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Interactive comment on "Multi-decadal river flows variations in France" *by* J. Boé and F. Habets

Anonymous Referee #2

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General Comments

This paper investigates the existence of multi-decadal variability (MDV) in annual and seasonal flows in French rivers, and explores some possible driving mechanisms (precipitation, large-scale atmospheric circulation, oceanic variability, hydrological processes). The analyses presented in this paper are of interest for the hydro-climate community and are quite convincing. I would therefore recommend publication, subject to the following moderate revisions:

1. I agree with Reviewer 1 that the paper would benefit from improvements in its structure. An easy-to-implement improvement would be to use level-2 or 3 subtitles to make the overall structure more apparent to the reader. For instance, section 2 could be split into 2.1 station data (Q then P), 2.2 reanalysis data (P, SLP, SST, hydrology), and 2.3. statistical tools (filtering and testing). Section 3 could be split into evidence

of MDV / link with P and T, section 4 into P / SLP / SST, and section 5 into motivation / model efficiency / impact of soil moisture on summer flows / MDV in soil moisture. Some other reorganization might also be valuable, for instance the presentation of the data is a bit confusing in the present state, mostly due to the 3 distinct precipitation datasets. Rather than an organization based on "data product" as currently done, I would recommend an organization based either on the data type as suggested above (station data vs. reanalyses), or maybe on the hydrometeorological variable (=> all 3 P precipitation datasets in the same section).

2. The existence of MDV is convincingly demonstrated for spring and annual flows, based on the visual inspection of the series and the MTM spectral analysis. However, the demonstration is less convincing for other seasons, because these two specific analyses are not shown for winter, summer and autumn flows. The authors are mostly using the results of figures 3 and 4 to demonstrate the existence of MDV for these seasons, but I don't think these results are sufficient for this purpose. In Figure 3, the ratio is primarily controlled by the properties of the low-pass filter. As a very rough approximation, considering that the filter used by the authors is similar to a 18-years moving average, one would expect that for an iid series (hence displaying no MDV whatsoever) the ratio would be close to 1/sqrt(18) = 0.24. While the boxplots for MAM and YEAR are clearly larger than this value, suggesting some form of autocorrelation that may be multi-decadal, the boxplots for other seasons are actually quite close to 0.24. Maybe the authors could implement a Monte-Carlo analysis, by applying their filter to iid series, to evaluate a "critical value" for this ratio? In a similar vein, the fact that two periods yield significant differences (Fig 4) is not a proof of MDV. I guess the best way to convincingly demonstrate the existence of this MDV for other seasons would be to show figure 2 for all seasons.

3. I think a discussion on detrending would be useful. Firstly, I think it is introduced much too quickly in section 2 (p. 11865 lines 25-29): the authors need to explain the motivation behind it. The authors actually discuss this issue later in the paper (p. 11876

lines 21-29), but I would recommend moving this discussion in section 2. Secondly, this discussion also clearly highlights potential caveats of detrending, and I'm personally not convinced that detrending is a good thing in the MDV context of this paper. According to the authors, "it is necessary to remove the potential effect of long-term anthropogenic climate change (. . .) by detrending the data". Somehow, this implicitly assumes that the computed trend is indeed caused by anthropogenic CC! This sounds like a massive assumption in the absence of any attribution study. Moreover, in the presence of MDV, a trend computed on raw data is likely to be spuriously created by the MDV behavior – ideally, removing the MDV would be needed before computing the trend! This is obviously a chicken-and-egg situation that has no easy solution as far as I know, but I think it deserves some discussion. Lastly, removing a linear trend on such long periods is also questionable, as discussed by the authors for SST (p. 11867 line 1). My feeling is that detrending makes sense for temperature variables, which indeed display consistent long-term trends which have been attributed to anthropogenic CC. I'm much more circumspect for hydrologic variables (precipitation and runoff), since the literature generally shows weaker and much less consistent trends. I'm not suggesting the authors should redo all analyses without detrending, but rather that they should justify their choice more precisely, and discuss potential caveats. Lastly, it is sometimes unclear whether or not detrending has been implemented (e.g. in figure 2). In some cases, detrending is specified in the figure caption but not in the text (e.g. for figure 4). In order to avoid ambiguity, I would recommend systematically writing "detrended" whenever needed (and even maybe "undetrended"!).

Specific Comments

p. 11865 line 9: Are you using civil or hydrological years for the annual mean? Please make it explicit (although I don't think it would change anything given the low-pass filtering).

p. 11865 lines 11-14: Did the authors consider simply leaving these values as missing? I think correlations and filtering can accommodate missing data quite easily – I'm less

sure for MTM. The authors should elaborate a bit on this (what technical difficulty with missing data led to this decision of filling?), because I agree that the filling approach is a bit crude (and temporal interpolation would probably not be better indeed).

p. 11865 lines 19-21. To be honest, I find the decision to keep strongly influenced stations disputable. It's probably acceptable at the annual scale, but at the seasonal scale, the effect of dams may be to transfer flows between seasons. Moreover, although these stations are indeed flagged in figure 1, the interpretation of the results in the following sections does not really use this information. Lastly, it seems that some of these strongly influenced stations are not present in all figures. For instance, the point near Paris (is it the Seine@Paris?) seems absent in figures 4-5, appears again in Figures 6-10, then disappears again from Figure 11. This needs to be clarified.

