Dear Prof. Luterbacher,

We thank you and the Reviewers for your time and valuable comments reviewing our manuscript. We believe that, thank to the constructive comments, our newly revised manuscript is significantly improved. Below, you can find the detailed point-by-point answers to the Reviewers' comments. To facilitate your assessment, we marked our responses in bold and highlight changes in the manuscript.

Response to the Reviewer 1

Reviewer:

1) I am still a little bit worried about the robustness of these resulting conclusions based on a small network composed of only three sites.

2) In addition, chronology of different proxies and their climate response have uncertainties.

3) A related discussion on these is helpful and should be indicated in abstract and conclusion sections.

Answer:

1) We agree with the Reviewer that a network with three sites is not complete, but it is the largest network existing to date. Therefore, while accepting possible limitations, we would like to underline some of the key strengths that this paper has with respect to what we knew previously:

i) The three sites cover a vast area of Siberia (being 1500 and 3400 km apart), including northern latitudes and high elevations. As such, they cover a rather vast territory within the boreal ecosystem. The fact that our results show that tree-ring responses to volcanic events are not consistent over time and space should help to raise questions regarding the validity of a unique volcanic forcing in climatic and dynamic global vegetation models;

ii) The use of a multi-proxy approach covering stratospheric volcanic eruptions over the past 1500 years remains an important effort that can only be produced at locations for which such chronologies exist. Considering the large amount of resources invested, we believe that it is still more appropriate to base our study on a set of "homogeneous sites" (boreal environment, genus *Larix*) than expanding to other regions or species.

iii) The multi-proxy approach proposed in a context of responses to several volcanic events is unprecedented and provides valuable results.

2) It is true that tree-ring proxies are characterized by "uncertainties". These are usually quantified via the assessment of the common signal and from the strength of the correlation with the climatic and/or environmental signal. In addition, there can be some additional uncertainties due to the suboptimal references of the climatic data if these are not collected at the site, as is the case in our study. This information was only partially supplied in previous versions of the submission (see results section). To make this more transparent we added the following to the text: "Mean inter-series correlation (RBAR) and EPS values of stable isotope chronologies were calculated for the period 1950-2000, for which individual trees were analyzed separately. We show the common signal with an EPS > 0.85 and series have RBAR ranging between 0.59 and 0.87. Before 1950, we used pooled material only. For all other

tree-ring parameters, the EPS exceeds the threshold of 0.85, and RBAR values range from 0.63 to 0.94." (P. 12, L. 224-228, revised version).
Moreover, we added the following text explaining possible uncertainties in regards of weather station data (see P. 7, L. 156-161) "Due to the remote localization of our study sites, we used meteorological data from monitored weather stations located at distances ranging from 50-200 km from the sampling sites. Temperature data from these weather stations are significantly correlated (r>0.91; p<0.05) with gridded data (http://climexp.knmi.nl). However, poor correlation is found with precipitation data (r<0.45; p<0.05), most likely as a result representing local effects (Churakova (Sidorova) et al., 2016)."

Each proxy carries a specific, seasonal information, one may thus expect that the responses can be different among proxies and events, also due to delay in responses related to the location and seasonality of the eruption on the one hand and the time at which environmental changes become tangible at the study sites. Our previous work in Russian Altai (see Sidorova et al., 2012 Climate Dynamic DOI 10.1007/s00382-010-0989-6) already highlighted that different proxies can carry different signals. This has been explained by the influence of different climate parameters in temperature-limited environment, by the different seasonality and by the different response patterns to temperature and precipitation changes in the permafrost zone.
***Reference:* Sidorova OV, Saurer, M, Myglan VS, Eichler A, Schwikowski M, Kirdyanov AV, Bryukhanova MV, Gerasimova OV, Kalugin IA, Daryin AV, Siegwolf RTW A multi-proxy approach for revealing recent climatic changes in the Russian Altai Clim Dyn DOI 10.1007/s00382-010-0989-6**

3) We have added more explanation on these aspects in the discussion (P. 3 L. 55-56; L.65-67, P. 12, L. 224-228, P. 7, L. 156-161). Similarly, the conclusion section was revised accordingly to the Reviewer's suggestions (P.31-32 L. 614-627).

