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Climate of the Past Discussions

Interactive comment on "Differing pre-industrial cooling trends between tree-rings and lower-resolution temperature proxies" *by* Lara Klippel et al.

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(1) comments from Referees, (2) author's response, (3) author's changes in manuscript

Comment 1: (1) With regard to 1, the authors appropriately discuss the latitudinal gradient in the orbitally forced trends in temperature. They nevertheless do little to describe and investigate the latitudinal sampling biases in the two populations that they explore, namely the dendro and composite proxy populations. This sampling bias is obvious in Figure 1 and in the sample sizes listed in Figure 5. And yet figures like Figure 2 are presented with little caveat. Such a figure is misleading, given that the composite records are biased toward the high latitudes and the dendro records are

biased toward the midlatitudes and the lower midlatitudes in particular (incidentally, it is not mentioned anywhere whether these means are themselves weighted by cos(lat), as they should be). How should we interpret these time series given that the explored effect intensifies at the higher latitudes? Splitting Figure 2 into time series representing different latitude bands would help (30 degree boxes may be too large for this), as would a scatter plot of trends vs. latitude for each of the two populations. While not definitive, it would be helpful for understanding how spatial sampling of a spatially-dependent temperature trend may be biasing the mean trends estimated from the two populations. I suspect that the authors will bring up Figure 5 as a rebuttal to what I am pointing out, but please see my comments on my second principle concern below. (2) Figure 2 is merely a reproduction of a figure shown in the Pages publication (and in response with suggestions of reviewer 1, only the NH is shown). The figure is not part of our analysis, but demonstrates that we can reproduce the Pages trends. The latitudinal sampling bias is indeed tested Figure 5, but we added another splitting as suggested. (3) Supplement figure added showing composite chronologies after splitting by latitude.

Comment 2: (1) Before I get to that, however, I would add that another overlooked bias is that of the proxies comprising the composite records. They all sample different seasonal windows, some reflect marine temperature changes as opposed to continental changes, and many have their own biases tied to representation of low-frequencies. The authors take the composite proxies at face value, presumably because they fit their assumptions about latitudinal trends (in most cases), but it is insufficient to do so. (2) This is obviously a critique on the data composition and/or papers using this by combining proxies, which is exactly what we try to make readers aware of. Here we point to systematic differences among proxies, and even include a splitting by season. (3) No changes made.

Comment 3 There may be biases in these other records that promote spurious trend estimates that the authors do nothing to highlight. One observation that may point to such biases is the increase in the percentage of significant mid-latitude trends in the

composite records relative to the high latitude records, which is of course counter to the expected spatial dependence. These factors are not sufficiently discussed. Regarding concern 2, the authors present Figure 5 as a measure of the latitudinal differences in the significance testing of trends in the dendro and composite records. The percentage of each population with significant and insignificant trends is nevertheless hard to interpret. Some additional significance testing would go a long way toward helping to interpret this figure and the results. The first question that should be addressed is: given the expected magnitude of trends estimated a priori from the orbitally-forced changes in insolation (signal), time series with the level of variance representative of the proxy series (noise), and the size of the sample populations, how many times would one achieve significant positive/negative trends and insignificant positive/negative trends for different realizations of noise? For instance, it may be the case that for 16 time series and the level of variance that is estimated in each, the trend percentages in the dendro high-latitude bin is actually what you would expect for a modestly detectable trend. Moreover, how should we interpret the comparison between the percentages associated with the dendro and composite series in a band like the high latitudes in Figure 5? It may be that the PDF of the percentage distributions spans the differences shown in Figure 5 and the results are fully consistent with each other. Put differently, for multiple noise realizations, how robust is the separation between the trend percentages in the dendro and composite records? It is impossible to answer these questions from what the authors have done and they should better characterize the statistical likelihood that the differences they describe are more than just noise. All of the above is fundamental for two reasons. The first is that the difference in the number of statistically significant trends is the primary metric by which the authors argue there are differences in the representations of trends between the two populations. If the physical expression of those trends is latitudinally dependent and their spatial sampling is heterogenous and biased in the two populations, it must be controlled for. Secondly, the robustness of the differences must also be statistically constrained so that real differences (statistically speaking) can be separated from differences that can arise simply by chance.

