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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Weak sensitivity of the terrestrial water budget to global soil texture maps in the ORCHIDEE land surface model" *by* Salma Tafasca et al.

Anonymous Referee #1

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Review of the manuscript "Weak sensitivity of the terrestrial water budget to global soil texture maps in the ORCHIDEE land surface model" by Tafasca et al. [hess-2019-305].

GENERAL COMMENTS

The paper by Tafasca et al. uses the ORCHIDEE land surface model to test the effect of using different soil texture maps on the water budget at the global scale and concludes that, given the similarities between the tested maps, the choice of input soil texture map is not crucial for large scale modeling (compared to the bias due to the choice of, for example, meteorological forcings). I think that the study of the impact of biases in the estimates in soil properties on water and energy budgets is of great importance for

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Discussion paper

assessing the accuracy of LSM simulations as well as to guide their parameterization. However, the manuscript by Tefasca and co-authors, in its current form, lacks a clear definition of its objectives and novelty. Most of the paper is devoted to showing the correct performance of a widely used model in modeling rainfall partitioning in different soil texture – this seems more a reality check for the model, than a novel analysis. The model is then used it to conclude that, given the similarity of the tested maps, at the scales under consideration the resulting bias in the hydrologic response is negligible (a result that could have been guessed without performing heavy numerical simulations). I believe the manuscript would better benefit from a more detailed (and quantitative) analysis of the relationship between the soil input bias and resulting hydrologic bias across scales, as detailed below.

MAJOR COMMENTS:

1. It is not clear what the novelty and the overall goal of the paper is. As it stands, it seems more of a modeling exercise using different soil maps, but without a clear scientific objective being proposed.

2. The authors use soil texture maps that are similar and conclude that they give similar results. If the soil maps are indeed not too different, how could the authors expect to observe any difference in the results (especially in terms of global fluxes where the main local differences are averaged out)? Along these lines, while the global/average water budget is similar, how different are the extremes (i.e., where the maps actually differ, what is the bias in the results)? In these terms, I think that a more detailed analysis of the biases induced in those areas where the maps differ would be more useful.

3. Lastly, at what scales do local differences in soil texture maps and the associated fluxes start to differ substantially? Can the authors define thresholds in these terms?

MINOR COMMENTS:

Abstract:

- Lines 10-11: “Here, we investigate the impact of soil texture on soil water fluxes and storage at global scale”. What is the novelty here? The impact of having different soil texture (clay vs. sand) on infiltration/runoff partitioning is well known and a large scale application only seems a modeling exercise without added scientific value. I think the abstract (and paper) would benefit from the definition of a more precise research question/objective.

Introduction:

- Line 35: SoilGrids database is available at higher resolution too (250 m).

- In general, I think the Introduction lacks some clarity: It is not clear whether the focus here is on testing the LSM at the global scale, or on the effect of PTFs, or on the comparison of different soil texture maps. The paper would largely benefit from a more detailed introduction where the novelty and the goals of the paper are clearly defined in relation to state of the art knowledge on the subject.

Methods:

- Lines 66-67: at what depth are the soil texture maps? SoilGrids provides, for example, texture properties at different soil depths - why are the authors assuming an exponential decrease of Ks instead of evaluating it from textures at different depths?

- Lines 67-68: please provide a reference for both the exponential decrease with depth and the exponential distribution horizontally.

- Line 70: please provide references for the evapotranspiration model.

- Line 91: what is the error due to selecting only the dominant soil texture? Did the authors investigate the effect of upscaling by using some average (or weighted average) soil properties?

- Line 133: “network owing to machine learning” – please rephrase.

Results:

- Lines 133-135: the partitioning of rainfall in infiltration (soil moisture) and runoff differs among soil textures in a way that is well known and studied - I don't see the novelty here. Are the authors simply testing the model?

- Lines 153-154: "Switching. . .variables". If the maps are similar a priori, why would the authors expect any differences in the global water budget? It would probably be more useful, in my opinion, to focus on those areas where the maps are actually different and discuss the resulting biases in the hydrologic response in those areas.

- Lines 170-177: the results discussed here could have been expected without running massive simulations: the partitioning of rainfall in infiltration and runoff with different soil textures is well known. The exercise here seems more of a reality check for the model than some novel analysis.

- In general, I think the paper lacks a proper quantification of the differences between the soil texture maps and the related bias in simulated fluxes. If the authors could provide a clear quantitative link between the bias in soil maps and the resulting bias in hydrologic partitioning this would actually allow to extrapolate something from the analysis. As it stands, the analysis only seems a modeling exercise without any useful application. I believe it would be more impactful if the paper could provide answers to questions like: how much does the hydrologic response (e.g., runoff, infiltration, etc) change if the soil texture differs by a certain percentage? How do the probability distributions of the water budget components vary with the distributions of soil texture?

- The authors showed that hydrologic fluxes are more sensitive to changes in climatic forcings rather than soil texture maps. But how different are the climatic forcings used compared to soil texture maps? If the bias in the climatic forcing is, a priori, much higher, it is likely that the resulting hydrologic behavior will differ more.

Conclusions:

- Lines 197-198: the fact that the model has a realistic behavior should not be a main result. The orchidee model has been widely tested, and its ability to reproduce hydrologic fluxes properly in relation to different soil textures is not a novel result.

- Lines 210-212: What is the point of using spatially similar maps to see if they have any discernible effects on the hydrologic fluxes? If, a priori, the maps are similar, what is the point of the entire exercise?

- Line 214: Did the authors try to test some weighted average SHPs thus accounting for spatial variability instead of using the dominant soil texture in each cell?

- Line 225: A detailed analysis of the difference between the various maps should be given upfront. This only appears with Fig. 8 but it would be beneficial to have an in depth analysis of key differences among these maps (as well as of differences resulting from adopting different strategies for upscaling the higher resolution maps) at the beginning of the manuscript.

- The clay bias that is only briefly discussed in lines 225-230 seems actually a quite interesting point. If the prevalence of loamy texture in the texture maps is – in part – an artifact due to upscaling procedures and averaging, what would the bias be in the hydrologic partitioning if the actual texture in some grid cell was not as loamy as assumed?

- Lines 239-240: Some products (e.g., SoilGrids) have vertically variable information on soil texture – why didn't the authors use this information to relax the hypothesis of vertically homogeneous texture?

- Lines 236 – 244: most of the paper focused on soil texture, while only two PTFs were tested. Why is the conclusive paragraph of the manuscript on PTFs and inclusion of additional factors in currently used PTFs, while the manuscript only slightly touched this point? Although this is an interesting topic, I wouldn't embark into a discussion on PTFs at this point of the manuscript (as the authors didn't actually do an in depth

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analysis of the bias induced by different PTFs).

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