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Interactive comment on "Skill of a global forecasting system in seasonal ensemble streamflow prediction" *by* Naze Candogan Yossef et al.

Anonymous Referee #3

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General comments

The objective is to investigate the total skill of seasonal streamflow forecasts for 20 of the largest rivers in the world, produced by a global hydrological model (PCR-GLOBWB) forced with the ECMWF S3 system. This manuscript got my attention because I think that ensemble streamflow prediction, as a product of coupling GCMs to Global Hydrological Models, is very promising. In addition, initiatives that can operationally provide seasonal forecasts and conditions about water shortages, occurrence of floods and energy production, in a global scale, are valuable especially for data scarce regions and countries where river basins extend beyond the political boundaries. In this context, I believe that the scope of the study is of interest of the scientific

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Discussion paper

community. In general, the methodology is sound and the manuscript is well written, providing an extension of previous studies about the skill of the PCR-GLOBWB to predict streamflows (Candogan Yossef et al. 2012, Candogan Yossef et al. 2013). Although I am not familiar enough with the state-of-the-art research regarding seasonal hydrologic forecasting, some questions arose after reading the manuscript and I was not fully convinced by the conclusions obtained in the study. Therefore, in an attempt to motivate further discussion, I have made some appointments that should be addressed/answered by the authors in order to the article be acceptable for publication.

Specific Comments

Firstly, I missed a better support of literature in the introduction, and authors should make an exercise of providing a suitable number of references. For instance, at page 2, lines 11-29, there is a long explanation about climate models, drivers of global climate patterns, atmospheric chaos and the use of ensembles for streamflow forecasts, supported by a single reference. You have mentioned that the capability of the global models to simulate streamflow was demonstrated in several cases, but what has been done in respect to [seasonal] streamflow forecasting? The study should be positioned among other existing research in this field, especially for continental (e.g. Zhao et al. 2016) and global scales (e.g. Yuan et al. 2015a). Did the experiments of coupling GCMs to hydrological models show skill on seasonal streamflow forecasting when compared to the ESP approach on those scales? It gives the reader an overview about the subject and allows to verify if the conclusions of your work agree to the existing studies or are pointing to the opposite direction. Maybe the article of Yuan et al. 2015b can be a good start to carry out this exercise.

Despite of using a specific workflow of the Delft-FEWS, I believe that the ESP generation procedure should be better clarified in text. In my understanding, the authors are producing a 32-member ensemble for each calendar month based on meteorological forcing data between years 1979-2010. This means that the ESP ensembles contain all the information of the observed meteorology (bias corrected ERA-Interim reanalysis with GPCP monthly rainfall observations) of the 'almost' same hindcasting period (1981-2010). In this sense, the comparison of ESP ensembles to ECMWF S3 seems to be unfair, because in a real situation we do not have perfect information about meteorology in the future. Did the authors leave the target year out when producing the ESP ensembles? The forecast years resampled from the historical record are required to be independent from the forecast target year, in order to avoid the inclusion of a perfect forecast. Maybe it is more convenient to generate ESP ensembles by selecting a random number of years from the historical period, i.e., not the entire period, excluding the target year (e.g. Mo et al. 2012; Yuan et al. 2016).

Another issue that I am concerned about is the definition of low and high flows. Thresholds corresponding to the 25th and 75th percentiles of the observed values and for both control and forecasting runs were respectively selected, meaning that streamflows are reaching values outside the "normal" conditions during 50% of the time. On the other hand, one of the advantages of climate models in respect to statistical methods is that the former is most suitable to predict extreme (rare) events, since the predictability of large-scale climate drivers such as the ENSO can be improved by assimilation of real time observed data in the numerical models. Especially for development countries, knowledge about the possibility of extreme droughts and floods are, indeed, much more important to predict than a simple detection of high or low flows. Would the results be the same if thresholds corresponding to 10th and 90th percentiles (still conservative for rare events, but enough for statistical significance) were selected?

