

Interactive comment on “Last nine-thousand years of temperature variability in Northern Europe” by H. Seppä et al.

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We acknowledge the constructive and detailed comments of the two referees. We have made the following changes and amendments.

Referee 1 Abstract: Ref. 1 asks about the winter humidity. Given the word limitation of the abstract, it is not possible to add information about the winter humidity, but we have added a sentence “Estimates of late-Holocene winter precipitation changes exist (e.g. Bakke et al., 2008), but their comparison with pollen- or chironomid-based Tjul or Tann reconstructions is more ambiguous” to the discussion (page 11). We have retained the second part of the last sentence because this issue is discussed briefly in the last paragraph of the conclusion. Results and discussion: -the magnitude of the HTM is really higher in the annual mean temperature data than in the summer mean temper-
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ature data. This suggests that during the early Holocene other factors than insolation alone were important. We do not raise this topic in the discussion, as it is slightly out of the focus of the paper. -it is true that Seppä & Birks 2001 provided a pollen-based precipitation reconstruction from N Fennoscandia, but we do not in general regard the pollen-stratigraphical records particularly accurate for precipitation reconstructions in Fennoscandia. Temperature appears as the more critical parameter for vegetation than precipitation and the calibration models for precipitation have poorer performance than the models for temperature and the errors of the reconstructed values are large. -we do not think that the dating uncertainty and sampling resolution of our stacked records influence the visual comparison with the residual ^{14}C -record. They would if the comparison was based on one or few temperature records, but in the stacked records with 2000-3000 values the influence of dating uncertainty should be smoothed out. Editorial comments: -these have been carried out, apart from “rotate right offset axis that numbers read according to left offset axis”. We do not understand what this means. Comments related to references: -these have been amended.

Referee 2 1) Ref. 2 raises an important issue by pointing out that no dedicated analysis of the potential cycles in our data has been undertaken. We are aware that the statistical test of the significant features, such as potential cycles, is a relevant aspect of studies such as ours. Some preliminary work in this respect was done during the processing of the data. However, this task is methodologically non-trivial, and differing results can be obtained depending on various levels of smoothing, data amalgamation, and the selection of statistical technique used for the analysis. It is our aim to carry on with the statistical analyses in the future to further analyze our stacked records. We have therefore added a short paragraph to 14 (end of the “Forcing factors” chapter) saying that “A test of the statistically significant features, including potential cyclicity, in our stacked curves is currently underway. This task is non-trivial, however, given that the test must account for the statistical errors of the reconstructed values, as well as the dating errors in our stacked records constructed from individual records dated with various dating techniques and accuracy”. In addition, as suggested by the referee, we

have downplayed the issue of cyclicity in the introduction by deleting all references to the terms “cycle” or “cyclicity”. 2) The subject of Telford & Birks 2009 paper is to analyze and discuss the role of spatial structure in the calibration models. This is relevant for more realistic understanding of the factors that influence the predictive power of the calibration models, but would not help to strengthen the case which climate parameters should be selected from different parts of the study region. The basis for this choice is more ecological than statistical. 3) We have added the following text about the use of Cl concentration as a record of atmospheric circulation: “The GISP-2 ice-core Cl ion concentration record is often inferred as an indicator of predominantly marine airmasses over Greenland and therefore a proxy for strong Icelandic low and westerly airflow in the North-Atlantic–Eurasian region (Mayewski et al., 1997) (Fig. 5d). There is some correlation, albeit weak, between the Cl record and our temperature record, especially during the cold period at 3800-3000 cal yr BP, but this support for the suggested circulation dynamics is tentative at most, especially because the relationship between the ice-core Cl concentration and atmospheric circulation over northern Europe is relatively poorly constrained.” 4) We have not yet looked at other forcing factors such as volcanism or greenhouse gases. This can be done in the future, once a more rigorous statistical analysis of the features in our data has been carried out. Technical corrections: These have been amended, apart from making the filled circles completely black. The black and white circles seem easier to observe than completely black. We think that the Table 1 is clear, as it clearly indicates whether Tann or Tjul is reconstructed from each site.

We hope that the paper can now be accepted for publication in *Climate of the Past*.

Sincerely,

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