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Discussions

Interactive comment on "Estimating the degree of preferential flow to drainage in an agricultural clay till field for a 10-year period" *by* David Nagy et al.

Anonymous Referee #1

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The present study applies the DAISY model in its one-dimensional form to simulate infiltration into an agricultural tile-drained field consisting of clay till as part of the Pesticide Leaching Assessment Program of Denmark. The central idea is to investigate the role of macropore configurations on the soil drainage behaviour by validating different macropore parametrisations against measurement of the soil water balance and tracer experiments conducted with Br^{-}. Model parameters were determined by a multiobjective function calibration procedure making use of the highly monitored experimental site. Results demonstrate the feasibility to reproduce drainage dynamics of the experimental field based on the chosen modelling setup and the final set of best parameters determined by model calibration.

I think the topic is of high relevance for HESS and the modelling was performed with

high technical and computational effort definitely justifying its publication. Unfortunately, the manuscript is in a poor overall state considering its structure and writing. As such, I struggled a lot in always finding the details necessary to understand what was done and by which means in order to correctly rank the results. In my impression, the manuscript rather represents a preliminary draft than a manuscript ready to hand in. I am convinced the authors could have done a much better job considering the great efforts taken to achieve these results.

In my opinion following major concerns needs to be addressed:

1. Thorough textual and grammatical revision of the complete manuscript. The entire text is structured without paragraphs. References in the text sometimes mislead to the wrong Figure/Table (for example L127) and their general order is not stringent (Fig. 2 referenced after Fig. 5 for example). The headings not always fit the content (sections 2.1 and 2.2 for example mix up with contents belonging to each other). A lot of sentences need to be rephrased because they are either grammatically wrong or their meaning is unclear. All these points might be of minor nature but their frequent occurrence within the manuscript clearly distracts from focusing on the storyline and weakens the paper's overall relevance.

2. The title is somehow misleading. Although the field measurements cover a 10-years period, the modelling period used for estimating the degree of preferential flow to drainage covers solely two years if subtracting the calibration period. Placing this information at the end of the methods section is also far too late and irritates a lot in understanding the model setup. It is a central point of the entire study and has to be stated at least at the end of the introduction and when the model application is introduced.

3. There is some misfit throughout the entire manuscript between the presented and required information. This relates in particular to the scatter of supporting

information over eleven figures in the manuscript and 17 additional figures in the supplement and less to some missing information. For some points I had to go through three to four figures until I finally found the desired information. This procedure is quite cumbersome and counteracts in promoting the findings which is certainly not within the authors' interest. I suggest including one central figure and table in the early methods section. The figure illustrates the experimental field, similar to Figure S3 but only including points with measurements used during the model calibration and validation. In addition, if known, the points for soil sampling should be marked. The table gives the relevant meta information like the number of replicas and measurement principle. As such, the data used for modelling would be a central part of the methods and the reader would not be requested to go through several figures to gather the information relevant for understanding the modelling objectives.

4. I miss some critical review of the applied calibration procedure. Fitting a model to more than 30 parameters is a clear subject to the equifinality problem (e.g. Beven, 2006). I see some attempts in benchmarking the calibrated parameters against field-measured and validating the model against a single heavy precipitation event to check for a correct preferential flow process replication, i.e., the drainage responds in the model. However, I would like to see some broader discussion on this topic since the study heavily relates to model calibration.

5. What are the lessons learned from this study and what is the novelty of this modelling approach? Especially in the context of other studies related to preferential flow from tile-drained fields (e.g., Gärdenäs et al., 2006; Klaus and Zehe, 2010; Klaus et al., 2013; Zehe and Flühler, 2001; and references in the manuscript) and preferential flow simulations in general? There are some general points at the end of the manuscript pointing into the direction of preferential transport. For sure, the correct preferential flow implementation is crucial for a correct solute transport simulation of macroporous soils. However, I would clearly broaden the

implications of this study. Most fields are not monitored so comprehensively and still less have coherent time series of soil moisture, groundwater level and tile-drain outflow for such long periods. So, can any recommendations be made for scarcely monitored agricultural fields? On what parameters should be concentrated if some monitoring is initiated on other sites? What can be concluded about the influence of macropore topology on preferential flow beyond the direct shortcut from surface to the tile? Other studies for example demonstrated a clear increase in soil water storage by dead-ending macropores (Urbina et al., 2019) and thus likely influences the solute transport. Answering some of these questions might clearly strengthen the relevance of the presented study and definitely help other studies investigating preferential flow and linked solute transport by means of modelling and field measurements.

I attached the manuscript with specific notes on major points criticized here as well as purely technical corrections.

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Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-665/hess-2019-665-RC1-supplement.pdf

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