Is there a specific reason for choosing the BSS to assess skill of ECMWF S3 over ESP method? Since BS is a metric for discrete events (two categories), the problem is that it does not account for the distance of ensemble members to the threshold used for low or high flows (BS values are too much dependent of those thresholds). Thus, why not including performance metrics such as the Continuous Ranked Probability Skill Score (CRPSS), which has been adopted in several recent studies involving seasonal streamflow forecasts (e.g. Arnal et al. 2015, Zao et al. 2016, Yuan et al. 2015)?

Particularly, I am very opposite to manuscripts that use an excessive number of tables to present results. The way you presented here sounds more like a report (10 tables x 20 rivers = 200 tables) than a scientific paper to me, which can discourage people to continue reading. I think the authors must demonstrate the ability to summarize the information produced and present the results in an appropriate manner; otherwise, it can be very difficult to draw conclusions. So, I am wondering: are all those information really necessary? If your objective is to assess the skill of the forecasting system, perhaps is better to focus in your Skill Score (BS can be moved to supplementary data). One possibility is to constrain your assessment to dry and wet seasons instead of all months. Conversely, if the authors really want to show results for each calendar month, I suggest to take a look at papers such as Schepen et al. (2016) and Yuan et al (2016), which are also handling results for many rivers. Finally, all those tables comprising results of BS and BSS for each month of the year can be moved to supplementary data.

Page 13, lines 34-36: One of the findings is that: "the apparent potential for improvement in seasonal hydrological forecasts by using better meteorological forecasts cannot be realized as yet with the model PCR-GLOBWB and the ECMWF S3 dataset". However, there is only a short description about the quality of meteorological forecasts, regarding some verifications conducted with the ECMWF S3 seasonal forecasting system (page 5, lines 16-24, without listing any references). I think that more information is needed to provide a clear understanding about the improvement of the ECMWF S3 over the ensembles of meteorological forcing used in the ESP. Thus, it would be better to verify the skill of ECMWF S3 (precipitation / temperature) forecasts prior to the hydrological assessments as it can be helpful to support your conclusions.

Technical issues

There is a misunderstanding about skill and Brier Score (BS) in the manuscript, so terms should be revised to avoid confusions. The Brier Score measures the magnitude of the probability forecast errors (authors are referring this to be skill), which is strongly influenced by the climatological frequency of the event in the hindcasting sample. For instance, if thresholds are selected for rare events, a good performance for the Brier Score will be obtained no matter if the forecasting system is less or more conservative, so low values of BS not necessarily implies in existence of forecasting skill. Conversely, skill must reflect the relative accuracy of the forecast over a reference forecast. You can do this by comparing a score (like BS) of a given forecasting system relatively to the BS of an unskilled forecast (conditional persistence, climatological forecast) or to another forecasting system, as done when computing Brier Skill Score (BSS) for BS ECMWF S3 / BS ESP. Therefore, you cannot say that skill is obtained through a comparison of forecasts to observed values or a control simulation (because these variables are used to compute the Brier Score, not skill). Authors should make clear that "Skill assessment was conducted in terms of Brier Skill Score" and also make the necessary adaptations.

I guess the term "actualized" is not well suitable in: pg. 10, line 35 "the percentage of theoretical skill actualized". I would recommend changing to "ratio of actual to theoretical skill". (There are other occurrences in the manuscript that should be changed)

Some parts of the text are too much didactic and do not fit well in a scientific paper. For instance, page 8, line 30 "In Table 2 for the Amazon, the color-coded first part which presents the theoretical skill for low flow shows that most of the BS values are coloured blue. This indicates that the ECMWF S3 forecasts are significantly more skilful than the ESP forecasts, i.e., the difference between the BS values is higher than 0.05." However, after handling the excessive number of tables, I guess this technical comment will be addressed.

Page 12, lines 34-35: "is the lowest in this continent", I could not understand if you are referring to semi-arid regions or to Murray-Darling basin. Also, add "-Darling" after "Murray" in line 36.

In the abstract, authors should include more details about results and conclusions. The current form is too vague to be presented.

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