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Hydrology and Earth System Sciences Discussions

Interactive comment on "A past discharge assimilation system for ensemble streamflow forecasts over France – Part 2: Impact on the ensemble streamflow forecasts" *by* G. Thirel et al.

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Interactive comment on "A past discharge assimilation system for ensemble streamflow forecasts over France – Part 2: Impact on the ensemble streamflow forecasts"

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General comments

The topic of the paper is relevant as it seeks to contribute to improving operational medium-range ensemble streamflow predictions and it analyses the effect of discharge

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assimilation to this respect. Although discharge assimilation is already known to improve deterministic short-term predictions, development of assimilation methods for ensemble predictions and evaluation of these methods across the short- to medium-range forecast horizons are subjects of the currently ongoing research in the hydrological ensemble prediction research community (e.g. HEPEX). The paper is thoroughly prepared and well written.

Specific comments

1. The title of the paper shows that the authors aim to analyse the impact of the discharge assimilation system on the ensemble streamflow forecasts. However, the analyses presented make it difficult to draw conclusions about the isolated contribution of the streamflow assimilation. First there is the difference in SAFRAN analyses for the No-Assimilation runs on the one hand, and the IS1 and IS2 runs on the other (with more precipitations observations). The effect of the more recent SAFRAN analysis used in IS1 and IS2 is not known. The authors acknowledge this, stating that there was not enough time to re-do the No-Assimilation with the updated SAFRAN. Yet, it would add much to the paper. If it is by now possible to add this analysis to the paper I would recommend that, if it is not possible than I would welcome some more comments on the expected effect of this difference in SAFRAN analysis. Secondly, it would be interesting to see the results of using only the improved physics of IS2 without using the discharge assimilation.

2. Most results are presented for the 1-day and 10-day forecast horizon. From an application point of view the authors could consider whether it is not more relevant to (also) show forecast horizons in between (e.g. 3 and 5 day horizons). This because the ECMWF EPS are known to have aimed to perform well for the medium-range (and not for the first 24-48 hours) and because a 10-day forecast horizon is for many applications not needed. Applications, e.g. in flood warning, concern pre-alerts from early warnings on the basis of medium-range predictions. Therefore, I suggest to include results for 3-day and 5 -day horizons. If needed these can replace some of the other presented

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results.

3. The presented results are (except one) concerning scores averaged over stations from different catchments in France. To my opinion a water manager would want to look at the results per catchment (for his own catchment) and per application (e.g. flood warning). For example, to know the benefits of applying the streamflow assimilation in a flood-warning application, results have to be presented for a particular river. Then for that river more information is needed about sample size, how many relevant events of high streamflow are in the analysis period. This kind of information is currently lacking. Therefore I recommend to add 1 or 2 hydrographs and eps plumes, and contingency tables for the three different EPSs for a large and a small, flood-prone, river basin for high-flow events.

4. Are the discharge Quantiles taken per individual river, or after combining the discharge records of all rivers? (in other words: Does the Q99 include the maximum discharges of each river or only of the biggest rivers, representing the maximum discharges in France?)

5. The conclusion section ends with an outlook to application in flood alert services. Since this is the intended use perhaps it is worthwhile emphasizing just before (on P 2476) the need for more research to improve the performance for the high discharges in particular.

6. In the introduction the authors could consider referring to the review paper on EPS in the Journal of Hydrology, by Cloke and Pappenberger, 2009: “Ensemble Flood Forecasting: A review”, Journal of Hydrology 375 (2009) 613–626

7. Section 6 Conclusion is perhaps a bit long. Consider taking up the detailed first part where results are repeated in section 5. And splitting up section 6 in more paragraphs for readability.

Technical corrections

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Page 2456, line 2: “Such systems” consider replacing by “Ensemble prediction systems (EPS)” Page 2457, line 5/6: ...enables them to take risk-based decisions. Page 2457, line 20: “This results in poor performance” consider changing into “This may result in poor performance, in particular for the short-range forecast horizons” The current statement is too strong, as there are also water system models in which drift is limited such that daily streamflow assimilation has little to add. Also many applications concern the medium-range forecast horizons for which performance is less sensitive to initial conditions (in particular in controlled water systems) as is also shown in this research by the limited improvements by the discharge assimilation for the longer forecast horizons. Page 2462, line 17/18/19 “The improvement..”: Please explain in text why. Page 2465, line 24 “significant”: should be “significantly different” Page 2472, line 28 “186”: maybe this should be “148” Page 2474/2475 lines26/27/1: This sentence is not very clear. Page 2475 line 9 “increase in the RPSS”: consider “increase in the RPSS with forecast horizon” Fig 5. Caption or in figure: state the probability threshold used (90% / 10%)

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