measure of gas diffusion.

References

Hayward, P. M. and Clymo, R. S.: Profiles of Water Content and Pore Size in Sphagnum and Peat, and their Relation to Peat Bog Ecology, *Proceedings of the Royal Society B: Biological Sciences*, 215, 299–325, <https://doi.org/10.1098/rspb.1982.0044>, 1982.

Weber, T. K. D., Iden, S. C., and Durner, W.: A pore-size classification for peat bogs derived from unsaturated hydraulic properties, *Hydrol. Earth Syst. Sci.*, 21, 6185–6200, <https://doi.org/10.5194/hess-21-6185-2017>, 2017.