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Climate of the Past Discussions

Interactive comment on "1200 years of warm-season temperature variability in central Fennoscandia inferred from tree-ring density" *by* P. Zhang et al.

Anonymous Referee #1

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This is a paper where the authors, Zhang et al., use tree-ring proxy of maximum wood density (MXD) to reconstruct summer temperature of the past 1200 years in their study region, central Fennoscandia/Sweden. This data contains several benefits and it is good to see that the research of this type of material is developed in the region. In its present form, the paper lacks many aspects of rigirous scientific research. With this unclarity, it is not possible to fully explore the research and discussion of the paper and the authors should provide more explicit information of their data and methods. I hope the authors find this commentary as encouragement for improved research!

First major source of criticism: it is unclear which part of the MXD data is new, which portion of the MXD data used in this paper originates is actually the same as G11

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data (the abbreviation the authors use) or data produced and analysed previously. The authors are recommended to provide this information. It should be mentioned explicitly already in the abstract, how this paper advances the research in the region of central Fennoscandia and Sweden by introducing new data, what are the new insights produced by this paper in comparison to G11 study and Linderholm et al. (2014a), and the other past MXD studies in this region (also the new MXD papers from Lapland incl. Esper et al. (2014), Pritzkow et al. (2014) and Matskovsky and Helama 2014) are studies to be introduced here), what are the advancements produced in this paper in comparison to G11 w.r.t. the MXD data, the standardisation and reconstruction methods, how is the reconstruction produced in this paper advancing the science w.r.t. G11 paper. Without this information the rearder of the paper is confused and the value of the current paper is questioned.

Second major source of criticism: the methods used in this paper are not sufficiently described. In page 6, the authors refer to G11 paper w.r.t. setting of ITRAX method. And they also refer to standard techniques (line 16-17). It is hard to believe there is such a thing as standard method. If any adjustments were made, in G11 paper or in this paper, to modify the MXD data from Walesch (the authors spell this differently in lines 15 and 16) and ITRAX techniques, this should be mentioned if it was done in G11 paper or by the authors of this paper. It is interesting to find out that the authors are also adjusting (page 6, lines 18 onwards) the absolute MXD values as dictated by temperature lapse rate using a method which they have developed previously in yet unpublished paper (Zhang et al. 2015). As this paper is not yet published, it is not possible to judge if this method is reasonable at any level and what are the requirements and actual statistical procedures to attain this adjustment. It recommended that if this paper is not yet pubslihed, the authors make an illustration of the method in the supplementary portion of the paper. If not done so, the current paper is done using fully unpublished methods and this is not following any scientific requirement. This is important because the adjustments of this type can introduce low-frequency variability to the reconstructed temperature data. The authors should also include all the statis-

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tical tests and their verbal illustration in the methods section. Now there are statistical measures and tests done to the data (Table 1 and 2) which are not described in the correct section of the text.

It is also suggested that the language of the paper is reviewed by native English speaker. At this point, there are several inconsistencies in the text needing clarification.

Smaller points of criticism:

p2, l6, it is unclear what “mean adjusted” actually means p2, l7, RSFi is mentioned as a method with no other information what this actually means and stands for p3, l15, once again the author mention something new, Delta-Density, but this is not described p3, l20, dry deadwood is also subfossil wood p3, l21, for the language, it would be good to decide if G11 means the study or the data used in the study p4, l2, what is this study, what did it do, and how it relates to your study p4, l13, it is uncertain what is the uncertainty that the authors mention here p5, l3-12, the authors are recommended to add papers that actually show the influence of all these factors to the study region p7, l24-27, really unclear sentence p8, l14 onwards, when using RCS methods, do the authors always apply the RCS method using a single or multiple curves. It is surprising to that the author skip the papers of Melvin & Briffa (2014, 2014b) “CRUST: Software for the implementation of regional chronology standardisation: part 1. Signal-free RCS” and “CRUST: Software for the implementation of Regional Chronology Standardisation: Part 2. Further RCS options and recommendations”, where the M & B demonstrate the importance of using multiple curves in RCS.

Interactive comment on Clim. Past Discuss., 11, 489, 2015.