Reviewer 2 (Daniel Siegel)

Dear Dr. Siegel, we thank you very much for taking the time and effort to review our paper. You have provided some very interesting reflections upon the greater ecosystem and hydrology within which the acrotelm and the specific sampling location is situated and its potential influence upon our conclusions. In spite of your closing comments "*Perhaps all that is needed may be for the authors of this paper to state something like that [preferential flow]. Recognize that preferential flow may be the driver at larger scales but in its absence, their conceptual model works*", we have answered each of your topical paragraphs below in a sequential manner. However, we would like to point out that our improvements on the manuscript have not lead to fundamental changes in our manuscript, because this would completely shift the focus from a detailed and intense study on the (unsaturated) soil hydraulic characteristics and property functions of the investigated peatland profile to a general study about peatland hydrology on a large scale, including preferential water flow and transport and its effect on pore water composition. This was clearly not the intention of or paper, and we hope that with the changes, we have made it clearer now.

As a general point, we would like to stress that (as also addressed in the reply to reviewer 1) we apparently used an originally ambivalent description of the peatland situation in our paper. To improve this, we have amended the first paragraphs of the Material and Methods section which now reads:

> *"The samples were collected at an ombrotrophic peat bog, the Odersprungmoor, Harz Mountains, Central Germany (UTM 32U 608000 mE 5737000 mN; 800 to 821 m a.s.l.). The Odersprungmoor formed on a saddle with an average downslope of 3 % in the SE-NW direction. In the SW-NE direction it is located in a gentle trough position (Fig. 2; Jensen, 1990). […]. The Odersprungmoor shows features of a poor-fen in some small areas where it is slightly influenced by minerotrophic water on a small strip on the North-Western flank (indicated by the arrows, Fig 1, bottom). Most of the incoming water from the shallow soils in the North-East is diverted past the bog along the northern rim of the bog towards the North-West (Border and Biester, 2015); thus our sampling location is situated in the ombrotrophic part of the bog. Broder and Biester (2015) provided information on the geochemical composition of the substrate and pore waters which supports this. "*
>
> We hope, the brevity with which we present the current understanding of this particular peatland is more than sufficient to comprehend our study. Futher, we follow your suggestion on a statement on the dominance of preferential flow on the large scale by adding the following statement to the end of the results section 3.3 now on P9 L28-29.
>
> *"Without contradiction to the presented model, we point out that preferential flow may be an important driver for saturated flow on the very large."*

P1C1: Having said this, I found the authors appear to have missed quite a bit literature on peatland hydrology. Decades of research on peatlands in northern Minnesota and in Canada show that ombrotrophic bogs are not necessarily supported by only precipitation. Clymos, Ingram's and Boelter's early work are dated related to peat soil hydraulic properties. Groundwater discharge can force water upwards into the base of domed bogs, supporting them during drought. To find out if groundwater discharge from regional flow systems provides hydrologic underpinning of bogs, hydrologists now sample pore water into the catotelmic (highly humified peat) and measure at least its pH and specific conductance. The authors might do a search on papers by Paul Glaser, Jeff Chanton, and myself (Donald Siegel). Paul's and my work showed how the hydraulic conductivity of peat may not change with increasing bulk density because of the secondary porosity issue.

P2C1: These studies and others also show that much of the water that moves through peat can occur through preferential flow paths along fibrous roots and the like, far down into the catotelm. This funneling of water makes modeling the actrotelm using the Richard's equation questionable except at the largest of scales (big areas). Doing experiments on cores won't necessarily capture the system processes. I note that the authors of this paper describe their bog forming in a valley, which would suggest that upward groundwater discharge into it could play a role—not just lateral which they show. And, if groundwater does enter at the bottom of peat profiles, it can dilate the peat pores opening them up and increasing the hydraulic conductivity (D. Ours and Siegel). I see that the authors cite others who have worked on the same bog before, and I recommend they write a few paragraphs describing what others found and from what approach.

The reviewer is certainly right with respect to the general characterization of peatland hydrology and the composition of pore waters in such systems. However, our paper is focused specifically on the characterization of the soil hydraulic properties and their model representation, i.e. models for the water retention and hydraulic conductivity of the variably saturated zone in topmost 40 cm of an ombrotrophic bog. We intentionally do not present our study as a peatland hydrology research.

We acknowledge that the flow paths in peat bogs might lead to an influence of minerotrophic groundwater, but Border and Biester (2015) do not support this for our site on a hydrogeochemical analyses of pore water from the top part of the saddle (our sampling location). To address this, we have rewritten the description of the site location.

