

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

Anonymous Referee #1

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Summary: This manuscript presents an analysis that applies Optimal Ranking Regime (ORR) analysis to primarily stream flow reconstructions, but several other hydroclimate reconstructions are included, for the purposes of identifying spatially coherent drought and pluvial conditions in the western US. ORR identifies several periods of droughts and pluvials that are coherent across a wide majority of the reconstructions since 1500 CE. The method is also used to analyze PDO, ENSO and North Atlantic conditions to try to identify dynamic states of variability that can explain the centennial-scale variability that is identified in the hydroclimate conditions.

General Remarks: This is a very clearly written paper that is well organized, thorough and placed well into the context of previous research. I uncharacteristically have little to criticize. The paper could almost be published as is, but I provide a few minor points

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below. The manuscript would also benefit from a thorough reading for typos. There are not a lot, but there are certainly some to be weeded out.

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. 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. It also should be noted that it is no surprise that there is some coherency across these reconstructions in the instrumental interval, the time during which they were all calibrated on the instrumental data. 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? 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.

2. 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.

3. 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

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the streamflow records. Something should be mentioned along these lines.

4. 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.

Pg. 764, Ln 15: Step ii

Pg. 768, Ln 15: Remove the second dry periods.

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