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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Northern high-latitude climate change between the mid and late Holocene – Part 1: Proxy data evidence" *by* H. S. Sundqvist et al.

Anonymous Referee #2

Received and published: 20 August 2009

General comments

This manuscript presents a collection of 124 proxy-based temperature or precipitation reconstructions from 71 different locations north of 60 deg N for 6 ky BP and for 1750 AD. A key aspect of the study is the calculation of errors for the climate estimates. The joint presentation and comparison of the climate reconstructions is highly informative and in principle justifies the publication of the manuscript. However, the study has two considerable methodological weaknesses. Firstly, the calculation of the errors is based on assumptions that can not be expected to hold, and secondly the error components

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Discussion Paper

that are taken into account do presumably not include error sources that are due to potential instabilities of the transfer functions, which are likely to contribute substantially to the total error. Although these latter errors are mentioned at the end of the paper, they are completely ignored when designing and discussing the methodology.

While providing a comprehensive estimate of climate reconstruction errors is a major problem in palaeoclimatology that the authors can not be expected to fully solve, it needs to be discussed much more systematically and critically in the paper what the study can actually deliver with respect to error estimates. The way the errors are calculated provides at best a very first step towards conceptually sound error estimates, and by clearly discussing the likely consequences of making assumptions that do not hold, and of the consequences of data limitations for estimating errors, the manuscript could be a useful contribution to the development of error estimates in palaeoclimatology. In contrast, not addressing the critical points only adds confusion about the issue.

Although some of my concerns are very similar to those raised by reviewer 1, which have been published before this review, they reflect an independent opinion that has not been influenced by review 1. I will however avoid unnecessary repetitions and refer to review 1 for some points.

Specific comments

1.) p 1823, l 15-23: It is mentioned that the study uses mostly terrestrial records and that the transfer functions are based on current spatial covariability of proxies and climate, and that for reconstructions it is assumed that these transfer functions are also applicable for covariability over time. Although this is a reasonable and often the only possible approach it is not clear at all to what extent this assumption actually holds. The errors in reconstructions associated with a violation of this assumption could be large and contribute considerably to the total reconstruction error. Given that errors are a focus of this manuscript, it is not appropriate that this assumption is not critically discussed.

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2.) p 1824, l 16-22: The use of varying weights over the 100 year periods is not motivated. The current explanation is circular as it says 'the weights were introduced to ensure that observations closer to the midpoint of the time slice got a higher weight'. This is obvious, but why has this approach been chosen?

The distinction into 'preliminary' and 'final' weights is not needed, as it is clear that in a weighted mean the sum of the weights needs to be 1.

3.) p 1825, l 7-12 and p1862, l 1-5: As mentioned above it is a key question how well it is justified to apply transfer functions that are either derived based on spatial variability, or based on temporal variability over a relatively short time (as for instance in the case of tree ring data) to centennial and millennial variability. In some cases the problem can also be stated as 'is the transfer function stable over time?'.

I assume that the 'calibration uncertainty' in most cases only describes the residuals from a fitting data set, and at best the results of an independent validation of the transfer function on a very limited (in space or time) dataset that has not been used for model fitting. This error thus does not include the error introduced through changes in the system (for instance non-climatic, low-frequency influences on the proxies, as they are discussed at the end of the manuscript) and the resulting change in the link between the proxies in the climate.

4.) p 1825, l 12: As already pointed out by reviewer 1, the assumption of normally distributed and independent errors is unlikely to hold in reality. What are the consequences?

5.) p 1826, l 15: It may be reasonable to assume independence of the errors with respect to the two time slices, but the errors within a time slice are very likely to be dependent. So Eqn. 7 can not hold.

6.) p 1827, l 1-7: This is at best an ad hoc, rough guess for the dating uncertainty, and it needs at least a better motivation and discussion of its limitations.

7.) p 1827, l 15: Eqn. 7 seems wrong, it should be

sigmaˆ2 = 0.5 Yˆ2

8.) p 1827, l 22: This equation assumes independent errors, which is not the case (see also comment by reviewer 1).

9.) p 1832, l 24 - p 1833, l 8: As mentioned above, the errors discussed here seem not to be included in the error estimates.

Technical comments

10.) p 1821, l 25: The cooling effect of the ice sheets during the early Holocene has also recently been discussed in a simulation-based paper by Renssen et al., 2009, Nature Geosciences, 2 411-414

11.) p 1821, l 29: clarify the meaning of 'sea-ice insolation feedback.

12.) In the main text and figure captions often the word 'change' is used for the difference between the reconstructed climate at 6 ka BP minus 0 ka BP. This is confusing, as at least in my understanding a 'change' is forward in time, so has the opposite sign to the 6 ka minus 0 ka difference. I suggest to replace it with 'difference'.

13.) P 1830, l 1-3: Rather than only saying it is the goal of the companion paper to understand the observed changes, a short statement on what has actually been found should be made.

14.) p 1834, l 8: Where does the apparently unrealistically small total uncertaintly of 0.05K for one of the records come from? This seems to be an example for error estimates that are clearly smaller than what can realistically be expected.

15.) p 1834, l 16-28: Non-climatic influences on proxies and the spatial representativity of reconstructions are completely different problems and should thus be discussed in separate paragraphs.

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16.) Please comment on what kind of proxy data sets have been used for PMIP1 and PMIP2 in high latitudes.

Interactive comment on Clim. Past Discuss., 5, 1819, 2009.

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