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Hydrology and Earth System Sciences
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 #3

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The paper analyses the impact of large-scale atmospheric drivers versus land parameters for modelling the terrestrial water systems over Europe. It is concluded that substantial parts of terrestrial water dynamics are controlled by atmospheric forcing which dominates over land parameters. The manuscript discusses in its first part the scale separation between small- and large scale phenomena and consequently proposes the "Constant Land Parameter Hypothesis" (CLPH) which assumes, based on runoff observations in Europe, that one single set of model parameters is valid at every location in space. The CLPH has been tested against several alternative hypotheses.

While I do appreciate the scale separation discussion in Section 2, the tests of the

CLPH are not very convincing. It is not clear enough which explicit and implicit assumptions are made with CLPH and thus which of them allow the conclusions presented as results, in particular in relation to the discussed small and large scales. Instead, a lot of technical detail in the appendix does not necessarily contribute to a better understanding of the main testing strategy. For which years has the CLPH been tested? What was the training data set and what the independent verification data? What is the reference model used in the skills core estimates?

As I am not an expert in land surface modelling and thus am not able to substantially comment on the physical aspects of terrestrial water dynamics, a my criticism also concerns the methodological approach used in the study. The skill of monthly water dynamics estimates over Europe are purely based on deterministic skill measures which to me seems to be in stark contrast to the unavoidable uncertainties related to land surface modelling parameters and observations. Why do the authors not consider probabilistic measures of performance which explicitly take into account the uncertain nature of the subject?

I was surprised to see the parameters in Table 1 where the fraction of variance explained by the small and large temporal and spatial scales are equal. How does this fit to the main results of the dominance of the large-scale atmospheric forcing?

The topic of the paper is an interesting one but I doubt that the manuscript can shed much convincing light on the subject. The paper is not particularly well structured, see comments above. Some of the graphics are too small for me to be meaningful (Figures 3 and B1) and could perhaps be improved.

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