Review of Brandthorst et al. 'Coupling saturated and unsaturated flow: comparing the iterative and the non-iterative approach'

We thank the reviewer for the effort and time to revise our manuscript and in the following want to respond in detail to the constructive comments he provided.

Major comments.

The contribution of the paper is purely computational. This can be seen from the test cases, all of which are highly artificial. I do not consider this a problem. The paper is focused, does not claim more than it delivers, and what it delivers is relevant for the HESS readership and substantial. The paper is well organized and generally clear. In the minor comments below I indicate where I was not sure if I understood everything.

We thank the reviewer for this rating. We agree that the test cases are entirely artificial. The aim was to apply and compare the methods under fully 'controllable' conditions. Evaluating the performance of the models in a more realistic setting or even a real-world case would be interesting work for the future.

Minor comments.

The conclusion in the abstract that the more elaborate iterative coupling is better and more robust but slower than the non-iterative coupling is a bit underwhelming. Perhaps being more specific would make it more informative.

We propose to reformulate this sentence to: "The non-iterative approach is faster but does not yield a good accuracy for the model parameters in all applied test cases whereas the iterative one gives good results in all cases. Which strategy is applied depends on the requirements: Computational speed vs. model accuracy."

The overview in the Introduction of the various coupled unsaturated-saturated models, their coupling strategies and the role of the specific yield is insightful and thorough.

Thanks.

l. 124- 131. If you assign the same 1D unsaturated zone columns to multiple grid cells of the underlying 2D groundwater model, I believe you are making the implicit assumption that the weather is uniform over the 2D extent of the groundwater level, or at least over the regions assigned to each of the 1D columns. If this is indeed the case, it may be good to mention this.

Yes, this is the case. We will change l.127-129 to "The atmospheric forcing of the unsaturated zone model as well as the recharge that is calculated from the unsaturated zone model and passed to the groundwater model are assumed to be constant within each zone. "

l. 156-173. If I understand correctly, if the resizing of an unsaturated zone column shortens the column, you have to add the storage in the part of the column that is cut off to the amount of water in the saturated zone to conserve mass during the resizing operation. When you increase the size of an unsaturated column, some water is transferred from the saturated to the unsaturated zone. I believe this is done in steps 4 and 5. In that case, I believe the ratio r should normally be equal to 1 to ensure mass conservation. You give several reasons to deviate from this. Equation (6) gives the expression for r you used, capped at 1. If you consider the entire system, does capping r at a maximum of 1 whilst allowing values smaller than 1 not necessarily lead to mass losses when resizing all unsaturated columns?

Incidentally, for this reason I do not understand why Erdal et al. (2019) set r to 0 instead of 1, but I am not reviewing that paper here. But it seems to indicate that I missed something here, unless a shortening/lengthening of an unsaturated zone column led to a corresponding increase/decrease in its volumetric water content to keep the total amount of water present in the column unchanged.

Indeed, the r=0 assumption is questioned by us as well, and that is why the new ratio is used. One needs to consider that the models are not strongly coupled. Setting the ratio to 1 causes a lot of numerical problems following the one-step-behind adding of the lost water. An example is oscillating groundwater tables with increasing magnitude. We therefore settled for the rather ad hoc solution presented in the manuscript. As the capping occurs both on elongating and shrinking of the unsaturated zone, it will have both a positive and a negative effect on the total water in the system, so the comment of the reviewer is not quite right. We see that this was not well explained in the original submission and it will be improved in the revised version. In particular, we will write explicitly that a factor r=1 would be needed to keep strict mass conservation.

Perhaps it is possible to have a 'before and after' figure, table, or water balance for the saturated and unsaturated domain (1 column only) in which you track what amounts of water go where during resizing.

While this is in principle an attractive idea, it should be noted that the non-iterative model, which, as the reviewer also points out, is taken from a previous publication on speeding up model spin-up (Erdal et al 2019), in this work serves mainly as a comparison to the iterative one. It is a conceptually quite ad hoc, but very fast model. Hence, we do not wish to spend more work on the inferior model, but rather focus on the much better performing iterative coupling, and we hope the reviewer can understand our point of view.

l. 158, 221. What is point i? Do you mean point 1?

Yes, it should be point 1. This is an artifact from the previous template using roman numerals.

Figure 2. Perhaps use a different color for the horizontal line denoting the phreatic level. (Minor point, please don't bother if this takes too much time.)

This is a good suggestion. We will change it.

l. 203. Equation (10) forces the specific yield to conform to the simultaneous gain/loss of water and the resulting change in in the groundwater level. Essentially, the specific yield is no longer a model variable but the ratio of the calculated water gain per calculated groundwater level change. The water level change is calculated using the 1D Richards' equation, which does not have the specific yield as a parameter. You thereby to a certain extent impose the unsaturated zone model upon the groundwater model and adapt one groundwater model parameter (the specific yield) to match both models. Interesting approach.

