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Climate of the Past Discussions

Interactive comment on "Heterogeneous response of Siberian tree-ring and stable isotope proxies to the largest Common Era volcanic eruptions" *by* Olga V. Churakova et al.

Anonymous Referee #1

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Review of Churakova et al. "Heterogeneous response of Siberian tree-ring and stable isotope proxies to the largest Common Era volcanic eruptions" for Climate of the Past Discussions

This study takes measurements of multiple tree-ring parameters (including stable isotopes and cell wall thickness) for larch chronologies at 3 sites in N and S Siberia. The measurements are mostly focussed on 20-year periods centred on 6 major volcanic eruptions, as well as during the post-1950 period for comparison with instrumental climate observations. The experimental design is good as it makes sense to first focus these measurements (some of which are expensive to do in both time and money) on

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periods where we might a priori expect a strong climatic signal to reveal interesting insights into the multi-variate response across tree ring variables and infer multi-variate changes in climate (temperature, precipitaiton, humidity, sunshine). The findings could then help guide future resources to the sites and parameters that would add most value, as well as learning about the complex responses to volcanic erutpions that likely go beyond the summertime cooling that has already been established.

However, there are problems with the data analysis and presentation of results which severely limit the value of the paper as it currently stands.

1. The lack of superposed epoch analysis.

No superposed epoch analysis is presented (despite the mention of it on line 305) to establish a best estimate of the "typical" response to a large eruption, instead the volanic epochs are only considered individually. The statistical testing on the individual events is therefore of limited statistical power because the size of the volcanic signal has to be equivalent to the 5 (or 10) percentile of the estimated variability of the time-series for it to have even a 50% chance of being identified as significant. [Also, Fig. S1 where these tests are reported is illegible at the size and resolution provided – I'm surprised this wasn't picked up by the journal technical staff for correction prior to starting the review process.]

It is good to see the responses to individual events – and I like the overall design of Fig. 4 for this purpose – but it is also necessary to see the composite behaviour because of the additional statistical power that compositing (superposing) the events will bring and the different statistical testing that would then be applied. While the purpose may be to illustrate the varied behaviour after each event, it is first necessary to see the aggregate behaviour. Once this is established, the heterogeneity can be considered.

2. There is no discrimination between (i) different responses to each event and (ii) "random" sampling variability that will make each case appear different anyway.

If the purpose is indeed to demonstrate the heterogeneous responses between events (instead of, or in addition to, the differences between sites and between tree-ring/climate parameters) then the analysis needs to consider how to discriminate truly different responses from sampling variability. Different values will occur due to internal climate/weather variability as well as error variance in the data. The statistical tests compare each value to the timeseries variability to see if they are significantly different from zero – but what is needed is to see if they are significantly different from either each other or from the composite mean (see point 1). That would demonstrate the heterogeneity between events is real and not just down to sampling variablility.

3. Errors and limitations in the presentation and discussion of the results in Fig. 4.

The results are hard to follow and their description/discussion is not presented concisely. In part the structure leads to duplication, e.g. the process-based description of stable isotopes is split between 2.6 before the results and 4.1.2/3 after the results but the results don't really confirm or alter our understanding of these processes so it seems unecessary to split this into the two sections (that would be appropriate if it was to say what our understanding was before this study and then to present the updated understanding after it, but that isn't really the case here). I wonder if the sample sizes are too small for the CWT and isotopes to really be confident in the findings – can anything further be done to show whether the sample sizes are adequate? Are there enough samples to calculate RBAR and EPS as a measure of the common signal and chronology confidence?

