

Interactive comment on “Documentary evidence of droughts in Sweden between the Middle Ages and c1800” by Dag Retsö and Lotta Leijonhufvud

Anonymous Referee #2

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The manuscript is an interesting attempt to discuss how historical documentary evidence can be used to reconstruct droughts in Sweden between ca 1400 and 1800. This is, to the best of my knowledge, something that has not been undertaken before and so it is a welcome contribution to the field of historical climatology. Parts of the discussion is well written, in particular the part between line 163 and 275, where details in what the historical documents can tell us are presented. As a whole, however, the article has major problems that must be addressed before the evidence discussed can be acceptably presented. Thus, my overall recommendation is that the manuscript needs major revision.

Below, I will outline the most important aspects at large. I will not comment much on various minor things because I believe that the manuscript after a major revision will

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necessarily look quite different in its details.

Method

I do not find the method description satisfactory. Essentially all that is said is a vague mentioning of a 7-point index scale ranging from -3 (extremely wet) to +3 (extremely dry). Nothing is said of how the authors have made in order to decide which value on the scale to choose for a particular year. Because their method is not clearly described, it is impossible to understand how they constructed their index time series. A clear method description is needed, preferably preceded by a discussion of relevant aspects (both strengths and weaknesses) and methodological problems encountered in similar investigations made by other scholars (in other countries) in the past.

The authors state on lines 81-82 that they used an already existing index of wet years (from Retsö 2015), which they complemented with indexed notes on drought. Nothing, however, is said of how the existing index and the new information was combined. This must be made clear.

Because it is relevant here, I also read the Retsö 2015 paper and I note that the table in Appendix A there presents much clearer information, where the reader at least can obtain a direct view of what kind of statements in the historical documents that lie behind the choice of a particular value on the index scale. Something similar here should help readers of the new work to understand it better, even without a strictly defined methodological rule.

I find it confusing that the authors refer to their constructed index several (seven) times as a "precipitation index" instead of a drought index, which one would expect that they should call it given how they start their account. Is it a drought index, or is it a precipitation index?

Documentary data

There is insufficient information in the manuscript regarding which documentary evi-

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dence that were used. There are indeed some archival sources mentioned and the reference list contains several elements. But it is anyway impossible to understand which the actual underlying data are.

At four instances the authors refer to "the database"; on line 12 (in the abstract), 85 (section 'Method'), 137 (section 'Instrumental measurements'), and 363 (section 'Discussion and conclusions'). However, they provide no information of where this "database" can be found or how it has been constructed. This is not acceptable and it does not match the Data Policy of the journal, which says that "it is particularly important that data and other information underpinning the research findings are "findable, accessible, interoperable, and reusable" (FAIR) not only for humans but also for machines." In order to comply with this policy, the authors are advised to make their database, including sufficient metadata to understand the actual data, available to the public in a recommendable digital repository for research data, and provided with a persistent identifier (digital object identifier).

Instrumental data

The authors use early instrumental temperature data from Stockholm in order to calculate correlation coefficients with their index series. This is fine and it illustrates that their index series has some correlation with summer temperatures. However, they say twice (lines 141 and 357) that they have no overlapping precipitation data. This is surprising given that they have used more recent precipitation data from Stockholm starting in 1859, which they have downloaded from the SMHI Open Data resource. Maybe the authors simply are not aware of it, but the same SMHI resource that they already have used actually also contain freely available digital monthly precipitation data from relevant stations that do overlap in time with their own index series. The following station precipitation data are available:

Uppsala 1723-1732 and 1739-present, Stockholm 1786-present, Risinge 1730-1740.

Even if early precipitation data certainly may have their own problems, it would be

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much better to use them - in particular the long Uppsala record - to calculate correlation coefficients with the index series than not doing this. It would also be advisable here to refer to the recently published global inventory of pre-1850 instrumental meteorological records by Brönniman et al. (2020) (<https://journals.ametsoc.org/doi/10.1175/BAMS-D-19-0040.1>). The catalogue in the supplementary material to that paper does include the above mentioned precipitation data from Uppsala, Stockholm and Risinge.

I recommend the authors to use the available overlapping monthly precipitation data in order to study correlations with their index data. Once this has been done, it may have a noticeable impact on how they discuss and interpret their index series.

Statistical analysis

The authors calculate correlation coefficients between instrumental temperature data and their index series, and between instrumental and precipitation data. However, this part of their analysis lacks a clear motivation, a clearly stated goal, and also statistical rigour. The authors are advised to be more stringent and apply appropriately chosen significance tests and use the subsequent result in their discussion.

The content of the tables with correlation coefficients is difficult to understand and I find it superfluously accurate to present three decimals in the calculated coefficients. Two decimals are enough in this context.

Figures

All figures are unfortunately rather poorly produced. Please provide more professional-looking figures.

The content of Figure 3 is confusing. Why are data before 1400 presented there at all? And which are these data? If they are needed in the discussion, this should be argued for and references to the data should be given.

Discussion and conclusions

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There are several issues that need to be better discussed. I will not mention everything I can think of here because I believe that the discussion after a major revision naturally will be quite different.

I find, however, the following statement (lines 360-363) particularly noteworthy:

"Correlation between the precipitation index and summer temperatures are higher than between summer temperatures and precipitation, so it is possible that the precipitation index is rather a "good-summer-weather-index". We think that the ideal would be to extend the documentary database until – at least – early 20 th century."

If the index series is a "good-summer-weather-index" rather than a precipitation index (or a drought index, which I think is what the authors should call it), then how much scientific information value do the conclusions that emerge from this paper have? In particular, how should one understand the claimed time periods of droughts that are discussed and even highlighted in the abstract? Are they periods of drought or periods of "good" summer weather? If their interpretation is that uncertain, should one really present them as periods "particularly struck by summer droughts" in the abstract?

Why do the authors "think that the ideal would be to extend the documentary database until – at least – early 20th century." It would be interesting to hear some discussion about what such an extension could help us with.

Given that the authors seem to not be convinced themselves that their derived index really shows us when droughts occurred, it would - overall - be advisable that they put more efforts on discussing the question of whether it is possible to develop a reliable drought reconstruction from Swedish documentary data.

As a last remark, I should say that I do encourage the authors to re-consider how to approach the documentary data and how to present and discuss them. The topic of drought reconstruction is clearly of interest, not the least in light of the fact that Sweden only two years ago (2018) experienced an exceptionally warm/dry summer. Scientific

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papers about the 2018 summer in Sweden and nearby have recently started to appear in the climatological literature. It could be a good starting point in a revised discussion and analysis of the here presented material, to relate it to what meteorologists and other climate scientists recently have considered be of importance in this context. Historical documentary data have the potential to tell us whether similar weather conditions have happened anytime during the past five hundred years or so.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2020-25>, 2020.

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