Reply on RC2
Amar Halifa-Marín et al.

Author comment on "A multivariate-driven approach for disentangling the reduction in near-natural Iberian water resources post-1980" by Amar Halifa-Marín et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-565-AC2, 2022

The study presented in this manuscript investigated winter precipitation and reservoir inflows on the Iberian peninsula with the objective of attribution of recent declines in these inflows in particular to climate and landcover changes. While the topic is generally interesting to the region, the manuscript unfortunately does not present the material clearly enough for publication.

Thank you very much for your kind words and for the invaluable time spent in a detailed and insightful reading of our manuscript.

The title indicates where more clarity and rigor is needed: neither is "resource scarcity" clearly defined and analysed, nor does it become clear what is meant by a multivariate-driven approach'.

Thank you for your comment. The title has been modified, as "A multivariate-driven approach disentangling the abrupt reduction of near-natural Iberian streamflow post-1980", and we think that it adequately addressed the topic of our study, given that the manuscript aim/study the contribution of multiple variables to the water inflows (streamflow) declining.

Multiple variables as drivers of change are studied, but not together in a multivariate approach. An overall attribution approach is not clearly developed or at least not understandably presented. Overall, phrasing of the manuscript is often imprecise and will require a lot of improvement before this manuscript could potentially be considered for publication.

A lot of changes have been applied to the manuscript, addressing your recommendations. We hope that we made significant improvements to the manuscript.

Major concerns are:

- There is no clear attribution methodology and a mix of assumptions regarding the type of time change: break points, but then correlation between variables (assuming linear relations over time), quantile-quantile tests, .... Many different indices are used, transformations and individual analyses are carried
out. Unfortunately I did not understand how they together should arrive at a result regarding attribution of reservoir inflow change (abrupt) to different causes with different time change signals. A combined hypothesis and truly multi-variate analysis of cause-effect relations is missing.

Thanks for your comment. Our work assesses the variability of wintertime Iberian streamflow in a long-term analysis. We quantified the trend magnitudes in order to analyse the recent evolution of streamflow in the target catchments (NENWIRES). This procedure allows us to confirm whether our data shows similar patterns of tendency which has been noticed in the literature. But our study particularly focused on the analysis of abrupt changes in water inflows (streamflow) post-1980 (echoing the call made in some references cited in the manuscript, for assess the potential propagation of the abrupt change registered in wintertime precipitation into streamflow series within the Iberian Peninsula). Given that our results noticed that water inflow changes cannot be essentially associated to precipitation variability in all NENWIRES series (target catchments), we thus evaluate the contribution of other variables. Nonetheless, we think that the Q-Q analysis and linear relationships also provide results necessary to establish our conclusions. Anyway, the drafting has been amended so as to render the text clearer with regard to readers.

- Overall, phrasing of the manuscript is often imprecise. The introduction reviews many aspects not really addressed later and does not really follow a clear flow of thought. In the last paragraph of the introduction the scientific aim of the study does not become clear. The paragraph states three "secondary aims" but no primary aim. A dataset and method (mentioned before, but not clearly named objective or aim) cannot be a research aim, but are necessary materials and tools. Aims: 1) Attribution of what to what exactly? 2) Propagation OF ? the winter precip deficit INTO ? - wrong prepositions make this impossible to understand. Into the annual net reservoir inflow? 3) What is meant with "confluence"? How can drivers flow together? This choice of terminology makes no sense.

Thanks for your comment. Phrasing of the manuscript has been modified attending these recommendations, as well as the aims of the manuscript currently are: “And this contribution aims at: 1) quantifying the tendency of near-natural water inflows (recent evolution); 2) assessing the potential propagation of the post-1980 abrupt WP reduction to the water inflows series; 3) analysing links between the abrupt changes detected on hydroclimatic variables and the NAOi enhancement; 4) reporting the catchments where water inflows are not mainly modulated by WP/NAOi changes; 5) disentangling other factors that contributed to the water inflow changes; and 6) identifying the principal trigger of water inflow changes depending on climate conditions in basins”.

Another example is line 221-222 where an aim is phrased in the method section (should be in the introduction), followed by "this approach..."...but which approach (method) is meant is unclear as none is named yet. The order of sentences is not logical to the reader, I am afraid, and this would make more sense as a concluding sentence at the end perhaps?.

Right, we try to use a more accurate term in the new version of the manuscript. Thank you for pointing out.

- As already noted by Rev. 1 the use of terminology should follow common climate risk research conventions.

- "Water scarcity" is usually used from a human perspective of water demand exceeding water supply (usually for people, not really used as a term for
We have tried to use a more accurate term in the revised manuscript. Thank you for pointing out.

- p2 line 39 "vulnerability of freshwater" - only people, society, economy etc. can be "vulnerable" to climate change impacts such as e.g. a lack of water. I am not sure what is meant here but correct might be: ...study the vulnerability of the Iberian P. to a decline in freshwater supply (as an impact of coupled climatic and landcover change?). However, this would imply that the social vulnerability is studied and that appears not to be the case - here the impact of climatic change on water supply is studied (not the social aspects of the demand, which might define vulnerability of certain sectors).

Right, thanks for your recommendation. This sentence has been amended in the manuscript.

- Data and Methods contain much unnecessary details about file formats and statistical analysis, but do not state what the methods do regarding an overall method of attribution of changes in y to changes in x. Many methods are missing - the results section provides many surprises for the reader.

The Section 2.3 (analysis procedures) and Section 3 (results/discussion) have been amended in order to attend your suggestion. Thanks.

- In the 'Results' quite often 'drought' is mentioned, but how is drought defined and how do droughts (as temporary events rather than permanent changes) function within a hypothesis of cause-effect relations to be 'disentangled'?

The revised manuscript mentions “permanent drought conditions” referring to SPEI6 and SPEI12 estimations. We assume that negative values of SPEI confirm “drought conditions” in the previous 6/12 months due to precipitation(temperature) was registered below(above) the normal records (climatology on the study period). It has been clarified in the Section 2.3 (analysis procedures) and Section 3 (results/discussion).

- The NAOI analysis maybe provides robust results, but why the conclusion that the NAOI shift has been "encouraged by T and ETP change" remains unclear to me. The results appear to introduce new analysis that has not been introduced clearly enough in the methods section. Readers need to be guided more which analysis tests which hypothesis and how this adds to the greater aim.

Thanks for the recommendation. This sentence has been modified in the manuscript. We referred to the probable rise of maximum temperature caused by the increase of insolation. But we do not mention this sentence in the revised manuscript so as to render the text clearer with regard to readers.

- Similar to Rev. 1 I could not follow the forest-change argument. A review of knowledge on the issue and a clear process-cause-effect hypothesis building on it would help perhaps.

Both recommendations were attended, and the text has been modified.

- Further technical deficits include (not an exhaustive list) the wrong terminology in figure captions ("hydrograph" in Fig.2 is not a hydrograph for example), no discussion section, a conclusion list that does not contain congruent arguments.
Our hydrograph (Fig 2.) shows the contribution of monthly water inflows to annual records (as conventional hydrographs). A clarification has been added in the caption of Fig. 2. We have tried to use a more accurate terminology in the revised manuscript. Thank you for pointing out. Discussion is included in the Section 3 (Results and Discussion). The final remarks have been amended to reflect adequately our principal findings.