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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

Anonymous Referee #1

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The title of this piece was very encouraging as the evaluation of the relative contribution to uncertainty of land surface parameterization versus model structure is certainly a hot topic in hydrologic science at the moment, but I was sorely disappointed by this manuscript which does not live up to the excellent reputation of the authors. Certainly the article does NOT move us towards new paradigms in modelling strategies and the assertions that physical process development in land surface/large scale models is not necessary is unfounded based on the provided discussion (and most probably at all). The manuscript is not helped in that it is not clearly formulated and difficult to follow in parts. The language and style is quite obtuse and the structure is very strange and

complicated. Why all the appendices? Where is the coherent story?

The most important point however is that nothing has been evaluated on temporal or spatial scales that actually matter. I do not understand how if you use spatio-temporal scales greater than where runoff dynamics are thought to be influenced by local varying land properties (fig2) you can say anything about tools that consider locally varying land properties. Surely it is totally obvious that the high spatial resolution processes will not have an effect. Here there is only evidence that the CLPH can provide a simple rainfall-runoff transformation of the precipitation at gridscale and at low temporal and spatial resolution, i.e. a large scale overview which does not consider storage or routing. This is not surprising or new. The comparison with observed has only been done against short residence times and there is no consideration of terrestrial water storage and longer residence times. Thus I find the conclusions are totally overstated. If CLPH-RFM can be used as a pragmatic estimator of continental scale terrestrial water dynamics, then so can the meteorological variables directly. Surely. What is the point? Why is the RFM appropriate to use here? This seems like a horribly overparameterized methodology, perhaps even rivalling the more complex hydrologic models and land surface models in terms of free parameters. Why do you need this complexity? This is not obvious and seems at odds with the aim to free modellers from uneeded parameterization. Also there can be no physical basis after the RFM has been implemented and this makes tracing back processes impossible. Why is this a useful method?

The whole point of the additional complexity in land surface models and large scale hydrological models is that they might want to do something more complicated than just predict gridscale runoff, for example, (i) act as an Earth System Model and feedback to the atmosphere or biosphere (water, energy, and carbon balance/cycle together), (ii) interact with changes in landuse or other environmental change, (iii) undertake higher spatio-temporal forecasting where the hydrograph dynamics, variations in soil column water, groundwater variability (sub-monthly and routed down the river) are essential to capture. "However, the mismatch between their temporal and spatial resolution raises the question of whether this can be successful" – what do you really mean by temporal and spatial resolution here. What has one got to do with the other? What is the physical basis for this comment? Hydrologic physics is full of non-linear transformations which makes these generalised relationships between time and space unhelpful. What if the models were applied at higher spatio-temporal resolution (as many LSMs/GHMs are undertaking right now)? I agree that even higher spatial resolution does not solve all the problems cf Beven & Cloke 2012, but it doesn't follow automatically that we should remove all land parameterization completely.

The evidence provided distinguishing the bias in forcing compared to the land parameterizations (table C1) is not convincing. What happens if you do bias correct the precipitation and other variables per model instead of the bulk WATCH correction as might be more typically done in climate impact studies? What about the additional uncertainty from the bias correction itself? What happens if you let the evaporation evolve as in a land-atmosphere coupled model instead of forcing it with constant forcing? Typically you would expect the model performances to change and yet these things have not really even been mentioned let alone carried out. In addition the observed (transformed) runoff has been used here as a benchmark directly and this is not usually a totally fair comparison as it is not in the 'model world'. I would have suggested that there should have been further comparisons with long-term model climatologies of runoff (routed discharge too) and/or reanalysis products which integrate observational data with the model structure. Also the analysis with the reduced meteorological variable set seems rather tagged on at the end when really this forms quite an important part of the analysis as many models are only driven by precipitation and temp/evap etc. This again undermines the conclusions.

"We note that issues common to all statistical applications can limit the interpretation of the presented results. Uncertainty in the used data, correlations between atmospheric forcing and land parameters, as well as an incomplete list of possible explanatory variables can influence the analysis". Quite right – surely this should have been tested

then – what is the sensitivity of your results to these? "these limitations do also imply that the effect of the considered land parameters on large scale features of terrestrial water dynamics may have a similar order of magnitude as the mentioned disturbing factors" – I don't understand how this can be justified as this has not been evaluated.

So overall I find that althougth the manuscript has opened up a debate on complexity issues in land surface/global hydrologic modelling, which is a good thing, unfortunately the paper itself is not of a high scientific quality, the conclusions are not supported by the evidence presented and it is written in a way which is very complicated and difficult to understand.

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