Hydrol. Earth Syst. Sci. Discuss., 10, C7745–C7754, 2014
www.hydrol-earth-syst-sci-discuss.net/10/C7745/2014/
© Author(s) 2014. This work is distributed under
the Creative Commons Attribute 3.0 License.

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Do land parameters matter in large-scale terrestrial water dynamics? – Toward new paradigms in modelling strategies" *by* L. Gudmundsson and S. I. Seneviratne

L. Gudmundsson and S. I. Seneviratne

lukas.gudmundsson@env.ethz.ch

Received and published: 4 February 2014

Reviewer #5 raises several interesting questions regarding some assumptions underlying our manuscript. In the following we provide a point by point reply to the reviewer's comments. For the sake of clarity we first repeat the reviewer's comments (*in italic*) and then provide our response.

Comment 0: *The original premise of this manuscript aligns with the conceptual versus physical model paradigm. However, there are too many underlying assumptions and failed evaluation that lead to at least major revisions and probably a complete rethinking*

of the manuscript, its assumptions and conclusions.

Below are the main issues that need to be addressed:

Reply 0: We appreciate the constructive criticism of Reviewer #5 and clarify here the concerns mentioned below.

Comment 1: Time scale *The entire premise of this manuscript is based on the assumption that the monthly time step and 0.5 degree spatial resolutions describe the need for heterogeneous land parameters. However, it is quite clear that there are many important processes (surface runoff, evapotranspiration, ...) that although at the monthly time step will average out a lot of the daily and sub-daily signal, does not mean that heterogeneous parameters do not matter.*

Reply 1: Although the scope of the presented study is modelling "large-scale terrestrial water dynamics" (cf. title), we fully agree with the fact that many interesting aspects of terrestrial water dynamics are governed by processes occurring on small spatiotemporal scales. It is, however, not clear to what extent these processes dominate the phenomena that are targeted by models operating at continental to global scales, often in a climatological context. These models are receiving increasing attention, as changes in terrestrial water availability are relevant for ecosystems as well as human activities on climatological time scales. Consequently there is a need for building models, that are applicable for large spatial domains and can efficiently been run covering long time periods (see also our response to Reviewers #1 & #2).

To clarify the scope of the article decided to remove the subtitle and have changed the title to: "Do land parameters matter in large-scale monthly terrestrial water dynamics?"

(see also general answer to all Reviewers).

Comment 2: Comparison to LSMs *The authors compare a machine learning algorithm (purely statistical) to semi-physical based LSMs. From the manunscript, it appears that no prior calibration was performed on the LSMs and instead the trained* *random forest was directly compared to the LSMs. This is quite misleading. For this comparison to be "fair", the LSMs should also be trained.*

Reply 2: The reviewer's comment highlights that we failed to communicate important aspects of our study clearly enough and have now expand the article along the following lines:

1. It is important to note that the comparison of the machine learning model with the LSMs does only constitute one part of our analysis. As clearly written in the manuscript (p. 13202, l. 5 - 13) we confront the CLPH-RFM by (Equation 9) with an alternative statistical model taking land parameters into account (Equation 10). The results (p. 13205, l. 9 - 14, Figure 6) show that the skill of both statistical models is not distinguishable.

2. It is true that it remains an open question whether the comparison of calibrated models and un-calibrated models is fair. However, calibration would render the rigorous physical interpretation of LSMs difficult (cf. equifinality; p. 13198, l. 25 ff) putting them in a category comparable to machine learning tools.

3. In addition it is important to note that the skill of the statistical models was only quantified at locations that were not used for model training (cross validation in space, p. 13202, l. 8 - 16). This procedure results in conservative estimates of model skill, making the comparison with LSMs as fair as possible.

Comment 3: Paper Structure *The only sections that seem ok are the abstract and introduction. The section on scaling takes up too much space for a subject that could be discussed in a sentence instead of a significant part of the manuscript. The results in the figures are not adequately discussed and instead just simply stated. Finally, the appendix it too convoluted and with too much information. The paper would need to be re-synthesized for publication.*

Reply 3: We thank Reviewer #5 for the detailed feedback regarding the structure of the paper. The manuscript has been revised to make its overall structure clearer for the reader. In the following we go step by step through the detailed comments.

Comment 3.1: Abstract *Unclear which processes and parameters to include? I don't think that is accurate. I think it is more appropriate to emphasize that we don't fully understand the underlying processes and are unable to accurately define the parameters to include*

Reply 3.1: We thank Reviewer #5 for this suggestion and will consider rephrasing.

Comment 3.2: line 11-12 *This might be true at the monthly time step over a coarse grid, but it really depends on the objective.*

Reply 3.2: The scale dependence of hydrological processes and the related phenomena is an important part of our argumentation (See response to Reviewers #1 & #2 and Section 2 & 3). Please note also that we repeatedly emphasise the "large-scale" and "monthly" focus of the study in the manuscript, starting with the title. We now also explicitly mention in the conclusions that the suitability of ignoring small-scale processes applies to the considered spatial and temporal scales and depends on the considered objectives (i.e. no focus on high-resolution processes).

