

Clim. Past Discuss., referee comment RC2  
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## Comment on cp-2021-190

Anonymous Referee #2

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Referee comment on "Prospects for dendroanatomy in paleoclimatology – a case study on *Picea engelmannii* from the Canadian Rockies" by Kristina Seftigen et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-190-RC2>, 2022

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The paper „Prospects for dendroanatomy in paleoclimatology – a case study on *Picea engelmannii* from the Canadian Rockies" by K. Seftigen *et al.*, explores the paleoclimatic potential of a broad set of dendro anatomical proxies, in particular focusing on the relationships between "new" proxies (mostly related to wood anatomy) and proxies that are relatively more known, such as X-ray maximum latewood density (MXD) and blue intensity (MXBI), and already used in the boreal environments of North America. My general impression about this manuscript is very positive, the paper deals with a timely issue, and such an assessment of the strengths and weaknesses of dendro anatomical proxies is a much-needed help for anyone working with "alternative" tree-ring paleoclimatic proxies. It is also notable that long chronologies of wood anatomical parameters are finally emerging even for the North American continent. For this reason, I can easily foresee that this work has the potential to become a highly cited reference in the field.

That said, I think there are still a few points that require attention.

### Major issues

The first major issue is related to the chronology development of wood anatomical parameters: in lines 159-165 it is specified that wood anatomical parameters were calculated as the 75<sup>th</sup> percentile within 20 µm wide bands parallel to the ring boundary. Nothing to say against this tree-ring partitioning approach, but it is not clear how the data obtained for each band were treated: usually, when the tree ring is partitioned, then multiple chronologies are developed. Since I do not see this in the manuscript, I assume that these data were somehow averaged. If so, it should be specified. In any case, the procedure of chronology development should be clearer, avoiding pointing to third papers.

Secondly, I have major concerns related to the procedure for testing temporal stability of dendroclimatic relationships in dendro anatomical proxies (Ln250 - Ln259 & Ln452 - Ln

473):

1) if the goal is to test the temporal stability of dendroclimatic relationships obtained for the monthly data, why shift from using monthly data to daily data, using a different climatic dataset (the Berkeley Earth dataset) that is, in addition, experimental and that You seem not to trust completely (see Ln. 461-464)? I think this is simply wrong and cannot be accepted unless a thorough comparison between the two datasets (CRU TS v4.03 and Berkeley Earth dataset) is performed to check if there exist substantial differences between the two.

2) if daily data are going to be used (and I am a huge supporter of daily data, whenever available, especially with wood anatomical data) why use them in 30-days fixed windows (so basically reconducting to a monthly analysis)? This is not the approach proposed by Jevsenak and Levanic (2018), which I would actually recommend following, testing moving correlations over the same 30-year period but using temporal windows starting from i.e. 14 days up to whatever length You'd like to test (30, 60, 180 days). That would help better target the temporal windows of climatic sensitivity described in Figure 3 and Figure 5.

A third major concern is related to the overall presentation of the paper. Despite being well written and accompanied with high-quality pictures, the manuscript is, in my personal opinion, exceptionally long, and at some point, allow me to use this term, becomes exhausting to read. I refer, for example, to the analysis related to the biases in the MXBI technique that are introduced in lines 190-199 and then discussed in lines 411-450. Honestly, I do not see the point of this long analysis and discussion. I am not criticizing its soundness nor questioning its interest. But to me, it looks like material for a stand-alone paper, a deep technicality related to the blue intensity with weak (or at least not immediate) implications for dendro anatomy within the context of this manuscript. I believe that the strength of this work is that it is exploring important technical details related to those dendro-anatomical proxies that are less known in this region. It should not focus on going into deep technical details related to proxies or techniques that are already relatively more known and applied. Another example is in lines 368-385. This long paragraph discussing the climatic response of EW features could be easily avoided, since it is evident throughout the entire manuscript, and according to Table 1, that EW signal is not predominant at this location. Since the paper is already very long, this long (and sometimes repetitive) discussion on EW density and lumen area could be avoided. The last example could be section 3.1, where several times concepts discussed in the following sections are already mentioned. In general, I suggest careful evaluation throughout the manuscript if certain paragraphs and/or concepts could be removed or rephrased to improve the readability of this paper.

Minor issues:

The paper introduces the longest dataset of dendro-anatomical parameters for North America but there is not a figure showing such 1585-2014 chronology in the paper for any parameter. I suggest adding it, even if the analysis is focused on the 1700-1994 period.

Moreover: why is the chronology truncated in 1994? And the data in Table 1 refers to the 1585-2014 or 1700-1994 timeframe? Please clarify.

Lines 320-331 Could be integrated into the next section.

Lines 360-361 "Even though the parameters describe two temporally distinct temperature signals, both are encapsulated within the short June-July-August period". What are the two temporally distinct temperature signals? Please clarify.

Lines 363-364 "...average monthly temperatures rise above 0 °C only in four months of the year". In Fig 1c it is actually five months.

Lines 364-366 "This window is substantially shorter than the single but wide target season observed in the latewood anatomical traits of *P. sylvestris* growing in temperature-limited environments in northern Scandinavia". I don't understand this sentence (i.e. what is the "target season observed in latewood anatomical traits"?), please be more clear.

Lines 368-385 Regardless of my previous comment (see above), I would like to add a few words about this. The reversing relationship observed between June-July temperature and EW density and lumen area might be due to an effect of precipitation, or in general of moisture availability, at that time. The initial stages of wood formation (hence, the anatomical features of the cells formed firstly along the ring) are highly dependent on water availability, which determines cell turgor in the enlargement phase. This is generally reflected in the EW lumen features, such as lumen area or lumen radial diameter. As a general remark, I would not say that EW features (i.e. lumen area) are not suitable climate proxies (or at least I would properly contextualize it): this might (is) true for this location and for temperature limited environments, but in arid environments earlywood (not latewood) features (in particular lumen size) are a crucial climatic proxy (not cell wall thickness).

Figure 4 The r coefficients listed on the right side, how are the aggregate months' correlation computed? Is it an average of the two r? Why some values are listed for both single and aggregated months, and in other cases not, even though the correlations seem equally strong?

Lines 458-459 Please clarify the meaning of "peripheral ends" and of "elusive".

Lines 484-489 This is a key methodological aspect that lead to one of the main findings of this paper, hence I was surprised to find it here and not in the Methods section. Please move it to the Methods section.

