Review bg-2019-63

The Li et al. paper presents a novel method to remove ontogenetic effects from tree-ring width series for the computation of a productivity index. Their advocate the need of a new method based on an existing sampling biais in classical detrending methodology caused by an over-representation of old trees in site-specific master chronologies at any given period. According to the authors this biais contaminate climate-growth correlations computed using detrended tree-ring series, especially those computed at site level. They provide a nice north-hemispheric analysis based on the *Picea* and *Pinus* tree-ring series extracted from the ITRDB.

GENERAL COMMENTS

This well written paper falls in the scope of the journal. I found pleasant to discover a new born methodology to handle tree-ring series in order to perform unbiased climate-growth analyses. However, even if this novel method looks appealing from a first perspective, it requires quite a substantial amount of parameters to be applied, compared to conventional methods (L307). I am not convinced that the application of this novel method is straightforward, despite what the authors say (cf. L307), particularly for other less common tree species or genii.

The high positive correlation between P* and RWI makes me think again that the classical detrending applied in dendrochronology would still be easier and simpler to use. To really advocate in favor of the use of the herein presented method, it would require to compare the climate-growth analyses presented in Figs 6 and 7 with similar climate-growth analyses computed with 'traditionally detrended RWI.

I have some major concerns that I hereafter discuss:

- Study area and environmental gradient

I am skeptical regarding the gradient over which they perform their analysis. The authors take two conifer tree genus from the northern hemisphere as examples, *Pinus* and *Picea* species. The tree populations include sites and pine and spruce species from the mediterranean, temperate and boreal contexts that respond quite differently to climate (e.g. *Pinus laricio* spp. vs *Pinus sylvestris*). For example, it is well known that growth responses to summer temperature move from being negative in warm and dry contexts (mediterranean biome) to positive in cold and wet environment (boreal biome). So I would like to see the analyses, especially the climate-growth analyses, ran for each genus over the three above-mentionned biomes.

This would be even more needed as the authors assume that "bioclimatic parameters have a consistent impact on growth" (cf. L212-213). This assumption is wrong, and it is even more wrong to assume that this is true regardless of sampling period or targeted tree species (cf. L213). Growth responses to climate are by nature not constant through time and space for a given tree species, due to non-linear interactions between environmental factors (e.g. climate and soil). The authors mention themselves (cf. L 220-221) that $mGDD_5$ has a non-linear impact on P*.

- 'the sampling biais of age distribution'

The authors ground their study in correcting the under-representation of young trees in growth chronologies. Therefore I have hard times to understand why the authors remove all rings before the turning point in growth trends (i.e. all juvenile rings). By doing so, they also contribute to the sampling biais of age distribution in their site chronologies. To me it seems that this removal of juvenile growth rings is only done to allow the approximation done in Appendix C. In traditional detrending, juvenile growth rings are kept.

A question arises: What is the average number of years (juvenile rings) removed by this step? How does age distribution look like after this step compare to the original age distribution (Fig. 1b)?

- Simplification of equations

The authors are simplifying quite substantially the equations they present in Appendix C based on different assumptions. Would these assumptions be applicable to other tree species, e.g. broadleaves species? If not, the herein presented method couldn't be applied to some tree species, and therefore would be useless in an more isolated geographical context.

- Detrending of tree-ring series

The authors use a mean fonction to detrend raw tree-ring width series but this is not optimal to remove ontogenetic effect. A mean function will not change the shape of the series, therefore the declining growth trend will still be present after detrending…which makes detrending useless in this case. A modified negative exponential would be better, especially if the authors decide to only keep the decreasing part of the raw ring width series (cf. L148). Morevoer, I think that the mean detrending is mostly responsible for the fact P* is higher than RWI over the most recent period and lower in the earliest years (L242-243).
Why no power transformation has been performed before detrending TRW to stabilize the variance? Why no autocorrelation removal has been performed after detrending?
These two steps (not perforemd here) are usually performed to maximize the link between growth patterns and climate. The authors say themselves that P* was designed to reflect climate impacts on radial growth (L211). It would therefore be more optimal to compare it to properly standardized RWI (incl. power transformation, negative exponential detrending and autocorrelation removal).