Reviewer: The chosen six volcanic events do not represent the largest volcanic eruptions. Why did not they choose the top 6 in terms of stratospheric sulphur injection?
Answer: Indeed the injection of sulfur into the stratosphere by explosive volcanic eruptions is a very important cause of significant climate variability. The selection of volcanic events was based on Table 2 in Toohey and Sigl (2017), who listed the top 20 eruptions of the past 2000 years in terms of volcanic stratospheric sulfur injection (VSSI). Our sub-criteria also include events that were well reported in tree-ring proxies (to exclude events that might have not affected the growing season and thus plant growth) and a certain temporal distribution over the last 1500 years. Therefore, we added more detailed information about the criteria used with the relative references (P. 8, L. 179- 182): "Identification of the events analyzed in this study was based on volcanic aerosols deposited in ice core records (Zielinski 1994; Robock 2000), and more precisely on Toohey and Sigl (2017) where the authors listed the top 20 eruptions from the past 2000 years in terms of volcanic stratospheric sulfur injection (VSSI) in a new ice core-based volcanic forcing reconstruction".
Our sub-criteria is based on literature review of reconstructed VSSI and events well reported in tree-ring proxies that may have had a noticeable impact on the forest ecosystems from high-latitude and high-altitude regions (Briffa et al., 1998; D'Arrigo et al., 2001; Churakova (Sidorova) et al., 2016; Büntgen et al., 2016; Gennaretti et al., 2017; Helama et al., 2018). Therefore, based on our previously published TRW and newly developed MXD, CWT,

$\delta^{13}C$ and $\delta^{18}O$ in tree-ring cellulose chronologies, we selected the years, characterized by strong volcanic eruptions with far-reaching climatic effect, namely the years CE 535, 540, 1257, 1640, 1815, and 1991. Therefore, to investigate climatic impacts of these eruptions in Siberian regions, we selected periods around (± 10 years): CE 525-545, 1247-1267, 1630-1650, 1805-1825, and 1950-2000, with the latter being used to calibrate tree-ring proxy versus available climate data (Table 2). (P. 9, L. 183-193)

Response to the Reviewer 2

Reviewer: Review of revised manuscript by Churakova et al. "Siberian tree-ring and stable isotope proxies as indicators of temperature and moisture changes after major stratospheric volcanic eruptions" (revised title) for Climate of the Past Discussions

As with my original review, I am supportive of this study because of its value in bringing together multiple tree-parameters and to infer multiple climatic parameters. My criticisms arose from the lack of a superposed epoch analysis (SEA), a focus on the heterogeneity of responses to different eruptions which hadn't been firmly established, and incorrect discussion of how the results from various poor quality figures had been described in the main text.

I am pleased that the authors have addressed many of my previous concerns. However there are still many problems with the manuscript. Individually, these are minor and could be easily addressed by the authors. Most are still about incorrect discussion of the results in the text that does not always match the values shown in the figures (the figures are improved, thank you). I'm annoyed that a whole section of my original review was ignored and not responded to, since some of these errors I had already highlighted and now I found them all over again!

Here is the section of my original review that was ignored (nothing mentioned in the authors' response). Of course the line numbers refer to the lines in the original 'discussions' manuscript. I repeat some of them later for the new manuscript but all the original ones should be checked anyway.

Answer: We value reviewer's support and appreciate his careful reading. Specifically, as reported in our previous version, we indeed performed a superposed epoch analysis (SEA; See Fig. S1 in the first submission and Fig. 3 in the R1 revised version). Based on the Reviewer's comments we have tried to improve visibility of Fig. 3 in the current revised version and produced separate plots for each proxy and site separately (see revised Fig. 3, P. 20).

We also apologize for missing a full section in our previous revision. This was due to a mistake when merging several versions of co-authors revisions. We thank the Reviewer for his careful check and these have been now addressed in our point-by point response. The match between text and figures has been checked and inconsistencies have been solved.

Reviewer: 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).

Answer: We double checked the results and figures, and revised accordingly.

Reviewer: 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)."

Answer: Summer 1991 in YAK was very sunny according to the weather station data (not Fig. 5). To avoid misunderstanding, we now carefully link our text with Fig. 5 (revised version), "Conversely, summer 1993 in ALT was very sunny (Fig. 5)" (see P. 26, L. 481).

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?

Answer: Yes, we used instrumental data (weather station data) to confirm our statement (P. 25, L. 461-462).

Reviewer: 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?

