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

Anonymous Referee #2

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Like Balazs Fekete, I was greatly interested when I received this paper, and like him, I have been surprised by the content. As I far as I am concerned, I have been disappointed. The main problem is that the paper does not deliver what the title promises, namely gridded runoff estimates based on observations over Europe. The paper did not convince me either to achieve a significant step toward this goal.

From my point of view, an important reason stems from the ambiguity between runoff and streamflow in paper, where they are used as equivalents, although they are very different quantities, with different units (typically mm/s vs m3/s), and different dynamics, since streamflow results from the routing of runoff in watersheds, along hill slopes, under ground, and in the river network. What the authors are using as their reference for runoff is streamflow divided by the contributing area. This certainly leads to a runoff unit, but not to a runoff dynamic, as the operation does not deconvolute the effect of transfer times within the catchment. The paper would greatly benefit from clarifications about these concepts, and from some reviewing of the various attempts to estimate runoff based on streamflow observations, not overlooking the targeted timescales. In particular, I raise the attention of the authors to Gottschalk and Sauquet, who addressed these questions in quite many papers (e.g. Sauquet, E., Gottschalk, L., and Leblois, E., Mapping average annual runoff: a hierarchical approach applying a stochastic interpolation scheme, Hydrological Sciences Journal, 45, 799-815, 2000).

The above point is essential when comparing the “runoff estimates” to LSM products, which are runoff stricto sensu. Grid-cell runoff in a LSM is the spatial average over the grid-cell of point-scale runoff in mm=kg/m2, without any transfer function to the river network, so that it does not have the same dynamic as the selected streamflow data (even if the contributing catchments are smaller than the 0.5°x0.5° LSM grid-cells). I believe this is a major reason for the large differences between “runoff estimates” and LSM runoff in the paper, so that these differences may not invalidate the LSMs, but rather the proposed “runoff estimates” as the appropriate data against which assessing LSMs.

Another problem is the representativeness of the selected streamflow stations/catchments to constrain gridded runoff estimates over Europe by extrapolation. The streamflow measuring stations mostly come from mountainous areas, Germany, and UK. Non mountainous Mediterranean climate is almost absent, except a couple of points in Spain. The performances are not differentiated based on hydroclimatic regime, and Fig. 8, the only one where it might be possible to assess the extrapolation power of the RFM in ungauged areas (I mean here areas with different hydrologic regimes than the ones of the selected streamflow stations, i.e. areas on which the RFM has not been trained), is almost useless: scatter plots where the color points corresponding to the different large-scale river basins cover each other, colors that are difficult to distinguish, log scale hiding the differences, no average summary by basin.

All this casts doubts on the main conclusions: (i) validity of a unique RFM for extrapolating “runoff” over Europe, (ii) negligible effect of land properties (they might have a larger effect in water stressed areas, in which the RFM is not trained).

Other comments regarding the methods: (1) Why not test the effect of land use and vegetation in the model setup? (2) R2 is not the correlation coefficient but the determination coefficient, with a major difference, since R2 cannot be negative. (3) I’m not sure the cross-validation procedure really leads to an independent evaluation, since different sub-periods in one stream flow record are not independent (we would used paired statistics to compare them). Random sub-samples of different stations may also show some dependence given the hydroclimatic similarities raised above.

Results: (1) The sections on model selection and validation are very short: no discussion of the parameters values (time lags); Fig 4 is a very condensed summary of the three models’ performances: couldn’t we compare the pdf of the local RMSEs for instance? Fig. 7 suggests poorer performances in mountainous areas, which is not commented. (2) Still regarding validation: why not comparing to an independent estimate, such as provided by Fekete et al. (2002)?

Eventually, I find abusive the conclusion that the selected RFM is capable, performs reasonably, compare well with observations, is an ideal candidate for model evaluation (all these expressions come from the submitted paper), and I don’t recommend the publication of this paper in its present form.

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