

Interactive comment on “Dendroclimatology in Fennoscandia – from past accomplishments to future potentials” by H. W. Linderholm et al.

Anonymous Referee #3

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I. General comments

First of all, I welcome this initiative summarising dendroclimatological research in Fennoscandia and indicating directions for future research. A lot has been achieved in this region since the first tree-ring chronologies were built in the early 20th century, and there is steady progress. Hundreds of papers have been published varying in the scientist's background and aims, applied methods, studied regions, environments and species. Thus, I am aware that a review article on this topic is not a trivial task, and maybe therefore my expectations were rather high. Unfortunately, my impression is that this attempt lags behind what might be possible and desirable. Many paragraphs bear the character of reporting results without reaching the point of clear syntheses and conclusions. Thus, though CPs review criteria might not be well-suited for a review

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paper, my overall rating for the paper in its present state is “fair 3”.

Scientific Significance: Fair (3 - not a "substantial" contribution to scientific progress)

Scientific Quality: Fair (3 - approach and balance can be improved)

Presentation Quality: Fair (3 – could be clearer, more concise, and better structured)

1. The paper addresses relevant scientific questions within the scope of CP; reviewing the use of a terrestrial archive for reconstructing centennial to seasonal-scale atmospheric dynamics during the mid to late Holocene.

2. As a review article, the paper is not supposed to merely focus on presenting novel research aspects. However, the expectations triggered by the phrase “future potentials” in the paper's title were not fully met.

3. In my opinion, more efforts should be made formulating substantial conclusions. The review part would profit from a more critical and thorough approach. With the exception of the northern Finnish multi-millennial pine chronology, no weaknesses in previous work have been pointed out. Some paragraphs bear the character of listing results without any attempt of summary and synthesis (e.g., Chapter 4). Furthermore, the logic/reasoning in several cases could be clearer.

4. This paper does not allow an in-depth discussion of scientific background and basic methodology. In the form given here, such information is not satisfactory and should be omitted (Ch. 2).

5. Are the results sufficient to support the interpretations and conclusions? The paper is based on results published elsewhere. However, in at least one case, results obviously are reported erroneously (Kirchhefer 2001, 1439:7ff). Also, several figures are reproduced from articles which still are in preparation (Seftigen 2009(sic!)). I have difficulties, allowing such data being reported in a review article. The same might apply for figures reproduced from a master thesis (Björklund 2009), which potentially has not been subject for a peer-review process as rigorous as demanded for an international

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publication.

6. The description of experiments and calculations does not apply here (but see point 5).
7. The authors give proper credit to related work and clearly indicate their own new/original contribution. However, the second part of this paper (“recent accomplishments” cf. 1417:24) relies strongly on unpublished in-house results. This causes bias. Solutions might be a) changing the title to “Dendroclimatology in Sweden with references to adjoining regions” or b) including (conferring with) scientists being active in other parts of Fennoscandia.
8. The title reflects the contents of the paper. However, the aspect of “future potentials” would deserve more attention in the paper. Also, one might argue that, not surprisingly given the affiliation of the author group, the paper is concentrating more on Sweden than the entire Fennoscandia (see point 7).
9. The abstract provides a concise and complete summary.
10. The overall presentation could be better structured. The aims of the article are clearly stated (1417:18-26), however, the manuscript does not strictly follow this three-fold structure. Some sections/paragraphs should be omitted, others moved (see below).
11. The language generally is fluent and precise. Nevertheless, in some occasions I question the proper use of terms and phrases. The manuscript would profit from a final language check by a native speaker, as well as a common effort of the author team to eliminate spelling errors, particularly also in the reference list.
12. As far as I can see, mathematical formulae, symbols, abbreviations, and units are correctly defined and used.
13. Some parts of the paper (text, figures, tables) should be clarified, reduced, combined, or eliminated. See below.

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14. The number and quality of references are appropriate. Again, I would avoid too heavy weight on unpublished work. Certainly, a complete list of dendroclimate publications would not be practical. Alternatively, the focus of this article would have to be narrowed to, e.g., just climate reconstructions.
15. No supplementary material is given.

II. Specific comments (individual scientific questions/issues)

Chapter 1

1416:7f, large areas of old-growth forests: This description might be rather optimistic, depending if you see the forest with a forester’s or dendroclimatologist’s eye. And (cf. 1417:21, virgin forests), there are only few, scattered areas which are untouched by man.

1416:22f, geographical coverage: How is the geographical coverage of study sites for dendroclimate reconstructions?

1416:26, sediments: Most sediments are not high-resolution archives.

1417:6 reconstructions & 1417:11-16 low-resolution: Are there more recent references?

Chapter 2

1418:1-18: This paragraph needs a better structure, e.g. (1418:6f, forcing factors/nutrients), the term ‘forcing’ implies that you are discussing growth limiting factors that are relevant to climate research and thus particularly those which are variable on an inter-annual scale. I do not regard nutrient availability as such factor.

1418:19-24: These lines would be better placed in the introduction.

1418:25 – 1 421:24: I don’t regard this information relevant for this publication. The manuscript would profit from omitting this section, thus providing space for more de-

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tailed information on applied science. For this reason, I do not comment here on details in section 2.1. Maybe it would be advised even discarding the entire chapter 2, but giving, where necessary, specific information in the following chapters.

Chapter 3

1422:1, dendroclimatical: The heading should reflect that these studies deal with climate-growth responses, not reconstructions.

1422:2-11: Are there any references to these earliest Fennoscandian tree-ring studies?

1422:8ff, Reventlov: Was this study performed in Fennoscandia or, e.g., on Lolland/DK, and did he relate tree-rings to climate?

1423:1ff, RCS: What then about early attempts of hand-drawn 'splines'?

