Reply for comment of Reviewer #2 on "Estimating the degree of preferential flow to drainage in an agricultural clay till field for a 10-year period".

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1 Introduction

We have addressed and carefully considered the constructive comments/inputs by Reviewer #2 and made the suggested revisions and modifications where we find them appropriate. We very much appreciate the extensive review of our manuscript provided. Altogether we believe the manuscript with the modifications has improved with the revisions made and we very much hope that you will consider the revised manuscript for publication. We agree with the Reviewer 2 that this manuscript do not only address the scope of the special issue on "Water, isotope and solute fluxes in the soil–plant–atmosphere interface: investigations from the canopy to the root zone" and would hence be misplaces in this issue.

2 Main concerns

1. *"In these articles I feel lost in the abundance of data and parameters and goodness of fit measures, which obscures the message you want to convey. It also seems that you have used determined a rather large amount of sensitive parameters and subsequently used many different parameters to calibrate on and different model performance criteria."*

   The reviewer has a point, the article consists of several information on parameters and goodness of fit, besides a comprehensive 10 years hourly dataset on climate, drainage and solute transport. We tried to limit the complexity of the presented information, however some of the information such as different performance measures have to be presented to provide measures for a discussion regarding model calibration and evaluation. Concerning the abundance of parameters presented in the manuscript, we would like to emphasize that the presented macropore concept in DAISY having a well-documented crop-development description has not been evaluated and documented in a peer-review article, why our extensive model performance focus.

2. *In the end it would be nice to see a bit more discussion about the model concept and what can be learned from this study and the field data on preferential flow.*

   We agree and have among other things revise the Perspective in regards of explaining the preferential transport effect comparing all model parametrization.

3 Detailed response to the minor concerns

Line 1: We agree with the reviewer, this sentence can be modified and we can remove the redundant part.
Proposed change:
To be able to estimate the degree of water percolating rapidly through vertical macropores, or slowly through the low-permeable matrix, is vital for being able to assess the leaching risk of contaminants like nitrate and pesticides through clay tills to drainage and groundwater.

Line 4: The study explore three different macropore settings with two different lower boundary condition. We do not only test one dual-permeability concept by Mollerup (2010), but also different conceptualization of the underlying pedology and hydraulic condition. No changes will be made.

Line 17: We have corrected our Latex code and fixed the paragraph error.

Line 18: With the proposed change:
In low-permeable tile-drained clay tills preferential flow and solute transport occur given the presence of macropores / discontinuities / continuous voids such as earthworm burrows and fractures.

Line 47: Thank you for bringing our attention to the SWAP model, we will try to incorporate the work of Kroes et al. (2008) into our introduction.

Line 70-71: The experiment of Tofteng et al. (2002) showed that macropore flow appeared below -20 to -30 cm H_2O potential, which is outside of the the expected 0 to -10 cm. Therefore, it cannot be described by a general retention - conductivity function such as the Mualem - van Genuchten retention model. The following text has been added instead
"To be able to estimate the degree of water percolating rapidly through vertical macropores, or slowly through the low-permeable matrix, is vital for being able to assess the leaching risk of contaminants like nitrate and pesticides through clay tills to drainage and groundwater."

Line 86: We have corrected our Latex code and fixed the paragraph error.

Line 87: The sentence has been modified into:
The field monitoring was started in 1999 and prior to that, it has been used for agricultural purposes since 1942 and systematically drained since 1966.

Line 96: Unfortunately we overlooked that the Latex code applied has mixed up the addresses of the references for Figure S2-S4.

Line 130: Figure S1-S4 have been combined into Figure 1 taking outset in Figure S3 though only showing the points with measurements applied in this study and the point for soil sampling and the original Figure 1 are changed into Figure S1.

Line 138: We understand the concerns of the reviewer, however we wanted to present in one figure the overall span and quality of the ten years measurement. In term of higher resolution we dedicated a subsection (3.5.1) where we are discussing the model concepts' capabilities of depicting the hourly transport in drainage in contrast of the hourly soil water change by screening a heavy precipitation event.

