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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Assessing the value of seasonal hydrological forecasts for improving water resource management: insights from a pilot application in the UK" *by* Andres Peñuela et al.

Anonymous Referee #4

Received and published: 27 April 2020

Dear authors,

Thank you for this interesting research, written up in a well-organised and clear paper. Overall, I have not hesitation to recommend your paper for publication. I do agree with you, that this kind of research, with continuous simulation of operational water management to test new information sources, methods, or strategies, is valuable for science and in particular for bringing findings forward to practice in an informed and iterative way.

I appreciate in particular your Conclusions section, and clear description of data used, methodology, and presentation of results.

General comments

I have the following main comments:

- The authors assumed the water demand to be known in advance and to be equal to the observed reservoir releases (line 220). This may be an important assumption. If more than needed was pumped-in for storage, this would perhaps also lead to releasing more than the actual demand. Could the authors reflect on this? The actual releases are the result of the current water management priorities, which focus on water resources availability. Could this have led to the forecast value also being maximum for the rap scenario's? Could the authors address this with a limited sensitivity analysis, varying water demand? (if time, sensitivity analysis of other aspects would be interesting as well, e.g. towards the set-up and settings of the NSGA optimisation experiment.)

- To my view, the results show that the bias correction applied, did not work in this particular case study, Figure 3 (only changed sign of MAE from under- to over-prediction, as authors also indicate in line 364). Could the authors reflect on this in their section on limitations of the research? What may be the reason? Could other bias correction methods work better or is this not to be expected (e.g. perhaps higher forecast skill is needed to begin with, for post-processing methods to be effective)? The poor performance of the bias correction also connects to the following comment.

- The Discussion section contains notes and even recommendations on the use of bias correction. In my view, the poor performance of the bias correction in this case study, and the fact that, as the authors point out, indeed there is only one particular case study analysed here, do not warrant such discussion on the merits of bias correction. Could the authors reflect on this, and depending on whether they agree or disagree, adjust the Discussion accordingly.

- The Discussion section also recommends use of ensemble (probabilistic) forecasts in operational management, which is supported by the research findings, but then con-

nects this to UK policy recommendations on long-term water resources planning. This I think is a bridge too far, and not needed. I would favour the Discussion to be less broad, and stay focused on the research findings presented (see my detailed comments for specific suggestions on where and how to make the Discussion section more specific). This leads to my next comment.

- I miss a more in-depth discussion on the forecast skill of the DSP used, and the influence of forecast lead time throughout the analysis chain. The CRPSS results nicely show that only for the first two months the uncorrected forecasts have skill (the bias-forecasts do not have skill). Is this positive skill utilised by the operational water management strategies simulated. Could the authors suggest ways on how to capitalise more on this positive skill, e.g. by using DSP for the first 2-month lead time, and using ESP for months 3 to 6?

- Lastly, to come back to the motivation of the authors to bridge science to practice, I would like to see observed and simulated releases for sample priority scenarios and years. These actual releases throughout a season is what operators will recognise and this will enable a discussion on how and to what extend the use of ensemble seasonal forecasts would lead to changes in operation.

Detailed comments:

- line 275: Indeed the bias corrected DSP "improves" skill for longer lead times, but only from negative skill to less negative skill. I would suggest to point that out.

- line 281: Yes, with bias correction there is "some improvements for some lead times", but still with negative skill. Rather than pointing out "some improvements for some lead times", I think it is more relevant to point out here that the bias correction as applied here, in this case study, is not working and even has adverse effect on forecast performance.

- line 311: The question is why? Again (See my first general comment, and note that

resource availability in the results varies only with 1-2%) this may indicate a constraint set-up, favouring a focus on rao. Please reflect on this.

- line 382: "Our results show that on average bias correction slightly improves the DSP forecast skill (as measured by CRPSS and mean error)". I do not agree here. When looking at the results, also on average, bias correction reduced the CRPSS for the first 2 months lead time where the DSP had skill (bias correction made skill less negative for further lead times, but still negative), and it changed the sign of the average error but did not reduce it, so bias correction was not working well.

- line 392: I agree with the authors. Based on this particular study, not much can be discussed on the merits of bias correction. Instead I would recommend to focus discussion in the paper more on the skill of the ensemble DSP (slightly positive for the first 2 months), how and why this is or is not being used in the water system operations simulated (see my last two General comments above).

- lines 399-404: While I agree with this statement in general, I do not think the research presented provides sufficient supporting evidence. Nor is it a surprise or a new finding.

- lines 413-416: This is quite a strong recommendation not substantiated with any analysis/numbers on what these 'costs' are. The authors could consider leaving this out.

- lines 433-436: I am not sure if the link with Long-term water resources planning is appropriate, and also I think it is not needed to make the case of risk-based operation on the basis of ensemble (probabilistic) forecasts. The results of your study do show this nice enough. The authors could consider leaving this out.

- lines 458-467: Yes, developing such toolkit and making available to the water management organisation is very valuable. Indeed question here would be to what extend the toolkit is customised to the specific case study, and how much time/effort customisation to a new case study would require. Could the authors reflect on this in the text?

- line 475: DSP is now more readily available from international weather forecast centres and more easily processed, such that this by-sentence on "ESP being more easily derived", in my view is perhaps becoming less relevant. Also because, as the authors describe, they have provided a Toolkit for ease-of-use.

Please see for suggested technical changes (editorials) the annotated pdf.

Thank you and with best regards.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2020-89/hess-2020-89-RC4-supplement.pdf

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