Comment 3.3: Introduction line 25 *I don't think that "relate" is the appropriate verb here. I would replace it with simulate. Relate implies an empirical relationship which is not what most land surface models actually do.*

Reply 3.2: We thank Reviewer #5 for this suggestion and will consider rephrasing.

Comment 3.3: line 14-15 *I would be very careful in stating the exact grid cell dimensions. Land surface models are not specific to a certain resolution. As long as you incorporate the appropriate processes, you can use land surface models at multiple scales. Given that in this study you are mainly concerned with coarse grid, I would just make sure you emphasize the "macro scale" component of the land surface modeling* *you are discussing in this paper.*

Reply 3.3:

1. We refer to model resolutions that are currently used for continental to global scale applications in a climatological context. We clarify this point in the revised version of the manuscript.

2. As highlighted in Section 3 (Equations 5 & 6) it is not clear if the process descriptions used in the current generation of LSMs are scale invariant, i.e. that it suffices to change resolution (See also reply to Reviewer #1). It is indeed an assumption but our results point to the fact that this leads to superfluous complexity at scales that are relevant for state-of-the-art Earth System Models (typically 200km or more).

3. We repeatedly emphasise the "large-scale" focus of the study, starting with the title.

Comment 3.4: Separation of Scales *It is unclear when you read the paper for the first time why this section even exists. The main emphasis of this paper is to determine the need for spatially heterogeneous land parameters but yet you spend time discussing the time scaling of soil moisture? Please explain.*

Reply 3.4: We thank Reviewer #5 for highlighting the fact that the importance of this section may not be clear on first reading. We will address this issue in the revised manuscript.

Comment 3.5: Section 4.1.1 *Althougth the monthly time scale might work to understand the impact of land parameters on GCMs, it definitely does not explain the sub-monthly time scale. For example, LSMs act as boundary conditions for numerical weather models at the hourly scale. You state that this is an assumption, however you*

fail to go into detail on how this assumption drives all your conclusions. This needs to be addressed.

Reply 3.5: We discuss the expected effects of spatial and temporal resolution in great detail in Section 2 & 3, where we develop the CLPH (p. 13199, l. 7-22). We also repeat the expected scale effects on p. 13200, l. 17f where we introduce the resolution of our analysis with: "Spatial and temporal resolution are chosen to be well above the space and time scales at which land properties are expected to have a dominating influence on terrestrial water dynamics (Fig. 2)."

It is however true that the expected effects of the considered spatiotemporal scales did get out of sight in the last part of the article. Therefore we have expanded this discussion, focusing on the effects of spatial and temporal resolution.

Comment 3.6: Section 4.2.1 *Again, it is not surprising that the model does well at the monthly time step against evaporation from LandFlux. At these scales, the daily and sub-daily processes are averaged out and you are relying on seasonal and annual variability. Given that at these scales, net radiation and rainfall will be the main drivers (atmospheric forcing), this result is not surprising.*

Reply 3.6: In Section 4.2.1 we simply report on the ability of the CLPH-RFM to capture several aspects of terrestrial water dynamics: (i) runoff at grid-cell scale, (ii) discharge from continental scale drainage basins and (iii) the spatial pattern of mean annual evapotranspiration. This section is meant to provide insights to the strengths and shortcomings of the CLPH-RFM , in particular with respect to variables that have not been used for model training ((ii) discharge from large drainage basins, (iii) longterm mean evapotranspiration).

These results highlight the general validity of the CLPH at the considered spatial and temporal scales, also for variables that were not used for model calibration. The results may not be surprising if you consider the scale relationships we discuss in the article. In our opinion it is, however, surprising that that our results suggest that the respective processes could possibly be represented in a parsimonious way at these spatiotemporal scales. Especially in light of the fact that it is usually assumed that very complex LSMs are needed to capture these patterns.

Comment 3.7: Section 4.2.2 *This comparison is misleading. The purpose of heterogenous LSMs is to include available datasets to depict the heterogeneity of the processes and not need calibration. I understand that available datasets make this a large challenge. However, you are making a comparison of physical based LSMs to a pure machine learning algorithm. While they both have positives and negatives, they also have different purposes. Not stating those different purposes and instead doing a 1 to 1 comparison sends the wrong message.*

Reply 3.7: Please note that we did not only compare the CLPH-RFM to LSMs but also to an additional statistical model, taking land parameters into account (Equation 10). The skill of these two statistical models is not distinguishable (p. 13205, l. 9-10). This result does not allow to reject the CLPH as the alternative model would need to be significantly better.

See Reply 2 for a discussion on the comparison of statistical models and LSMs.

