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Interactive comment on "The potential of tree-ring cellulose content as a novel supplementary proxy in dendroclimatology" *by* Malin M. Ziehmer et al.

Anonymous Referee #2

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The paper by Ziehmer et al. highlights the possibility of using cellulose content in tree rings as a proxy for temperature. This paper is a rather technical paper that has two components: 1) a methodological aspect in which the authors discuss how to measure cellulose content in trees and 2) the application of using cellulose content as a proxy for temperature. While I believe that the approach of the authors is interesting and might even be promising, the authors have not convinced me of the accurate measurements of cellulose content. Lots of errors can be introduced in the method (which to a certain degree the authors discuss), but the paper lacks a clear estimation as to what the error on this method is. This could for example be accomplished by doing replicate sampling on the same tree. Another possibility is to split the paper in two papers, one which discusses the methodology and one which discusses the chronologies.

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Major comments

While there are few grammatical and/or spelling mistakes, the paper should be improved for clarity. At times the paper is just very confusing. I suggest the authors try to shorten their paper and remove certain sections that make the paper unnecessary long and confusing (e.g. the discussion on whether to use dry weight before or after cutting, see more explanation below). In addition, the result section is also very confusing (see more details below)

Introduction

p2 L3-9: The authors argue that alpha-cellulose is the preferred substance for isotope analysis due to its long-term stability. I believe this is rather vague and the authors could give more details about the low mobility of cellulose, the fact that alpha cellulose is a singular chemical compound and the fact that it is also that the pathway from photosynthetic products to cellulose formation is more direct than the pathway to any of the other extractives (additional fractionations).

p2 L21-37: In this section, the authors discuss the fact that subfossil or fossil wood can have degradation of different wood components. The authors stress how this influences the isotope ratios and can have an effect on the ratios of the individual components. This is a major limitation of the study, but although the authors mention this, they don't seem to be worried that this might affect their study and there is no further mention of this in the rest of the paper and not even in the discussion.

Overall, the authors should bring in more discussion on the physiological aspects of the different components of wood formation in order to give the reader background into the possible limitations of the method. For example, cellulose/lignin/extractive ratios are known to differ between juvenile and mature wood and between heart wood and sapwood. It is also known to differ between normal wood and reaction wood (see for example Saka, 1991, Chemical composition and distribution, Ch 2 in Wood and Cellulosic Chemistry, Second Edition, Revised, and Expanded, as well as Rowell et

al 2012, Handbook of Wood Chemistry and Wood Composites (Second edition), CRC Press, London (2012), pp. 48-51)

The authors need to discuss this in the paper and need to address how this could affect their data.

In addition, the authors also should research additional papers studied on similar subjects. The following paper discusses lignin content as a proxy for temperature. Since lignin and cellulose are the two main components of wood, it seems logic that a change in one will also affect a change in the other. Gindl, W., Grabner, M. & Wimmer, R. 2000. The influence of temperature on latewood lignin content in treeline Norway spruce compared with maximum density and ring width.Trees 14: 409-414.

Results

P6, L5: The authors discuss that a determination of sample weight after cutting is essential. Considering that the study relies on cellulose content measurements, I believe that this is rather obvious. It is more logical to use dry weight after cutting rather than before cutting. I think it is a good idea of the authors to point it out and to discuss it, but I suggest the authors remove it from the methods (section 2.5). This will make that section much less confusing.

P6, L 11: The authors discuss the fact that a systematic error is introduced while the samples are unpacked. This is indeed a good addition, but the authors don't mention what they consider this error to be. Since it is a systematic error, the authors argue that the variability between samples should not be affected. However, the error means that small differences in cellulose content between samples cannot be interpreted. Therefore, it is very important that the authors discuss/estimate the error. Especially considering that they are looking at rather small differences in cellulose content. When looking at Table 4, it seems that the maximum weight loss during unpacking of the sample (after extraction) is 5.3 %. The authors could use this as a %error on their data. More accurately, the authors should determine the error by using replicate sampling of

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the same tree.

P 6 section 3.1 and further: the use of % for cellulose content as well as % to express differences between sites is rather confusing and makes the paper difficult to read. Is there any way the authors can make this clearer? For example: p 6 L 22: UAZR1 and UAZR2 values are 10 percent lower than the other two trees. This could mean that their cellulose content drops from 40% to 30% (which is not the case), or that the cellulose content drops from 38 to 34 % (roughly 10% of 38% $\sim$4, so a 4 percent drop, which seems to be the correct interpretation here (?)). Another example: P6 L30: an increase in CC % over time by $\sim$5%. What does this mean? from 35 to 40 % or from 35 to 36.8% (reasoning that 5% of 35 is $\sim$1.3)

Discussion and Conclusion

The authors should revisit the methods used and include a discussion on the limitations of their method. Also, a discussion on the practical aspects of this method should be included: It is definitely not easier than measuring ring widths, so what is the advantage? Is there other information that has been revealed?

Minor comments

P4 section 2.5: this section is extremely confusing. If possible it should be rewritten P4 L32: I think the authors mean “1. dry weight” in the equation? P4, L33 “weighing” instead of weighting P5, L3: replace cellulose with sample P5, L19: add the . . .obtained in the form. . .

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