

Review:

Is blue intensity ready to replace maximum latewood density as a strong temperature proxy? A tree-ring case study on Scots pine from northern Sweden

Björklund et al.

General Comment

There is no doubt that the relatively new and essentially untested parameter Blue Intensity holds great promise and Björklund et al. present a timely paper on utilising this parameter (along with modifications - i.e. ΔBI) for dendroclimatic reconstruction of past summer temperatures.

My major worry about this paper is their insistence that BI must be converted/transformed to proxies of density. Why? Firstly, are BI and MXD actually measuring the exact same wood properties? I think wood density is related to cell wall thickness which is in turn controlled by cellulose, hemi-cellulose and lignin content. As far as I understand the theory, BI is measuring the absorption and related reflectance of the light from lignin at the surface of the sample which is strongest in the blue part of the frequency spectrum. Therefore, MXD and BI are similar parameters w.r.t. wood properties they measure, but ultimately are measuring slightly different things. Therefore, converting BI to density seems an unnecessary step and in my mind they should NOT be treated in the same way and we should not expect both parameters to show exactly the same characteristics. This ultimately does not change the main result of the paper w.r.t. the use of ΔBI and ΔMXD but would cut out some of the complexities and rambling text (in some sections) of the paper.

Also - the authors bounce between 1st difference transforms for response function analysis (RFA) and showing RCS processed chronologies for MXD but not BI. I do not see why they do not do the RFA using the non-1st differenced transformed chronologies. And why not also consider individual series data adaptive detrending options such as linear or Hegershoff functions. The RFA will be susceptible to biases in the mid-lower frequency domain and 1st differencing removes that information. Also, RCS could be used on the BI data, but the sub-fossil and living data would have to be divided into two groups to take into account the different "reflective" properties between these sample sub-sets.

Finally, the authors do not mention ring-width at all. I find this rather puzzling. Although I would agree that the inter-annual climate signal in TRW is weaker than MXD and BI, I am not sure this is the case at decadal to centennial scales - especially when replication is high (presumably the authors have 250 RW series). Esper et al. (2012) have hypothesised that there could be millennial scale biases in TRW versus MXD, but that hypothesis was only generated from the N-SCAN data and has not yet been tested using any other data-set yet. So - if the problem ultimately with BI is in the mid-longer time-scales, surely this can be partly tested by comparison to TRW data as well. This seems to be a missed opportunity.

Ultimately, this paper should be accepted after appropriate revision.

However, the authors need to better rationalise why the raw BI data needs to be transformed to a proxy of density. This seems a needless step in my mind and just makes the whole paper more complex than it needs to be.

CP specific questions

1. Does the paper address relevant scientific questions within the scope of CP?

Yes

2. Does the paper present novel concepts, ideas, tools, or data?

Yes

3. Are substantial conclusions reached?

Yes - but conversion of BI to a proxy of MXD seems an irrelevant step.

4. Are the scientific methods and assumptions valid and clearly outlined?

Mostly - but again - I think conversation of BI to a proxy of MXD seems an irrelevant step.

5. Are the results sufficient to support the interpretations and conclusions?

Focussing on ΔBI and ΔMXD - yes.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

No - see my detail comments below.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes

8. Does the title clearly reflect the contents of the paper?

Yes

9. Does the abstract provide a concise and complete summary?

Yes - but I believe changes will need to be made w.r.t. clarification of how BI can be measured from a grey scale image and why BI data needs to be transformed to a density proxy in the first place.

10. Is the overall presentation well structured and clear?

Yes

11. Is the language fluent and precise?

Mostly - have made some minor successions below.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Mostly - although possibly some confusion between g/cm^3 and g/dm^3 and the 0 - 255 scale for intensity. See comments below.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

See detailed comments below.

14. Are the number and quality of references appropriate?

Yes

15. Is the amount and quality of supplementary material appropriate?

na

Detailed Comments

P. 5228, Line 13: Grey scale? Surely you mean Blue scale?

P. 5228, Line 25: change "e.g." to "specifically" and place a "(" before Schweingruber.

P. 5229, Lines 15-20: Another reason for inverting BI is that one could use the same single series data adaptive detrending methods for both MXD and BI.

P. 5230, line 8: Can the authors clarify why they used the mean density of the earlywood and not the minimum density value? Was this option tested?

P. 5230, line 17: Reword to, "....are favourable and result in dense/dark..."

P. 5230, line 20: How can blue light be measured from a grey scale image? Surely you need to start with a full colour image? Actually, this is a potentially important observation. I had a quick play in Photoshop and once a figure has been converted to grey-scale, NO COLOUR information can be gleaned from that resulting image. Is it possible the authors are not measuring blue intensity, but rather grey intensity?

P. 5230, line 26: Delete "the" before "similar"

P. 5230, line 28: Can "wood in a volume" be better worded?

P. 5231, line 1: Why proxies for density? Why can't BI be used as a proxy of lignin content. In fact, this is my one major discussion point about the whole of this paper (see general comment above). I do not see any advantage in viewing BI as a proxy of MXD. Why is the transform needed in the first place? Why not simply use BI data as a measure of latewood lignin content which is controlled by summer temperatures.

P. 5231, line 6: w.r.t. Figure 2. I am pretty sure light intensity scales are from 0-255 - not 256.

P. 5231, line 7: w.r.t. " and consequently if a direct comparison between MXD and Blmax is going to be made, Blmax must also be transformed into density " WHY? Again - I do not see why these two TR parameters cannot be treated independently and the resultant reconstructions from them compared. Comment also relevant for line 21.

P. 5232, line 11: Not sure "finest" is an appropriate word to use. How about best calibrated? or even longest? I always thought Tornestrask was the gold standard - why was this site chronology not used as well?

