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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

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We thank Reviewer #4 (Stefan Hagemann) for openly commenting on our manuscript and highlighting issues that require clarification. In the following we provide point by point answers to his comments. For the sake of clarity we first repeat the reviewer's comments (*in italic*) and then provide our response.

Comment 1: *The authors present an interesting study on simulating monthly water dynamics over small catchments in Europe. They compared results from land surface models and global hydrology models (both are referred to as GHMs in the following)*

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to a so-called data driven approach where they used Random Forest method, a machine learning tool, to calculate runoff. Their results highlight that substantial parts of terrestrial water dynamics are controlled by atmospheric forcing, and it is suggested that land parameters play a negligible role.

Reply 1: We appreciate this concise summary of our research.

Comment 2: *Some major clarifications with regard to the characteristics of the Random Forest method used to create the CLPH model are necessary. How I understand the method from what is written in the paper is that it basically creates a discharge solution Q for the point x from forcing variables and discharge observation from the surrounding 9 EWA catchments (n=10). Thus, this means that the method creates an impulse response function h that yields discharge (or river runoff) from the forcing variables. This is rather similar to what has been done in rainfall-runoff modelling for many years. The new point in the present study is that this function is created from surrounding areas of x and not at x itself such as it is commonly being done in rainfall-runoff modelling. In both cases, any parameter dependencies are hidden in the 'black box' of the impulse response function. In rainfall-runoff modelling, it is no surprise that the calibrated function h usually yields better runoff estimates than an explicit model. In the present study, the CPLH model or the respective functions h also mostly yield better results than the GHMs, but here h is based on surrounding areas. This can be interpreted that the hidden parameters are similar for neighbouring catchments, which is a concept that has been previously applied in regionalization studies (Some of those studies and results should be cited and discussed in this respect).*

Reply 2: Unfortunately there is a misunderstanding with respect to the formulation of the CLPH model. In contrast to the reviewer's perception, the parameters of the statistical model implementing the CLPH are not derived from the 9 neighbouring grid cells. As clearly stated in Equation 8 and Equation 9, the CLPH based model (F^*) is independent of spatial location x and the model is identified using data from the entire spatial domain of the study. The parameter n mentioned by the reviewer is a time-lag

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parameters as clearly stated on p. 13201, l. 4-5.

Further we would like to highlight the following points:

1. We introduced key principles of machine-learning in more detail in the revised manuscript. Note, however, that the choice of Random Forests (RF) over other machine learning tools is not exclusive and other machine learning methods such as Support Vector Machines (SVM), Neural Networks or Gradient Boosting could have also been used.

2. In hydrology, models are usually calibrated for individual catchments and it is a common perception that the resulting parameters reflect the catchment properties. Consequently it is assumed that locally calibrated models cannot be applied at new locations (we point to this issue on p. 13198, l. 25 ff). Our application differs fundamentally from this approach, as we identify one single parameter set applicable at any location within the domain of the study.

3. It is true that the models defined in Equations 9 and 10 closely resemble impulse response functions. However, the resulting models differ from typical impulse response functions such as the unit hydrograph in several important points (see reply to Reviewer #1).

Comment 3: *It is very interesting to see that this concept actually works quite well on the European scale for small catchments. But it does not mean that parameters are not important.*

Reply 3: We do not state that the land parameters do not matter in general, only that their importance is found to be very limited at the investigated large spatiotemporal scales (monthly, ≈50 km). Note additionally that we test the CLPH (Equation 8 and 9) explicitly by confronting it with an alternative statistical model taking land parameters into account (Equation 10).

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Comment 4: *This would only be the case if you train/derive a function h in one area and apply it in a totally different area, e.g. deriving the function in the Mediterranean and applying it in Northern Finland, or by using one function h for all catchments. I doubt that in this case the GHMs would still be generally worse than the CPLH model.*

Reply 4: This is actually what we did and we hope that our comments above clarified this sufficiently. The model defined with Equation 9 is independent from the spatial location. Nevertheless, the resulting statistical model significantly outperforms the LSMs/GHMs suggesting that physically based models could have substantially more skill than they have now solely based on taking water and energy availability into account.

Comment 5: *The issue of scales (spatially and temporally) on which the study results are valid also need to be better highlighted. Given that my interpretation of the RF method is correct, the results indicate that the "local" variations of parameters are not important (thus, regionalization is applicable). But this does not mean that "regional" parameter variations are not important, e.g. variations from the semi-arid Mediterranean region to the snowmelt dominated areas of Northern Finland. Here, the definition of "local" and "regional" should be made clear and quantified if possible.*

Reply 5: As already highlighted in Reply 2, 3, and 4 we are drawing our conclusions from a statistical model that has constant parameters through the entire domain (although it has admittedly a very large number of such parameters, and thus many degrees of freedom). This model is valid in both the "*Mediterranean region*" and "*Northern Finland*". Consequently this suggests that geographically constant parameters are sufficient to reasonably describe large-scale terrestrial water dynamics for the considered region.

