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Comment on hess-2021-565

Anonymous Referee #2

Referee 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-RC2, 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. 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'. Multiple variables as drivers of change are studied, but not together in a multi-variate 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.

Major concerns are:

1) 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 ananlysis of cause-effect relations is missing.

2) 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 prepostitions 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.

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 perhpas?.

3) 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 rally used as a term for vegetation)

- 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).

4) 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.

5) 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'?

6) 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.

7) 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 perhpas.

8) 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.