Initial assessment of a multi-model approach to spring flood forecasting in Sweden

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The authors are presenting a relevant study on seasonal forecasting in a snowmelt-governed region of Europe . Seasonal forecasting is emerging as one of the key challenges in hydrological predictions (http://hepex.irstea.fr/hepex-workshop-on-seasonal-hydrological-forecasting/).

In this "initial assessment" the authors focus on different setups for forecasting spring discharge volume (May to July). I find the assessment well documented and the evaluation fair and straight forward. The main novelty of the study is the evaluation of a combination of the presented methodologies. The limited number of single realizations able to top the traditional approach based on the climatology ensemble (CE) and the high number of cases where the weighted approach tops CE is an interesting communication for the reader of HESS.

I have a general concern with respect to the title of the manuscript. Why multi-model? HBV is the only model, you have only multiple approaches for selecting and/or generating your forcing data. Why flood? You are just looking at the cumulated discharge volume during the snowmelt-season.

I guess I would have called the study: "**Initial assessment of spring discharge volume forecasting in Sweden: evaluation of multiple strategies to select atmospheric forcing**"

I admit, that this is less fashionable than your original title, but I am convinced, that this is a better choice to declare the work you present.

Issues to be addressed (Page(s) – Line(s)):

General comments:

You present here several ways to upgrade your seasonal predictions, all of them focussed on the meteorological input. Are there any reason for ignoring for instance the initial conditions of the model (e.g. selecting years with similar snow water equivalent at the beginning of the forecast period)?

As well pointed out by Peter Krahe in his review, many of the data you are using are affected by instationarity, lack of homogeneity and biases. For instance the use of raw output of IFS (and of any numerical model) is something you really need to focus on. We recently presented two studies demonstrating the value of calibrated meterological data (Fundel and Zappa , 2011; Jörg-Hess et al., 2015).

6082 – 10-12: You declare here "In this study we focus on forecasts of the accumulated discharge in the spring flood period (May–July)". In my opinion the accumulated discharge

during the flood period cannot be declared as "spring flood forecast". I think it would be more adequate to declare your product as "spring discharge volume".

6083 – 13-17: As I understand here you declare the outcomes of model calibration and also present them in Table 1. I would expect here also some words and numbers on model verification.

6092 – 2 – 9: I come here a little bit in trouble with respect to your definition of lead time. I think you should introduce a second figure presenting the setup concerning lead time and evaluation period. E.g.:

Initialization	1. January	1. February	1. March	01. Apr	1. May	1. June	1. July	01. Aug
1. January								
1. March								
1- May								

Yellow: Model running

Blue: Model running and evaluation of discharge volume.

6100: The weighted multi model is of course an interesting section of this paper. I really like the simple way you present here to assign the weights (based on ranking). Is there a publication you can cite that presents an assessment of this for me very elegant approach? Do you plan to evaluate other methods to assign the weights (e.g. Bayesian averaging)?

Minor comments:

General issue: I'd like to learn more about the variable you are forecasting. How large is the variability of these spring discharge volumes? Could you add the minimum and maximum discharge volume (May-July) within all years of your experiments in Table 1?

6080 – 22 to 6081 - 9: This paragraph sounds more like a methodological section. Is there any way to formulate this more generally to introduce the goals of your study and move the present formulation in the methods section (e.g. at the beginning of page 6085)?

Final considerations:

I am struggling with the recommendation for this manuscript. I acknowledge the value of these preliminary results and I can also support the fact that you present the data as they are without any post-processing, because this is the starting point and one should be aware of this. I think this manuscript would profit to a "demotion" to "technical-note" with a title sounding like "Technical-note: Spring discharge volume forecasting in Sweden - Evaluation of multiple strategies to select atmospheric forcing". I recommend therefore major revision to re-shape the manuscript to be published as technical note.

Best regards

Massimiliano Zappa

Birmensdorf, 04 August 2015

References:

Fundel F, Zappa M. 2011. Hydrological Ensemble Forecasting in Mesoscale Catchments: Sensitivity to Initial Conditions and Value of Reforecasts. Water Resour. Res., 47, W09520, doi:10.1029/2010WR009996.

Joerg-Hess S, Kempf SB, Fundel F, Zappa M. 2015. The benefit of climatological and calibrated reforecast data for simulating hydrological droughts in Switzerland. Met. Apps. 22: 444–458. doi:10.1002/met.1474