

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2022-112

Anonymous Referee #2

Referee comment on "Pore network modeling as a new tool for determining gas diffusivity in peat" by Petri Kiuru et al., Biogeosciences Discuss.,
<https://doi.org/10.5194/bg-2022-112-RC2>, 2022

Soil gas diffusivity and its controls are important issues in understanding greenhouse-gas dynamics of ecosystems. Knowledge gaps in this area are more obvious for organic soils than for mineral soils. Therefore the study of Kiuru et al. covers a relevant topic and principally aims and scopes of Biogeosciences. Although the study touches many several relevant aspects of the research area, nowhere a really satisfactory scientific depth is achieved. Therefore I suggest a complete revision of the manuscript **with a stronger scientific focus on aspects that are well covered by the measurements and can be amended by a sufficient theoretical base.**

Introduction: Here I miss any review about specific soil-physical features of peats in contrast to mineral soils. The state of the art, especially considering water retention but in parts also for gas diffusion would allow to formulate specific hypotheses for the application of diffusion models.

line 36: Consumption of O₂ higher than the supply is not precise, you mean that diffusion cannot maintain O₂ concentration above a critical level (which can be even zero in extreme cases).

line 52: This applies only when gas diffusion through the water is disregarded.

Material and methods: Description of standard methods can be substantially shortened (by 50%)

line 87: Reference soil group Histosol, is it order from UsSoilTax or reference soil group from WRB? Anyway, classification should be done somewhat deeper (WRB principal

qualifiers).

line 104: Shrinkage has been measured, but not reported.

line 119: I wonder about the extremely long closing times (60 and 120 minutes). With the very high D_s values this could lead to high measuring errors.

line 130: What was the exact criterion to discard measurements? How do you know that the other measurements were correct?

line 133: Why specific models have been chosen? The Millington-Kirk models have a mechanistic base with randomly disrupted capillary systems. The TPM-model is mostly empirical. Do you have ideas, which of these models are more sound to organic material?

Is there a reason why CO₂ has been measured?

line 175 following, pore network:

It is an impressive mathematical toolbox that has been used. However, I miss any critical view (this could be also in the discussion) about the validity of the completely artificial pore network model. Peats consist of fibres, clusters with a possibly strong anisotropy due to the good compressibility. Why the throat-bubble model should reflect the pore network? Are these assumptions robust or do they easily create biases in estimations? This can be checked by simulations (sensitivity analysis) of exemplary datasets.

Results part:

line 251 following: The database is weak and results do not contain surprising or interesting patterns, so strongly shorten!

Shrinkage is a very critical issue but is completely disregarded in the measurements. I would suggest to correct epsilon for the reduced total volume.

Table 1: two digits are sufficient

Table 2: "significant differences" between what?

line 295: Hysteresis has been only modeled with a non-checked pore model with swelling-shrinkage disregarded. This is no relevant scientific contribution.

Discussion:

I would expect a critical view on the scientific progress including some theoretical thoughts that support the rather weak empirical base.

line 315 to 354: Is completely trivial and can be omitted, better is to check for critical issues in measurement quality.

line 363: The problem is, that the pore network model does have a real theoretical foundation.

line 393: See above, hysteresis problem/shrinkage swelling