Based on our results we conclude that the difference in terrestrial water dynamics between Northern Europe and Southern Europe is sufficiently captured by differences in the availability of water and energy. It is, however, true that the expected effects of

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the considered spatiotemporal scales did get out of sight in the discussion and we now include more details on this topic in the revised article.

Comment 6: *If regional variations would not be important either, then the application of globally constant parameters would be sufficient, but many studies in climate and land surface modelling have shown that moving from globally constant to heterogeneous (regionally varying) parameters led to improvements in corresponding model simulations.*

Reply 6: It is true that early model development showed an improvement when moving from globally constant parameters (e.g. for water-holding capacity or stomatal resistance) to varying maps of these parameters. On this point, we need to remark that our study is focused on Europe, hence suggesting that at continental scale these variations in parameters may be less relevant than when comparing different continents (e.g. South America vs Europe). This is possibly not all to surprising since we are not investigating a region with very large vegetation or climatic gradients (for instance, only few catchments are included in the Mediterranean area). This is an important caveat that we now note in the revised manuscript.

Comment 7: *It also should be noted that GHMs are usually made to be applicable for larger catchments and not necessarily adequate for small scale catchments with the size of one (or a few) 0.5° model grid box (boxes). Thus, there is already an inherent scale limitation of the GHMs itself.*

Reply 7: We are fully aware of this issue and designed our study accordingly:

1. In our understanding the grid cells in LSMs/GHMs basically partition the coupled energy and water balance, where runoff is the excess water leaving the system. Consequently we do not expect LSMs/GHMs to precisely capture daily streamflow dynamics. We argue, however, that grid cell runoff can be compared to observation-based runoff rates at large time scales (e.g. monthly) at which pro-

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cesses such as channel routing are expected to play a minor role. (See also reply to Reviewers #1 & #2).

2. We designed the "observation-based runoff rates" used for the analysis to be as comparable with grid cell runoff as possible (see p. 13201, l. 21-26 and response to Reviewer #2 for details).

3. We do not expect LSMs/GHMs to be perfect at every location in space and therefore focus our evaluation not on skill at individual locations but on the distribution of skill estimated at many locations.

In the revised manuscript we provide additional text, explaining the allocation of catchments and grid-cells in more detail.

Comment 8: *The use of the terms runoff and discharge is not fully consistent and partially misleading.*

Reply 8: Throughout the manuscript we try to apply the following definitions

runoff: The average amount of water draining from a small land unit (e.g. grid cell).

streamflow: The amount of water flowing through a gauging station of a stream (small river).

discharge: The amount of water flowing through the gauging station of a large river, where lateral transport of water is important.

We have worked through the manuscript to ensure that this terminology is applied consistently.

Comment 9: *Especially it should be pointed out that the lateral transport of water (which transforms runoff into discharge) is not considered in the present study. For*

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discharge it is known that the slope is important for the travel times of water. As only small catchments are considered on monthly time scales, travel times do not play a role in the present study. But it needs to be highlighted that the results of the present study with regard to the non-importance of locally varying parameters cannot be transferred to large catchments as here the travel times of water in the river network become important, and those strongly depend on locally varying parameters such as the slope and on the presence of wetlands, lakes and artificial reservoirs in the river network. Consequently the CPLH-RFM does not provide a reliable basis for estimating river discharge from Pan-European rivers, such as noted on p. 12304 – lines 11-12. It can only be used to estimate the total runoff for these catchments as the lateral transport is not accounted for. The difference between runoff and discharge also needs to be properly taken into account in Fig. 3, Appendix C and Table C1.

Reply 9: This is a misunderstanding, as we actually did consider discharge in our study. In both the main body of the text (Section 4.2.1 Figure 3d-g) and the Appendix (Appendix C, Table C1) we compared observed monthly river discharge to estimates derived from spatially averaging runoff from all grid cells within the river basin. This spatial averaging implies lateral transport. Consequently we have presented empirical evidence, suggesting that a CLPH based model can capture monthly discharge from large rivers in Europe reasonably well. We do, however, assume that this may break down if phenomena with a higher (e.g. daily) resolution are considered. For these phenomena processes such as the kinematic wave propagation in river channels are certainly important and we will highlight this in the revised version of the manuscript.

