

Interactive comment on “Saturated and unsaturated salt transport in peat from a constructed fen” by Reuven B. Simhayov et al.

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Reply to Reviewer #1

We thank the reviewer for his/hers comments. We found the review had some helpful comments, which we address fully. However, we apparently failed to convey the appropriate contextual information in a way that s/he could appreciate our goals and methods. We have done our best to adopt suggestions, and otherwise have provided further explanation or a rebuttal. Our answers to the review are as follows: Answers to comments by reviewer #1 (Structure: quote of reviewer and an answer starting with — in the line below the quotation).

1) “Characterization of peat samples: Peat soils are divers as mineral soils. Their

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exact characterization is crucial to classify any experimental outcome. I couldn't find any basic characterization of the investigated soil, even not the organic matter content nor the Van-Post value.”

—We already did give explicit reference to previously published studies describing the structure of the peat on page 4 lines 101-106 (we will add text - refer also to comment 2)).

2) “Moreover, the peat is characterized as sedge peat, but Figure 1 shows moss with hyaline cells.”

—Please refer to page 15 lines 348-350, where we explicitly state that: “The peat of the Nikanotee Fen watershed was moderately decomposed sedge peat containing small amounts of Sphagnum moss (Nwaishi et al., 2015)” in the Results section. It explains Figure 1 which shows how the remnant sphagnum appears in the sedge peat. With respect to comment 1+2, we will explicitly describe the peat material in the Materials and Methods earlier on in the manuscript.

3) “The conclusion drawn is unjustified because soil structure was compromised upon sample preparation (sieving and re-packing). Relevant and recent studies clearly showed that plant residues (embedded in an intact peat structure) in undecomposed peat may serve as a preferred solute pathway (Liu and Lennartz, 2015; Liu et al., 2016).”

—We disagree. The comment correctly identifies, that we reconstituted the peat. However, the field site is characterized as a recently replaced and reconstituted site. In this respect, the laboratory protocol is similar to what has happened in the field, since undisturbed samples (i.e. naturally grown peat) does not exist in our system (See lines 102-105). More specifically, the peat had been disturbed a few years prior to sampling. It had been taken from a pile that was created using heavy machinery. We think it is very likely that these disturbances had a much greater effect on the peat properties than the lab preparation. The preparation was designed to reduce the variability caused

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by the disturbances. Furthermore, nowhere in the text is “sieving” mentioned. The peat was not sieved, as suggested by reviewer 1, but coarse material (leaves and twigs and stem from the aforementioned disturbances which accounted to about 2% in volume) was carefully removed and the peat was gently mixed to increase homogeneity. We don't believe this caused a change to the cellular structure of the cells. However, we do agree that we perhaps did not make it sufficiently clear to the reader that our samples are from a stockpile of heavily disturbed peat, which was replaced in the newly created artificial peatland. Thus, we will be careful to address the full characterization of the peat (also cf. comment 1) and 2) in the revised manuscript.

4) “Peat soils differ from mineral soils in their ability to retain compounds that are generally considered as “conservative” or inert such as chloride anions (Hoag and Price, 1997; Caron et al., 2015). It has been likewise observed that the application of sodium-chloride to peat samples may cause a rearrangement of pore structure based on a pore dilation effect (Ours et al., 1997). Both aspects shall be discussed in a publication on sodium-chloride transport in peat soils”

—We agree with the reviewers' comment, and in recognizing the potential for sodium to cause flocculation and/or pore dilation, we flushed the column with NaCl as a pre-treatment of the samples to minimize the effect during experimentation. We stated this explicitly on lines 152-155.

5) “Model evaluation: The authors employed the mobile-immobile solute transport concept and the according solution of the underlying equations to obtain parameter values by running optimization algorithms. From a huge body of literature (for instance Parker and van Genuchten, 1984; Bond and Wierenga, 1990; Gao et al., 2009) it is evident that the simultaneous optimization of the pore water velocity (v) and the fraction of immobile water (β) may lead to invalid parameter values because both parameters account for the position of the BTC on the (dimensionless) time axis. Keeping v fixed at the measured values would probably have produced a different outcome and a different conclusion.”

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—Of course, fixing parameters will lead to different conclusions, we are aware of this. The method the reviewer proposes is for exactly that same reason a very unsatisfying method. Pre-fixing either the pore water velocity (v) or the fraction of immobile water (β) is subjective. Moreover, if parameters are fixed a priori, the inverse modeling will not provide evidence on the flow phenomena, but merely “prove” the conceptual model which was used to fix the initial parameters in the first place. For this reason, we took a different approach; we refrained from fixing parameters, and used the information content of the experiments. Thus, we attest on whether or not the mobile-immobile solute transport model is an over-parameterization or an adequate model choice. Moreover, simultaneously fitting v and β is possible and has been done previously (e.g. Zurmühl, 1998; Tang et al., 2009 and references herein). Considering the implied 1:1 representation of v for β (or vice versa) close inspection of inspection of Eq. 1 shows that what the reviewer implies cannot be the case. Division by β leads to β being part of each of the three summands on the right-hand side of the Eq. 1, and therefore contradicts the reviewer's assumption. We believe our approach is appropriate.

