

## ***Interactive comment on “An operational hydrological ensemble prediction system for the city of Zurich (Switzerland): skill, case studies and scenarios” by N. Addor et al.***

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Overall this is an interesting and thorough case study of an HEPS for the city of Zurich. The study provides quantitative and graphical evidence which is used to assess the usefulness of probabilistic forecasts in this context. The paper is balanced and the discussion is interesting. I find the new graphical representation of probability forecasts useful (if a bit cluttered – but then I’m not currently forecasting floods in Zurich!). I find no major problems with the manuscript and suggest publication after some minor corrections.

1. Can you provide more information on what 2-way feedback you have undertaken

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with your stakeholders to date (not including the new module – page 739)? This is not entirely clear from the manuscript and would be interesting information to have to place your study in context.

2. How are the 16 representative members selected? Do you have any evidence regarding to what extent these 16 preserve the information contained in the 102? If this is contained in a reference somewhere then please make this clearer. (page 721, line 22). What are the implications for your results of the underdispersivity noted (page 732)? The discussion of COSMO-LEPS could do with some more depth here. Is any of the underdispersivity transferred from the original ensemble?

3. Can you further discuss the implications of using proxy observations (HREF) in lieu of actual observations (e.g. as per discussion in Pappenberger et al (2008)). There are advantages and disadvantages which could be further discussed, especially if you focus on the end product of the operational rules of Lake Sihl. (Page 723, line 24). Have you statistics (rather than just the graphs, fig 8 etc) to demonstrate the relationship?

4. The discussion of F and FAR might benefit from more contextual discussion about the requirement for a long time series. For what length of time series, number of events etc. do these indicators stabilise. The crux is – when can they be trusted? (Page 727...)

5. The discussion of uncertainty in hydropower would surely be more useful earlier (page 730), alongside a clearer discussion of all uncertainties in your method/data.

Typos etc.

- i. Page 716, line 5. Should be “model chain” – please check throughout manuscript
- ii. Page 716, line 11. Should be “framework with which to compare”
- iii. Line 17. “overall COSMO-LEPS-based ...” instead of “overall outperform”
- iv. Page 718, line17 “For the August...” – please check throughout manuscript
- v. Line 24. “forecasts from an economic”

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- vi. Line 27 “economic evaluation”
- vii. Page 719, line 1. “consisting of the combination of calibrated”
- viii. Page 720, line 9. “strives to achieve the following”
- ix. Line 27. “for a comparatively”
- x. Page 721, line 8. “setting of the Sihl”
- xi. Line 9. “enables the illustration of”
- xii. Line 18 “Probabilistic atmospheric”
- xiii. Page 722, line 19. “nested in the ECMWF”
- xiv. Page 723, line 17 “method is an intermediate”
- xv. Page 724, line 26. “extreme discharge forecasts”
- xvi. Page 725, line 3. “assessing the model performance for”
- xvii. Page 727, line 9. “threshold exceedance”
- xviii. Line 11. “probability of exceeding Q”
- xix. Page 729, line 5 “exceedance”
- xx. Line 16. “Only low water”
- xxi. Page 730, line 20 “corresponds to”
- xxii. Page 731, line 1. “forecast”

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