Line 139: We have now checked the manuscript in regard to consistent on the use of unit as pointed out by the reviewer.
Changed to:
various depths (25, 60, 90, 110, 190 and 210 cm)

Line 143: *measured* is removed and *measurements* is added. *due to* is removed and *because* is added.

Line 144: Word *containing* is changed to *contain*.

Line 148: According to Germann (1985), the terminology is *wetting shock front*. No changes is made.

Line 174: *Here a* is removed from the sentence.

Line 187: We understand the confusion of the reviewer, although the required geometrical model parameters of macropores is defined in line 175. We have now added the signs of the parameters behind their description. Line 175 has been changed into:
Changed to
...characterized by physical properties such as length, diameter (d) *and density* (ρ).

Line 193: We agree that the sentence does not convey the information as intended. Therefore, we accepted the reviewer proposal for this sentence.

Line 195: The word "concept" as described in our answer to the reviewer's comment in Line 4, we refer to the different parametrization of the model setups. We see that it might increase the misunderstanding between conceptualization of processes and conceptualization of model setup, therefore we intended to use parametrization instead of concept.

Line 197: The *horizon* term might not be an appropriate term for describing the parametrized soil layers as it has a direct connection to the pedologigal surveying. In this case we agree with the reviewer to use layers instead of horizon.

Line 198: The soil layers description for the models has been enumerated. We have though kept the capital letters naming system (A, B, C, and D) of the soil layers to avoid a recreation of all figures in relation to the model parametrization.

Line 215: We agree with the reviewer to separate this subsection into two paragraph.

Line 217: By dividing this section into two paragraph starting with the macropore settings (MSETs), the reader is helped to associate it to Figure 2 as it is cited after the sentence, why we do not see a need for changing MSET into MPset.

Line 220: The Daisy macropore module classifies two type of macropore feature based on their lower boundary conditions, whether it is matrix ended or drain connected. These classes can be use to create user defined composition of classes, which can be added to the model. Thus, by using the predefined classes we created 6 different macropore type which we believed is a good approximation of reality, such as surface connected or buried macropore ended in a matrix or drain. No changes is made.

Line 243: Please see the answer of line 187.

Line 255: We agree with the reviewer to move this paragraph to the discussion part of the manuscript and remove *addressing the field*.

Line 220: In order to alleviate the confusion, the part of the sentence is rephrased to:
with the normalization of the difference of the maximum and minimum of the measured value of inspected period

Line 311: In the calibration methodology we did not want to involve crop calibration, since it was not the scope of the study. Here we wanted to inform the reader, that the Daisy crop library has extensive description of different crop, but it might be prudent to be adjust the development rate for certain crop if phenological data is available (DSrate1 and DSrate2 please see Daisy manual - www.daisy.ku.dk).

Line 329: Yes, that was our intent to show the reader how different the model outcome can be if one only uses a single permeability model.

Line 331: In order to make it clear, the 26, 32 and 35 parameters based on MSET *a)*, MSET *b)* and MSET *c)*, respectively, were identified to be screened for sensitivity but it is not the outcome of the sensitivity analysis.
The sensitivity screening gave sensitive parameters of 14/16, 13/15, and 19/19 for model with MSET a), b) and c) with lower boundary P3/P4, respectively.

Line 357: We agree with the reviewer's statement, that MM (Matrix ended macropores), "that the release of the water into the matrix is likely to be highest in relatively dry soil". Although, we would like to clarify that the statement "*The MM macropores contribute very little thereafter to the rest of the hydraulic environment*", only show the role of MM macropores in this particular modelling case in terms of the sensitivity screening.

Line 360: According to Tofteng et al. (2002), macropores with diameter smaller than 5 mm, tend to flow with pulse flow, by utilizing the full size of the macropore, whereas macropores with diameter above 5 mm tend to flow by film flow. In order to estimate the degree of preferential flow, one needs to know an average macropore size of a given area, where the flow conducted in the macropore at maximum capacity. This information will be added to the manuscript.

Line 365: Noted, and changed to *b)*.

Line 367: The word *but* is replaced with *and*.