Answer: We corrected the sentence related to Fig. 3 as follows: "In addition, at ALT a positive relationship is observed between March precipitation and TRW (*p<0.05*) (r=0.37), MXD (r=0.32), while April precipitation is related positively with CWT (r=0.34), respectively." (P. 22, L.417-419).

Reviewer: Specific comments for the revised manuscript follow.

L77: 0.5C cooling is for NH land cooling not global cooling.

Answer: we removed "global" from the text.

Reviewer:L176: this says these events are VEI>6, but L136 says VEI exceeding 5. Table 1 has one with VEI=5 (Parker). Please be consistent, or if VEI was not the criteria for selection than don't say that it was. I understand that you've already made the selection and made the measurements, so I am not suggesting you make a different selection I'm just asking that you be consistent in stating how the selection was made. It doesn't matter if there is not a simple rule (VEI or something else). It is fine if you selected the largest plus Parker for another reason (data/samples were available, etc.) just say it.

Answer: To avoid misunderstanding related to the VEI, we revised section 2.2 "Selection of volcanic events and larch subsamples" P. 8-9, L. 178-193: "Identification of the events analyzed in this study was based on volcanic aerosols deposited in ice core records (Zielinski 1994; Robock 2000), and more precisely on Toohey and Sigl (2017) where the authors listed the top 20 eruptions from the past 2000 years in terms of volcanic stratospheric sulfur injection (VSSI) in a new ice core-based volcanic forcing reconstruction. Our sub-criteria is based on literature review of reconstructed VSSI and events well reported in tree-ring proxies that may have had a noticeable impact on the forest ecosystems from high-latitude and high-altitude regions (Briffa et al., 1998; D'Arrigo et al., 2001; Churakova (Sidorova) et al., 2016; Büntgen et al., 2016; Gennaretti et al., 2017; Helama et al., 2018). Therefore, based on our

previously published TRW and newly developed MXD, CWT, $\delta^{13}C$ and $\delta^{18}O$ in tree-ring cellulose chronologies, we selected the years, characterized by strong volcanic eruptions with far-reaching climatic effect, namely the years CE 535, 540, 1257, 1640, 1815, and 1991. Therefore, to investigate climatic impacts of these eruptions in Siberian regions, we selected periods around (± 10 years): CE 525-545, 1247-1267, 1630-1650, 1805-1825, and 1950-2000, with the latter being used to calibrate tree-ring proxy versus available climate data (Table 2).”

Reviewer: L228-231: "the remaining YAK sample size was too small" Should YAK be replaced by "TAY"? Otherwise I don't see how the YAK sample size causes the TAY 536 CWT value to be missing. Perhaps these sentences would be more clearly written as: "Unfortunately the remaining sample material for the CE 536 ring at TAY was insufficient to produce a clear anatomical signal. As a result, CWT is missing for CE 536 at TAY (Fig. 2)"

Answer: We revised the sentence according to the Reviewer suggestion: “Unfortunately the remaining sample material for the CE 536 ring at TAY was insufficient to produce a clear anatomical signal. As a result, CWT is missing for CE 536 at TAY (Fig. 2).” (P. 12-13, L. 240-242).

Reviewer: L240: Should it say "(g/cm3)"?

Answer: corrected to g/cm^3 (P. 13, L. 251)

Reviewer: Various anomalies are discussed in the results section. These do not always match the values shown in the figures, suggesting that you have not checked the values very carefully. This isn't helped by using incompatible colours in Fig. 2 and in Fig. S1 for TRW (pink vs. black) and MXD (black vs. purple).

Answer: Colors are revised according to the reviewer’s suggestions and correspond now to those in Fig. 2 and Fig. S1. Specifically, purple (Fig.2 for MXD) vs. purple (Fig.S1 for MXD), black (Fig.2 for TRW) vs. black (Fig. S1 for TRW). We have also double-checked all data and confirm that information in the Figures corresponds to data in the text.

Reviewer: Some examples:
L317-318: You have added a sentence that is not supported by Fig. 2 or Fig. S1 ("Regarding CWT, a strong decrease is observed in CE 536 at YAK.").
Answer: Regarding CWT, a strong decrease is observed in CE 536 at YAK and ALT. P. 16, L. 328;
Fig. 2 reference line and x-axis are corrected.

