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Climate of the Past Discussions

# *Interactive comment on* "Millennial-to-centennial patterns and trends in the hydroclimate of North America over the past 2000 years" *by* Bryan N. Shuman et al.

**Anonymous Referee #2**

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The authors have put considerable effort into collating and summarizing a large pool of proxy hydroclimate data from Central and North America using an impressive suite of statistical techniques. The focus was on centennial and longer trends over the Common Era (CE) with particular emphasis on contrasting the first and second millennium as well as the Medieval period (800-1300 CE) and Little Ice Age (1400-1900). The paper is clearly written and well structured with excellent figures. However, my concerns relate mainly to what has not been included and I have outlined these concerns in sections below.

a) Comparison to tree-ring based reconstructions (drought atlases). I was very sur-



prised about the lack of any direct comparison to published drought atlases derived from tree-rings (e.g. Cook et al. 2008 and Stahle et al. 2016). This is something that has also been raised by another reviewer. Clearly, the authors have the analytical skills to have addressed such comparisons. Their comment that tree-rings fail to preserve low frequency variance (lines 20-26, page 2) and therefore ignored, fails to recognise significant recent advances in tree-ring reconstructions (e.g. see "signal free" standardisation described in Melvin and Briffa 2008, 2014; and the methods applied to studies such as Stahle et al. 2016 and Cook et al. 2010, 2015). The lack of any direct comparison also ignores the fact that the drought atlases describe multi-decadal to centennial length periods of drought and pluvials. The authors also discuss the possibility (Section 4.1, page 10) that some of their records may have also failed to capture long term trends due to detrending so this fact along with their inclusion of ice accumulation records makes the omission of the tree-rings seem arbitrary. My concern is, despite many approaches to analysing the data no clear spatially coherent patterns seem to emerge and I believe without the direct comparison to the tree-ring records the validity of the presented results remains hard to assess. I believe the paper needs to include comparisons and discussion about the published drought atlases given their clear geographical overlap.

b) Selection of windows of time. Several analyses involved the selection of time periods (or "windows of time") without any indication of the reasoning behind it. Let's start with Section 2.2.1. Why specifically 100-year bins? I know that a consistent window-length had to be chosen across all sites, but why was it 100? Given some sites dating uncertainties, how conservative is 100-years? This window length results in a maximum of 20 bins covering the last 2k. Was there a cohort of sites (say 25 or more) that had the potential for 50-year bins? In the opening introduction (line 29, page 2) the authors state "many have decadal resolution. . .". I would like to see the inclusion in Table 1 of information about the dating resolution and time-span covered by each site (and perhaps their autocorrelation). I would also like to recommend the inclusion in Table 1 of the Gini coefficient (Biondi and Qeadan, 2008) to help indicate those sites which show

strong records of past environmental variability. These additional parameters would then provide a basis that could be summarised (plotted?) and used as the rationale for the selected bin-window. Was there a missed opportunity here to present a smaller cohort but at higher temporal resolution or even split the database into two cohorts at different temporal resolutions? How many missing bins were there (an expanded Table 1 would help provide this information)? Why pick a 5-bin (500yr) moving window – how strong was the autocorrelation? The next aspect I think needs addressing is the selection of the CE time period. I understand the desire to present something aligned to the PAGES 2K initiatives but does this then stretch (i.e. require missing bins) the data of many sites or does it undervalue the temporal strength of many proxies? There is obviously a strong cohort of long proxies in the collection enabling the evaluation back through the Holocene (Section 3.5) so some discussion about the adoption of the CE period is I believe warranted. There is no discussion or comparison made about the nested approach used with the subset of the long cohort – e.g. what influence does the long cohort of sites have on the CE analyses (i.e. Figure 2 and 3)? I like the idea of epoch differencing between the LIA and MCA (despite the lack of any spatial coherence in Figure 4) but don't understand the rationale for the first versus second millennia comparison. The latter seems arbitrary, especially since it dissects the MCA window – a widely acknowledged period of climate importance. I would like to see epoch differencing over other windows. So for example, what about comparisons of the earlier period of 300-800 CE to the LIA and the MCA? We would then start to be able to appreciate how significant or different the LIA and/or MCA were. Is it possible to do epoch differencing of a moving 500-year period compared to the MCA (and the LIA)? I wonder also why a 500-year window was chosen for the MCA and LIA – how does the pattern change if a shorter window is chosen of say 400 years (900-1300 CE and 1400-1800 CE)? Is it possible to have a selected 500-year moving window that is compared to 5 randomly selected 100-year bins (like bootstrapping). If the millennia comparisons want to be retained, then perhaps do the same approach using only the long cohort of sites so that multiple millennia can be compared.



c) Selection of groupings Again, the selection of 9 geographic regions is presented without any explanation of the reasoning behind it. In Figure 1 the 9 different geographic regions are shown but only 6 of these are illustrated in Figure 5 presumably due to the lack of sites being located in the remaining 3 areas. I wonder then if some sort of biogeographic merging could be done so there aren't the gaps and to increase the associated sample depth (e.g. arctic plus boreal). Could a description of the characteristic climatic regime also be added about each of the regions as means of explaining why they have been used. As it stands, I agree with comments from the other referee about the PCA-by-region being somewhat inconsistent given the lack of regional coherence in the EOF results. The thorough statistical exploration of the database did not extend into looking at the specific proxy-types on their own, despite them being described in the methods (see Sections 2.1.2 - 2.1.5, pages 4 & 5). Could principal component analysis be done on the proxy types alone? This might help inform on the geographic patterns and / or the leading modes of variability in the whole dataset. I think the other referee also made a good suggestion of looking in more detail at the calibrated records.

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