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Interactive comment on "Climate indices in historical climate reconstructions: A global state-of-the-art" *by* David J. Nash et al.

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Response to Anonymous Reviewer #2 [RC2]

We thank the anonymous reviewer for their time and thought, which will help to improve significantly the overall quality of the manuscript. We respond to each question raised in turn:

[RC2] Regarding objective one [of the paper: 'provide a global state-of-the-art review of the development and application of the index approach in historical climate reconstruction'], the review is exhaustive reflecting most of the previous work based on ordinal indices that consider the departure from normality as the main criterion to produce an anomaly scale with several levels of intensity. However, references to other ap-

proaches to build ordinal indices are missing. For instance, several papers have built ENSO chronologies from documentary evidences from different areas of S America reporting different impacts associated to ENSO (Quinn and Neal, 1992; Ortlieb 2000; Garcia-Herrera et al 2008). In my view this type of approach should also be acknowledged in the paper.

[Response] Thank you. This is a very valid point. We will add information about index-based approaches to the development of ENSO chronologies to the most relevant part of the manuscript (Section 5, dealing with 'Climate indices in the Americas').

[RC2] Lines 777-778 In the recent years directional wind indices over the oceans have gone beyond decadal reconstructions of wind force trends, as stated in the paper. This methodology has allowed the generation of the longest series of the wind circulation in the North Atlantic and generating new indices for circulation patterns as the NAO or the East Atlantic pattern (Mellado-Cano et al 2020). Besides, they have been useful in studying different features of the global monsoon system: the impact of volcanic eruptions on the West African Summer monsoon during the 19th century (Gallego et al 2015), the onset of the Indian Summer Monsoon (Ordoñez et al 2016) or secular trends in the Australian Summer Monsoon (Gallego et al 2017) among others. Lines 865-873. Over the Oceans the uncertainties associated to the limited sampling in a given area and period have been also quantified, see for instance Gallego et al (2015).

[Response] Thank you for these very helpful comments. We will review each of these studies and update sections 7, 8.6 and 9.1 where appropriate.

[RC2] In my view the second objective is not achieved because there is not a critical analysis of the work described in sections 2-9. Consequently, the link from the recorded evidences to the identification of the best practices is missing. This should have been done in section 10, but this is again very descriptive. Tables 5-7 do not identify best practice, instead they just summarize the variables studied in every region or the number of classes used. Having missed this analytical part, many of the statements lack

of support. The authors claim that they are based on two previous reviews and 'also incorporate insights from this study'. This is not evident at all from the text, because of this lack of critical analysis in the manuscript. For instance (lines 950-952) why do the authors "recognise that the most widely used approaches such as the Pfister method would require modification to be useful for temperature and/or rainfall reconstruction in all regions"? Which of the previous papers are the support of this statement? What are the main reasons for this recognition? The authors do not provide any evidence of the limitations of these approaches and they should do it based on their extensive previous review. Is it because indices derived from a certain type of documents and for a given climate cannot be applied mimetically to different documentary sources and climates? If so, the authors should provide supporting evidence. Otherwise this is just their opinion.

[Response] We take the reviewer's point about lines 950-952 and will expand the text as suggested. We will also review section 9.1 in general to ensure that statements are backed up with examples from the preceding sections. We do not, however, agree with the other views - particularly the suggestion there is insufficient critical analysis of the examples discussed in sections 2 to 9. We embed critical analysis throughout the manuscript. The global coverage of historical climate studies is such that, for many parts of the world, there are not overlapping series that would allow for a direct comparison of the outcomes arising from the use of different methodologies. Where historical studies do overlap, for example in Europe and Africa, we have commented on similarities and differences. Indeed, figure 2 explicitly presents the results of two different studies of overlapping areas in a European context. The purpose of section 9, and section 9.2 in particular, is to not only synthesise the different approaches used to reconstruct climate indices in different parts of the world but also to identify weaknesses. Many of these weaknesses feed directly into the recommendations in section 10.

[RC2] I find several problems with the guidelines. Firstly, they should be clearly sup-

ported from the previous review, which is not the case. The review should allow identifying best practices and the analysis of these cases should lead to the guidelines, but this sequence is not followed in the paper. Bets practices are not identified, and, consequently, guidelines are not supported by them.

[Response] As discussed in our previous response we do not agree with this viewpoint. Further, section 10 is intended as a conclusion and synthesis. Adding supporting evidence to underpin each of the 12 recommendations would add unnecessary length to an already very long manuscript.

[RC2] Additionally, I think that a climate component is missing in some of the guidelines. According to my previous experience, the final indexation should be a compromise among the historical records characteristics, their availability and the climate of the region to be studied. The mere translation of indices built for a certain climate to other areas may lead to biases or inadequacies if applied to other regions. For example, the translation of precipitation indices developed for central Europe should be applied with care to Mediterranean climates, where most of the precipitation is concentrated between September and April and occurs mostly in the form of a few intense events. Thus, I think that guideline 2 could be rephrased from: Researchers should be familiar with the strengths and weaknesses of each of their historical sources prior to their use in climate reconstruction. To something as: Researchers should be familiar with the local climate and the strengths and weaknesses of each of their historical sources prior to their use in climate reconstruction.

