EGUsphere, author comment AC2
https://doi.org/10.5194/egusphere-2022-475-AC2, 2022

Reply on RC2

Simon Cazaurang et al.

Author comment on "Numerical assessment of morphological and hydraulic properties of moss, lichen and peat from a permafrost peatland" by Simon Cazaurang et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-475-AC2, 2022

Dear Madam, dear Sir,

On behalf of all the co-authors, I wish to thank you for your careful review of this manuscript. Please find below the answers to the questions raised following the review. In order to make the answers accessible to everyone, we have decided to answer each question individually.

Sincerely yours,
Simon Cazaurang

General comment:

[AR]: The submitted manuscript deals with the assessment of the porosity and hydraulic conductivity of Western Siberian Lowland ground vegetation samples (lichens, Sphagnum mosses, peat). Twelve samples are analysed throughout a numerical method instead of a classical experimental field determination (that they have also carried out). Based on digital X-CT reconstructions the study confirms the high values of porosity presented by such a biological media. Due to numerical constraints, a single numerical method could not be implemented and the authors used both Direct Numerical Simulations (DNS) and Pore Network Modeling (PNM), which did not provide the same results… but are closer together for the evaluation of the hydraulic conductivity, which is several orders of magnitude lower for the experimental field method.

The subject of the study is interesting and suitable for publication in HESS and as the authors reported in their introduction, the results could be of great importance to model the evolution of these natural environments impacted by climate change. The manuscript is globally well written and of good scientific quality. I think that a few modifications can still be made to improve its quality.

- *References are numerous, and I guess that they cover the state of the art.*
- *The purpose of the study should be more clearly stated.*
- *The station can be described briefly, the field work is important and deserves a few*

words.

- *A complete flowchart describing the different steps of the methodology will be appreciated.*
- *The quality of the figures is good, but the use of colors is not always clear and understandable. The works in the supplements are not highlighted enough.*
- *The distinction between homogeneous and heterogeneous is not clearly defined, although this aspect seems to have an impact on the typology and the possibilities of implementing the proposed methods.*
- *Some misunderstandings in the presentation of the results for porosity.*
- *The definition of the sample typology is done twice and is not consistent (?)*

[Authors]: The authors want to warmly thank the reviewer for the careful and thorough reading of this manuscript. One can find the answers to the raised comments below. Each question is answered through a dedicated paragraph for the question.

Comments:

[AR]: L 97: I am not a specialist of the topic, but what about the study by Potkay et al. (2020) ?

Potkay, A., ten Veldhuis, M. C., Fan, Y., Mattos, C. R., Ananyev, G., & Dismukes, G. C. (2020). Water and vapor transport in algal-fungal lichen: Modeling constrained by laboratory experiments, an application for Flavoparmelia caperata. Plant, cell & environment, 43(4), 945-964.

[Authors]: The authors want to thank the reviewer for this suggestion. There are some publications about lichen membrane transport properties assessment and modelling (Linefold et al., 1990, Voortmann et al. 2014 and for instance Potkay et al., 2020). However, most of these publications are dealing with transmembrane transport and not macro-scale transport. Nonetheless, we will add Potkay et al. (2020) for the sake of completeness of the state of the art. Making the link between transmembrane transport and macro-scale transport is a significant clue for understanding the whole atmosphere-geosphere transfer phenomenon.

[AR]: L105: what is the main purpose of the present study? The authors want to assess hydraulic properties of lichens and Sphagnum mosses by numerical methods, but I do not understand the "justification" of this way? What kind of problems / difficulties arise from the experimental measurements? The question is of primary goal and I think that the issue of measurements scale should also be mentioned, especially if the results are intended to be used in a modelling approach.

[Authors]: Indeed, we do not emphasize enough the main purpose of the study. The authors want to thank the review to penlight this aspect. We will add a supplementary paragraph to make the link between the state of the art and the main workflow applied for this study, insisting on the gains in terms of reproducibility and inter-comparison capability.

[AR]: L122: please indicate the coordinates of the station where samples were collected. Besides, I would appreciate synthetic details on the climatic condition of this location.

[Authors]: The authors will add some supplementary information about the scientific station where the samples were collected and more broadly on northern taiga. Some details are available through various publications such as Payandi-Rolland et al. (2020), Raudina et al. (2018), Soudzilovskaïa et al. (2013).

[AR]: L161: you refer to the drying experiment carried out by Kämäräinen et al. (2018). A different drying temperature (40 °C instead of 50 °C) has been applied for the preparation of samples that have a larger size in your case. It is not exactly the same protocol. The most important finding is that the morphological structure is preserved, and since you don't expect to know the actual water content of your various samples, this is probably not a critical point.

[Authors]: Actually, the sentence is not well-written. It would be more accurate to write "an analogous method than the one used in Kämäräinen et al. (2018) rather than saying that it is the same method. We will correct this sentence in the revised version of the manuscript.

[AR]: L190: in the main text, Fig. 4 is used before figures 2 and 3? It is quite difficult to distinguish colors in your picture. Is there a link between the colors used for your planar porosity plots and the averaged versions? I don't think so, but it's not clear.

