

## ***Interactive comment on “Influence of multidecadal hydroclimate variations on hydrological extremes: the case of the Seine basin” by Rémy Bonnet et al.***

**Anonymous Referee #3**

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Influence of multidecadal hydroclimate variations on hydrological extremes: the case of the Seine basin Bonnet et al.

Overall this is an interesting paper that develops reconstructed flows for the Seine basin back to the 1850s. Variability in the reconstructions is then assessed and linked to SSTs. While this work will be of interest to the readers of the journal I do not think it is ready for publication. It is difficult to read at present and clarification is needed at times as to what is done. A fuller discussion of the limitations, uncertainties and assumptions of the work is needed. Discussion of other modes of climate variability that could be influential would also be welcome.

I highlight some issues below which the authors should address. I hope that these are seen as constructive, as I believe this could be a nice addition to the literature.

C1

Title – hyphen multi-decadal

In addition hydro appears twice in the title, while the paper really only deals with SSTs as a source of variability. Suggest title change to “Influence of multi-decadal variability on hydrological extremes: the case of the Seine basin. You could include SST before variability if you wish.

Overall the manuscript needs to be thoroughly edited. There are lots of problems with the use of plurals throughout eg. Precipitations, which should always be singular.

It is not clear to the reader what you mean by multi-decadal phases. This appears throughout the paper and I strongly suggest you define use of it early on or use a different term. I assume it means wetter than average or drier than average periods.

The abstract states the obvious a little too much, eg. Wet periods are conducive to more flooding, dry periods to droughts. I would be more interested to learn what specifically your reconstructions offer and the new insights they provide.

In addition, the final sentence of the abstract gives the impression that this paper looks at dynamical chains in how ssts influence atmospheric circulation. It does not.

The literature review mentions the AMV as an important source of variability, but what about others modes of variability eg. The NAO, East Atlantic Pattern etc. There is more than just the AMV at play and it is my view that this should be reflected in the literature review. These other modes of variability could also be examined for their relationships with the extremes in the reconstructed flows or discounted if the literature has previously addressed these issues.

In your critique of 20CR on page 2 line 30 you explicitly state that that “this approach is far from optimal, as it does not make use of the long-term meteorological observations that may exist”. I would strongly recommend a citation for this or remove.

There are uncertainties not considered in this paper related to input data, hydrological modelling, other datasets used etc. It would be welcome if the discussion could, in a

C2

paragraph, flesh these out.

As mentioned, the methodology is complex and difficult to follow. The use of analogues to derive daily precipitation seems to move from 2800 samples to 60 to 3. Why three, why such a decrease in sample size and are you left with a large enough sample?

Similarly there are assumptions made that things like channel dimensions and conveyance capacity remain stationary over time. Recent research has shown that they also react to variability Slater, L.J., Khouakhi, A. and Wilby, R.L., 2019. River channel conveyance capacity adjusts to modes of climate variability. *Scientific reports*, 9(1), pp.1-10.

In terms of your methods does your use of analogs constrain the variability to that of the shorter record? The methodology is complex and hard to follow as it is written. Previously published work on which this data is based needs to be shortly summarised.

In addition it would be helpful to the reader to provide a flow diagram of the key steps in the workflow. For example, I have read the paper multiple times and at the start of the results I am unsure what the reference simulation is.

I don't like the word anthropized – heavily impacted would work just as well.

It appears that the reconstructions persistently underestimate the annual maxima (fig 4). What does this say about your reconstruction, does it affect your results and how might this be improved?

Have the piezometric level data been quality assured?

It is stated on page 7 line 29 that “The length of the reconstruction allows to show that multidecadal variations are also present before the 20th century. . . .” Is this not expected. In the same sentence what do you mean by negative phase.

Throughout you could quantify these above and below average phases in terms of the magnitude of their anomaly.

C3

Indeed what can your work tell us about an appropriate baseline for assessing changes in the Seine flows – one that accounts for the types of variability you see. Thirty year baselines often only sample one component of multi-decadal variations.

There are a number of statements that require clarification or a more precise wording, such as: multi-decadal phases? P7 line 10 over what period are correlations derived? Page 8 line 23 A partially captive part of the aquifer is not represented in the model – what is a partially captive part? Pg 11 line 11 a large number of stations have a ratio between one and two. . . .with a ratio of 1.1 consistent with the reconstruction in that region. I cant interpret this. Pg 11, line 24 strong positive multidecadal phase?

Is it fair to call a day with flow above the 95th percentile a flood day. Is it fairer to say a high flow day? The same issue applies to drought days, are these more fairly described as low flow days? In reality you are not strictly looking at floods and droughts in this part of the analysis.

It would be useful to outline a potential or plausible chain of causation as to how SSTs in the North pacific relate to Seine flows.

Do your extended flows add additional insight into AMV related variations in flows than done in previous work. Has the magnitude of anomalous periods been the same or different in your reconstructions relative to previous work?

Section 4.1 is hard to follow, it might be worthwhile organising by season. At times the text jumps from season to season

I think section 6 needs to come earlier in the paper, given its importance to the message of the paper.

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Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, <https://doi.org/10.5194/hess-2019-320>, 2019.

C4