Line 382: The data from PLAP refers to the original dataset acquired from the field in 1999 and published in Lindhardt et al. (2001). We used that SWR dataset and fitted the vGM SWR and conductivity model on it. That is how we acquired the PLAP SWR range (grey ribbon). As it is stated in the manuscript, that we had an understanding, that the original PLAP dataset might not represent well enough the retention range of the plough layer of the field.

We extended the SWR parameter range for the plough layer, in light of the studies of Katuwal et al. (2015), Norgaard et al. (2015) and Naveed et al. (2016). These studies were conducted at the same field, and they had a more extensive pedological and sampling survey of the plough layer of the field. We fitted vGM SWR and conductivity model on their measurements and that is how we got the extended parameter range for the SWR of plough layer. In order to alleviate the confusion about the origin of the data of Katuwal et al. (2015), Norgaard et al. (2015) and Naveed et al. (2016), we will describe it more accurately in section 2.1.

Line 391: Please see the answer for Line 138.

Line 450: We understand the reviewer, we will consider to move this section earlier to the manuscript.

Line 467: The 70% refers to the amount of drainage originated to the macropore flow. We understand, that it is difficult to see from Table 4. Therefore the following text has been added:
"...estimating a high macropore contribution to drainage (113 mm ; 70% of total drainage 160 mm; 47 mm from matrix; Table 4).

Line 470: The range of 34-36% can be acquired by summing up the matrix and macropore percolation percentage from Table 4. by MSET and lower boundary condition. In order to help the reader, we modified the sentence to ......*The sum of percolation from matrix and macropore to the groundwater was similar for all macropores....*

Line 483: The Daisy crop model is capable of simulating water uptake based on demand, root crown water potential, and Darcy flow. Nutrient uptake is based on both convection and diffusion. It is also possible to export the amount of nutrient or substance, which is taken up by the crop. Although we have no measurement on bromide concentration in the harvested crop, in our assessment, we compared our results with the reported ranges from studies like Kohler et al. (2005) and Shtangeeva (2017).

Line 507: We assume the models are able to depict the water and bromide transport satisfactorily. The models are able to simulate the soil water content well through out the entire simulation period. Therefore we assume that the soil water propagation is following the reality. Later in the companion paper we could show the nitrate concentration simulation at 1m and 2m deep is matched with the measured concentration.

Line 531: Even though we do not have soil water bromide concentration, we had nitrate concentration. In the companion paper, we were able to model the transport of nitrate to the deeper layer (1 and 2 meters b.g.s.). In that case the nitrate were originated from plough layer, due to restricted denitrification left excess nitrate in that soil layer. In regards to the amount of water and solute transport from the plough layer, we understand there are limitations in the applied model concept.

With the one-dimensional DAISY model it was not possible to allow precipitation to enter the macropore domain directly at the soil surface nor was it possible to include a fracture domain in a physical acceptable manner here among allowing water and solute to leave the fracture domain directly at the bottom of the model domain like in the model study by Rosenbom et al. (2008). Having such drawbacks in the modelling code lead to that the macropores initiated in the plough layer had the highest impact on leaching in this study. Even though the model outcome may not be completely realistic given the conceptual setting of the macropore and matrix domain, the model have been restricted by a realistic upper and lower boundary condition, soil water contents in the profile and sink-term (measured drainage from the field), it emphasis the high degree of preferential flow and transport to drainage.

Figure 4: Please see answer for Line 382.

Figure 7: Agreed, out of this four evaluation case the MSET *a)* and MSET *c)*, performs better than MSET *b)* during the season of 2000-2001 with P3 lower boundary condition, but in all other cases MSET *b)* outperform MSET *a)* and *c)*. Thereby, we can state that "MSET *b)* performed better than MSET (*a)* and *c)*". This has been added to the manuscript.

Figure 8: We agree with the reviewer, that exploring additional heavy rain events would be interesting for this manuscript. We tried to present this extensive model study and high resolution dataset from as many aspect as possible. For further revision, we will consider the possibility to extend that part of manuscript by screening data from the evaluation period.

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