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Discussions

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

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General comments

This paper describes the application of the Model Output Statistics (MOS) method to monthly streamflow forecasting in the Rhine River basin. The MOS method is based on empirical relations between predictors (past observations and GCM forecasts) and predictands (monthly mean streamflow) that are obtained through linear regression. Results of several variations of the method using different combinations of predictors are compared in terms of forecast skill.

The paper is well written and clear. In my opinion the paper addresses a relevant scientific topic related to finding methods for streamflow forecasting at the medium

range time scale, which falls within the scope of HESS. Nevertheless, I have a few comments to this paper, as specified below, that I believe should be addressed before I can recommend its publication.

Specific comments

The MOS method is presented as an option for streamflow forecasting at the seasonal time scale (Page1-Line 16, Page 2-Line 18, Page 3-Line 9). However, the results show only forecast skill relative to climatology for the first month ahead. This is usually not what is called seasonal forecasting (rather medium- or extended-range forecasting). So the conclusion must be drawn that no skill was found at the seasonal time scale for any of the models (including ESP and H-TESSEL). This is indeed concluded for the MOS models (Page 17-Line 15), but the suggestion that the performance may be better for particular calendar months (Page 17-Line 15,16) is unfounded and should be removed. Also, the conclusion that H-TESSEL is an interesting option for seasonal (i.e. beyond 1 month lead time) streamflow forecasting (Page 1-Line 13, Page 17-Line 26) is not supported by the results shown in Figure 2 (at least not clear to me).

Moreover, if the MOS method is to be considered for operational streamflow forecasting, it would need to be tested against the more traditional approach, which uses ESP- or GCM-driven hydrologic models. I am not sure which version of H-TESSEL you are using, but since you mention it does not include routing (Page 16-Line 26), this must be a relatively limited model. When comparing the MOS method to this H-TESSEL for zero lead time, the results are not very convincing: according to Table 4 the MAE of the S4* models is worse than for H-TESSEL at Lobith and only marginally better at Basel. Given these results, would the conclusion not be that the MOS methods are not a viable option for operational streamflow forecasting? This conclusion is missing on Page 17.

My final concern with this paper is that it is not clear how the skill of the various models at zero lead time is composed. Probably, the forecast skill for the first 5 days is higher than for the last 5 days of the 1-month lead time. By averaging over the entire first month this information is lost. It could very well be that the average positive skill for lead times 1-30 days is entirely due to the positive skill for the first few days. Moreover, this positive skill for the first few days may be a result of the persistence of weather patterns in the GCM, similar to that for a normal short range weather forecast.

Related to this is the ESP-revESP analysis. The paper states that there is no clear difference in skill between the ESP and revESP at zero lead time (Page 16-Line 2). But the zero lead time is actually an average for lead times of 1 to 30 days. A separate analysis for lead times of 5, 10, 15, etc days would probably reveal a cross-over from dominance of initial conditions (higher skill of ESP) for short lead times to dominance of meteorological forcing (higher skill of revESP) for longer lead times. But this cannot be seen in the monthly average. Therefore I encourage the authors to do an skill assessment at higher temporal resolution.

Detailed comments

Page 3-Line 23: What approaches do these earlier studies use? Are these (bias-corrected) hydrologic model forecasting studies or do they use MOS/PP?

Page 6-Line 2: I believe the reference for H-TESSEL should be Balsamo et al., 2008 or 2009.

Page 7-Line 6: Sample size is 31? 1981-2011 is 31 years, but you leave out the year of forecast and (according to Section 4.1.3) also the two preceding and subsequent years, so n must be 26.

Page 16-Line 14: It is found that the S4PT model outperforms the ESP model for subcatchments with smooth terrain and weak influence of initial conditions. Can you explain why? I would expect the opposite: the S4PT model (which includes forecast temperature) should do well for catchments that are dominated by snowmelt (rough terrain and strong influence of initial conditions).

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