In summary, I suggest accepting the paper for publication after major revisions have been made

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Minor Comments

MC 1: Title *The second part of the title, 'towards new paradigms in modelling strategies', may lead to the impression that new modelling strategies are introduced in the paper, which is not the case. Thus, I suggest modifying the title.*

Reply MC1 In the view of the numerous comments of the reviewers on this point, we agree with the reviewer regarding this issue. As stated in our general answer to the reviewers, we realise that the title as previously stated could be misunderstood to imply that we proposed a new modeling strategy, while our main purpose was to highlight the fact that such new modelling strategies could be developed by the community as a whole (aiming at the "right" amount of complexity). For this reason, and as highlighted in our general answer, we have removed the second part of the title, as well as included an indication that we focus on the monthly time scale in the final title: "Do land parameters matter in large-scale monthly terrestrial water dynamics?"

MC 2: p. 13193 - line 14 *... for discrete land ...*

Reply MC2 Thank you for spotting this typo, we corrected this in the revised version.

MC 3: p. 13195 - line 8 *... where features of the atmospheric ...*

Reply MC3 We consciously formulated "where large features" to discriminate against local atmospheric phenomena occurring e.g. at the boundary layer scale

MC 4: p. 13196 - line 10 *Note, however, that Skøien et ...*

Reply MC4 Thank you for noting this typo, we corrected this in the revised version.

MC 5: p. 13197 - line 2 *It is written: ... using Morans / ... What does that mean?*

Reply MC5 In HESS-D typesetting it is difficult to distinguish the letter "I" from the slash "/". As stated in the text Morans I is a well established measure of spatial autocorrelation. In the text we cite the original reference and a textbook, documenting the definition and the application of this method.

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MC 6: p. 13201 - line 22 *... catchments were first ...*

Reply MC6 Thank you for noting this issue, we corrected this in the revised version.

MC 7: p. 13202 - line 13 *... inclusion of locally ...*

Reply MC7 Thank you for noting this issue, we corrected this in the revised version.

MC 8: p. 13210 - line 11-12

It is written: "However, catchment-scale hydrological modelling is usually based on precipitation (the sum of rainfall and snowfall) and temperature only"

This statement is too general, as the scale and model type need to be clearly defined. For rainfall-runoff modelling types this is true, but with regard to the WATCH GHMs, it is wrong, as here most of the GHMs use more forcing variables than precipitation and temperature.

Reply MC8, part A Indeed we have been writing about "*rainfall-runoff modelling*" as it is typically applied for individual catchments in many practical applications. We clarify this point in the revised manuscript.

Here, it should be noted that even though PT forced GHMs may yield reasonable results for today's climate, they potentially fail for future climate conditions, especially over tropical and sub-tropical areas as pointed out by Hagemann et al. (2011).

Reply MC8, part B Quite true, but this topic is not within the scope of the presented study.

MC 9: p. 13210 - line 22-23 *It is written: "The fact that BIAS and BIASlog computed for the RFM are hardly distinguishable from the LSMs/GHMs shows ..."*

It should be noted that Fig. B1 shows that one RFM model (unfortunately I can't read the legend) is clearly worse than all (or almost all) GHMs.

Reply MC9

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1. Thank you for spotting this inconsistency, the model RFM you are referring to is in the configuration "FULL-LP$_{cv-time}$. We will comment on this in the revised version of the manuscript.

2. The reason for the bad readability of Figure B1 is related to HESS-D typesetting, squeezing figures designed to fill "portrait" oriented pages to a "landscape" format.

MC 10: p. 13212 - line 4-5 *This is an important result that should not be hidden within the appendix. It also supports findings of Haddeland et al. (2012) who investigated the impact of bias correcting other forcing variables than precipitation and temperature on the simulated hydrology. In this way it adds to the discussion whether the bias correction of these other forcing variables is really necessary for hydrological applications that has also be taken up in Hagemann et al. (2013).*

Reply MC10 We agree that this is an interesting result and therefore documented it in the appendix. However, the result it self is not within the scope of the presented study: the influence of land-parameters on large-scale terrestrial water dynamics.

MC 11: Table C1 *Why you do not show the multi-model mean of the GHMs?*

Reply MC11 Good point, we have changed this in the revised version of the manuscript.

MC 12: Fig. 2, 3,4, 6, B1 *Many figures are too small so that it is very hard to read the legends.*

Reply MC12 This is related to the HESS-D typesetting. These figures are designed to be "double-column" figures in HESS. In this case the annotations should be easily readable.

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