

Interactive comment on “Long-range forecasting of intermittent streamflow” by F. F. van Ogtrop et al.

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Thank you everyone for some excellent comments on this paper, I believe this is possible to move forwards. I think the exchange between the authors and the reviewer comments has been very productive and clearly generated some interesting discussion. My only issue is that I want to make sure this is captured fully in a revision, I am happy if this is done the paper should not need to go through a review process again as the reviewers were favourable to publication.

Perhaps I'd like to highlight the changes I see as critical to complete this process:

1) I think the cross validation between results (Ashish Sharma) does need to be im-

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proved from the paper. I would like the authors to think about how to do this (with more clarity than is currently realised) without considerably lengthening the paper. But there is scope for presenting a more detailed analysis I think. Please can the authors work on this, I think it adds value to the paper (as the authors agreed)

2) I want to make sure the discussion of the IQQM data (David Post) to drive this research is captured in the discussion sections. The response of the authors bring up some interesting points but they do not say what they will do in a revised manuscript. This needs more clarity and it might mean some investigation of the magnitude of the flows generated is warranted. Please can the authors address this issue of using IQQM more clearly in the revised manuscript

3) I do believe it's important to comment further in the paper about the issue of different time lags (David Post). I note the authors state in their paper “Different lag times or combinations of different lag times may also be considered, but this is not further pursued in this paper” I realise the paper is focused on 12months as being the most relevant for the authors interest but I feel some comments are warranted in connection with the literature noted and how this might be extended (and the difficulties of doing so) where strong seasonal signals exist. I wouldn't expect any methodological developments here (I am sure a different paper could develop this) but the authors did not respond to this comment and I think it's useful for giving the results some context

4) I do not believe the authors address the final issue (David Post) about if the probabilities decreased in other catchments. Perhaps they could explain how this will be dealt with in a revised manuscript (I wasn't clear from their response). It may well be dealt with from some assessment that is discussed in a minor way for clarity. I did not see this as a major need for the papers current focus.

5) Can the revision make sure the points in p688l3 are included in the discussion in the paper. It wasn't clear how this would be achieved

Thanks again for everyone's time on this paper, I look forward to receiving the revi-

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sions for the paper and confirming these have addressed the main points raised, kind regards, Jim

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 681, 2011.

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