

Interactive comment on “Historical droughts in Mediterranean regions during the last 500 years: a data/model approach” by S. Brewer et al.

Anonymous Referee #2

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This paper attempts a very interesting data-model comparison for the last few centuries by comparing a cluster analysis of drought index patterns in models and proxy reconstructions (based on tree rings). I was not familiar with this type of diagnostic previously, and so I appreciate the explanation of how it works (though it could be made a little less technical in places). However, I find the specific application in this case and the conclusions drawn somewhat unconvincing - principally due to the lack of any demonstration of statistical significance or assessment of explained variance. Thus while I think this approach is promising, this paper is not yet acceptable for publication.

Major Points:

Chronology. It is clear that the paradigm of a multi-century LIA is not appropriate for

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these kinds of high resolution studies. The variance within the defined LIA is easily as large as the difference between the LIA and any other period. At this timescale therefore, a much more useful breakdown is by calendar year or period. Additionally since the forcings used in the models have timescales of decades to multi-decades, the issue of cause and effect can be much better addressed, rather than having vague and undefined references to a 'LIA'. I therefore suggest removing most references to the LIA in the results and discussion, and replacing them with more exact calendar year periods.

Fuzzy-logic: I don't see what is fuzzy about the Hagan distance, and I find the description of how it is calculated extremely opaque.

Clusters. I don't know why you expect that the first 5 clusters in the model would resemble the first five clusters from the data. Even if the patterns were similar, the ordering would probably be rather dependent on segment of the run analysed and the forcings used. However, it is clearly the case that the clusters aren't similar, since in many of the cases one model cluster is most like 3 or 4 of the data clusters and vice versa. Given then that there is no clear one to one correspondence in general between the model clusters and the data, this brings into question much of the following analysis. Possibly the authors should focus only on results where there is a clear correspondence?

Clusters vs. NAO. The only mechanistic exploration of the clusters is in terms of the previous winter's SLP pattern - which, as is well known, dominated by NAO variability (~20% of the interannual variance). However, this doesn't seem to me to be satisfactory - what about correlations to summer temperature, SLP or rainfall? Possibly these do not explain much of the patterns of the clusters, but super-posed epoch (SPE) analyses of all these fields could show some statistically robust connections that would be important for the interpretations. I am particularly concerned about the use of a 3 year mean winter SLP field around individual years to assess the connection between the SLP and the clusters. This is not sufficient to average out uncorrelated noise (a t-test

could be used to show that lack of significance for instance). A much better approach is the SPE analysis using all years when particular clusters are important.

Analysis of Individual years (p785-786). This is quite troubling. In any realisation of a climate model the weather in any one year is generally a random (chaotic) component and is not a robust feature of the run. i.e. an identical run with different initial conditions will have very different weather. Only in the case where you have an extreme short period forcing (such as a large volcanic eruption) would one expect to see a clear correlation in a single run. In most cases, the impact will only be seen in an ensemble mean. I am aware that the authors do not necessarily have access to multiple ensemble members, but there are other ways to average over the weather noise and assess the forced component (super-posed epoch analysis etc.). As it stands, whether 1741 (for instance) is a good match between data and model is highly likely to be purely coincidental and the authors should remove any such simplistic analysis.

Minor Points:

Abs. We hardly need this paper to demonstrate 'the importance of forcings'!

'occurs most frequently in data over this period' which period? the LIA or prior?

p 773. PMIP is certainly not the only project to have ever compared climate models to paleo-data! The 6kyr BP discussion is irrelevant. remove.

p 773. "As the internal climate variability generated by the external forcings of this period is relatively small, coupled ocean- atmosphere models are required to accurately represent the variability of the past few centuries." – makes no sense. Do you mean that external forcings are small over the last few centuries? In which case, there is no reason to expect that transient results in a model would resemble the real world at all. The only comparisons possible would then be with respect to the statistics of the simulations - not the transient path.

p774 Pseudo-proxy approaches were also pioneered by Rutherford et al (2003), and

in papers by Amman and co workers.

p 775 'yearly' data is likely to hide significant forced variability since there are significant seasonal differences in responses to particular forcings (Shindell et al 2004).

p 777 model grid boxes are not pixels

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