Considering the relationship between saturated conductivity and bulk density, we agree. For this reason, and also by addressing a respective comment of reviewer 1, we have deleted the sub clause "*as is often done for saturated conductivity*" in the second but last sentence of the conclusions. Moreover, we acknowledge this fact in the rationale of our study, stating that "*the saturated conductivity is not necessarily a good predictor for the unsaturated hydraulic properties*." (now P2 LL24-25). We furthermore now clearly state that this might be an effect macro-pores can have on the saturated conductivity, by adding the following sub clause to the sentence: *", since the contribution of macro-porosity on preferential flow can be substantial in the saturated case*". (now P2 LL25-26).

P3: I don't have the time to review all the other papers. It could very well be, that this bog in question formed from lake pedogenesis. In this case, the preferential flow issue may not be as important as in the vast mires I and others have studied. The deep peat in paludafied lakes largely comes from decomposing algal remains to my understanding, superceded on top by mosses eventually. So the bog may look like a bog in say, Siberia, but hydraulically it might behave very differently. Paludified catotelmic peat really gets dense and humified compared to peat in other settings and may behave more in line with the Dickey Clymo model. I think the authors need to address this question head on. What kind of bog it is and how did it form?

The focus of our work is different. We do not want to give a generalized characterization for a certain type of peatland, but want to show that under the site of our investigation, we can characterize the depth profile of soil hydraulic properties with a smooth and gradual transition from the three-modal porosity properties of the living moss to the bimodal porosity of decomposed peat. Certainly, it would be desirable to assess the generality of this finding with respect to different types of peatland, but this would require investigations at different sites with an associated enormous amount of work. Our paper reports results from a very intense and detailed study at one specific site. How general this finding is, and to what extent it can be applied to similar or other types of peatlands, is not part of the study.

Our site is not and cannot have been formed from lake pedogenesis, since it is situated on a saddle with a watershed divide at it's crescent. We have addressed this point in a more detailed manner by adding quite a bit of explanation to the Site description in section 2. From this, we hope to clarify this issue. There is already quite a bit of description in P3 L25-P32 of the original manuscript and from the isolines in Figure 2. We have also addressed this in the comments to reviewer 1 by making the site description less ambivalent. Sadly, there is no information in the literature on genesis of the Odersprungmoor, and peat coring and subsequent description is not the scope of this paper. We do acknowledge, that a study on this presents itself as a very interesting research question.

P4: Ironically, the same problems with the Richardson equation applies to mineral soils in forested terrains. Check out Jeff McDonnel's and Keith Bevin's recent papers on this issue. They argue that new theory has to be developed to address how water moves through unsaturated soils of all types, at least at the scale short of regional synthesis. Indeed, preferential flow is so important that Zeno Levy recently published a paper showing that solutes can move from active pore spaces into dead pores to create strange geochemical signals in the catotelm (a dual porosity issue). Chanton's isotopic work showed that much of the methane generated in wetlands may come from modern labile carbon that is driven down deep though the acrotelm into the catotelm. Bacteria use this labile carbon to form methane at depth and the methane episodically blows out when the water table drops, and in the process, no doubt creates preferential flow paths all the way to the top.

So, no—I don't buy into the notion that mosses only take water from the upper few cm of peat soils. This makes no sense from what I know of peatland hydrology.

To the best of our knowledge, we do not state that mosses only take water from the upper few cm of peat soils. We describe the uppermost part of an ombrotrophic peatland, in its transition from living moss to decayed peat, as a porous system that can be successfully characterized with the framework of the classical continuum description of water flow in porous media with hydraulic properties that gradually change with depth. We like to stress that this result is new with respect to detail of the characterized pore systems and representative for the local situation. The specific hydraulic dynamics in this system will of course depend on the boundary conditions, and water flow will occur upwards and downwards under transient boundary conditions.

We do not extend our study to a hydrological modelling of the whole catchment.

We are familiar and aware of the solute transport properties of porous media exhibiting preferential flow paths. But since we are looking at soil matrix water storage and conductivity, we think that our types of experiments are well thought out and designed.

I fully understand that the authors of this paper only looked at the very fibrous peat in the upper 50 cm of the peat column. And that's ok. But they neglect preferential flow and I think this makes their work less useful. The experimental method also strikes me as problematic. In all the years I have taken cores of peat, we take care NOT to freeze them. How can freezing not affect porosity of peat, since frozen water notoriously creates porosity though expansion. Plant cells burst. I see this every time I accidently leave an orange in my freezer too long (for use in drinks). I think the authors need to address this more, rather than cite a paper saying there is no problem.

We do not see any problem, here. Of course, freezing might have an effect on even unsaturated samples. However, the *Sphagnum* moss and peat we used freezes every winter in the field. So we are treating it in a way nature does every winter anyway. In terms of the method itself, it is quite common to freeze peat bog samples (just one of the many examples is McCarter and Price, 2012).

