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Climate of the Past Discussions

Interactive comment on "Optimal Ranking Regime analysis of TreeFlow dendrohydrological reconstructions" *by* S. A. Mauget

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Rev1-1: The dynamic analysis is the weakest part of the manuscript, but I believe that is more the consequence of the reconstructions that the author characterizes. I think this point could be made more strongly. There is just little agreement in some of these dynamic mode reconstructions and it is not a surprise that little can be determined conclusively from their joint analysis.

Reply: The Appendix A analyses of the PDO, AMO, and ENSO reconstructions were done mainly to see if there was any evidence of Fig. 4's Active-Dormant-Active variability pattern. Also, it seemed like a natural thing to check out given Hidalgo's (2004) earlier work citing the influence of the AMO and PDO over the interior western U.S. It turns out, as pointed out by others (Mantua and Hare 2002, D'Arrigo and Wilson

2006), the PDO reconstructions were in pretty clear disagreement. Given these basically negative results I was weighing whether to include the dynamic analyses in the paper, but in the spirit of full disclosure decided to include them in an appendix. I note they disagree, but I'm unsure how the point could be made more strongly. I've considered some reasons why they might disagree (see Reply to Rev1-2), but I'd rather not dig into it in this paper. This might be a topic for another paper, but I think it would be a diversion here.

I agree we can't draw much from the Appendix A results, but I was surprised that the PDO reconstructions were so dissimilar. In principle, they should agree with one another. They each claim to reconstruct PDO variation – which is a known climate signal during the instrumental period, and is assumed to have a definable history during the pre-instrumental period that they are trying to reconstruct. If necessary conditions for reproducing the PDO are met (see Reply to Rev1-2), then I would expect these reconstructions to agree more closely.

Rev1-2: It should also be noted that some reconstructions are based 20th-century characterizations of the teleconnections between a given NA region and the oceanic states. If these teleconnections are not stable over time, it is also possible that the associations that the author is looking for are not reflected in the reconstructions because of a breakdown in the teleconnections.

Reply: I agree that its possible that teleconnections may change over time. But it also seems that the entire idea of trying to reconstruct ocean mode variability from dendrochronologies is based on the assumption that teleconnections are stable over time. If that assumption wasn't true, there would be no point in trying. Although there may be more, I can think of three necessary conditions for reconstructing the AMO or PDO from tree-ring data.

1. Tree rings respond to rainfall and that response is the same in the instrumental period and the pre-instrumental period. 2. During the instrumental period the PDO or

AMO has a significant effect on rainfall over the region where a dendrochronology is formed. For example, +PDO → more rain → more growth, -PDO → less rain → less growth. 3. Stable teleconnections, i.e., the general rainfall effects of the PDO or AMO teleconnections for a region are the same in the pre-instrumental period as they are in the instrumental period.

My guess is that the PDO reconstructions disagree because (2) or (3) were violated. It is possible that PDO teleconnections are non-stationary, but some of the reconstructions were based on tree-ring data from regions that may not have the strongest PDO signals (Southern California and Northern Baja California, Alberta).

Rev1-3: Additionally, it would be useful for the author to note the independence between the dynamic reconstructions and the streamflow/hydro proxies that are used. Is there any overlap between the dendroclimatic series used in the streamflow reconstructions and the dynamic mode reconstructions?

Reply: Overall, very little. Most of the results in the streamflow reconstructions of Fig. 4 are based on tree-ring chronologies from the Upper Colorado basin, while the PDO reconstructions were based on tree ring records from Southern California and Northern Baja California (Biondi et al. 2001), Asia and Siberia (D'Arrigo and Wilson 2006), and California and Alberta (McDonald and Case, 2005). The Shen et al. (2006) PDO was based on a summer drought/flood index derived from Chinese historical documents. The Gray et al. (2004) AMO reconstructions used tree-ring records from eastern North America, Europe, Scandinavia, North Africa and the Middle East. There may be some overlap with the ENSO proxies used in the McGregor et al (2010) PCA analysis, as some were located in interior western North America (their Fig. 2).