[Response] We agree fully with the reviewer here. However, the recommendations need to be considered as a whole. We already discuss the idea that indices should be developed for climatically homogeneous regions in guideline 1. We do not anywhere suggest that a one size fits all approach to index development would be appropriate. There are numerous examples in the manuscript of where approaches have been tailored to suit climatic variability in an area of interest.

[RC2] Guideline 3 reads: "Researchers should select an appropriate temporal resolution for their index series according to the quantity and richness (in terms of climate information) of available historical sources. This may be monthly, seasonal, annual or longer, although for information-rich areas, a monthly resolution is the most desirable." Again the climate factor is missing, for instance if you build monthly series for precipitation in the Mediterranean, you should be aware that during the dry months, the signal-to-noise ratio should be very low and this can bias the results. This guideline should be rephrased, as, for example: "Researchers should select an appropriate temporal resolution for their index series according to the quantity and richness (in terms of climate information) of available historical sources and the local climate. This may be monthly, seasonal, annual or longer, although for information-rich areas, a monthly resolution is desirable depending on the climate type and variable studied."

[Response] The key aspect to this particular guideline is that researchers should select an appropriate temporal resolution for their index series based on their data. If, due to the climatic characteristics of an area, observations are relatively sparse for particular seasons then it may not be appropriate to adopt a monthly time scale, regardless of how rich the observations are for other periods of the year. We think that this is explicit in the guideline as it stands.

[RC2] Guideline 4 reads: "Whether to develop a three-, five- or seven- (or more) point index series will also depend upon data quantity and quality but may be influenced by the legacy of previous studies in a region if direct comparisons are required". Two comments here. I do not understand the mention to the previous legacy, I find this confusing. Do you mean that things should be done as they were done in the past, just to compare? Even if you have identified problems in the legacy? This needs to be clarified. Applying previous indexation without a careful examination of its adequacy to a given climate and data set is not a good practice. Apart from this, a mention to the climate is also required, since the number of points in the scale may also depend on the type of climate and climate variable studied. So, for instance, this could read

something as: "Whether to develop a three-, five- or seven- (or more) point index series will also depend upon data quantity and quality, the local climate and climate variable to be indexed".

[Response] We take the reviewer's points about reference to previous studies and will clarify the text. We disagree, however, about the need to include reference to climate and climate variability in the guideline. The temporal resolution of any index series hinges on the richness of available data across the year. If this varies seasonally then the resolution of the series for the whole year should reflect this.

[RC2] Guideline 9 reads "To maximise their wider usefulness, index series should, ideally, overlap with runs of local or regional instrumental data to permit calibration and verification. Where instrumental data are not available, overlaps with independent high resolution palaeoclimate records may be used for calibration" I think that using palaeo-climate proxies to calibrate an index is not the best recommendation. These proxies have their own weaknesses and uncertainties and using them as the 'truth' to calibrate an index may introduce unexpected biases. Calibrating an index with a proxy, implies two transfer functions from the variable to the proxy and from the proxy to the index, posing additional uncertainties. I think that a comparison with proxies is fine, but using them to calibrate is far too dangerous.

[Response] The reviewer correctly identifies that there is a controversy here. We will address this controversy through the insertion of additional text describing, for example, Andrea Kiss' or Martin Bauch and colleagues' work comparing written records and indices with the Old-World Drought Atlas, as this illustrates the issue well. We do not, however, wish to modify the recommendation. The key words here are "high resolution". We would not recommend calibration using low resolution palaeoclimate series.

[RC2] Summing up, I think that the paper requires and extensive revision before being acceptable for publication. The good practices need to be well identified in the text and the support of the guidelines must be clearly linked to the previous evidence. The

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[Discussion paper]

authors have made a highly valuable effort in compiling the previous work. Improving the analysis by better illustrating the good practices and providing a clear background and support to the guidelines, would lead to a highly interesting paper, but these issues need to be solved.

[Response] Thank you for this. We will adjust the text as outlined above.

[RC2] Minor issue Some of the authors references are made in a strange way. For instance, line 455 'Garza Merodio who was a student of : : :.' Is this so relevant? Why are not the other academic linkages mentioned? To me this is relevant if you want to tell the history of the researchers involved in this topic, which is not the case. Line 401 'the work of Coleen Vogel'. Line 407,' Sharon Nicholson' and several others. Why some authors are cited by their full names (not the usual practice) and other just by the surname? Not clear to me.

[Response] First names are used sparingly throughout the manuscript to flag up key researchers who made important contributions in specific regions and/or to identify distinct schools of historical climatology that have transmitted certain methodologies.

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