Editor’s comments

After the secound round of reviews, I'm pleased to tell you that your paper entitled "Multicentury mean summer temperature variations in the Southern Rhaetian Alps reconstructed from Larix decidua blue intensity data" has just been accepted for publication in C.Past.

Both reviewers are positive and think your paper interesting as it is based on the Blue Intensity, a new method able to document summer temperature changes.

Only minor revisions are now needed (see both rapports below).

Could you please answer carefully to each point and send me the improved version of your paper? I recommend to to take into account the reviewers suggestions to improve the discussion part (better discuss the limitations of your approach, discussion on extraction) ; I strongly agree with the reviewer 1 about the fact that the paper is referring too many times to materials that are not inside the paper.

Reply: *Thank you for your message. We are pleased that our work has been accepted for publication in Climate of the Past. Below, we provide our responses to all the comments made by the second-round reviewer.*
We have expanded the discussion on resin and extractive extraction, as well as the heartwood/sapwood transition. Additionally, we have reduced references to the Supplementary Material, although we believe that including as many results as possible provides a valuable data reference for future research. However, we now clarify in the text that establishing a standard protocol for BI on European larch is beyond the scope of this manuscript.
For these reasons, while the reported results are important for the manuscript and future readers, they are not directly focused on the main topic of the text but serve as fundamental steps to support the protocol used. Furthermore, including these results in the main text would detract from the primary narrative and reduce the overall focus, especially considering that some of these additions were specifically requested by an anonymous reviewer during the first round of revision.

Best regards,
dr. Riccardo Cerrato on behalf of all authors

Anonymous Reviewer #3 – report #1 comments

I critically read the paper of Cerrato et al., and I found the paper interesting, opening new possibilities in dendroclimatic reconstructions in the EU Alps based on European larch. It's true that many analyses tend to produce many graphs and possibilities of interesting deepening into data, however I personally do not like the supplementary material philosophy, and in my opinion the paper is referring too many times to materials that are not inside the paper. I understand this is a trending way, also supported by the journals.
Reply: *We thank the Reviewer for the kind comment. Regarding the amount of data included in the Supplementary Materials, as the Reviewer noted, "many analyses tend to produce many graphs and possibilities of interesting deepening into data." This is particularly true for studies that aim to apply a relatively new method to a new species and observe its response. However, we believe that including all the analyses in the main manuscript would make it excessively long and less focused. Moreover, as Dr. Björklund noted in one of his comments, we have made an effort to be as transparent as possible, which may occasionally come across as tedious. Nonetheless, we consider this transparency essential, especially because more studies on the BI of European larch are needed. Reporting as many results as possible provides a valuable data reference for future research, even though establishing a standard protocol for BI on European larch is beyond the scope of this manuscript.*

That said, we have made an effort to limit and reduce references to the supplementary materials throughout the text wherever possible.

Besides this consideration, I have some minor indications to provide to the Authors:

1) A network of 3 site chronology at maximum distance of approximately 8 km, lead to a reconstruction of climate variability over thousands of km around. Sites were selected along a modest altitudinal gradient, line94. Before passing to PC1, did you check if you had different climate-BI responses separately at the three sites? Please better discuss.
Reply: *Yes, we checked, and the results are reported in Figure S6 of the Supplementary Material. As shown, all three sites exhibit a positive response to June, July, and August mean temperatures, with the strongest correlations observed with June–August (JJA) mean temperature. In two out of three cases, the correlation between PC1 and JJA mean temperature is higher than that observed for the individual site chronologies (i.e., Pearson's r increases for BARC by 0.09, and for PALP by 0.16). However, when comparing the correlations of ANBO with JJA mean temperature to those of PC1, the latter is slightly lower by 0.09.*

We have updated the discussion to reference this analysis. The revised first sentence of paragraph 5.1 now reads: "The correlation analyses highlighted the strong influence of the summer (i.e., June to August) monthly mean temperature on the individual site chronologies and especially on the PC1 (ANBO+BARC+PALP) chronology (Fig. 3a)."

Regarding the observation that chronologies from three sites located within a few kilometres in the Alps show a consistent positive response to regional-scale temperature variations, this is an expected result. This consistency is attributed to: i) the spatially homogeneous behavior of temperature; and ii) similar findings for other species in different parts of the world (e.g., Zheng et al., 2023; Seftigen et al., 2020; Heeter et al., 2019; Wilson et al., 2017).

Heeter, K. J., Harley, G. L., Van De Gevel, S. L., and White, P. B.: Blue intensity as a temperature proxy in the eastern United States: A pilot study from a southern disjunct population of Picea rubens (Sarg.), Dendrochronologia, 55, 105–109, https://doi.org/10.1016/j.dendro.2019.04.010, 2019.

