Reply on RC3
Daniel Regenass et al.

We thank the reviewer for taking their time to review our manuscript. First, we would like to stress that the scope of this paper is not on ‘improvements’ but rather to systematically assess numerical issues with the Richards equation as implemented in today’s state-of-the-art land surface models (LSMs) coupled to weather- and climate prediction models, and to highlight their interaction with increasing precipitation intensities. Since both reviewer 1 and 3 seem to have understood our manuscript as a suggestion for a new solver, we acknowledge that the actual scope has to be made clearer in the introduction. Any feedback on how to make the scope and aim of this paper more understandable would be greatly appreciated. It is clear to the authors that the state-of-the-art in many if not most land surface models (e.g. Heise et al., 2003, Balsamo et al., 2008) is vastly different from the state-of-the-art in infiltration models used in the vadose zone community (e.g. Šimůnek et al, 2016). Yet, it must be kept in mind that these LSMs are run under operational constraints, with severe limitations on the execution times. This is well documented in the review by Vereecken et al. (2019) and we thank the reviewer for pointing us to this review. While Vereecken et al. (2019) touch on the subject of numerical implementation, they do not discuss numerical convergence analyses, and nor the role of such analyses for establishing the magnitude of respective numerical errors. We thus still believe that systematic convergence analyses of the Richards equation yield added value for the discussion on how to treat the infiltration problem in LSMs. Moreover, the interaction of numerical errors in the solution to Richards equation with precipitation intensities in the context of weather and climate modelling has so far received little attention and are this topic is thus novel for the LSM community. Systematic analyses of numerical errors give us a better baseline for the decision, what to spend the limited execution time on.

In the following, we will address the individual points raised by the reviewer. The reviewer also mentions flaws that they pointed out in hand-written annotations in the manuscript (see supplement to RC3). There is a considerable overlap between the points raised directly in the review and the hand-written annotations in the supplement. We will thus try to address the hand-written comments, where they are not typos/ formal issues and where we feel that they do not correspond to a point raised in the review and mark the corresponding answers with (IA) for ‘inline annotation’.
Yes but the feedback to the atmosphere will take place on hourly to seasonal time scales. This is what the weather/ climate modelling community is mostly concerned with.

Here we mean grid spacing.

We think that this will become clear from the experiments, but we could say fast increase in layer thickness in this case means exponential increase in layer thickness as is the case for geometric grids (Table 3).

The solver in the LSM will not change when changing the horizontal resolution of the LSM. But in a coupled setup, the LSM will be subject to a different precipitation forcing. This stems from (a) the effect of smoothing of the simulated precipitation in a coarser atmospheric model and (b) the effect of the parameterization of convection. While parametrized convection in large-scale models tends to produce more drizzle and less high-intensity precipitation, in kilometer-scale models the convective precipitation is simulated explicitly leading to more high-intensity precipitation. Both effects are further explained in the introduction and disentangled in chapter 4.3.

The equations of a dynamical core in the atmosphere are strictly based on conservation laws (mass, momentum, energy) that are described by some simplification of the Navier-Stokes equations. We do not have an according set of equations to describe conservation laws for the land surface. Therefore, land surface modelling (along with other sub-gridscale parameterizations in weather and climate models) is subject to approaches that are more heuristic. While this is reasonable given the complexity of the matter, it makes the interactions of the equations at hand (and their numerical representations) harder to understand and quantify.

We deliberately choose an implementation of Richards equation that is used in a weather and climate model (Schlemmer et al., 2018). Depth dependent profiles of hydraulic conductivity may be found in similar forms in other LSMs (e.g. Decharme, 2006; Ducharne, 2016). We fully agree that infiltration processes are often poorly represented in weather and climate models. We are however convinced that the solution to the governing equations must be represented accurately by the solver, and to this end convergence studies such as the one presented in our manuscript are important and should be carried out complementary to the implementation of additional processes and a better process representation.

Again, this is mostly a problem of the forcing precipitation data in the coupled model. Here we are specifically concerned with the infiltration process in weather and climate models, where rainfall intensities are not solely subject to spatial aggregation (i.e. the size of the grid cell or LSM tile in case the rainfall is disaggregated), but also the way precipitation is represented. Mesoscale weather models and regional climate models are often operated on the kilometer-scale, where convective processes need not to be parameterized in contrast to global models. There are marked differences in precipitation intensities between so-called convection permitting (or convection resolving) models and models with parameterized convection processes (Ban et al., 2014). The scaling of hydraulic properties such as hydraulic conductivity is beyond the scope of this paper, but we agree that the subgridscale heterogeneity should urgently be addressed in future research. Thank you for pointing us to the reference.

We fully agree with the reviewer that the mixed form of Richards equation should be used and we are currently revisiting our analysis for the mixed form. However, as can be seen in Vereecken et al. (2019), Table 2, four out of seven of the listed LSMs use the diffusivity (saturation) form of Richards' equation. Moreover, five out of seven of the listed LSMs generate surface runoff when the precipitation rate exceeds saturated hydraulic conductivity. Therefore, a convergence analysis on such an implementation is of practical
relevance to the LSM community.