[Authors]: We intended to highlight samples' homogeneity classes through the proposed color scheme. However, this layout was not optimal for black and white prints. A new color palette will be developed to cope with black and white prints and to enhance readability for samples' nature and homogeneity classes. A sentence will be added in the revised manuscript to emphasize on the manner the color scheme was though out.

[AR]: L202: please indicate that p_{open} refers to the open porosity proportion. In table 3, this variable should be multiplied by 100? And to be consistent with equation (2), you should indicate "%" in Table 1.

[Authors]: The authors want to thank the reviewer for the rigorous reading. We made the choice of keeping popen as a decimal number to avoid confusion with other percentages (especially porosity). We will correct equation (2) accordingly to what is shown in Table 1 and the results of Table 3.

[AR]: L258: You should refer to Supplement A. Also, I think you did not mention in the main text supplement B1 where some pictures present the evolution of porosity with the size of measurement. I'm wondering how you fix the final value of the sample's REV of porosity. It is not obvious to understand your results (final sizes) when comparing curves, for instance, for samples Hollow2.7, lichen1.3 and lichen2.1.

[Authors]: Indeed, there are missing links to Supplement A and B1. We will make the links between the Supplements and the main text clearer for the reader.
Assessing the REV maximal size is one of the main challenges in REV computations. We presented REV in part 2.4 of the manuscript. Yet, we will reiterate the criteria in this subsection as well as in Supplement B1's caption.

The final REV of porosity size is computed according to the standard deviation values for each study size reduction. Each time when the standard deviation overpasses a certain arbitrary threshold (here, we have decided to take 1, 3 or 5% of porosity variation). In the case of Hollow2.7, Lichen1.3 and Lichen2.1, we observed that these samples are more homogeneous than the others.

To enhance the classification, we added average and standard deviation values for each sample class in Table 3. This confirms that X and Y values have very low variability (under 1% for Type I samples, around 1% for Type III and less than 4% for Type II). In terms of vertical porosity, Type I have a lower variability than Type II and Type III, the latter types concentrating porosity variability.

[AR]: L265: I don't think that figure 3 is required.

[Authors]: Indeed, permeability measurements using pressure gradients (constant and variable head permeameters) are often used in the literature. However, we assume that no standardization is made for such numerical resolution. In this way, we wanted to emphasize boundary and initial conditions to clarify numerical aspects. We will move this figure to the supplemental section, as it is not mandatory for the manuscript itself.

[AR]: L280: I guess the numerical method developed by Patankar (1980) has been improved to solve faster the single-phase flow problem. I understand that the numerical aspects are not a key point of this study, but your choice induces limitations in the sample processing capacity (only type I) and that's a bit bothering.

[Authors]: The actual solver to conduct our computations is simpleFoam, built on the open-source computational fluid mechanics toolbox OpenFoam. The simpleFoam solver is far apart from Patankar's original algorithm. Nonetheless, simpleFoam still uses the same numerical solving scheme to resolve Navier-Stokes equations, therefore we thought that it was important to cite Patankar (1980). simpleFoam is nowadays highly parallelizable on many processors so that even big simulations can be conducted fairly easily. In our case, the number of simulations per sample (672) is rather the most limiting factor than the cost of the simulation itself.

However, the sentence could lead to misunderstand our methodology. We will correct this sentence in the revised version of the manuscript.

[AR]: L282: how did you select your 4 REV sizes? If I clearly understand, choosing a small REV size involves performing many simulations. However, are 4 sizes enough to detect the impact of the size on the fluctuation of your variable?

[Authors]: In this study, we assessed hydraulic conductivity REV using an analogous method of the one developed for the REV of porosity. Nonetheless, for the hydraulic conductivity REV, the sizes are assigned from the beginning to shorten computational times. Indeed, this study took 10 days per sample to be conducted. Making a continuous scanning as made for porosity is still prohibitive in terms of computational efficiency.

The sentence on line 282 can be misleading. We will reformulate this sentence in the revised version.

[AR]: L344-354: The conclusion is that DNS and PNM did not lead to compatible results for hydraulic conductivity measurements. The authors should probably indicate a way to - a priori - select the better method for each sample. Is there a link with homogeneity of the sample (the authors have indicated that in the manuscript, L276 for instance) and how can they mathematically or physically define the frontier between homogenous and heterogeneous sample?

[Authors]: Indeed, our study spotlights the fact that it is not possible to study a sample in the same way if there is homogeneity or heterogeneity. In our study, we assume that samples' variability is conditioned upon porosity spatially varying or not. This condition is a prerequisite, especially in the presence of natural samples, potentially very heterogeneous. Homogeneity criterion is based on the existence of a Representative Elementary Volume. Property homogeneity is required to find a Representative Elementary Volume. Consecutively, if a sample admits no significant variation for a given direction, then we can consider the sample homogeneous for that given axis (i.e., 1D-homogeneous). In the same way, if this low variability is observed on the three axes, then this sample is considered as homogeneous on the whole sample (i.e., 3D-homogeneous). Otherwise, convergence cannot be reached, and therefore the method using Representative Elementary Volumes will fail. To illustrate this, it is possible to take the example of samples Peat2.2 and Hollow1.2 that do not satisfy sufficient homogeneity to converge to a Representative Elementary Volume. In this case, apart from doing a computation on the entire sample (which is prohibitive in terms of computational costs), choices are reduced. For both samples, and more widely for the Type II and III samples that are 2D-homogeneous, Pore Network Modeling is appropriate in the absence of another efficient volume averaging method.