Seftigen, K., Fuentes, M., Ljungqvist, F. C., and Björklund, J.: Using Blue Intensity from drought-sensitive Pinus sylvestris in Fennoscandia to improve reconstruction of past hydroclimate variability, Clim. Dyn., 55, 579–594, https://doi.org/10.1007/s00382-020-05287-2, 2020.

Wilson, R., Wilson, D., Rydval, M., Crone, A., Büntgen, U., Clark, S., Ehmer, J., Forbes, E., Fuentes, M., Gunnarson, B. E., Linderholm, H. W., Nicolussi, K., Wood, C. V., and Mills, C.: Facilitating tree-ring dating of historic conifer timbers using Blue Intensity, J. Archaeol. Sci., 78, 99–111, https://doi.org/10.1016/j.jas.2016.11.011, 2017.

Zheng, Y., Shen, H., Abernethy, R., and Wilson, R.: Experiments of the efficacy of tree ring blue intensity as a climate proxy in central and western China, Biogeosciences, 20, 3481–3490, https://doi.org/10.5194/bg-20-3481-2023, 2023.

2) Working on PC1 then of course cut-off parts of the different climatic influences at the site level. Fig. 3: I see colored bars (raw, low and high –'frequency domain' could be written somewhere), I see here and there the gray bars, but what are the white bars depicting?

Reply: *We agree with Reviewer #3 that the use of PCA reduces the ability to distinguish the specific climatic influences at each site level. However, as noted by the Reviewer #3 in the previous comment, the sites are geographically close to one another. Therefore, since they are all sensitive to the same climatic period (i.e., JJA), the primary differences we can hypothesize between the sites are: i) a potential altitudinal gradient that influences the response of the trees at the different stands. It should be noted, however, that this gradient is limited, as also mentioned by Reviewer #3 in the previous comment; and ii) the effect of outbreaks of Zeiraphera diniana Gn., which affect European larch in the European Alps in a non-synchronous manner (Bjørnstad et al., 2002). Furthermore, the impact of defoliation due to these outbreaks is altitude-dependent, affecting the three sites differently (Cerrato et al., 2019). Thus, the application of PCA likely retains, in the first principal component, the factor that most strongly influences the dataset's variance. Based on our results, this is most likely JJA temperature.*

Regarding the white bars, they represent the correlations that does not reach the 0.05 level of significance.

Bjørnstad, O. N., Peltonen, M., Liebhold, A. M., and Baltensweiler, W.: Waves of larch budmoth outbreaks in the European alps, Science, 298, 1020–1023, https://doi.org/10.1126/science.1075182, 2002.

Cerrato, R., Cherubini, P., Büntgen, U., Coppola, A., Salvatore, M. C., and Baroni, C.: Tree-ring-based reconstruction of larch budmoth outbreaks in the Central Italian Alps since 1774 CE, iForest - Biogeosciences For., 12, 289–296, https://doi.org/10.3832/ifor2533-012, 2019.

3) Please better discuss the discoloration issue. As far I can see, the strong declining trend in the sapwood corresponds to a positive trend in the recent period for DBI that perfectly fits with the recent period of temperature rise. It is true that other authors found DBI effective, but since this is a climatic reconstruction, I would have personally excluded this latter period from the model (and also from the sensitivity analysis) to avoid the possible inclusion of biases. If the same trees will be sampled in 20-30 years, the areas now in sapwood will be included in the heartwood: would the nice positive trend disappear? I am not asking to go back to see how the reconstruction is affected without the most recent period, however the Authors should better discuss what limitation could face the proposed reconstruction.

***Reply**: We agree with the Reviewer #3's comment. Heartwood/sapwood transition could be an issue but currently it is impossible to know what will happen in the future. However, we account for this in the revised version of the manuscript by adding a paragraph in the section 5.2. Now the manuscript reads as follows: "Regarding the recent observed trend, it is important to note that it coincides with a period characterized by the heartwood/sapwood transition in most of the samples used. Although the use of the DBI mitigates the discoloration issue, the notably good agreement between the BI and meteorological data may still be influenced by these conditions. However, it is currently not possible to determine whether, in the future, when the current sapwood will become part of the heartwood, the strong correspondence between the proposed proxy and the meteorological data will persist.".*

4) Especially because the Authors propose reconstructions based on different combinations of site chronologies (up to including only one chronology, ANBO; Fig. 5), I strongly recommend to include the sensitivity analysis for each site chronology separately.