1423:12-16, autocorrelation . . . however: Don't Ording and Jonsson agree?

1423:20-27, Schove . . . /Furthermore . . . : These statements are contradictory.

Chapter 4

1424:1-6, differences in . . . climate: Here, you could add facts. How big are the differences in climate?

In the end of this chapter I would expect a summary of the observations.

Chapter 5

1425:24ff, calibration-verification, explained variance: Here, I would regard the explained variance for the total calibration period as sufficient. A table summarising the main characteristics of the reconstructions discussed would be most helpful (similarly to Tab. 1; number of trees and/or chronologies, months reconstructed, length calibration period, R2 etc, length reconstruction, region). This would also apply for precipitation, ch. 5.2.

This chapter (5.1) you conclude with a recommendation. Where would you go for the

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"few new key sites" (1427:25), and how many sites would you have to sample to find those?

1427:28–1428:17: I would move this paragraph to Chapter 4.

1428:5, convective precipitation: I regard this as an inland phenomenon.

1428:6, Thus . . . /site: Which kind of site would you look for to capture spatio-temporally highly variable precipitation?

1429:1f, dry period 1810-1835: Was this observed by Helama and Lindholm (2003), too?

1429:9f, trend during 20th century: This would be obvious from the instrumental record alone. Is this trend extraordinary compared to the past 400 years?

Chapter 6

1430:6, dense network: Is there a contradiction between the requirements for "a dense network" and "a few more key sites"?

1430:10, living trees >1000 years: This is most probably correct. Unless you regard juniper a tree (and useful for climate reconstruction).

1430:24, warm and moist: This applies for winter conditions.

1431:18, Jämtland chronology: You might mention that this chronology is, though multi-millennial, still not continuous. The same applies for climate reconstructions based on this record.

1432:8ff, large parts . . . likely been collected: Can you provide more precise informations?

1432:9, Schweingruber et al. 1993: Is this citation appropriate here?

1432:24, not continuous: Isn't this true for the Torneträsk record, too, and weren't the gaps in both series first closed by mutual Swedish-Finnish efforts?

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1433:3f, homogeneity and robustness: You could address this question also for other records, e.g., the Torneträsk and Jämtland chronologies in Fig. 3. The material for the latter records is gathered from a much smaller geographical area, but still replication (as typical for tree-ring records) is rather poor for certain periods.

1433:8f, common ... high-frequency variability: You could easily visualise this by adding a running correlation to Fig. 3.

1433:18, replication: See above. Please add to Fig. 3.

1433:26, decrease: The scale of Fig. 3 is not best suited to show this detail, but the "decrease" appears rather as a growth depression, not a declining trend.

Chapter 7

1434:9, Fennoscandia: Is this true for the entire Fennoscandia?

1435:15, AD 1706: My task here is not to review Folland et al. 2009, but I notice that 1706 is the first year (thus one tree only) of the shortest tree-ring chronology contributing to the reconstruction. So how robust is the early part of that reconstruction?

1435:15, explained variance: How strong is $R^2 = 39\%$ compared to reconstructions of winter NAO?

1438:2, pooling: Most readers might not be familiar with this concept.

Chapter 9

1439:7, growth increases: The c. 1950 growth maximum is nearly absent at the coast of Norway, which is also seen in the investigations of Kalela-Brundin. I assume that the explanation of this growth surge is not straight-forward.

Chapter 10

Most "future prospects" outlined this chapter describe already ongoing activities. Rather, I hoped for some guidelines or visions where, according to the authors, dendro-

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climatology should head. For instance, what is lacking to the "perfect" set of chronologies/reconstructions covering entire Fennoscandia? Which present key sites need consolidation in terms of internal replication and homogeneity? Just to address two simple questions.

1440:9, spruce: Wouldn't oak belong here too?

1441:26, exactly: This ambition might be unrealistic.

1442:2ff, PMIP2 & model validation: Could you provide any references?

Tables and Figures:

Tab. 1: Have you observed any overlap between these studies (multiple use of data)?

What about several tables of this kind, particularly on climate reconstructions?

Fig. 2: Until the original publication (Seftigen in prep.) is published, I suggest replacing this map by a another map showing correlation coefficients achieved in various previous studies (precipitation, temperature, NAO etc.)?

Fig. 3: The outlier around 500 BC in Jämtland most probably is caused by low internal replication. Could you please indicate which sequences of the two chronologies actually are comparable, e.g., by adding sample depth (r_{Bar} , EPS)? A running between-chronology correlation could demonstrate how well these records cross-date.

Figs. 4 to 6: See comment on Fig. 2. If published here, the caption of Fig. 6 needs to be improved (what is "the area above 63°N"?).

III. Technical corrections (typing errors, etc.)

1417:4, modes: phrasing?

1417:13f, analysis: records

1417:17, method: archive

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1423:20, limitations: Principles?

1428:19f, few... Nevertheless: only few... For instance... ?

1429:10, centurial: centennial/secular?

1433:1: Lindholm

1435:2f, in subsequent years: (remove)

1435:14, western Norway: coastal North Norway (west Norway = Bergen area)

1435:26, positive...: Something is missing here.

1437:6, intra-site: between-site?

1440:22, multi- analysis: Something is missing here.

References:

- Many references are not listed in the proper alphabetical order.

- There are rather many spelling errors, particularly in the non-English and non-Swedish references (e.g., Hustich).

- Several manuscripts which are in preparation or in review are referred to as "2009" in the text.

Cook & Kairiukstis 1989: Erroneous year.

Linderholm et al 2008: Publisher?

Raven 1999: There are two co-authors as well as a newer edition.

I hope these comments are just and useful.

Best regards!

Interactive comment on Clim. Past Discuss., 5, 1415, 2009.

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