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Discussions

Interactive comment on "Long-term ensemble forecast of snowmelt inflow into the Cheboksary reservoir under the differently constructed weather scenarios" *by* Alexander Gelfan et al.

Anonymous Referee #2

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This work describes a long-term streamflow forecasting system developed for Cheboksary reservoir inflows (Russia). The authors use the ECOMAG semi-distributed hydrological model to general ensemble forecasts via two different approaches: (1) the Ensemble Streamflow Prediction (ESP) method, and (2) weather generator (WG) based hydrologic forecasts. Forecast assessment is focused on four main metrics: (i) April-June runoff volumes, (ii) maximum inflow discharge, (iii) number of days with inflow discharge above mean observed discharge, and (iv) number of days with inflow discharge above mean maximum observed discharge. Hindcast evaluation is conducted for the period 1982-2016 (i.e., 35 water years) – using deterministic and probabilistic metrics –, and a final evaluation for 2017 is presented. The authors conclude that ESP

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hindcasts are slightly better, although they provide larger confidence intervals than the WG-based technique.

Case studies like these are needed by the community in order to understand the strengths and weaknesses of existing approaches in different hydroclimatic regimes. However, I think that a fundamental flaw of this study is the lack of an overarching science question driving this effort. As a result, the manuscript reads like a technical report rather than a scientific paper, with little interpretation of the results, or even redundancy in the set of metrics selected and number of figures. Therefore, I think the manuscript needs a substantial revision before it can be published in HESS. I would like to encourage the authors to (1) re-think what is the gap that they intend to fill with this work – in terms of methods, information used, or forecast properties, for instance –, (2) clearly state their questions/hypotheses, and (3) re-think their current experimental design to accept or reject their hypotheses. The following comments could guide the authors to improve their work:

Major comments

1. Introduction: The authors state that "the purpose of this paper is to present the performance assessment of a long-term ensemble forecasting system of water inflow into the Cheboksary reservoir of the VKRC". I suggest re-formulating that purpose based on one or two science questions, whose answers could be found using the aforementioned system. Moreover, most of the text refers to the VKRC, with limited connection with recent literature on long-range hydrological forecasting (e.g., Schepen and Wang 2015; Mendoza et al. 2017; Beckers et al. 2016; Najafi and Moradkhani 2015; Demirel et al. 2015; Yossef et al. 2013; DeChant and Moradkhani 2014). A better link with current approaches will help readers to understand what is the contribution of this study.

2. Methods: The authors mention that the first long-term forecasts for the VKRC are dated back to the 1930s and 1940s (P2, L8). In my opinion, the authors should include

one or two benchmark methods – e.g., direct water balance methods, or index-based methods – to understand the added value of the proposed methodologies, ideally for several forecast initialization dates. Also, it is really hard for this reviewer to understand – from the information provided in the supplement section – the differences in forecast ensemble spread between ESP and WG-based technique. I think it would be helpful to see WG results contrasting boxplots or CDFs with observations for monthly precipitation amounts or temperature averages.

3. Probabilistic verification: The authors include both BS and BSS (having climatology as a reference) in Table 4, although they don't need both metrics to conclude that the WG-based approach is better for the event occurrence analyzed (similar to RPS and RPSS in table 5). Also, I strongly suggest to include some metric and/or graphic device for the assessment of forecast ensemble spread, since this is something that the authors point to without a solid quantitative basis (e.g., P16, L14). This could be done, for instance, using QQ plots (e.g., Thyer et al. 2009; Renard et al. 2010) or rank histograms (e.g., Hamill 2001; Delle Monache et al. 2006). The authors could further assess the ability of their forecasting system to distinguish between occurrence and non-occurrence by using discrimination diagrams (e.g., Clark and Slater 2006).

Minor comments

4. P6, L14: I think that the authors should provide a short description of the calibration method, since the paper should be self-contained. Also, the authors state in P6-L9 that "most of the parameters are physically meaningful". I think that statement should be re-visited, because even measurable parameters have uncertainties associated with (i) observational errors, and (ii) their applicability at spatial scales that are different to those for which physically-based equations were developed.

5. P8, L16: Please provide a reference for the Cholesky's decomposition method.

6. Verification metrics: it would be helpful to condense them in a table, including equation, possible range of values and references.

7. P10, L27: The authors state that maximum inflow discharge is well simulated by the hydrologic model, although the plot (Fig. 6) still shows considerable spread around the 1:1 line.

8. Figure 6: Instead of using "lower-left", "lower-right", etc., I suggest using panels (a), (b), (c) and (d).

9. P12, L15: "1000-year Monte Carlo generated time series". Do you mean 1000-member ensemble? Please re-word.

10. Please clarify forecast initialization dates and forecasting approach in the caption of tables and figures.

11. Figure 9: In my opinion, the results from this figure could be better communicated using time series with ensemble forecasts as boxplots, including a line with observations (e.g., Bracken et al. 2010).

12. P20, L2-25: This should be moved to the Methods section.

13. Forecast example for 2017: although this is a very interesting demonstration, I strongly encourage the authors to include verification metrics in their analyses.

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