***Reply**: The analysis has been performed and reported in the Supplementary material as Figure S6. The motivation that drove this choice is based on the evidence that the PC1 resulted to be more sensitive to climate in two out of three cases (BARC and PALP). This is probably due to the fact that BARC and PALP are located at lower altitudes and thus probably more prone to larch budmoth outbreaks. The higher number of outbreaks and the higher severity could partially hamper the climatic signal inside these chronologies (even if procedures to try to attenuate this effect has been applied, as described in Materials and Methods section). However, albeit the LBM fingerprint, the chronologies of these valley still retain a significant climatic signal (alpha = 0.99). Thus, as explained in a previous reply, we decide to focus the narration in the manuscript on the more important results, but to be transparent with other researcher, we decide to include as many results as possible in the Supplementary Material providing a valuable data reference for future research*

Dr. Björklund Jesper's comments

This study expands the research using Blue Intensity to a new species, Larix decidua, growing in the Rhaetian Alps. New evidence from BI is always interesting, and new data on past climate are useful to consolidate current understanding of the climate system and climate change or if it challenges conventional understanding. The study is pleasant to read and uses for the most part standard protocol methods. Considering that it is a study using BI to explore climate, there may be a shortage of protocol experimentation, because BI is not state-of-the-art but an affordable shortcut to comparable information. I specifically think of the lack of resin extraction of the samples, which may or may not be influential on the conclusions. That said, it is a nice study that only needs some minor revisions in my view. Below there are some comments that might be useful in this process.

***Reply**: We thanks Dr. Björklund for his kind comments. We acknowledge the limited protocol experimentation in the present study. However, to our knowledge, this is one of the first studies to use European larch BI for climatic reconstruction purposes. We believe it was important to demonstrate that BI performs well with this species in the European Alps, as this could encourage further research on European larch. We agree that more detailed methodological studies are necessary to fine-tune the protocol, obtain cleaner data, and achieve more accurate reconstructions from European larches.*

L67-70 This sentence is too dense, split up in two or three sentences. The terms “spectral analysis” and “nonlinearity” are used in ways that do not make them easy to understand for a reader not intimately familiar with the cited literature.

***Reply**: We followed the suggestion of Dr. Björklund and modify the text as follows: “In fact, BI data, derived from the reflected-light spectral analysis of tree-ring samples, provide climatic information that is virtually identical to that acquired through MXD. The BI and MXD data reflect cell wall dimension rather than the TRW or cell wall compounds, and thus show strong similarities in terms of temperature correlation strength and autocorrelation (Björklund et al., 2021; Ljungqvist et al., 2020).*”

L120-130 The description of the method here is exemplary, but please consider the terminology for BI proposed in Björklund et al 2024. The problem highlighted in this paper is that over the past decade(s), LWBI and EWBI have been synonymous with both inversion states. For future research it would be simpler if BI was dedicated for data positively correlated with density, and that Blue Reflectance (BR), i.e., the raw state, would be dedicated to the non-inverted state, which is negatively correlated with density.

***Reply**: We thank Dr. Björklund for highlighting this inconsistency. At the time of the initial submission, their work had not yet been published. We are pleased to revise the text in this paragraph to align with the proposed standard terminology. The revised text now reads as follows: “In this study, considering that cores with a diameter of 5.15 mm were involved, a frame width of 100 pixels (equal to 0.8 mm at 3200 dpi) was used to measure the minimum latewood Blue Reflectance (LWBR) and maximum earlywood Blue Reflectance (EWBR) values. Frame depths of 50 and 200 pixels (corresponding to 0.4 mm and 1.6 mm at 3200 dpi, respectively) were determined to be optimal compromises between the average wood structure width and measurement requirements. These frame depths were subsequently employed for measuring the*

LWBR and EWBR, respectively. The offsets of the frame were set at 5 and –2 pixels for the LWBR and EWBR, respectively (Fig. S1 in the Supplementary Material). For the LWBR measurements, we considered the mean values of the 25 % of the darkest pixels in the frame, whereas all the pixels within the frame were considered for the EWBRI measurements (Cerrato et al., 2023). For easier comparison with climate data, BR values were inverted following standard procedures to derive BI values (Rydval et al., 2014; Wilson et al., 2014), consistent with the '2024 BI standard terminology' (Björklund et al., 2024)."