To enhance the classification, we will add average and standard deviation values for each sample class in Table 3. This will confirm that X and Y values have very low variability (under 1% for Type I samples, around 1% for Type III and less than 4% for Type II). In terms of vertical porosity, Type I have a low variability as Type III and Type II

concentrates the variability.

[AR]: L349: Fig. 9-Right does not describe the hydraulic conductivity computation as mentioned in the manuscript.

[Authors]: Indeed, this is a typo error. We will correct this sentence in the manuscript.

[AR] :

- *L359: the lowest porosity value is obtained for the Mound1.1 sample.*
- *L365: according to Table 3, the average porosity for mound mosses is 65.8 ± 23% ?*
- *L373: Lichen1.3 has a total porosity lower than 85%.*
- *L375: correct definition is "medium high porosity" or "low basal porosity" (L193) for type II ? Besides, total porosity is not comprised between 70 and 85%.*
- *L376: for type III, the total porosity is lower than 73%*

[Authors]: The authors want to thank the reviewer for the careful reading and verification of the data. We will correct promptly these remarks in the revised version of the manuscript.

For mound mosses, the average porosity is 65.8 ± 23%, including Mound2.6. As the value span is quite significant, we will rework the type definitions to be consistent with the definitions given on lines 192-194.

[AR]: L469: Did Shirokova et al. (2021) make a link between their biological results and the properties you focused on? I don't think that they talk about porosity, hydraulic conductivity, and specific surfaces. Either you add a reference that show this link, or you add this part later in your discussion…

[Authors]: The citation of Shirokova et al. (2021) is not well inserted in our discussion, even though this paper is important because our current study and their study are well linked. Indeed, transmembrane transport properties are required to make a realistic flux and matter balance through arctic vegetation cover. For the sake of clarity, this citation will be presented in the introduction and the linkage will be reinforced.

[AR]: L472: your experimental results are quite similar than other studies mention in Table 2. I would include them (not only "published results" but experimental results in general, including your own).

[Authors]: The authors want to thank the reviewer for this suggestion. We will add our

experimental results for each sample in the manuscript. We will add the results of the field work in the discussion part to give some material to discuss the differences between field and numerical experiments. The authors want to thank Georgiy Istigechev for his help in the double-ring infiltrometry campaign. We will add him in the contributions as well as in the acknowledgements.

[AR]: L477: What is the interest of subsection "4.1 Numerical reconstruction after scanning" in the discussion part? I have the feeling that the main elements are given again between L 510 and 520?

[Authors]: Section 4.1 describes the drawbacks that were identified from the use of the tomographic plots themselves. Then, between lines 510 and 520, we introduce the differences between DNS and PNM. In our point of view, we think that it is more appropriate to keep these aspects separated, as they are occurring at a different moment in the process.

[AR]: L523-526: I think the main justification and interest of the present study lies in the fact that field experiments carried out to obtain hydraulic conductivity could be inaccurate because of an excessive compression of the biological media. I would appreciate references that could highlight this aspect, also the difference between natural rainfall and field experiments and finally the lack of such kind of experimental measurements. Besides, in the perspective of numerical modelling of these biological media located in the upper part of the soil, is it possible to hide unsaturated flows (cf. L488)? I guess it's not possible to directly measure water flow, but do you know of any experiments or field measurements where high velocity is consistent with your very high hydraulic conductivity?

[Authors]: Some few studies deal with compressibility issues occurring in the field measurement (for instance, Weber et al., 2017 and Golubev & Whittington, 2018). These references will be added accordingly in the manuscript. Moreover, this study is a preliminary step of a broader study of transfer properties of this biological media. Here, we assessed fully saturated hydraulic conductivity, which is something occurring in extreme conditions. Hydraulic conductivity will be studied for variably saturated porous media and will be the subject of another study out of the scope of this study.

Minor comments:

L52 "increases" ☐ Will be corrected.

L93, L109 and in Table 2:Ref. Hamamoto et al. à Date of publication is 2016 ☐ Will be corrected.

L159: IMFT ? ☐ Toulouse Fluid Mechanics Institute, will be corrected.

L386: 1.00 mm ☐ Will be corrected.

L722: "response" a typo in the original title?! ☐ Will be corrected.

L591: problem with the words in italics "Sphagnum" ☐ Will be corrected.

L690: problem with the words in italics "Sphagnum" ☐ Will be corrected.

L770:Date of publication is 2012 instead of 2014 ? (it is correctly mention in the main text: "McCarter & Price, 2012") ☐ Will be corrected.

L840-841: problem with subscript and italics in the reference title "Extending a land-surface model with Sphagnum moss to simulate responses of a northern temperate bog to whole ecosystem warming and elevated CO2" ☐ Will be corrected.