Hydrology and Earth System Sciences Discussions

Interactive comment on "Long-range hydrometeorological ensemble predictions of drought parameters" *by* F. Fundel et al.

Anonymous Referee #1

Received and published: 16 July 2012

This manuscript presents results of 18 year retrospective monthly low-flow forecasts for a catchment in Switzerland, based on the varEPS atmospheric ensemble prediction system and a calibrated hydrological model. This manuscript shows potentially interesting conclusions, but it clearly lacks some rigour in the presentation of methods and outcomes, which make the results difficult to assess. I would therefore recommend that the manuscript should be revised following the general and specific comments listed below.

1 General comments

- This paper definitely lacks some clarity, rigour and detail in the description of drought characteristics (section 2.4) that will be studied in the results section, in the formulation of adopted verification scores (section 2.5), in the description of the methods (section 3.1, 3.2), the definition of threshold, etc., that makes the manuscript hard to review and the results difficult to assess. See all the related specific comments below.

- I am not convinced about the relevance of studying drought events at the scale of 32 days. Drought events, and especially hydrological drought events, usually last several months (see Vidal et al, 2010 for example). Consequently, the drought event characteristics considered here therefore lack some relevance by considering only truncated events. The relevant time scale for studying droughts is the seasonal time scale, not the monthly time scale. This should be at least commented in the paper.

2 Specific comments

- The use of "parameters" in the title (and elsewhere in the manuscript) is in my opinion misleading. "Parameters" is usually used in the sense of "model parameters". I would rather recommend using "characteristics" or "features". Additionnaly, I would recommend using "monthly" instead of "long-range".

- P6859 L17: "(Cacciamani et al., 2007)" instead of "Cacciamani et al. (2007)"

- P6859 L19 (and following lines): the use of "coupled" is in this case not appropriate. The hydrological model is only forced by outputs from the atmospheric model, and there is no upward retroaction.

- P6860 L7: I would also emphasise the uncertainty associated to the choice of the hydrological model, and recommend to mention that the use of a multi-hydrological model and/or an multi-parameter ensemble could definitely be useful as well.

- P6860 L20: I believe that this catchment has a relatively natural regime (please give some reference if possible) and I would recommend to emphasise it, as low-flows are potentially highly influenced by any reservoir operation in a catchment.

- P6861 L9: "an" instead of "and"

- P6861 L18: I don't understand how the "954" figure is obtained. It should be clear in the paper how many members are available for the reforecasts (there are many different figures in this paragraph: 954, 51, 5). Without this clarification, it is not possible to assess the results presented.

- P6862 L9: some details about the downscaling method are referred here. Are they different from the dedicated paragraph in 2.3? Please specify it and move it to 2.3.

- P6862 L4-23: What is the time step of PREVAH?

- P6862 L12: redundant "a"

- Fig 1: is the y-axis actually in mm/h? Or in mm/day? Cf. the comment on the time step.

- P6862 L23: is it -0.003 m3/s?

- P6862 L23 and Figure 1: there is a discrepancy between the study periods. Is it 1991-2008 or 1981-2009?

C3079

- Fig.2: some central information about the drought event definition (the choice of the longest under-threshold period, timing definition, etc.) is given in the legend of the figure. Please move it to the text.

- P6863 L20-23: I don't understand these two sentences, with some potentially important information (lead-time dependency of low-flow threshold??).

- P6864 L4-5: "By doing so, the lead-time is no longer a possible source of forecats error." I don't understand this sentence.

- P6865 L1-12: The application of the 2AFC score to the data used here (period, lead-time, quantiles) should be described here (including mathematical formulation). Otherwise the reader cannot assess how it is applied and how useful it is. Additionally, it should be specified what is the climatology reference here; is it the 31days moving average over the whole period like in Fig.1?

- P6865 L23 and Fig. 3: it not clear at all how the 2AFC score is computed to get this figure. See my general comment on that and the comment above, and give an example, for a given quantile and lead-time. And how is the overlap (and the autocorrelation that follows) between lead-time (32 days) and time step (7 days) taken into account into this computation?

- P6865 L24-25 and Fig. 3: "Peak flow forecasts [...] show skill for up to 15 days". Given the color scale and the score definition, I would say that there is some skill (i.e., score above 0.5) until the end of the lead-time period. Please clarify this.