Comment 3.8: Section 5, p. 13206, lines 1-12 *I agree that the absolute values of the water balance equation will be highly correlated to the monthly atmospheric forcing. However, this is rarely (if ever) true at the sub-monthly time step. Although, I agree that the monthly average will only be affected by variables that depend at large time scales on the heterogeneous land properties, this still does not address the problem that we still need to depict the signal at the finer time scales to account for important hydrologic processes such as flooding, sensible heat, and latent heat at the daily to sub-daily scale.*

Reply 3.8 Please see Reply 1 for a discussion on the relevance of small scale processes, which are beyond the scope of the presented study.

Comment 3.9: page 13206, lines 13-22 *Actually, the land parameters used in macro scale LSMs come from upscaled (mainly by spatial averaging) of higher resolution product. This means that many processes in LSMs (e.g. richards equation) make sense at fine resolutions but are misleading at the coarse resolution. Combining high resolution modeling with available high resolution datasets (e.g. gSSURGO) will make the processes in current and upcoming LSMs more appropriate.*

Reply 3.9 Again we want to emphasise the large-scale focus of this study. That said, we are aware of the ongoing efforts on "hyper resolution" modelling (Wood et al., 2011) and follow this debated topic (Beven and Cloke, 2012; Wood et al., 2012) with great interest.

However, even if "hyper resolution" hydrological model will become feasible on a global scale, it is to date not clear whether this can also be achieved for long (climatological) time periods. For example, "hyper resolution" models will rely on "hyper resolution" estimates of atmospheric forcing. To date it is not foreseeable that such products, covering long historic records, will be available. (Current global estimates of atmospheric variables covering the past decades typically have a resolution $\geq 0.5^{\circ}$ and state of the art climate models typically operate at a resolution of $\approx 2^{\circ}$).

We have more text on this topic in the revised article to clarify the relation of our investigation to other ongoing efforts.

Comment 3.10: page 13207, lines 5-12 *I don't think that this statement says anything new from previous work. It is well known that an appropriate black-box model (i.e. random forests) when appropriately trained with enough data from representative regions will indeed beat in many cases physically based LSMs. However, this will only be applicable at the monthly time step and will fail to account for the evolution of the land surface over time and take account for variable climates. It is not surprising that the trained random forest worked well in this study, given that the climates used are similar. Taking this model and applying it over tropical regions would most likely lead* *to very different results.*

Reply 3.10 We disagree with this statement:

1. Typical applications of black box models in hydrology are focusing on individual catchments and the resulting parameters are assumed to reflect local land properties. The application of Random Forests in the presented study differs fundamentally from this approach, as the resulting model has only one parameter set and is applicable at any location within the domain of the study. We will highlight this difference in the revised manuscript.
2. The fact that no effect of slope and soil texture on monthly runoff dynamics in Europe (p. 13204, l. 9-11) could be detected suggests that these parameters (and their evolution over time) is only be of secondary importance. Possible influences of other land parameters where not assessed and remain a topic for further research, which we will highlight in the revised article.
3. Please note that the domain of the investigation covers different climate zones, ranging from dry and temperate climates in the south to cold, snow dominated climates in the north (although we agree that applying the model to another continent would likely not be successful because of the larger implied differences).

Finally we would like to emphasise that we do not suggest that statistics should replaced physics in modelling terrestrial water dynamics at large scales. However, our results highlight the fact that substantial model skill can be derived from atmospheric forcing (i. e. the availability of water and energy). Although seemingly trivial this has gotten out of sight in the current modelling practice.

References

Beven, K. J. and Cloke, H. L.: Comment on "Hyperresolution global land surface modeling: Meeting a grand challenge for monitoring Earth's terrestrial water"; by Eric F. Wood et al., Water Resour. Res., 48, W01 801, doi:10.1029/2011WR010982, 2012.

Wood, E. F., Roundy, J. K., Troy, T. J., van Beek, L. P. H., Bierkens, M. F. P., Blyth, E., de Roo, A., Döll, P., Ek, M., Famiglietti, J., Gochis, D., van de Giesen, N., Houser, P., Jaffé, P. R., Kollet, S., Lehner, B., Lettenmaier, D. P., Peters-Lidard, C., Sivapalan, M., Sheffield, J., Wade, A., and Whitehead, P.: Hyperresolution global land surface modeling: Meeting a grand challenge for monitoring Earth's terrestrial water, Water Resour. Res., 47, W05 301, doi:10.1029/2010WR010090, 2011.

Wood, E. F., Roundy, J. K., Troy, T. J., van Beek, R., Bierkens, M., Blyth, E., de Roo, A., Döll, P., Ek, M., Famiglietti, J., Gochis, D., van de Giesen, N., Houser, P., Jaffe, P., Kollet, S., Lehner, B., Lettenmaier, D. P., Peters-Lidard, C. D., Sivapalan, M., Sheffield, J., Wade, A. J., and Whitehead, P.: Reply to comment by Keith J. Beven and Hannah L. Cloke on "Hyperresolution global land surface modeling: Meeting a grand challenge for monitoring Earth's terrestrial water", Water Resour. Res., 48, W01 802, doi:10.1029/2011WR011202, 2012.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 13191, 2013.