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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Effect of preferential transport and coherent denitrification on leaching of nitrate to drainage" *by* David Nagy et al.

Anonymous Referee #1

Received and published: 16 February 2020

Dear Editor, The following is a review of the above titled paper for HESS. The paper documents an application of the DAISY model to the simulation of nitrogen transport with an emphasis on the role of preferential transport processes. This study is applied to data collected at a field site, part of the Danish Pesticide Leaching Assessment Program. The field site is well instrumented and has a long history of intensive monitoring and characterization, including unsaturated zone modelling of solute movement. The topic is of relevance to HESS readers and the conclusions have the potential to contribute to a better understanding of risks of nitrogen contamination of groundwater.

The authors adopted a modified version of DAISY, previously developed in Nagy et al., (2019). DAISY simulates N cycling through various N pools, water movement (now with preferential flow), crop growth and the energy balance. There are many param-

eters and multiple processes simulated and it seems there is significant uncertainty in specifying/constraining parameters a priori to require calibration of many of them. There is a need for studies like these, as learning from data via models offers the potential to better understand the significance of processes in the unsaturated zone.

After describing briefly the data and the model the paper documents the results of calibration procedures for simulations with and without the consideration of preferential flow.

This manuscript would benefit from consideration of the following criticisms:

1) There has been considerable characterization of soil and in particular preferential flow at the study site, both through observation of soil, through observations of pesticide and tracer transport, and several modelling studies. The manuscript would benefit substantially from relating how their "new" model considered these learnings and what new contribution it brings; how parameters related to those calibrated from earlier studies at the site (for example modelling studies exist of at least one of the other author's); and in order to broaden the appeal of this study, how parameters or transport metrics relate to studies from the wider literature. This is essential for both the N cycling and crucial for preferential flow.

2) The descriptions of the calibration process are unclear. The number of parameters calibrated is not described. The justification for narrow initial uncertainties of parameters is not described. How these paramaterizations might be justified from field data, independently of calibration where possible is not undertaken. It is not clear which calibration data are used or from where (publications) the data comes. Consideration of calibrations undertaken by earlier studies at the site should be given more detail and/or attempted. The staged approach to calibration could be made clearer. Also some critique of the approach to using a single objective summed from multiple model - data states (i.e. water flow, Br movement, N movement, crop yield) etc. as opposed to a true multi-objective calibration. Some consideration of the learnings about model

calibration the hydrological community has been struggling with for decades could be applied and considered, particularly when emphasizing an appraisal of the limitations of the proposed approach. There seems to be a mix of objective and subjective model calibration but it is unclear when one is conducted and the other is used. The method is currently not reproducible, neither the data, the model and its input files, nor a step by step documentation of calibration (code) been supplied.

3) The graphics are in general quite cluttered, containing text that is too small to read or data which are difficult to distinguish from simulations or are unclear as to what they represent.

4) The discussion should return to the literature on N cycling and preferential flow, but instead it remains focused on the results of model calibration efforts. Prior studies at the site and the wider literature are skimmed over. The outcomes of the paper have the potential to contribute to better understanding of model limitations but also to application of such models to emissions prediction and mitigation.

I have appended a marked up pdf offering suggested edits to text.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-666/hess-2019-666-RC1-supplement.pdf

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