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Climate of the Past Discussions

Interactive comment on "A tree-ring perspective on temporal changes in the frequency and intensity of hydroclimatic extremes in the territory of the Czech Republic since 761 AD" *by* P. Dobrovolný et al.

Anonymous Referee #1

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Review: A tree-ring perspective on temporal changes in the frequency and intensity of hydroclimatic extremes in the territory of the Czech Republic since 761AD

Dobrovolny et al.

General Comments There is no doubt that studying the temporal distribution of extreme climatic events within the context of regional and global climate change is of huge importance.

Dobrovolny et al. attempt to address the issue of long term changes in hydroclimatic

extremes using a new long TRW chronology measured from Oak samples from the Czech Republic. Although the author's aspirations are admirable, the results and discussion fail to convince me that they have addressed the main aims of the paper. This in no way reflects upon the authors themselves, in fact some of the analyses were rather novel and I believe the methods are potentially interesting. However, to put it bluntly, the oak TRW data they use are simply not up to the task and are not responsive enough to hydroclimate. I cannot see how these data could lead to any confidence about the "trends" in hydroclimatic extreme in the CR.

Ultimately, the main question that must first be better addressed is what environmental factors are driving the extreme annual values in the oak TRW chronologies. With a calibrated signal of only 18-20% to precip/SPEI, one must not forget that 80% of the variability in the chronologies is explained by something else. If the authors plotted the chronology ensemble and the main hydroclimate parameters, how many extreme years (positive and negative) actually agree between the proxy and actual data – not many I would guess. If there is poor agreement between these variables, then what can really be concluded about long term temporal changes in hydroclimatic extremes. The current analysis is an analysis of oak growth extremes – but not all of the oak extreme years are hydroclimatically driven. Although hydroclimate might be the dominant factor, it still only explains 20% of the TRW variability. The authors themselves spend much of the discussion on ambiguities and poor agreement of their data with different archives and in fact I am pretty sure that the historical observations are likely the superior proxy archive to address these issues.

I am afraid, in its current form, I do not think this paper should be accepted for publication.

Much more is needed to try and model better the controlling factors of the inter-annual variability in Oak from this region. Perhaps relevant information could be gleaned from tree-growth models such as VSlite (or other models).

I assume the authors have also examined more secular scale changes in the TRW data, when appropriately detrended, and I wonder if these data would be simply be better at decadal and longer time-scales?

Perhaps consider more comparison to extremes in other TR records such as Brazdil (2002) and further afield records (including Oak and conifer records from Germany??) and possibly the gridded multi-proxy (but dominated by historical data?) precipitation/hydroclimate products of the Luterbacher group (e.g. Pauling etc).

I believe the authors should also read the recent paper by McCarroll et al. in The Holocene, "Measuring the skill of variance-scaled climate reconstructions and a test for the capture of extremes" which could provide an alternative method of assessing how well the TRW data actually represent extreme climate.

I am sorry for such a harsh review but I think the TRW data used in this paper are simply not fit for purpose to address the aims of the paper.

Detailed comments Page 3110, line 27: we are a long way from modelling high frequency hydroclimate. Consider removing this statement.

Page 3111, line 26: . . ..essential for. . ...

Page 3112, line 14: . . ..have, to date, been. . ..

Page 3113, text w.r.t. Fig 1a, b: It seems to me that there is an obvious NW / SE split to the CR data-set with about 150-200kms between the centres of both regions. Although I am sure there would be some similarities in hydroclimate between these regions, there surely would be some differences as well which could impact on the analyses. Did the authors consider splitting the country into two regions? Some compromise might need to be made w.r.t. replication and chronology expressed population signal quality, but it could refine the regional relevance of such records? This is just a suggestion and maybe has been addressed in other papers.

Page 3113, line 22: Delete "quite"

Page 3116, lines 14-21: It would be useful to plot the chronology ensemble with the precip/SPEI time-series with an associated table listing the 10 or 20 most extreme positive/negative years in the instrumental data and how many of them the TRW data actually capture (including intensity?). With r values of around 0.4, I would imagine that they proxy data do not capture many of these extreme events well. Also – for correlations, state what period analysis was undertaken over, and were the instrumental data high pass filtered in the same way as the TRW data.

Page 3117, line 17: Considering showing a STD or RCS version of the TRW chronology for better assessment of long term trends with inference of decadal long drought and pluvials – with caveats that the TRW chron actually matches well with the instrumental data.

Page 3121, line 18: . . ...still an open. . .. . .. NB. Discussion on response of both species has already been mentioned earlier. These few lines could be removed.

Page 3122, lines 12-21: The authors state exactly my concerns about the record here. For these reasons alone, even the authors surely can see that this TRW record is not fit for this specific purpose.

Page 3122, line 26: w.r.t. Buentgen (2011), was pollution not a factor in the recent period for Fir? Could this not also be a factor for Oak as well?

Page 3123, lines 17-28: One of the difficulties with Oak is its deep tap root to it is likely that they truly only respond to drought when ground water is in significant deficit. Therefore, winter precipitation and ground water re-charge is surely a factor in modulating tree growth response during the spring/summer period. Possibly worth more discussion on this issue as I am sure it is a crucial factor in the poor response of these trees to hydroclimate. As I said above, tree-growth modelling may help with this.

Page 3124: It is not clear to me why there is so much discussion on the 1540 drought. Pauling et al. clearly showed this to be an extreme year and the CR TRW oak data

show a lagged response in 1541. Overall, as authors say, this just highlights that again, these data are not good for such analyses.

Page 3124, lines 26-27: Not clear what is meant by ". . .also disclosed by the results in this contribution"???

Page 3125: Much of this discussion simply highlights the mixed signal nature of the oak TRW record and the authors are arguing themselves into a corner as to the utility of these data to assess extremes.

Interactive comment on Clim. Past Discuss., 11, 3109, 2015.