- P6866 L3-4: Given this (right) statement, could the persistence be used as the reference when computing the score?

- Fig. 4: Once again, there is no information about the computation of the 2AFC score over the whole period, with the overlap, etc.

C3080

- P6866 L11-18: I'm confused here: you wrote above that "the 2AFC score is not affected by systematic biases", and then you define good or poor initial state by looking at errors between observed runoff and reference run runoff. Plus, P6863, you mention that "a separate threshold for forecast and observed runoff was used". All of this should definitely be justified, clarified and made explicit.

- Fig. 4: "The polygons show the 90% confidence interval, found by resampling 1000 times". I don't understand, and this method should be specified in the text of the manuscript

- P6866 L15: "For both groups the probability of exceeding the 85th and the 15th quantile is verified." I don't understand. Please explain.

- P6866 L21-23: " [. . .] which threshold would be best [...]" This comment is somewhat inconsistent with the paragraph on P6863 (which I definitely agree on) that comments on the different threshold needs for different users.

- P6867 L2: "stays fairly constant within the uncertainty bounds". Please rephrase.

- P6867 L4: "higher" instead of "heigher"

- P6867 L4-5: It seems essential to provide the reader with the number of events!

- Fig. 5: Again, please describe the resampling approach.

- P6868 and Fig. 7: I don't understand if this is a weekly or daily time series. If weekly, it is not clear how the overlap between lead-time and forecast time step is taken into account here.

- Fig. 7, legend: the "monthly varying 15th quantile"? Its is really confusing: is it "monthly" as here, seasonally as in P6863 L10 or daily (31 day moving average) as in legend of Fig. 1. Please clarify and make it consistent throughout the paper.

C3081

- P6868 L17-18 and following: "No seasonal dependency of low-flow event is apparent". What does that mean? How do you justify this statement?

- P6869 L5-7: "Higher value scores [. . .] detection threshold" How do you justify this statement?

- P6869 L7-8: "The highest value score [. . .] the event". How do you justify this statement?

- P6869 L9-10: "The maximum value score reached is generally independent of the duration, severity or magnitude of the event". This is not what is shown in Fig. 9. Please clarify this.

- P6869 L11-18: I don't understand this whole paragraph. This seems to be related to the next paragraph (section 3.5)

- P6869 L20-27: there is a confusion between "lead-time" and "timing" (which is not cleraly defined, is it the start, end or middle of the event?). Please rephrase this whole paragraph.

- P6869 L 27-28: "Of all forecast events [. . .] false alarms" Why is this statement given here? I can't see any relation with the event timing.

- Fig. 10, legend: again, confusion between "lead-time" and timing". Please define clearly (in the text) the studied drought event characteristics.

- P6870 L23-24: the importance of initial conditions in the skill of seasonal hydrological forecasts has been also recently emphasised by Singla et al. (2012) for French catchments.

- P6871 L1-7: This is the case with climatic forecasts (as opposed to meteorological forecasts) with lead-time higher than 15 days, from monthly to seasonal or even decadal.

C3082

- P6871 L20-24: I am still not clear about the impact (necessarily good) of defining separate thresholds for observed and predicted runoff. Please make it clear once for all.

- P6871 L25 and following: I am not convinced about the contribution of a higher resolution RCM for this kind of application. I would rather stick to much more flexible statistical downscaling methods, as used by Singla et al. (2012).

3 References

Singla, S., Céron, J.-P., Martin, E., Regimbeau, F., Déqué, M., Habets, F. Vidal, J.-P. (2012) Predictability of soil moisture and river flows over France for the spring season. Hydrology and Earth System Sciences, 16, 201-216. doi: 10.5194/hess-16-201-2012

Vidal, J.-P., Martin, E., Franchistéguy, L., Habets, F., Soubeyroux, J.-M., Blanchard, M. Baillon, M. (2010) Multilevel and multiscale drought reanalysis over France with the Safran-Isba-Modcou hydrometeorological suite. Hydrology and Earth System Sciences, 14, 459-478. doi: 10.5194/hess-14-459-2010

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 6857, 2012.