(3) We added a test and (supplementary) figure addressing the problem of latitudinal sample biases. We randomly (1000 times) drew 10 series from all tree ring series, and the composite (glacier, marine and lake records), at the latitudinal bands 0-90°N, 30-60°N and 60-90°N (below 30°N sample replication is too low), and calculated the percent of records showing insignificant/significant cooling and insignificant/significant warming trends.

Minor comments: Comment 4: (1) Pg. 1, Ln. 13: It should be noted here that the 692 proxies are the temperature sensitive records in the database (the full database is closer to 3000). (2) No, the database includes exactly 692 records. We refer to PAGES 2k Consortium: A global multiproxy database for temperature reconstructions of the Common Era, Nat. Sci. Dat., 4, 1-33, 2017. (3) No changes.

Comment 5: (1) Pg. 1, Ln. 24: There are a lot more reviews that speak to this issue than Frank et al. Consider adding Jones et al. (2009), North et al. (2006), Mann (2007), Smerdon and Pollack (2016), and Christiansen and Ljungqvist (2017). (3) Further references added.

Comment 6: (1) Pg. 2, Ln 28: The list of multiproxy reconstructions does not include the data assimilation work (e.g. Hakim et al. (2016) and Steiger et al. (2018)) nor does it include the PAGES products from 2013 and 2018. This should be corrected. (3) Corrected.

Comment 7: (1) Pg. 2, Ln 32: The list of review articles that discuss this should be expanded as above. (3) Expanded.

Comment 8: (1) Pg. 2, Ln 34: This is once again a limited list of papers that compare reconstructions and models. The authors should at least include the PAGES efforts from PAGES2k- PMIP Group (2015) and PAGES Hydro2K Consortium (2017), if not include some of the additional references that are discussed in those studies. (3) 3 references added.

Comment 9: (1) Pg. 4, Ln 6: Consider discriminating instead of critical (3) Changed, accordingly.

Comment 10: (1) Pg. 5, Ln 46: It seems strange to use cubic smoothing splines for standardization in the context of this investigation, given that they will explicitly remove the long-term trends. The effect is clearly visible in Figure 6 where even the 20th century trends have been removed. Incidentally, I find the bracket and description in Figure 6b a bit clumsy and hard to follow. The bracket in particular looks like it was drawn in by hand! (3) Spline detrending was removed and replaced by Signal Free detrending in line with suggestions made by reviewer 1. Bracket replaced with an arrow.

Comment 11: (1) Pg. 6, Ln. 84-86: Doesn't this contradict a central premise of the paper? This seems a lot more concerning than the attention it is given in the manuscript. (2) No, this is "just" related to removing cambial/biological age-trends inherent to tree-ring width and density data. (3) No changes.

Comment 12: (1) Pg. 7, Ln 90: The subset is described as 70 but there are multiple places where this number appears to be different. Figure 5, for instance, discusses 89 dendro series. Are these typos or am I missing something? (3) Further explanation added.

Comment 13: (1) Pg. 7, Ln. 5: How does -0.32 compare to -0.03?! (3) Removed.

Comment 14: (1) Pg. 7, Ln. 9: Consider preserving instead of conserving (3) Changed.

Comment 15: (1) Pg. 8, Ln. 37: I find the discussion starting here and extending to the end of the paragraph very confusing. It seems to be saying that the authors have demonstrated differences between proxies, but that there are no differences between proxies. With regard to the last sentence, I do not think the authors have demonstrated the lack of spatial sampling bias, based on the principle arguments I have provided above. (2) See above, response to comment #3.

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