Yes, this is exactly what we do and what to our knowledge has not been tested before. We think that the approach is well explained by the reviewer and will explain it this way in the revised manuscript.

l. 226. 'constant' or 'spatially uniform'?

We mean "spatially uniform". We will change this.

Figure 3. I was expecting a yes/no decision at the closure criterion, looping back to the start of the iteration in case of 'no'.

Right, this is missing. We will add it.

l. 271-272. You state that you have hydrostatic equilibrium as the initial condition (zero flow), but in the next sentence state that you state that in most of the profile, the matric potential is vertically uniform, which amounts to a profile with unit gradient flow.

We agree that the way it is written is misleading, we will change the two sentences to: "The initial condition assumes a hydrostatic pressure profile in the lower 6.5m of the soil corresponding to an initial position of the groundwater table at 3.95m below the surface. In the upper 3.5m of the soil unit gradient flow is imposed by setting the pressure head to -0.283m. "

l. 281-282: repeats l. 276-277.

Not exactly. L.281 – 282 refer to the 1D reference model and l .276-277 to the coupled model. We will make that clearer.

l. 325. Why did you assume uniform distributions for the van Genuchten parameters or their logarithms?

We did that because we want to test over the entire physically plausible range of these parameters. We could also use a normal or lognormal distribution, but this would to a certain degree be like implying knowledge about the soil type. We will change the sentence to "The parameters included in the sensitivity analysis are listed in Table 3. They are sampled from uniform distribution functions ρ to cover their entire physically plausible range. Table 3 also shows the limits of these ranges. "

Table 4. To increase the readability of the Table independently from the text I suggest to clarify that the calculation times are for a PC in the first two cases and a supercomputer in the third case.

We will add this information.

Section 4.2 Could there be an effect of the way you calculate *r* (Eq. (6)) on the accuracy of the non-iterative 2D model?

Yes, we agree with the reviewer that part of the mismatch with the non-iterative model could be an effect of our r-calculation method. As pointed out above, the method is rather ad-hoc and better ways may be possible, especially during the first coupling timesteps. Our used way of calculating r has though been showing the best overall performance in our test cases, which is why we use it. In the revised manuscript, the discussion about both the role of the non-iterative method as a comparison method rather than a suggested method, and our choice of r calculation method will be discussed in more detail.

l. 394. The spread in values of the specific yield is well beyond the plausible range. But since the specific yield is not really a parameter in your approach, I am not sure if that should be worrying or not.

In our opinion this is not a problem, which we try to explain in l. 469ff. Also, since this approach is novel there is no data, we could compare it to.

l. 407-407. I recommend that you mention the fact that the model is not designed to handle overland flow when you describe the models in section 2.

This is a good suggestion. We will add "The coupled model is a simplified subsurface model that can model flow in the groundwater and in the vadose zone, but no overland flow. It consists of a 2D depth averaged model for horizontal groundwater flow (Eq. 1) and multiple 1D models for vertical unsaturated flow (Eq. 2)." at the beginning of Section 2.2.1.

l. 461-465. You implemented unit gradient flow in the top of the profile and no flow (hydrostatic equilibrium) in the lower part of the profile, with an abrupt boundary between the two. In the first time step, this creates a rather hectic situation at the interface between these two regions, where Richards' equation needs to smoothen the transition and create some flow in the area with initially stagnant water. I can imagine this causes an initial jump in the importance of the van Genuchten parameters.

This is a good explanation. We will change l.464-465 to "This is most likely due to the rather artificial initial condition in the unsaturated zone which enforces unit gradient flow down to 1.25m above the groundwater table and no flow below."

l. 481-487. In your approach, the specific yield is no longer even a model parameter. The discussion of its physically impossible values of the specific yield is OK, but the rest of the paragraph is a bit contrived.

We partly agree and partly disagree with the reviewer here. The part was also commented on by reviewer 2. We will reformulate this paragraph and change it to:

"In the end, the specific yield is not a physical quantity here but a model parameter as it needs to compensate the influence of the lateral fluxes on the recharge, which cannot be quantified properly in this model, as well. Therefore, it should be treated as such and fitted for each application. This is a general problem, that does not only concern this model, as there is no good way of determining this parameter properly in advance. Calculating it within the iteration substitutes its calibration and is therefore an advantage over other methods. The sensitivity analysis for the parameters' influence on the calculated specific yield shows that it behaves reasonably. It depends on the parameters of the unsaturated zone models, especially on porosity ϕ and the saturated hydraulic conductivity K_{UZ}. The aim of the specific yield is to represent the missing unsaturated zone in the groundwater model, therefore a strong dependency on the unsaturated zone model's parameters is plausible."

l. 503-504. Unfortunately, the saturated hydraulic conductivity is also the parameter with the largest degree of spatial variation.

This is true but it needs to be estimated for the groundwater model anyhow.