Comment on hess-2021-426

Referee comment on “It rains and then? Numerical challenges with the 1D Richards equation in kilometer-resolution land surface modelling” by Daniel Regenass, Linda Schlemmer, Elena Jahr, and Christoph Schär, Hydrology and Earth System Sciences Discussions.

In this paper, the authors present a solver for the one-dimensional Richards equation and its application in a land-surface models to assess the partitioning of the precipitation in surface runoff and infiltration. I think that the present paper can be accepted after major revisions.

Main comments

1. In their model, the authors use the so-called saturation-form (or the theta-form) of the Richards equation. This is one of the three forms in which is possible to express the Richards equation. In appendix A the authors discuss the limit of the saturation-form. My concern is that the clearly state that the saturation-form is not the suitable to model water infiltration close to saturated condition (Farthing and Ogden, 2017). The paper aims to assess the partitioning of precipitation in surface runoff and infiltration, but I think more attention should be put in the definition of the surface boundary condition see for instance Tubini and Rigon 2021. Moreover, in presence of saturation it should be advisable to consider of the compressibility of water that allows to correctly simulate saturated hydraulic conditions (Simunek et al. 2012), since the Richards equation is appropriate only in the unsaturated domain.

2. At line 517 the author states that the saturation form is used successfully in LSMs, even if its use is mathematically not fully appropriate. This assertion seems to be not clear and a bit contradictory: there are sound mathematical reasons to not use the saturation form, but it is still applied succesfully. Please could you discuss a little more this sentence also referring to existing literature?

Minor comments

1. About Fig. (5) panel a b c could be merged in a unique panel and in panel d I would change the color of the reference solution: it should be highlighted more. Looking at panel (d) the model converges, but honestly 40s is a quite small-time step. This figure refers to the MVG or RIJTEMA model? In the caption the spatial discretization used is identified by Delta z but in the subsequent figure the x-axis contains the number of control volume. See also comments 2 and 3.
2. Fig. (6) the x-axis contains the number of vertical layers, while in the caption you refer to the mesh spacing Delta z. I would suggest replacing ‘as a function of Delta t and Delta z’ with ‘as a function of Delta t and n_z’. I would change the title of the left panel in RT.
3. Line 363-364 you refer to Delta z< 2cm while in Fig. 6 one reads the number of layers n_z. It is easier to follow the text if one directly reads the number of layers. Please, this comment as the above on the use of n_z and delta z are a hint to adopt a consistent manner to refer to the

spatial discretization of the domain. In my opinion the convention can change when referring to the equally spaced grid and the geometric grid.

4. In both the equidistant grid experiments, the NRMSD is lower than 20% only for Delta t<40 s. this is very small-time step considering that *soil moisture is a critical variable for determining the exchange of water and energy between the atmosphere and the land surface on hourly to seasonal time scales*?
5. Fig. (7) I would change the title of the left panel in RT.
6. Eq. (8) contains the evapotranspiration term (E_k) while at line 151 it is written that *E=0 for all further derivations and experiments in this study*. The same also in Eq.(10)and Eq.(11). I would suggest modifying line 150 removing for all further derivations.

References

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