P. 5232, line 17: Maybe include a reference for N-SCAND in figure 3 caption. Also does not N-SCAND utilise sub-fossil data from other sites as well?

P. 5233, line 7: 250 dated samples but only 140 used. Could the authors please clarify why only a sub-set of the samples were used? Was this simply related to cost - if so, you must say it.

P. 5233, line 26: reword to, "X-Ray analysis, WERE sanded with increasingly finer grit sandpaper, with 600-grit paper FOR the final round."

P. 5234, line 12: See earlier comments. I do not see the need to calibrate BI to MXD. Calibration to IT8 colour card already made (line 6). The BI data should therefore be simply a measure of light reflectance intensity on a 0-255 scale. I have never used WinDendro, so I might be missing something here.
change "like with" to "as with".

P. 5235, line 15: Consider re-wording "response analysis" to "response function analysis". This is the more standard terminology.

P. 5235, lines 21-23: The 1st difference transform is all well and good, but response function analysis will help partly evaluate the difference in the mid and low frequency domains if non-transformed chronology versions are used as well. At the very least, I would expect analysis using 1st differenced transforms, the RCS version (non transformed) and possibly even the use of so-called standard chronologies where regression, Hegershoff or [stiff] spline functions have been used for detrending.

P. 5236, line 4: Please define better what you mean by residuals. I understand it is the difference between two chronology series, but with the use of residuals in regression, and so called residual chronologies, the terminology might be a little confusing.

P. 5236, line 14-16: Why did blue light levels have to be adjusted? This again all comes down to the issue of transforming BI to MXD. I really don't see why this needs to be done and when I see word like "adjustment" in this context, I hear alarm bells. Please clarify.

P. 5236, line 16 onwards: All chronologies were compared and analysed raw. Is this why 1st differenced transforms were needed for the RFA? If so, why not at least undertake individual series detrending approaches? The authors then say that RCS is used on the MXD data but not on the BI data. Again - this is all rather confusing. Are the chronologies in Figure 10 raw means and not RCS detrended versions. This all needs further clarification.

P. 5236, line 26: There is one possible problem with Figure 5 and that is the images are presented using the full light spectrum. The methods is ONLY interested in the blue light reflectance, so why not filter the images in figure 5 to show only the blue part of the spectrum. My gut feeling is that the colour differences seen with the full spectrum colour figure will change considerably when filtered to blue only.

P. 5237, line 13: I am not sure what the authors mean by a "rational climate signal". Please re-word.

P. 5237, line 18: It looks to me as if it is ONLY MXD which shows an $EPS < 0.85$ around 1600. BI is fine.

P. 5237, line 23: Not fully clear. Can the authors clarify how the data are generated for figure 7. The EW and LW data are summed? Is that correct? Is that why the BI y-axis is not in the 0-255 range? Sorry, but this is all a little confusing. Please clarify. Why the * after BLI?

P. 5238, line 9: All the calibration r^2 values are using 1st differenced transforms - right? Can this be clarified in the caption. Ultimately, I think all the BI, MXD and their Δ versions should be detrended in more standard data adaptive ways to ascertain the mid-frequency response.

P. 5238, line 25: "point" should be "points"

P. 5238, line 27: insert "the" before "same".

P. 5239, line 1-6: Would Figure 10 be relevant if the BI data were not calibrated to density values???? Ultimately, I am struggling with this figure as I do not see why the BI data need to be calibrated and assuming that the statistical properties will be exactly similar to MXD is wrong.

P. 5239, line 11-17: Is there a chance that there was some timber extraction from this region between the 16th and 17th centuries that could also be partly to blame for the low replication for this period?

P. 5239, line 19: Replace "negative" with "irrelevant"

P. 5241, line 20-23: It is not clear if the CRA is undertaken using data transformed to 1st differences. If so, the authors cannot really talk about climate response at time-scales longer than year-to-year.

P. 5242, line 1: Replace "boosted" with "improved"

P. 5242, line 3: section sub-heading title (use of "complement") is not really consistent with the paper title which uses "replacement". Consider changing to "replacement".

Please note that much of the text of section 4.2 is a rather rambling affair and overall needs tightening. Focusing just on BI data, without transforming them to density, might simplify much of the paper in this regard.

P. 5244, line 15: reword, "to drastically improve THE spatial distribution and replication in highly climate sensitive tree-ring chronologies and lead to higher confidence in LARGE-SCALE climate reconstructions "

Comments on Tables and Figures

Overall, captions could have much more detail.

Table 1: Please clarify if these results are from using 1st differenced versions of the data. If so, it would be interesting to see results using detrended data - STD and/or RCS

Figure 2: change "over" to "of". Intensity scale should be 0 -255.

Figure 3: I think N-SCAND covers a greater region??

Figure 4: Throughout the paper the authors change between g/cm³ and g/dm³. In A, I think it should be g/dm³. Why a ** after BLI? Why are the intensity scales ranging from 0 - 1200 and not 0 - 255. Having never worked with WinDendro, this seems a little unclear to me. Surely the calibration with the IT8 card allows the intensity data to be on a 0 -255 scale?

Figure 5: full visual light/colour is perhaps not relevant??? Why not filter to show blue part of the spectrum.

Figure 6: State that these are raw non-detrended chronologies.

Figure 7: Not fully clear why Y-axis scale is not from 0 - 255 - maybe because the ED and LD values have been summed??

Figure 8: These are RFA results from using 1st differenced transforms. If so, state this in caption, but also consider including results from detrended data (STD and/or RCS)

Figure 9: These are RCS chronologies - correct? Please state this in the caption.

Figure 10: Please clarify if these are raw non-detrended chronologies.

Figure 11: This again all comes down again to the calibration of BI to MXD - if this is not needed, then figure 11 is not necessary I think.