L130 perhaps replace "devise different solutions" with "require attention"
***Reply**: We accepted the proposed changes.*

L135 Perhaps: Visual and statistical crossdating of ring width, from the core samples, ensured that all obtained BI-based values were also correctly crossdated
***Reply**: We accepted the suggestion modify the text according to it.*

L200-201 perhaps also check out Esper, J., Frank, D. C., Wilson, R. J., & Briffa, K. R. (2005). Effect of scaling and regression on reconstructed temperature amplitude for the past millennium. Geophysical Research Letters, 32(7). The regression deflates the variance, but scaling inflates the error.
***Reply**: We thank for the suggestion. We better explain the applied methodology that is a combination of regression and scaling. Text now reads as follows: "Then, the mean and the variance of the regressed DBI z-scores data were adjusted against the instrumental targets to avoid the typical loss of amplitude due to regression error (Carrer et al., 2023) and reducing the inflated error variance observed when only the scaling approach was applied (Esper et al., 2005).".*

L206-208 It is quite a large negative slope in the sapwood transition zone. Probably in part due to that ethanol extraction was omitted. It is very likely that EWBI and LWBI are affected by this transition, but is the effect completely neutralized in the delta parameter? This is difficult to say because the EWBI and LWBI have other calendar dated variances as well (climate, moths, stand dynamics..?). An initial experiment to test this could be to align all LWBI and EWBI on HW/SW transition date, and compare mean values before and after the transition. To facilitate the analysis further, the mean values of LWBI and EWBI can be set to zero in the HW zone by means of subtracting the HW mean from the timeseries. If the SW means of the LWBI and EWBI are the same, the effect is neutralized. However, since all HW/SW transitions occur at roughly the same years, this analysis will not completely neutralize the climate and other variances, so it will not be a perfect analysis. Note that I do not insist on such an analysis, but it could be informative as a test.
***Reply**: We followed the suggestion of Dr. Björklund and perform the proposed analysis on a single valley (ANBO). Preliminary results show that the creation of DBI only mitigate the effect of the HW/SW, thus specific designed scientific investigations are needed but are beyond the main aims of the present*

manuscript. This is now highlighted in the discussion also in reply to a following comment trying to specify that even if in the present study the resin extraction was not performed it does not mean that it is not necessary.

L230-231 seems like something is missing here, perhaps add an “and” correlates stronger with…? ..“and does not correlate SIGNIFICANTLY with”.. ?
***Reply**: We modified the text as suggested.*

L251 time series?
***Reply**: We replaced temporal series with the suggested time series.*

L252 DBI?
***Reply**: We modify the text as suggested.*

Figure 3 A moving window correlation analysis with smoothed data may not be so statistically sound. At least it will be very difficult to reach significance after the loss of degrees of freedom is considered (even when the correlation is close to 1). I would remove this from panel b and state that such an analysis will not be so informative, it is enough with the visual comparison in panel c.
***Reply**: We followed the Dr. Björklund’s suggestion and modified the Figs. 3, S5, S6, and S7 accordingly. The text was modified between line 246 and 249 and now reads as follows: “The moving window analysis between PC1 (ANBO+BARC+PALP) and the JJA mean temperature revealed significant and stable correlation values at the 0.01 level when considering the raw data and high-frequency domain (Fig. 3b). Regarding the low-frequency domain, interpreting its significance is challenging, particularly given the loss of degrees of freedom caused by the applied smoothing function.”*

Table 2 I appreciate the effort for transparency but I do not think it is necessary to include the regression tests for high-and low frequency filtered data.
***Reply**: We partially agree with Dr. Björklund’s comment. Since the high-frequency has been discussed in the section 5, we decided to remove from the Table 2 only the analysis regarding the low-frequency domain, according to what was done for Fig. 3.*

L322-340 The discussion on extraction is necessary because this study makes a step away from common practice. I think the authors have a time series which is sound, but I would be surprised if it would not be

slightly improved if resin extraction was included (especially in the high frequency rbar, and perhaps in the low frequency as well). Abundant resin can be very local and might not even be comparable in earlywood and latewood.

Perhaps something like this is visible in ANBO EWBI around 1800 CE in Figure S3. Probably this is something else, but in principle, this type of erratic behaviour could perhaps be mitigated with resin extraction? The discussion concludes that DBI is effective in mitigating HW/SW differences, which is fine. But surely the authors do not discourage from using resin extraction? Can a sentence or two be dedicated to this?

***Reply**: We agree with Dr. Björklund and added the following paragraph at the end of the section: "Although the data collected for this study suggest that the creation and use of DBI significantly mitigate discoloration issues occurring at the heartwood/sapwood transitions in European larch samples, this does not imply that the removal of resins and extractives is unnecessary. Instead, it underscores the need for further analysis because no information is currently available on how resins and extractives affect the EWBI and LWBI timeseries derived from European larch samples. Additionally, resin abundance can vary greatly at a local scale and may not be comparable between earlywood and latewood, and therefore it influences the DBI time-series. To better understand the influence of resins and extractives on BI – both in heartwood, sapwood, and their transition zone – a specifically designed scientific investigation is required."*