After writing some text that addressed this lack of overlap, I found it hard to come up with a reason why such overlap might be a problem. Even if there was complete overlap, e.g., one set of upper Colorado dendrochronologies that were regressed on both streamflow and the PDO, you would still be doing regressions on two separate

signals. You could only get significant regressions if there was a signal common to both the streamflow and the PDO in the dendrochronologies. If Rev1 could be more specific about why this might be a concern, I'd have a better idea of how and where to include this discussion in the paper.

Rev1-4: Finally, I do not think it is necessary to include an appendix in this paper. Why not make the ORR analysis of the climate indices a final section in the paper? It is not clear to me that the material should be presented in such a way, especially in a journal that allows for this extra length.

Reply: After thinking about this, I'd rather keep the paper as is. The analysis of the climate indices was included to check whether the available PDO and AMO reconstructions showed any evidence of Fig. 4's centennial scale Active-Dormant-Active cycle. It didn't, and in my view didn't reveal anything interesting – other than the fact that the PDO reconstructions clearly disagree. I'd rather give a quick summary of this in the paper and not move the current Appendix A into the paper itself. I tried this with an alternate draft and it seemed like a diversion at the end of the paper. If readers wants more details they can read Appendix A, but my thinking is that it's results are not really central to the paper.

Rev1-5: It should be noted that the reconstructions also have formal uncertainties associated with them. I do not think the author includes these in the ORR analysis, i.e. he has only used the mean estimate of the reconstructions. While not necessary here, the author should mention something about the possibility of including uncertainties about the mean estimate and how they might affect the results.

Reply: I imagine you could figure out a method of accounting for uncertainty using a ranking based analysis method. Maybe a Monte Carlo approach that randomly perturbed the reconstruction values within their 2σ limits, and then ranked those values. But without doing this sort of thing I can't really say how it would affect the results. My guess is that the fairly clear common signal in Fig. 4's analyses of the mean values

would “lose focus” and become weaker. I’m really not sure I can figure out how to say something meaningful and coherent about how data uncertainty would propagate through an ORR analysis without actually doing a test. Maybe this is a topic for a future paper

Rev1-6: In the context of future analyses, I presume that ORR could be applied to a gridded hydroclimate product like the Cook et al. North American Drought Atlas that would both complement the current analysis and sidestep the issue of spatial heterogeneities in the streamflow records. Something should be mentioned along these lines.

Reply: This seems like a likely next step. In the current draft an additional (5th) paragraph in the Conclusion was added, which includes the following sentence:

“Also, because these reconstructions are mostly representative of the upper Colorado watershed and the WY-UT-CO region (Table 1, Fig. 1), Fig. 4’s overall regime pattern may not represent variability over the broader western U.S. Whether this is the case might be resolved in the future via an ORR analysis of the more spatially continuous Cook et al. (2007) PDSI reconstructions. “

Rev1-7: Is there any overlap in the underlying tree chronologies that are used for the various streamflow reconstructions? A note on this issue would be helpful.

Reply: Many of the streamflow reconstructions were done by the same group (e.g. Barnett et al. 2010), and are from adjacent watersheds, so I imagine there is some overlap. Without digging into identifying the tree-ring chronologies for all the Table 1 reconstructions, I can’t say for sure. But to acknowledge the possibility, I’ve added the following sentence to paragraph 5 of the conclusion:

“Although the individual ORR analyses of reconstructed streamflow in Fig. 4 show consistent regime patterns, the Table 1 streamflow reconstructions may not be completely independent. As many of these reconstructions were done by individual groups (e.g. Barnett et al. 2010, Wise 2010) and are from adjacent watersheds, some may reflect

flow variability estimated from overlapping sets of dendrochronologies. As noted in Section 2, a number of the Table 1 reconstructions are for flow for the same rivers at various points in their watershed.”

Rev1-8: Pg. 764, Ln 15: Step ii

Reply: Changed to ‘step (II)’

Rev 1-9: Pg. 768, Ln 15: Remove the second dry periods.

Reply: That sentence has been changed to:

In some instances these dry regimes are of longer duration and higher significance than the same reconstruction’s previous dry periods in Fig. 4a, e.g., those in Fig. 4c’s R19, R24, R32-R36, and R40 reconstructions.

Interactive comment on Clim. Past Discuss., 11, 755, 2015.