- Computation of the $Hm_{(ICP)}$
Please provide a map of the location of the sites where the paired D and H measurements were taken and from which $Hm_{(ICP)}$ computation are derived. Are these sites representative, in term of stand characteristics and environmental gradient, of your study sites (those extracted from the ITRDB)? For me estimation of asymptotic maximum height should not be performed using the overall dataset, but should be performed along a climatic gradient or at least along an latitudinal gradient.

SPECIFIC COMMENTS

- Methodology
I have had hard time to understand at which stage the analyses move from a tree-level to a site-level. Some of the described analyses are hard to follow also because the authors seldom describe the statistical tools they used. One example of this lack of clarity is the 3.3. section. We don't know how the contribution of each tree to the site P* was computed.

L164- Which resolution does this dataset have?
Why using E-OBS when the WFDEI data look much more resolved temporally and spatially?

L195. Are you talking about standardized or raw annual diameter increments here?

- Statisitcal tools and methodology
Similarly I find that almost none of the methods presented described the statistical environment (program, packages, functions) in wihch analyses were performed.

Section 3.3. please provide the packages or statistical environment used for these analyses

L205. Please provide the statistical tools you used to standardize the raw tree-ring width series.

L262. The notion of first maximum has not be defined yet. Please define it or rather the 'ontogentic turning point' or 'peak in radial growth' you mention.

-References
I think that the authors are not enough supporting what they write by scientific references:
L101-102 Assumption 1: environmental effects presumed to influence the growth of all trees. Do you have a reference for this?

L51. Use a reference to support that tree growth is sensitive to environmental factors (even if we all know it).

L58-59 please use a reference for this.

L64. Please use a reference for this.

L68 Please use a reference for this

L77. Please use a reference for this

L171. Please use a reference for this statement. Who has defined the ‘effective carbon accumulation year’?

- Others comments

L148. Why would you remove earlier rings (youngest trees) if your main target is to correct for the sampling biais over old trees?

L 50. “because of their chronological accuracy”> please mention that this is achieved by applying the principles of crossdating.

L82. ‘we require a reliable method to process the large volume of tree-ring data’. I don’t agree with this statement. Numerous methods already exist for big data analyses (ANN, kNN, RF etc.) and these are already quite reliable.

L87-88. This seems logical to me. When you go sampling, no matter which tree you sample, the trees are older over 1970-2000 than over 1940-1969…Regardless of the structure of the stands…

L97-98. This statement can be argued. There is an endless discussion regarding the age vs size effect on growth trends. The discussion is not over. Please see XXX.
However, no one would argue with the fact that size is easier to measure and more accurately measured than age at a certain tree height.

L278-279. Conventional approaches are also based on equal contribution/weight of all tree to the master chronology…

L284. Add ‘positive response to alpha’

L290. Please compare the model $P^*= f(mGDD_5)$ with the model $P^*=f(mGDD_5 + mGDD_5^2)$, to prove that applying a parabolic regression is better than a simple linear regression.

Figure 3.
a- Larger differences between RWI and P* are observed at more productive sites. Do you have any explanation for this?
b- Please mention in your caption the change in x and y-axis scale across panels.

Figure 6.
a- I identify subgroups in the points clouds of some panels (ex. upper left panel on the extreme right of the cloud). Is this link to geography? Could you plot the sites from North America and Eurasia in different colors? It would be very interesting to provide us with the same analyses by biome (mediterranean, temperate, boreal). See my major comment about this.
b- I notice a higher variability in P* in the middle of the gradient in the middle panels of PPFD5. Do you have any explanation for this?

Figure 7.
a- I am surprised to see that the p value of the regression in panels CO^2 vs Pinus 1970-2000 and CO^2 vs Picea1970-2000 is significant. To me it seems that the linear regression does not present any slope… Do you have any explanation for this?