Lines 86-87 and 88-92: There are ongoing attempts to improve the representation of hydrological processes along several different pathways. None of those will be efficient on its own. We agree with the reviewer that the infiltration process is a particularly critical one, and it must first be represented accurately by the corresponding governing equations and their numerical representation. Otherwise errors in the dataset will interact with physical and numerical errors, which hampers LSM development.

Lines 92-93: The lack of awareness in the LSM community – or, more broadly speaking, the weather and climate community – is precisely the issue we want to address with our manuscript. This would allow for a discussion on ways forward. We agree with the reviewer that this 'knowledge gap' should be closed in order to foster the collaboration amongst hydrologists, soil physicists and LSM modelers.

Line 100: Variations in time-to-solution is problematic in the operational context of weather and climate modelling. This might at least partially be the reason, why parts of the LSM community are hesitant to adopt such approaches.

Line 133 and line 150 (IA): In TERRA ML (Heise et al., 2004, Schlemmer et al., 2018), E is a sink term in the root zone. We agree with the reviewer that it would be more elegant to include transpiration in the suction head (if this is what they mean). However most LSMs are simply not that far in their evolution yet.

Line 146 (IA): Q is a sink term that should emulate horizontal transport in complex terrain to some extent. We follow the implementation of Schlemmer et al. (2018). While this approach is of course very heuristic, it keeps the LSM one-dimensional and it is computationally efficient.

Benchmarking LSMs against infiltration models is an excellent idea and we are very open to this suggestion.

Subsection 2.2.1 “The system under consideration”: We are open to omit this section. We believe however that it nicely illustrates the problem with vertical resolution. If one compares the sharp moisture gradient across the wetting front with the vertical discretization in state-of-the-art LSMs, the issue as well as the effect on infiltration should be immediately clear.

Subsection 2.2.2 "Soil hydraulic model" (line 159): The values are adopted from the ORCHIDEE LSM (Ducharne, 2016). We are open to revisiting the analysis with different values, but it will likely not change the results of the convergence analysis tremendously.

Subsection 2.2.3 “Ground Runoff”: The parameterization of ground runoff in our version of TERRA ML (Heise et al., 2003) is described in Schlemmer et al. (2018). In contrast, both saturation excess and infiltration excess are included in the surface runoff. In our case it would correspond to a saturation of the upper soil and an exceedance of saturated hydraulic conductivity respectively.

Subsection 2.2.4 "Boundary condition": The answer to this point is partially included in the answer to the point raised concerning using the mixed form of Richards’ equation (line 82). As pointed out above, five out of seven of the listed LSMs in Table 2 of the review by Vereecken et al. (2019) generate surface runoff when the precipitation rate exceeds saturated hydraulic conductivity. While this approach is likely not valid for very dry soils, it is still of practical relevance as this assumption is included in many if not most LSMs and it is probably also a fairly good approximation for wet and moderately dry soils (see also Ogden et al., 2017). However, we agree that it is necessary for the LSM community to
transition to the mixed form of Richards’ equation and to revisit the formulation of the boundary condition.

Line 196: We agree with the reviewer that virtually all LSMs use non-equidistant grids. In Line 196 we state that most LSMs use a staggered grid. In the manuscript we treat the equidistant grid first to further strip the complexity of the solver. We do not stress the implementation of a non-equidistant grid (again, this is a community standard). However, we stress the coordinate transformation as it can be shown that a naïve finite difference discretization for non-equidistant grids leads to the introduction of first order numerical errors (Sundqvist and Veronis, 1970). In practice, we found these errors to be negligible compared to the errors introduced by inadequate grid spacing, which is why we do not discuss this issue any further.

Line 199: Indeed, in order to resolve the gradients in space, thin layers are required. Yet, as the two are dependent on each other (CFL criterion), this enforces the use of a small timestep to ensure numerical stability, or use of an adequate numerical method such as an implicit solver.

Subsection 3.2 “Experiment A”: This whole manuscript is on numerical convergence. Of course, this does not imply that the underlying physical processes are represented correctly. This is not the scope of a convergence study.

Line 273: In the case of a fully saturated soil column, the infiltration is further limited by the outflow at the bottom of the soil column, which is in our case given by the parameterization of Q. $\Theta_f$ is relative moisture content defined according to the equation in Table 2.

Subsection 3.3 “Experiment B”: We believe that the description of the forcing is important in this context as it directly relates the numerical errors to different setups that are typical for weather and climate modelling. In our opinion, it is important to show that in a practical context, numerical errors tend to aggravate with precipitation intensity because of the current evolution of weather and climate models towards higher resolutions. The different precipitation time series are chosen, such that they represent a typical example in the transition from convection parameterized to convection resolving models with the associated change in precipitation intensity.

Subsection 4.1 “Analytical Solution” (IA): In the presence of a sink term (Q), it is possible to define a zero-flux bottom boundary condition for K (Schlemmer et al., 2018). In a fully saturated soil, Q is then balanced by infiltration I. This balance controls the magnitude of the flux (i.e. K as $D=0$) in the soil column. This equilibrium may only be established, if the solver is mass conserving.

References


exponential profile of saturated hydraulic conductivity within the ISBA LSM: simulations over the Rhône basin, Journal of Hydrometeorology, 7, 61–80, 2006


