

Interactive comment on “Coupling a groundwater model with a land surface model to improve water and energy cycle simulation” by W. Tian et al.

Anonymous Referee #1

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The authors couple a 1D soil and plant growth model with a 3D variably saturated groundwater model. This is a laudable work as this type of consistent two-way coupled models is of relevance for the community in future research work especially when the impacts of land management changes or climate change are to be assessed. In the coupled model the lower boundary of the 1D soil model is given by the more regionally determined groundwater table while the recharge to the groundwater model is strongly influenced by plant water uptake in the 1D soil column. The stepwise exchange of information between the two models allows for a more consistent, self-contained modelling. The work is not innovative as coupled models exist already (e.g. MIKESHE by DHI, Hydrogeosphere by Therrien et al.). But they are either commercial or only executables are available. Here generally available open software components are used and coupled.

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There are a number of points of criticism which require revision. If addressed the paper could make a good publication in the end.

The paper claims that the model has been validated. This is incorrect. The model results have been compared with measurements at three sites. This involved a model calibration at those three points which is not specified clearly in the paper. No assessment of the model outside of these three sites has been made.

The coupling of the two models is awkward as the 1D-soil and plant growth model based on Richards equation is coupled to an unsaturated-saturated 3D groundwater model based also on Richards equation but in a different formulation. The paper sees this problem and addresses it. Presumably plant roots can only occur in the 1D-soil model reaching down in the applicatin to 5 m depth from the soil surface. So what about phreatophytes whose roots are much longer and extend to the groundwater table? A suggestion would be to adapt the depth of the SiB2 to the time-averaged depth to groundwater in each model cell.

The comparison of the SiB2 model with GWSiB leads to an obvious and trivial result which could have been reached with much less effort: Different boundary conditions at the lower end of a soil column lead to different fluxes of water. If the water table is shallow and reaches the bottom of the SiB2 soil column it will enhance evapotranspiration. A single column would have been sufficient to show this effect.

A regional comparison of coupling versus not coupling should rather use boundary conditions in SiB2 which reflect the depth to groundwater. As the groundwater table is changing slowly, the “uncoupled” case (SiB2 only) could use a long term average depth to groundwater to formulate the lower boundary conditions. The fully coupled case should then use the dynamically developing groundwater table as it is done in the paper. In such a comparison the results will differ much less and show whether it is really necessary to do the dynamic coupling over time.

One should not overestimate the accuracy of the model. The groundwater model in

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itself is rather uncertain. It depends on inadequately known hydraulic conductivities, porosities and other soil parameters. So the position of the groundwater table is not only influenced by the more consistent recharge flux in the two way-coupled model but also by the aquifer properties. Nothing is said about the regional performance of the groundwater model. Measured and computed groundwater tables should be given at least in the three sites. The depth to groundwater is not only influenced by the computed groundwater table (the accuracy of which is not given in the paper) but also by the DTM, which maybe wrong by up to 5 m. That is the depth of the whole soil zone.

As far as the overall accuracy of a regional model in computing evapotranspiration is concerned one should remember that the soil-plant model opens a can of new parameters in addition to the aquifer parameters which are also not adequately known over the whole area.

Two-way coupling seems only necessary for shallow groundwater table areas. So a lot of effort could be saved by having two way coupling only in zones with depth to groundwater smaller than say 3 m.

The horizontal (3 km) and vertical (1.6 m) discretizations are coarse. It is not shown whether the computation results are grid convergent. A doubling of resolution is recommended to check whether changes remain small.

The regional inaccuracy of the inputs is lower than that at the 3 locations chosen for validation.

In the three locations, the model shows that evapotranspiration is different in the two approaches as expected, when the groundwater table is shallow. In that case SiB2 is performing better than GWSiB. For a deep groundwater level, however, there are no differences.

In all sites the computed and (best) simulated evapotranspiration should be shown in a scatterplot in order to clearly see the correlation between the two items, which I suspect

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is not that good.

In the second site, the model seems to react to the rain before the rain has started. This should be checked.

In the third site evapotranspiration seems often strongly overestimated. Soil moisture in the model recedes much faster than in reality. I guess some tuning of storage related parameters could improve the result.

The English language of the paper could do with some polishing.

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