These limitations are compounded by errors in the description of the results in Fig. 4. Here are lines 439-449 with my comments in [CAPS]:

——— "Therefore, CE 536 was extremely humid in YAK and TAY, as well as 541 and 542 [NOTHING SIGNIFICANT IN FIG 4 IN 542] in TAY and ALT. CE 1258 was dry in YAK and ALT, while drier than normal conditions occurred in 1259 for all studied sites. CE 1641 [1641 ISN'T EVEN IN FIG 4] was dry in TAY; 1642 in YAK [NO, 1643!] and ALT

[NOT ACCORDING TO FIG 4]. A rather wet summer was in TAY during 1815 [THIS YEAR IS NOT IN THE FIGURE!] and 1816 years [NOTHING SIGNIFICANT HERE]. CE 1991 [NOT IN THE FIGURE!] was wet in YAK, 1992 in ALT [NO ALT DOESN'T APPEAR WET IN 1995 IN FIG 4] followed by a dry summer in 1993 [NOT ACCORDING TO FIG 4!] (Fig. 4).

3.3.3. Sunshine duration proxies Instrumental measurements of sunshine duration (Table 2) in YAK and ALT during the recent period showed a significant link with δ18O cellulose. Based on this we conclude that sunshine duration decreased significantly in 536, 541, 542, 1258 and 1259 in YAK, and 536 in ALT. Conversely, summer 1991 in YAK was very sunny [NOT ACCORDING TO FIG 4! BUT 1993 in ALT WAS, WHY NOT MENTION THIS?] (Fig. 4)." ———

For Pinatubo, the instrumental data could be used to confirm the multi-variate explanations, e.g. do they confirm the statements you give about 1991-1993 on lines 442-443, 449 and 517?

Some description of Fig. 3 looks incorrect too, e.g. identification of months with significant correlations:

L370: "with the exception of TAY for the latter" - and CWT too? L381: "May" - looks like "April"? L391: "March" - looks like "April" for CWT?

Specific comments and questions:

L58: "triggered by" implies a causal link, which hasn't been established by the analysis here.

L85: Briffa –> Briffa et al.

L120: Why do you expect increased humidity?

L146: Why 6 eruptions and not more to increase the sample size? Why these particular 6? L180-181 says these are not the top 6, why not choose the top 6 in terms of

stratospheric sulphur injection?

L190: virtual –> virtually

L191, 238-239: give the actual sample size for each site, period and eruption in a table (perhaps adding to table 1) rather than just "at least 4"

L193: "perfectly" isn't needed

L229: did you consider using CWT averaged only over the latewood, so it is closer to MXD?

L304-305: It doesn't look like you subtracted the mean prior to the eruption – this is standard for SEA.

L306, 345: 15 years? Only 10 are shown in fig 2

L325: -4.4 sigma is NOT less pronounced than -1.8 sigma

L327: -3.9 sigma CWT for YAK is not visible in fig 2, which eruption do you mean?

L334: no this is not the "response" to the volcanoes. This misconception is a repeated weakness of the manuscript: the complexity show may be due to a fairly stable response to the forcing, but with local climate variability and errors in the chronologies (sample size is small for some parameters) superimposed to give individual realisations that differ from case to case.

L340: Fig. 2 has a gap in the CWT series for TAY in 536. Explain why and what this means. Frost rings?

L362 onwards: Climate analysis comments:

You only analyse correlations between individual months but later you suggest Jun-Aug temperature response for some parameters. Showing correlations for a 2 or 3 month seasonal mean might be useful, it might give a stronger correlation because intra-seasoal variability would be reduced.

Did you detrend the climate data before calculating correlations?

You need to consider cross-correlations between the climate variables, since this could explain why some tree-ring parameters are correlated with multiple climate variables. e.g. L389, could the negative correlations with MJJ precip arise because precip in these months is negatively correlated with temperature at ALT?

L372: Fig. 3 axis labelling is too small and blurry.

L410: CWT label is missing for YAK.

L468: "strong" seems, err, too strong, when correlations are only 0.3 to 0.5. Also make clear it is SUMMER temperature.

L483-484: First time that the NUMBER of cells is mentioned, similarly for frost rings.

L509: what is "gs"?

L510: at TWO sites

L514: positive correlation with VPD is signficant at only ONE site

L525: or explained by a delayed/sustained climate response or by the aerosol forcing persisting and perhaps taking some time to reach the highest latitudes?

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