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Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Towards observation based gridded runoff estimates for Europe" *by* L. Gudmundsson and S. I. Seneviratne

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We would like to thank Mr Fekete for the open and very positive evaluation of our manuscript. 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: *Gudmundsson and Seneviratne presented new time varying gridded runoff estimates for Europe primarily based on observed river discharge combined with atmospheric forcing and land surface characteristics in a stochastic framework. I was quite excited about the paper when I was invited to provide my comments, because I anticipated that the authors will take my earlier work (Fekete et al. 2002, that*

the authors cited) to a new level. I strongly believe that discharge records are poorly utilized in continental scale hydrology despite their high accuracy compared to other "measured" components of the hydrological cycle, therefore I was looking forward to see new advancement in their utilization. To be honest, this paper was different than I expected.

Reply 1: We appreciate that Mr Fekete agrees with our evaluation that observed river flow is under-utilised in continental to global scale hydrology. We are, however, also surprized that he had strong expectations about the approach of our study. We believe that one way to advance the science is to assess multiple approaches to specific questions. Therefore we appreciate the positive attitude of Mr Fekete and would like to thank him for supporting the publication of the manuscript in the present form.

Comment 2: *In principal, I probably disagree with the authors that a calibrated black-box model (machine learning algorithms) is any closer to observations than physically based models. In that sense, I don't necessary see the difference between implementing machine learning algorithm using a series of input variables from physically based land surface models (LSMs). I personally have a preference using the later, partly because that is what I am more familiar with, but more importantly, physically based LSMs can give insight into how the water cycle works that a blackbox model can not. I admit that LSMs applied coarse resolution are possibly as much detached from physical reality than a blackbox model, but nevertheless they implement well understood physical processes.*

Reply 2: While we fully agree that physically based LSMs have their specific merits we would also like to emphasise that the current generation of models is not without flaws. Remaining challenges in current generation LSMs are e.g. related to incomplete process understanding and to observationally ill constrained parameters. As a consequence, the current generation of LSMs still exhibits large uncertainties (see e.g. the references in the submitted manuscript and Figures 6,7 and 8).Therefore we have been seeking an alternative for estimating long time series of runoff at ungauged locations. For this we adapted an empirical approach that is well established for estimating evapotranspiration (and other land-atmosphere fluxes) on regular grids (Jung et al., 2009, 2010, 2011). The lack of physical assumptions in the statistical model can also be seen as an advantage, because it means that it is purely empirical and not subject to prior assumptions.

In the submitted manuscript we also showed that the empirical runoff estimates are on average closer to observations than estimates stemming from LSMs (see Figures 6,7 and 8 in the discussion paper).

Comment 3: *I normally don't like to push my own work in reviewing, but I think the authors some- what overlooked our paper (Fekete 2002) that could be seen as a predecessor to their work. Although our 2002 paper only provided monthly climatology of gridded runoff estimates, we produced 10 year time series (1986-1995) for the ISLSCP-II initiative (http:// daac.ornl.gov/ ISLSCP_II/ guides/ comp_runoff_monthly_xdeg.html). I have to admit that the ISLSCP-II time series has serious problems as the number of dis- charge gauges drop out over time, but both the provided climatology and the delineated subbasisn and inter-basin discharge could serve as a reference to the present study.*

Reply 3: While we where aware of the runoff climatologies presented in Fekete et al. (2002), none of our searches led us to the data product mentioned above. This might be related to the fact that the data product is not associated with a peer-reviewed publication and only covers one decade.

Nevertheless we would like to thank Mr Fekete for pointing us to this interesting product. We have accessed the data and evaluated its ability to estimate runoff and river discharge dynamics in Europe. The results are shown in Figures A1 and A2. Overall the performance of the ISLSCP-II data is in line with that of the WATCH LSMs under consideration (see Figures 6, 7, 9 in the discussion paper).