I also have a problem treating peat porosity akin to soil porosity, talking about pore diameters as if they were spheres. They are not. Peat pores can be linear and vertical, horizontal etc. We are looking at a living system of plants in laid down decomposed plants. Perhaps at the end, some kind of uniform matrix pore distribution can occur, but even then, I see the preferential issue, so clear from a great deal of independent research. At least in mire peat.

We fully agree on your thoughtful comments on the size and orientation of the soil pores. Please note that soil hydraulic properties, also for mineral soils, always characterize **effective** properties. Any direct interpretation of the corresponding pore-size distributions as direct capillary diameters would be wrong. However, the interpretation of "pore size" just by the energetic state of pore water is common practice in soil physics, and we do not see any necessity to differentiate between peat and mineral soils in this respect.

Finally, peat can have lower saturated hydraulic conductivity in natural setting because of methane buildup in the pores of question. In natural peatlands, methane can fill up to 20% of lower acrotelmic and then deeper catotelmic peat at times. Check out a paper by Don Rosenberry on this. So flushing out small pieces of peat with DI or air destroys some of the process you want to figure out.

Please note that we characterize her ethe effective pore-size distribuition of the investigated peat soil as material property. Yes, entrapped gases block largest pores and thus can greatly reduce the permeability for the water. Therefore, in a hydraulic modeling of water dynamics under natural conditions, the possible formation of gas must be explicitly considered. However, again we stress that we give in this paper a sound soil physical base for hydrologic simulations. Whether these include methane buildup or not, would be dependent on the purpose of the related hydrological modeling. In any case, such an application would require an additional conceptual treatment of the blocking effect by gas formation. In our characterization, we specify just the permeability for the wetting phase.

While I have some issues with this paper, I do find some good things too. I liked the hypotheses being proposed and the math seems solid to me. The math results agree with their conclusions, but of course, they would have given the inversely fitting a model using the Richards equation can be done by using different suites of assumed parameterizations. The model is non-unique—as are all mathematical models of hydrology. Even given the lack of recognition for dual porosity and preferential flow being not incorporated into the equation and van Genutchen solution, I would want to see more sensitivity analysis to show that the parameterization chosen was the best of the lot.

Thank you for the good words. In contrast to your statement, we <u>did</u> incorporate dual- and tri-modality (which is able to map macroporosity in the framework of the Darcy-Buckingham law into soil hydraulic properties) in both soil hydraulic property model groups. We found that the model parameters are identifiable and unique, we added this by adding "*It furthermore corroborates the fact that even model parameters of three-modal functions can be uniquely identified*." to the manuscript on page 7 LL11-12.

While I'm uncertain the -100 cm head applied for peatlands in general, this result needs to be stated or addressed more clearly before the conclusion proper. I think that there may be more active porosity than what the authors think because of methane ebullition and plants other than sphagnum with roots penetrating deeper than 30 cm or so. Are there spruce or other trees in the peat system studied? In places where fen vegetation occurs, I have seen sedge roots go down far deeper, meters. These can funnel labile carbon to depth among other things. So, my recommendation is for the authors to clarify more for the readers (e.g. when citing a fact like freezing doesn't matter--say why), and address some of the issues I bring up.

We do not study any parts of the poor-fen peatland, and yes, some very small and few spruces can be found, while these have a potential large impact on saturated flow and solute transport of the larger hydrology, this cannot be expected of the unsaturated characteristics.

I recall a conversation I had with Sandy Verry, who with Boelter, did the earliest work on peat permeability from a small kettle lake bog setting. Paul Glaser and I were coming up with orders of magnitude greater values using piezometers and other sampling devices in the Glacial Lake Agassiz peatland. I asked Sandy what he thought and he said he was not surprised, that they only took samples where they saw no roots and other structures. He said his work reflected matrix and not bulk peat. Perhaps all that is needed may be for the authors of this paper to state something like that. Recognize that preferential flow may be the driver at larger scales but in its absence, their conceptual model works.

We addressed this

References

McCarter, C. P. R. and Price J. S.: Ecohydrology of Sphagnum moss hummocks: mechanisms of capitula water supply and simulated effects of evaporation, Ecohydrol., 7(1):33-44. doi:10.1002/eco.1313, 2012.

Yours Sincerely,

Tobias Weber, Braunschweig, 15 September 2017

--

Tobias KD Weber (Geoecologist and Soil Hydrologist)
Researcher in CRC 1253 CAMPOS
University of Hohenheim
Institute of Soil Science and Land Evaluation | Biogeophysics
Emil-Wolff-Straße 27 | D-70593 Stuttgart
tel.: +49 (0) 711 459-24019 (direct) | tel.: +49 (0) 711 459-23765 (secretary)
web: https://www.uni-hohenheim.de/en/organization/institution/biogeop