6) “In this context, the possible retardation of the “conservative” tracer needs to be considered (see above). ”

— We disagree that a potential Cl adsorption might be relevant on the scale of the flow through reactors. There is a theoretical potential, that an anion such as Cl- (or Br- used in other studies) might be attenuated, too, but this would contradict what is known about Cl- in peat. Since no studies exists (to the knowledge of the authors) of deuterium as tracer in peat, it is very speculative to state that Cl should not be at least very close to conservative as a tracer. Hoag and Price (1997) indicated Cl was subject to physical diffusion into inactive pores as evidenced by the Cl retardation factor greater than 1 ($R=1.1$). The reviewer is commenting on the classical definition of retardation (by adsorption). We acknowledge that while most studies treat Cl as a conservative tracer, there is a theoretical possibility that a small anion adsorption effect may exist, which can only be distinguished by known conservative tracers like deuterium. We will

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add text acknowledging the possibility.

7) "For instance, how can it be explained that the optimized pore water velocity ($v(fit)$) showed greater values than the measured ($v(measured)$) employing the (single modal) convective dispersion equation? A ratio of $v(measured)/v(fit)$ of less than 1 is a clear indication of a preferred solute transport situation."

—We think the reviewer refers to the effective pore water velocity. Of course, the comparison of $v(fit)$ with $v(measured)$ can be done, but differences are to be expected from normal measurement errors, experimental set up, and remaining air bubbles in the sample. In any case, ω [-], the dimensionless mass-transfer coefficient also indicated at the non-existence of mobile-immobile solute transport as it was found that $\omega > 100$ (In the paper we actually reported on the dimensioned mass transfer coefficient, α). We realize that our conclusions from our results of ω were not presented with sufficient clarity. The parameter ω could also not be identified by inverse modeling. In the inversion, ω ran into the maximum upper boundary limit of 100 in CXTFIT. This supports the conclusion, that the MIM solute transport model is an over-parameterization (corroborated by the CXTFIT results files, too), and that the CDE is sufficient for describing the observed breakthrough and solute transport. For this reason, we are convinced that our results and conclusions are not affected by this comment. In a revised manuscript, we will address this by including a brief discussion on ω , and present the values in Table 3, and hope this can strengthen the paper considerably.

8) "Solute concentration data shall be presented against exchanged pore volume instead of absolute time to better classify the breakthrough behavior."

—Both methods of presentation can be found in the literature (e.g. Tang et al., 2009). Moreover, we took great care in presenting and discussing our results indicating CDE solute transport, and therefore do not believe it will change the understanding of the chart or the processes. If the editor finds this change compulsory, we could consider this in a revised manuscript.

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9) "The result section starts with the presentation of the (adjusted) bulk density etc. That clearly belongs to Material and Methods. "

—A good point. While being more elaborate on the description of the peat, (cf. comment 1-3), we will address this, too, and make the Material and Methods more concise and complete.

10) "There are likewise some editorial flaws: Doubling of sentences, etc. (lines101-104). "

—We apologize for this and will make an even greater effort in addressing any potential editorial concerns, thoroughly, before submitting a revised manuscript.

11) "Suggestion: There is some potential in the work. It is, for instance, interesting to compare saturated and partially saturated solute transport scenarios. It is likewise valuable to combine transient evaporation with tension disk measurements to obtain soil water retention and hydraulic conductivity information."

—We agree this is interesting, but do not believe it is central to the manuscript; it would disrupt the flow, and in its extent is a separate study for another time.

12) "In any case, the experimental data base needs to be substantiate addressing the given points to draw definite conclusion. The study could be substantially upgraded if data from UNDISTURBED samples could be presented in comparison to the data from the disturbed samples. This holds true for the solute transport as well as for the hydraulic properties alike".

—We agree that comparison to undisturbed samples is always interesting; however, the peatland from which these samples were drawn was destroyed long before we began this experiment. So in that sense, we disagree, as undisturbed samples do not exist. The samples are obtained from stockpiled and heavily disturbed original material (during placement on site). Our focus here is to characterize the disturbed peat, so the implications for solute transport at the specific site can be discussed. In a revised

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manuscript, we will include a clear statement on the objectives, and, again, be much more precise on the nature of our samples.

References used in Reply to Reviewer #1:

Hoag, R.S., and Price, J.S. (1997). The effects of matrix diffusion on solute transport and retardation in undisturbed peat in laboratory columns. *Journal of Contaminant Hydrology* 28:193–205.

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Tang, G., Mayes, M. A., Parker, J. C., Yin, X. L., Watson, D. B., and Jardine, P. M. (2009). Improving parameter estimation for column experiments by multi-model evaluation and comparison, *Journal of Hydrology* 376, 567–578.

Zurmühl, T. (1998). Capability of convection–dispersion transport models to predict transient water and solute movement in undisturbed soil columns, *Journal of Contaminant Hydrology*, 30, 101–128.

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