As the information gain of the evaluation of the ISLSCP-II data is small compared to the

evaluation of the WATCH LSMs we propose not to include the above mentioned results in the article. Instead we suggest to provide Figures A1 and A2 in the supplementary information if this is deemed to be important by the editor.

Comment 4: *The fundamental difference between our work and this study is the use of large vs. small river basins. While large river basins are more prone to human alterations, but I would argue that having multiple discharge gauges along the rivers main-stems should be sufficient to isolate affected and "pristine" river reaches. A step forward would be the combination of the authors approach looking at small basins that are smaller than a grid box with large river basins. To some degree the authors accomplish this by considering major river basins across Europe, but I think, more fine grained subbasin partitioning should be feasible.*

Reply 4: The main aim of our study is to produce monthly estimates of the amount of water leaving relatively small land units (i.e. grid-cells). This is motivated by the fact that the partitioning of precipitation into runoff and evapotranspiration - as well as the retention of water in storages such as ground water and snow is primarily governed by local scale processes.

We agree that it would be in principle possible to route the derived runoff estimates through a river network. This would not only allow for a fine grained subbasin partitioning but would also facilitate the assimilation of observations from larger catchments, following the procedure suggested by Fekete et al. (2002). This however, would be a major independent research effort which would go beyond the scope the presented study. Instead we suggest to mention this possibility in the outlook of the paper, to motivate possible follow up investigations.

Comment 5: *I would encourage the authors to implement their work on a finer resolution grid. Half a degree resolution is pretty much the norm for global scale studies therefore one would expect higher resolution analysis for a single continent.*

Reply 5: The spatial resolution of the presented study is motivated by the spatial resolution of the WATCH multi model ensemble, which we use as a benchmark for our product. To keep the comparison between the WATCH models and the empirical runoff estimates as close as possible, we opted for using the WATCH forcing data for the presented study.

Comment 6: *Although, the paper did not quite meet my expectation in terms of delivering gridded runoff fields (where I have to admit that probably my expectations were biased), I found plenty of valuable research findings with respect of what variables are important for large scale hydrological modeling. This angle of the paper was more valuable to me than producing gridded runoff fields. I would argue that this sensitivity analysis is where the blackbox approach is more convincing. When sensitivity analyses are carried out with physically based models, one has to wonder if the derived results are indeed the characteristics of the physical world or the model.*

Taking the machine learning model and testing different sets of model inputs in a stochastic framework appears to be a better approach in telling which data are important. Although these results might be still misleading since high noise in certain input data and the corresponding over fitting might be a problem with the input data itself rather than its significance in the simulated processes, but nevertheless it is informative with respect of where one should put more emphasis in large scale modeling.

Reply 6: We thank the reviewer for this positive evaluation of our sensitivity experiments! We might consider to emphasise this in a revised title of the article.

Comment 7: *Although, this paper is quite different from what I hoped for, but it has substantial valuable results that is worthy for publication. I encourage to authors to take a closer look at the time series implementation of the composite runoff fields that we produced for the ISLSCP-II Initiative, but if the authors chose to stick to the current content I will support their papers' publication in its present form.*

We thank Mr Fekete for his open-mined evaluation of our study! As mentioned above we have considered the ISLSCP-II data product in the model evaluation process.

Figure Captions:

Fig. A1. Comparing the grid-cell level performance of the Random Forest model in different configurations and for both cross-validation experiments to the performance of the ISLSCP-II runoff composite. The skill of all models was estimated for the time steps at which the ISLSCP-II data are available. Details on the performance metrics can be found in Sections 3.3.3 and 4.2.1 in the discussion paper. The interpretation of the box-plots is analogue to Figures 6 and 7 in the manuscript.

Fig. A2. Comparing the basin scale performance of the Random Forest model to the performance of the ISLSCP-II runoff composite. Details on the performance metrics can be found in Sections 3.3.3 and 4.2.2 in the discussion paper. The skill of all models was estimated for the time steps at which the ISLSCP-II data are available. The interpretation is analogue to Figure 9 in the manuscript.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 12883, 2014.

