Review - Monthly streamflow forecasting at varying spatial scales in the Rhine basin

Specific comments

As I mentioned in the general comments, Joost has already raised some points especially with regards to the conclusions. I agree with most of his comments so to minimise repetition I will concentrate on other aspects unless where I disagree with him.

Is there a reason why the initial hydrological conditions are not included as predictors (page 3-lines 10-12 and table 2)? Predictors related to storages such as soil moisture content, snow, and reservoir/lake levels all impact future streamflow yet only meteorological predictors are used. I agree that many of these initial storages are affected by the antecedent meteorological conditions, but these connections are not necessarily linear or significant depending on the time frame used. For example, if only predictors for the preceding month are used then there is little connection to snow pack size or reservoir levels and therefore little added value. Thus I miss a description of the time period, and to a lesser extent the domain, for the predictors.

Similarly, I question whether the use of the terms ESP and revESP are technically correct in this paper as it stands. Without any information regarding the initial conditions at the forecast initialisation one can argue that this is not similar to what Wood and Lettenmeier (2008) meant. If it were possible I would suggest the authors include predictors that represented the initial conditions (soil moisture, snow depth, or even streamflow) otherwise they should add a paragraph explaining why the current approach is still an adaption of the VESPA methodology. I believe that the latter may be difficult to justify especially with respect to revESP.

I echo Joost's point where he suggests that the suggestion that the performance may be better for particular months (page 17-lines15, 16) is unfounded as the article stands now. However, I do expect this to be the case and therefore I disagree with him in that this should be removed. Rather I think it would be of interest to include some results or a section that addresses this variability. This can be done in part in the form of a figure along the lines of the one below (figure 1). Related to this, why are the authors concentrating only on the general performance throughout the year? The usefulness of these forecasts may be much higher, even only, during specific times during the year e.x. during the snow melt period or low flow period.

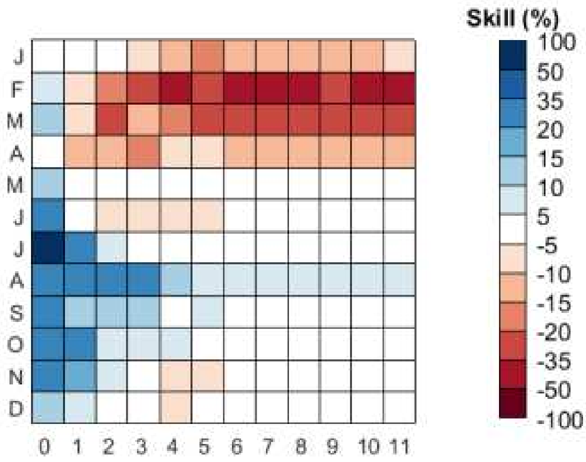

Figure 1. Forecast skill as a function of lead-time and initialisation date.

With regards to H-TESSEL, Table 4 shows that it has some skill, at least at the spatial level 1. Have the authors tested using these data as predictors in the MOS approach at levels 2 and 3?

I am unclear as to whether the S4* data is bias corrected. It is now almost common practice for some sort pre-processing or bias correction of the S4* forecast data before use in hydrological forecasting studies and work. The authors note that the quality of seasonal climate predictions for the study area are low (page 3-lines 20,21) but it is not clear to me whether any attempt to bias correct the data, and if I did miss it by what method.

Lastly, the authors mention how the uncertainties in forecasts can be reduced when the quantity of interest is controlled by teleconnection phenomena (page 1-line 17-19). I don't contest that this is true but rather question how it is relevant to the paper because there does not seem to be any more references to such modulation activity or its importance in the rest of the paper.

Technical comments

As mentioned in the general comments I feel that the article is well written so I have two only minor technical comments.

On page 9-line 12 the authors give a secondary citation where I feel that the original citation, or at least inclusion of the original would be strongly advised. Taylor's original article is: *Taylor, K. E. (2001). Summarizing multiple aspects of model performance in a single diagram. Journal of Geophysical Research: Atmospheres, 106(D7), 7183-7192.* The authors are encouraged to check their other sources.

Lastly, there are some minor grammatical errors in the paper; however these do not detract from the readability or arguments made therein. All the same I do suggest that the authors spend a little time to minimise them if time allows.