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Interactive comment on "Monthly streamflow forecasting at varying spatial scales in the Rhine basin" *by* Simon Schick et al.

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The authors thanks for the good and comprehensive reviews by Joost Beckers and Kean Foster, which reveal some important deficiencies of our article and hint some interesting ideas. Please find below a list of how we plan to work the article over if it gets accepted for a revision. Since we agree with the detailed/technical comments of both reviewers, we include here only the major issues.

Terminology

- We check the article for the correct usage of the terms 'season' and 'operational' in order to avoid wrong expectations. It must be clearly stressed that – on average

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– the prediction skill of the MOS approach is restricted to one month ahead.

- We will revise the naming of the different predictor combinations, e.g. preMet (preceding meteorology) and subMet (subsequent meteorology) instead of ESP and revESP (up to now, we do not have a better idea). However, we suggest to retain the link to the ESP-revESP framework for the motivation of such a preMet-subMet analysis (or whatever its name), since we originally aimed to mimic the ESP-revESP framework in a statistical context, and the underlying idea is not at all ours.

Introduction

- As pointed out by Kean Foster, we need to clarify the first paragraph about the 'slowly-varying and predictable phenomena' as well as teleconnections and why they are relevant for our study (page 1, line 16). Also we should mention why it is hard to make seasonal climate predictions for Europe compared to the tropics (page 3, line 19).

Results

- We add plots (and corresponding paragraphs) similar to the ones attached to our response of Kean Foster's review, which show the variation of the regression coefficients and time aggregation periods (maybe it can help to make the black box model a little bit less black) as well as the variation of skill versus the date of prediction. We suggest to put the former to the additional materials, while the latter is in our opinion appropriate for the results section.

- We add the MAE of the bias corrected H-TESSEL runoff for spatial levels 2 and 3 to table 4.
- Concerning the suggestion of Joost Beckers (variation of the predictand's time aggregation at zero lead time, e.g. streamflow averages of 1,3,5,10,15,...,30 days) we propose one of the following:

    1. We include such an analysis in the article, but drop the results about the comparison of skill versus geographic attributes (i.e. figure 4 and the corresponding paragraphs). We have the impression that otherwise the article gets too lengthy.
    2. We do not include such an analysis and stick with the 'skill versus geographic attributes' results, but instead discuss such an experiment in the discussion and conclusion section for further research.

  We have not yet tested the MOS approach at such small time average windows, so we have no clue about the direction of possible results. As we often read that catchments are complex, non-linear systems, we silently assumed that a linear regression model fails at such small forecast windows. On the other hand, studies exist in which ARIMA models are fitted to daily streamflows, so who knows?

  As already noted in our response to Joost Beckers' review, we think that such a preMet-subMet analysis for short time intervals would be an interesting experiment. However, there is simply no guarantee that the MOS approach does not crash (at least we can verify this point with the refRun model, which has access to the best available input data).

  Maybe the editor can provide some guidance regarding this point.

Discussion

- We add a section about pros and cons of MOS based streamflow forecasts, e.g. that we get a bias correction for free (in case of our article restricted to unconditional biases and conditional biases that are linear), that it is not possible to make predictions at ungauged sites, and so forth.
- If the reviewers and the editor agree, we could add some MAE or MSE estimates of deterministic hydrological models for the Rhine at Basel or Lobith. Up to now, we do not have any relevant reference from the scientific literature, but eventually we could find one or two references since climate change impact studies often report MAE, MSE, NSE and friends on a monthly or yearly basis. Clearly, such a comparison would mix the contexts of climate change projections and S2S forecasting, but could provide a rough order of magnitude.
- We need to stress that by using meteorological input variables only (aside from the S4Q model), we end up with a loose and fuzzy proxy for the initial conditions. Additional input variables most probably improve the prediction accuracy, but impose technical and practical disadvantages that we aim to avoid for the present study (inconsistent data availability, predictor dependence).

Conclusion

- We should more elaborate on why we think that the performance of the H-TESSEL benchmark is interesting: H-TESSEL is skillful against climatology and the MOS approaches in case of the Rhine basin, though H-TESSEL runoff (within the S4 system) is not intended to provide streamflow predictions. At this point, we think it is legitimate to have an outlook regarding operational applications: If

the setup of a hydrological model or the subsequent production of streamflow forecasts is not feasible, but streamflow observations are available, then a simple bias correction of H-TESSEL runoff from the S4 system could be worth to test.

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