p. 11866 line 6. Please indicate the spatial and temporal resolutions of this reanalysis (and of other subsequent reanalyses as well).

p. 11866 lines 11-24: the precise definition of SLPI should be given here rather than in a subsequent "results" section.

p. 11866 line 27: please indicate which low-pass filter is used.

p. 11867 line 7-9: this is a bit confusing – I understand that a linear regression between SST and CO2 is used; as a result, the evolution with respect to time is not linear any more. Is it correct? Please reword to avoid ambiguity.

p. 11867 lines 10-12: this sounds a bit speculative – unless of course the authors actually trialed other de-trending approaches and saw little difference, but in this case I would slightly reword this sentence to make it more explicit.

p. 11867 lines 26. Maybe the authors could summarize which variables will actually be used in the case study?

p. 11869 lines 4-5: I disagree that Fig. 3b alone shows the existence of MDV (see general comment 2).

p. 11870 lines 8-13: I think that this paragraph (and the corresponding figure) is a bit too short to bring any significant additional information: it could therefore be removed. Moreover, the wording is a bit confusing: a change in the mean is already a modification of the distribution of river flows! Do the authors mean a change in the shape of this distribution? But in this case, the changes reported in Figure 5 do not seem inconsistent with a simple translation of the distribution (shape and variance remaining unchanged).

p. 11870 lines 23-24: As noted by the authors, Mediterranean France is indeed quite distinct from the rest of the country – but in this case, wouldn't it make sense to use precipitation averaged over non-Mediterranean France as an explanatory variable?

p. 11872 line 14: except maybe in the Mediterranean area?

p. 11873 lines 13-17: I agree that this would be needed to formally established causality. However, given the difficulty of AOGCMs to reproduce low-frequency variability and its impacts (as mentioned by the authors in the introduction), is it really feasible? A short reminder of this difficulty could be added here.

p. 11873 lines 23-29: this is a bit confusing – maybe because the exact meaning of "direct causality" is unclear to me. In my eyes, the fact that negative precipitation anomalies in spring may impact summer flows through the catchment memory sounds like a direct causality link. Maybe "synchronous" or something similar would be better than "direct"?

p. 11874 line 10: the differences in Figure 4h are so small that I wouldn't use the adjective "larger" (especially in South-Western France where many streamflow stations are located)

p. 11875 line 20: good correlations may hide systematic biases – is it the case here? Maybe reporting Nash-Sutcliffe efficiencies would be more appropriate?

p. 11876 lines 1-2: I have to confess that I find this sentence very optimistic. . .there are

many examples in the literature of models "having the good answer for the bad reason" (and not only in hydrology!). So I wouldn't say that a good agreement with observed flows is sufficient to claim that other components of the continental water cycle are very likely well simulated. I would suggest moderating this statement.

p. 11876 line 8: Isn't it a bit risky to use a single-day SWI (31/05)? It probably depends on what is exactly the "soil" represented is SIM, and therefore what is the temporal dynamics of daily SWI. Can it react quickly to some moderate precipitation event? (therefore yielding a "high" SWI on 31/05 but that would decrease to a much lower SWI value in just a couple of days?). A few words of explanation would be valuable.

p. 11879 lines 1-14. I find the distinction between interannual variability and MDV a bit confusing. I have the feeling that the authors consider that using raw annual (or seasonal) data corresponds to studying interannual variability, while low-pass filtering is necessary to studying MDV. I would disagree with this: a MDV signal can be seen in unfiltered data, as nicely illustrated in Figure 2. Moreover, establishing a link between a climate indice with a marked MDV component (such as the AMO index) and unfiltered data is also a sensible way of studying MDV. Filtering is indeed very useful to make MDV behaviors much more visible in figures; however, significance is more difficult to reach due to the large autocorrelation induced by filtering. Consider for instance figure 9: correlations of about 0.8 are needed before reaching significance (with roughly 100-year long series). I wouldn't be surprised if the same figure based on unfiltered data yielded similar results in terms of significance, although with much lower correlation coefficients! Maybe the authors should try to clarify this distinction between interannual variability and MDV.

p. 11879 lines 15-16: see general comment 2.

p. 11880 line 6: I find this statement quite optimistic: if a large reservoir that transfers a significant part of the flow from one season to the next is built in the middle of the study period, the MDV assessment could be significantly impacted. This statement needs a reference in my opinion.

p. 11881 lines 23-29: references would be useful.

p. 11882 lines 5-8: Haven't such assessments already been reported in the hydrologic or climate literature?

Figures, general comment: axes labels are sometimes a bit small, please scan all figures and increase font size if necessary. Moreover, in figures with multiple maps sharing the same colorbar, it would be better to show a single large colorbar (in the present state, some replicated colorbars are very difficult to read, e.g. figure 4).

Figure 1: A histogram of catchment sizes would be useful, or at least the range of catchment sizes should be mentioned in the text.

Figure 3: I think it's worth showing the same figure for temperatures, given the comparison made in the following figure 4. Same comment for figure 6.

Figure 5: Is it after detrending?

Technical corrections

p. 11869 line 23: “or” rather than “nor”

p. 11873 line 7: robustly estimate

p. 11882 line 5 : Whether or not climate models are able to capture . . .

caption of figure 10: automn <-> autumn

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11861, 2013.