

Interactive comment on “Northern Hemisphere temperature patterns in the last 12 centuries” by F. C. Ljungqvist et al.

Anonymous Referee #1

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Review:

Northern Hemisphere temperature patterns in the last 12 centuries

By Ljungqvist et al.

General comments

Despite the myriad of large scale northern hemisphere palaeoclimate reconstructions, I believe the current study by Ljungqvist et al. is timely and provides a new fresh approach to studying large scale climate of the last millennium and importantly the spatial change in climate over this period.

This whole ‘genre’ of palaeoclimatology has been subject to fierce criticism and debate

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over the last ~10 years - both within the community and from “so called sceptics”. This is in part related to (1) many different groups perhaps not detailing their methods in a clear way, (2) the non- archiving of data (and/or code) and also a stubbornness by the sceptical community to accept very real environmental and climatic changes that more and more appear to be exceptional over the last 1000 years. The science by its very nature is uncertain, and it is easy for the sceptics to criticise and muddy the message. However, ultimately, the message is surprisingly coherent – as we see in Ljungqvist et al.

There is no doubt that each proxy type has its own inherent weaknesses (and strengths) and this study I believe addresses these issues in a sensible simple way without biasing the final outcome to one proxy type and/or location. I am sure a reviewer could potentially carp on about (1) the quality of some of the proxy records, and (2) the uncertainty in the dating of the non-annual proxies, but these arguments are honestly irrelevant in the context of this paper w.r.t. the methods employed and the robust result that comes out.

The focus of this paper is on the spatial change in relative temperature departures from the long term mean. Only Mann et al. has previously attempted to do this. The relative simplicity of the approach is this study’s strength. The final outcome(s) are not biased to (1) certain locations due to method (e.g. as has often been prescribed to Mann et al. 99 w.r.t. Bristlecone Pines and the use of PCA etc), (2) individual records (e.g. the debate over the use of the Yamal or Polar Ural tree-ring records) or proxy type. Figure 4 shows the common robust signal between different sub-sampling variants very well.

This is an important study and I know that it has been reviewed in other journals and in my mind has probably been, unfairly, rejected although its length might not have been ideal for some journals.

As it stands, this is a well written paper, the main article being commendably short, but with profuse amounts of Appendices and Supplementary Experiments for those who

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want to get into the detail.

I advise acceptance for publication in CP with only minor revisions.

1.Does the paper address relevant scientific questions within the scope of CP?

Yes

2.Does the paper present novel concepts, ideas, tools, or data?

Yes

3.Are substantial conclusions reached?

Yes

4.Are the scientific methods and assumptions valid and clearly outlined?

Yes

5.Are the results sufficient to support the interpretations and conclusions?

Yes

6.Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Yes – more so than previous studies probably.

7.Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes

8.Does the title clearly reflect the contents of the paper?

Yes

9.Does the abstract provide a concise and complete summary?

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Yes

10.Is the overall presentation well structured and clear?

Yes

11.Is the language fluent and precise?

Yes

12.Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes

13.Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

No – but see more detailed comments.

14.Are the number and quality of references appropriate?

Yes – the authors may want to check the reference list as it might not be fully correct or complete.

15.Is the amount and quality of supplementary material appropriate?

Yes – there is a lot, but I think it will address obvious potential questions from reviewers.

Specific comments

P. 3350, lines 16-17: There is reference to an accepted paper “Christiansen and Ljungqvist, 2011”. I just looked this up and it appears that the C+L (2011) series is derived from a sub-set (40 records) of the data-set detailed here. The authors should make some further reference to this study which appears to only focus on the reconstruction of large scale NH temperatures and the estimate of past amplitude changes (another important aspect of the reconstruction of past temperatures). In a similar vein,

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line 19 makes reference to Ljungqvist (2008). Again, this appears to be an earlier iteration of this work. I think this paper should be 2009 or 2010 as well. Overall, I think at some relevant location in the paper, the current article's results need to be discussed in terms of what else Ljungqvist has published. Presumably they are all complimentary??

P. 3351, lines 8-10: Please clarify what is meant by "global forcings". Do the authors mean external forcing such as volcanic or solar influences, or do they also include large scale synoptic internal forcing (e.g. ENSO, NAO etc). This statement needs also to be clarified in the context of time-scale as well. As their data are filtered to only look at centennial scales, volcanic and ENSO forcing, for example, are not particularly meaningful or relevant.

P 3352, line 1: Is it possible to make a direct comparison between the number of proxy records used by Mann et al. (09) and the current study during the Medieval period. Mann et al. (09) used substantially more records, BUT after reducing the number due to screening and then focussing only on those that are 1000-years in length, it might be worth stating actually how many series Mann used for the MP. This will potentially help emphasise the more (?) robust nature of the current study, w.r.t. number of records, compared to Mann et al. (09) over this period.

P. 3353, line 15: I would like to highlight the importance of the statement "...considered by their authors to be temperature sensitive...". Again, the authors are not making any subjective judgement of previous results and they are not performing an empirical screening of data. I think this approach should be applauded as all "locally" interpreted temperature sensitive proxies are included in the analysis. Of course, some are going to be more robust than others, but the authors clearly shows that sub-sampling of the data into different regions, seasons, proxy types etc, make little difference to the final outcome. That is a ROBUST outcome and should minimise counter arguments that we so often see that a particular outcome is related to the overweighting of one or two individual records.

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P. 3352, line 12: It is possible that I missed this, but at no point did I find a rationale why a 167-spline was used. Why not 150-years?

P. 3360, line 11: Surely some RW data-sets were used. Were they all MXD. I am not so sure. I think the D'Arrigo et al. (06) Gulf of Alaska series used only ring-width data for example.

P. 3361, line 5: Tuovinen et al. is a Scandinavian specific paper. I think a little more discussion is perhaps needed here and the second author of the paper is well placed to add in additional comment. The Lamont group (Jacoby and D'Arrigo) are adamant that it is in fact that RW data that is the integrator of longer seasonal time-scale information – hence their insistence that such data should be used at large scales to reconstruct annual temperatures, not summer. MXD is almost always restricted to the summer season but with specific weight to August (see Briffa 2002 synthesis papers).

P. 3367, lines 20-22: w.r.t. "Because the spatial coherence is expected to increase with increasing time-scales this comparison reveals that the proxy series exhibit a substantial amount of noise which motivates the use of spatial averaging of proxy anomalies." I am not sure there is a similar statement in the main article text. I think this is a very important observation and should be briefly mentioned earlier.

Technical corrections

P. 3350, Line 10: add in after "unprecedented", "...in the context of the last 1200 years) or something similar.

P. 3351, Line 18: Actually, spatial reconstructions were also developed by Mann et al (98/99)

P. 3354, line 15: "shown" not "show"

P. 3359, line 22: "acquired" not "had"

P. 3361, lines 7-10: This sentence is unclear. Please re-word. Insert comma after

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“resolved” in line 11.

Interactive comment on Clim. Past Discuss., 7, 3349, 2011.

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