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Comment on hess-2021-168

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Referee comment on "Robustness of a parsimonious subsurface drainage model at the French national scale" by Alexis Jeantet et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-168-RC1, 2021

General comments

This paper presents a simple hydrological model of drained soils, which combines an empirical single soil box ("lumped") model of evapotranspiration, water storage and recharge for the unsaturated zone with a classical physics-based equation for water flow in the saturated zone to a subsurface drainage system (the Boussinesq equation). The model is shown to perform acceptably well after calibration of four parameters using a large dataset of field drainage data from 22 sites in France.

I think the model is interesting and useful (within its limits) and the description of its application is comprehensive and reasonably transparent. In many places in the paper (e.g. line 429), the authors stress the value and advantages of model simplicity and in general I am sympathetic to this point of view. However, there is a downside to model simplicity which the authors have neglected. This is that errors and limitations in the use of the model are inevitably introduced when process descriptions are over-simplified (or even excluded from the model entirely). A more realistic, objective and open discussion of some of the limitations of the model is therefore needed (see points 3-7 below).

Specific comments

- Line 177: The origin and meaning of this equation is unclear and should be explained. Perhaps a figure could help? Where does the number 0.4 come from? In principle, this looks like another parameter to me, even if you assumed it here to be a constant.
- Line 186: in principle, a is also a parameter that apparently has been previously calibrated against experimental data and is now set as a constant. However, in principle, it should depend on soil hydraulic properties and is therefore not a constant for different soil types. It's not a very important point, but the author's claim that there

are only 4 parameters to calibrate in this model is in my opinion a little dubious. As far as I can see, it should be six.

- Line 202: You should also mention another important assumption here. You also assume that recharge to groundwater is negligible i.e. that all excess water is routed to the drainage system. This seems to be a reasonable assumption for your study sites, because the water balances are simulated quite well, but it will definitely not always be the case. For example, wide-spaced drainage systems are often installed in fields with slowly permeable subsoils (e.g. in soils with morainic parent material). Annual recharge to groundwater is definitely not a negligible term of the water balance in such cases. It should be discussed here whether your model could be adapted to account for this kind of hydrogeological situation (and if so, how).
- Lines 301-303: It would help readers to better understand the model limitations for clay soils if the authors also showed some plots of the results for the Courcival site at this point.
- Lines 310-311: do you mean that the model gets the timing of the start of drainage in clay soils wrong by a whole month? Please clarify. It would help to show this in a figure (see point 4).
- Lines 331-332 and 579-581: it will not be possible to accurately model pesticide or nitrate leaching based on the RU hydrological model, because the unsaturated zone is modelled as a single box. Leaching of nutrients and contaminants in soil cannot be accurately modelled using such a lumped hydrological model for the unsaturated zone. Physics-based modelling approaches are necessary. This should be acknowledged in some suitable way.
- Lines 498-500: Yes, most hydrological models work less well on clay soils and SIDRA-RU does not seem to be an exception. But it should be acknowledged that there are exceptions. For example, MACRO was originally designed to simulate flow processes in structured soils and it has been shown to work very well in heavy clays (Köhne et al., 2009). There are three main reasons for this. It accounts for non-equilibrium water flow in soil macropores as well as vertical heterogeneity in their hydraulic properties, while the drainage module also assumes horizontal saturated flow (Dupuit-Forcheimer assumptions) to a seepage surface above the drain (exactly as figure 11 shows).

Technical corrections

- Line 50: MACRO was not developed by the FOCUS group (it has been used by FOCUS, but that is not the same thing). Please cite Larsbo et al. (2005) here (rather than Jarvis, 1994), because this internal report from 1994 is no longer available.
- Line 55: It is unclear how the authors arrived at an estimate of 20 parameters to be calibrated in MACRO. I would be surprised if so many parameters have ever been calibrated in an application of this model. I think it is difficult (and unnecessary) to compare the two models like this. It is sufficient to write that only four parameters need to be calibrated in SIDRA-RU.
- Line 57: please replace “this model” by “SIDRA” to avoid confusion.
- Lines 172-173 and equation 1: Equation 1 is incomplete. Please join it together with the condition and equation in the text on line 172.
- Line 212: Please modify this text to: “they cannot be measured directly, nor easily correlated with any …”
- Line 243: Does KGE’ = 0 have any significance? If it does, this could be mentioned.

The paper is well organized and is also relatively easy to read. However, there are quite a number of (mostly) trivial grammatical errors in the text, which I have not listed here (line 205 is one example: it should be "responsiveness" not "nervousness"). It would not be difficult or time-consuming for a native English speaker to make very significant improvements to the text.

References

Köhne, J., Köhne, S., ŠimÅ¯nek, J. 2009. A review of model applications for structured soils: A. Water flow and tracer transport. Journal of Contaminant Hydrology, 104, 4–35.

Larsbo, M., Roulier, S., Stenemo, F., Kasteel, R., Jarvis, N.J. 2005. An improved dual-permeability model of water flow and solute transport in the vadose zone. Vadose Zone Journal, 4, 398-406.

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