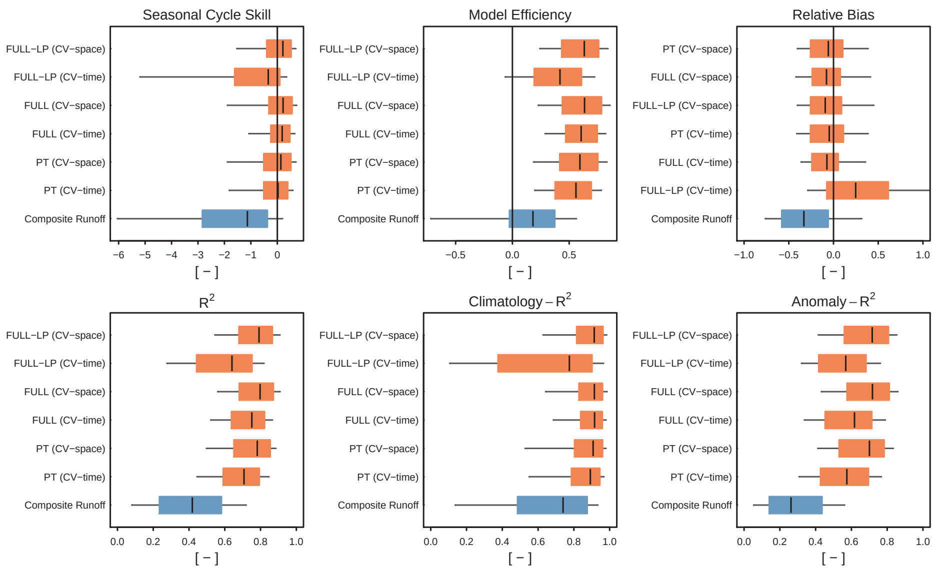
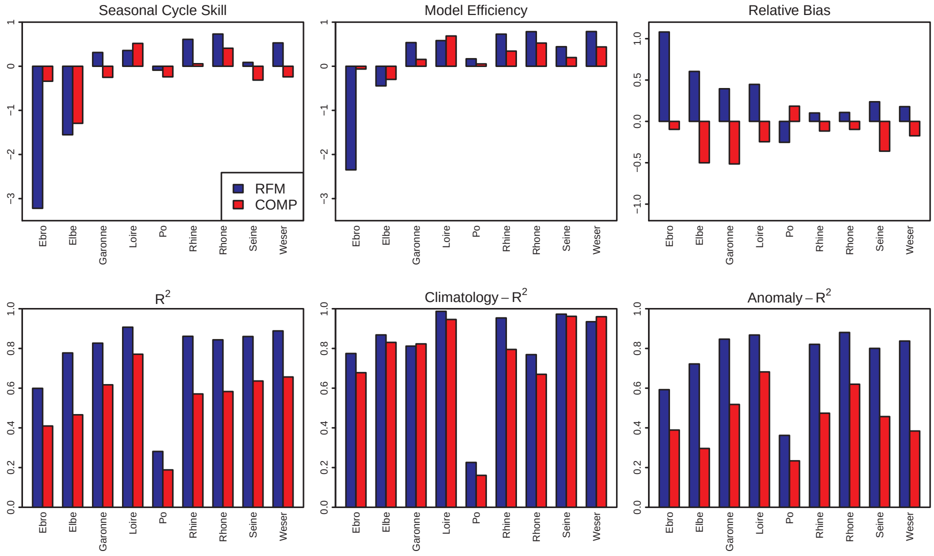


Fig. A1.



Seasonal Cycle Skill
Model Efficiency
Relative Bias
RFM
COMP
Ebro
Elbe
Garonne
Loire
Po
Rhine
Rhone
Seine
Weser
R2
Climatology – R2
Anomaly – R2

Fig. A2.