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Climate of the Past Discussions

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 #1

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1 General comments

In this paper, the authors seek to develop a quantitative understanding of the absolute change in climate between the mid-Holocene and the preindustrial period in northern high latitudes. As they say in the abstract, their aims are "to compare results from proxy data with results from several climate model simulations" and "to try to quantify the uncertainties in proxy reconstructions". Since the first of these aims is not met by the present paper (the authors defer this to a companion paper also submitted to CPD), the

primary focus of the present work is to quantify uncertainties. As a result, precisely how the authors handle uncertainty is crucial. The authors offer some nice discussion of key environmental/climatological issues which make the task of quantifying and tracking uncertainty extremely challenging. However, the methods they adopt for working with those uncertainties are *ad hoc* at best and, at worst, extremely naïve. My review focuses on these shortcomings.

2 Specific comments

- Without any motivation or justification, on page 1825, the authors assume that all uncertainties are normally distributed. This cannot be the case. For example, the date estimates obtained by calibrating radiocarbon determinations are seldom symmetrically distributed and are often multimodal. In addition, some of the climate variables must have nonsymmetric errors too; for example negative precipitation is unlikely (if not impossible) and extremely large amounts of precipitation are fairly common. I appreciate that the present authors are only following convention by assuming normal errors but, given their focus on quantifying uncertainty, it is not a sensible assumption.
- Equally crucially, the authors seem to be unaware of the fact that point-wise climate estimates at two individual points in time (or space) do not contain enough information to compute reliable estimates of the distribution of the change in climate between the two points (even if the point-wise estimates include reliable uncertainty statements). Put formally, the individual climate estimates at each time (or space) point are marginal estimates; concerning climate at each point separately. In order to obtain a reliable estimate of the change in climate between the two points, a joint statement of how the two estimates vary together is needed. Given that the two time points under consideration here are several

thousand years apart, this may be relatively unimportant, but the authors must convince us of that. However, even that would not be enough to validate the methodology used here since these authors are also working with climate reconstructions that are near to one another in space and the same concern about joint and marginal estimates holds here too. Given that the original papers from which the present authors got their data do not offer joint spatio-temporal estimates of the climate variables they reconstruct, the present authors simply do not have access to the information they need.

- The preceding observation means that the statistical methods used by the authors, which assume independence between the uncertainties on the available climate estimates at all points in space and time, are likely to over-estimate the uncertainties on the change in climate between the mid-Holocene and the preindustrial period. Given the large number of related variances that the authors are summing to obtain their overall uncertainty, this over-estimate could be substantial.
- Throughout the text, the authors use the term "statistically significant" without offering any formal discussion of what constitutes statistical significance in the context of the present problem or how such significance might be tested.

3 Technical comments

- Given my observations above, eqn. 6 (which contains no covariance components) cannot be right unless the relevant covariances can be shown to be zero.
- Eqn. 8 is fundamentally flawed, one cannot map uncertainty on the time axis onto uncertainty on the climate axis in this way. Indeed, this approach is not really tackling dating uncertainty at all. It is simply inflating climate uncertainty by

an unknown amount; since some of the uncertainty formalised by $\sigma^2_{c_{6ka}}$ is shared by $\sigma^2_{d_{6ka}}$ (because both derive from X).

- In deriving eqn. 10, why is "reading" uncertainty "assumed to correspond directly to one standard deviation of the climate variable"? Also, why is there no mention here of "reading" uncertainty on the date estimates?
- More motivation is needed for the method in eqn. 12. In particular, why do the authors divide by the sum of the variances?
- In Section 3, all results should be closely associated with an indication of sample size. Some sample sizes are very small, but this is not clear from reading the text.
- In Section 3.1 (page 1828), we are told that the results are an "unweighted average of $\Delta\bar{T}$". In what sense are they "unweighted" and why?
- The reference to the OxCal package (page 1824) is not appropriate since the authors have not used the part of OxCal described in that particular paper. Bronk Ramsey (2001) Radiocarbon 43(2A), 355–363 would be more appropriate.

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