EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-274-RC1, 2022

Comment on egusphere-2022-274

Anonymous Referee #1

Referee comment on "Assessment of the ISBA Land Surface Model soil hydrology using several lysimeters and four pairs of soil water retention and hydraulic conductivity functions" by Antoine Sobaga et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-274-RC1, 2022

The manuscript focuses on the use of a Richards-based solver to reproduce hydrological observations from multiple lysimeters in France. The case study is also used to compare three soil hydraulic models (i.e., Brooks-Corey, van Genuchten-Mualem, a combination of both). The aim is relevant for HESS and somehow interesting, however the manuscript possesses multiple methodological weaknesses:

- The choice to use the ISBA LSM model, which was conceived to operate on larger scales, to investigate a process at the lysimeter level (and prove a soil physics point: Brooks vs van Genuchten) is questionable. The model solves the Richards equation using a Crank-Nicolson scheme but there are no details about the spatial discretization, boundary conditions, etc. By reading this (https://doi.org/10.1029/2018MS001545), the model seems to use a multi-layer approach based on the finite difference. Widely used vadose zone hydrological model such as HYDRUS or SWAP use schemes that comply with the mass conservative approach proposed by Celia et al. (1990). These models have been widely tested, and would be a more rational choice to investigate processes at the lysimeter level and compare multiple soil hydraulic models.
- The whole methodology on the comparison between model predictions and observations is cumbersome to read, not novel, and weak.

- No error metric is reported to compare multiple soil hydraulic models. Besides fitting (which should be quantified), other metrics should be used to compare also the complexity of the models (e.g., at least Akaike Information Criterion)
- The calibration procedure should compare time series of modeled and observed soil water quantities (e.g., water contents). An objective function or a likelihood (e.g., NSE, Gaussian, etc) should be selected, and a numerical algorithm should be used to perform the model calibration. Further, parameters uncertainty should be assessed to see how informative are data, and whether the choice of a more complex model is justified. Only after having performed a statistically robust analysis, it is possible to try

to explain why BC+VG is better and when. As they are, methods don't support enough the conclusions, and neither represent a novel contribution to the field.

Specific comments:

L15-20 Not really. Drainage is the amount of water that bypasses the root zone.

L47-50 Nonlinearity cannot be a source of criticism, otherwise an endless number of equations used in environmental modeling should be "criticized". I would remove this part. Richards equation is not perfect, but we are still far from finding a viable, widely used, and extensively validated alternative.

L56 BC66 has that sharp singular point near the air-entry pressure that makes it not very stable. (https://doi.org/10.1029/93WR03238). Authors indeed discuss this point later. However, more specific references are needed to prove your point that BC66 is more numerically stable than VG80.

Data: Please add details about TDR sensors (e.g., type, accuracy, calibration type) and tipping bucket resolution

L124-125 Is the heat transport included in the numerical simulation of lysimeters? If yes, key equations should be provided. Otherwise, it should be removed from the text.

L130-140 This part should be moved after the Richards equation, and should describe how it is connected to the sink term S(z). Key equations should provided. Citing refs is good, but the manuscript should stand by itself.

L147-148 what is the discretization of the soil profile? What are the boundary conditions used?

Figure1. Very confusing. It is difficult to appreciate differences. What are the dashed lines? Figure+Caption should be self-explanatory

L176 There is not a single error metric to support the conclusion that one formulation is better than the other. It is really puzzling to see that.

L192 VG80 not stable for n<1.3?! Never experienced something like this. Indeed, I agree with the Authors that n>1.1 is a good constraint.

L197 Having a highly negative tortuosity is not recommended. Actually Schaap suggests a value of -1 (https://doi.org/10.2136/sssaj2000.643843x)