Reviewer: L321-322: MXD for YAK is not -2.8 sigma in CE 537 in Fig. 2. Maybe you mean 536 for YAK and 537 for ALT?
Answer: Corrected as follows: “…Furthermore, we revealed decreasing MXD values for ALT (-4.4 σ) in CE 537 and YAK (-2.8 σ) in CE 536.” (P. 16, L. 332-333).

Reviewer: L325-326: This sentence is not supported by the figures: "The δ18O chronologies show a distinct decrease one year after the eruptions for YAK -3.9 σ, in the year

of 1259, TAY -3.0σ in 537, and ALT -2.9 σ in 537 only (Fig. 2, Fig. S1)." First, 1259 and 537 are not ONE year after an eruption. Second, ALT extreme is in 536 not 537. **Answer: Thank you for this observation. We corrected as follows: "The ALT $\delta^{18}O$ chronology recorded a drastic decrease in 536 CE with (- 4.8 σ) (Fig. 2, Fig. S1). A $\delta^{18}O$ decrease for YAK was found after the CE 1257 Samalas eruption, but only in CE 1259, opposite to increased $\delta^{18}O$ values towards CE 1259 from ALT (Fig. 2). P .16, L. 335-338.**

Reviewer: L347-348: "No extreme anomalies are observed in CE 1816 in Siberia regardless of the site and the tree-ring parameter analyzed." Even if this is correct for your particular definition of "extreme" (lower 10th percentile) it is nevertheless worth noting that MXD at YAK does reach about -2.5 sigma in 1816, otherwise this sentence may be misinterpreted by readers. Also, I'm not sure it is correct: is -2.5 sigma really not in the lower 10th percentile (as shown in Fig. S1 for 1816 YAK). This would imply a very non-normal distribution which seems unlikely from Fig. S1 1816 YAK. Also, this does not agree with Fig. 2 of the main paper, where TRW (pink) is not less than -1 sigma, yet in Fig. S1 1816 YAK the TRW (now black) is -2.3 sigma. Also MXD is -2.5 sigma (black in Fig. 2) but -2 sigma (purple in Fig. S1). This all needs careful checking and perhaps correcting -- and perhaps Fig. 5 may also then need correcting to show cool anomalies for YAK in 1816 for either MXD or TRW depending on whether fig. 2 or fig. S1 is the correct one.

Answer: We carefully checked and corrected figures accordingly.

Reviewer: Fig. 3: it is good to now see an SEA analysis. However it's value is limited by two aspects: (1) by overlaying composites for the three sites means it is unclear what some of the values are (e.g. MXD in eruption+1 is negative for ALT and YAK but no idea what the TAY value is as there is no red bar; for eruption+2 ony the green bar is visible; etc.).

(2) no significance levels are shown -- a key advantage of SEA is that the anomaies can be tested against a null hypothesis that there is no volcanic signal. One brief mention of a statistical test is given in the text (L357-359) though it is unclear what is being averaged over. If it isn't possible to perform a statistical test of the SEA results then this should be clearly stated (e.g. because only selected years have been measured for some variables, rather than full timeseries) though I think this would only weaken the power of a test rather than prevent any test being performed.

Answer: (1) Fig. 3 was revised according to the Reviewer's suggestions. We separated figures to avoid overlapping between plots (P. 20). (2) We applied unpaired t-test statistics to check significance between each proxy and each site (P. 16, L.322-323).

Reviewer: L389-391: The results described do not seem to match those shown in Fig. 4 for d18O. There are no red bars for the months discussed at YAK and TAY. There are some for ALT but they don't appear to be in February and March.

Answer: We removed this sentence to avoid misunderstanding because the correlations were rather low and we did not include them in the graph, but tried to discuss in the text. Therefore, we did not provide the link to the Figure.

Reviewer: Fig. 5 is a very nicely designed summary. However the text describing it is not always consistent.
Answer: We revised accordingly.

Reviewer: L451-452: "Therefore, CE 536 was extremely humid in YAK and TAY, as well as 541 and 542 in TAY and ALT." The only coloured symbols in Fig. 5 for CE 542 are indicators or temperature or sunshine. How can you say it was extremely humid then?

Answer: In Fig. 5, $\delta^{13}C$ values, which recorded summer vapor pressure deficit (VPD) are shown as purple circles for YAK for the years of 536, 541, for TAY for the years of 536, 537, 538 and 541 in ALT. We revised this sentence as follows: "Accordingly, the $\delta^{13}C$ values showed humid summer climate conditions for YAK in 536, 541; for TAY in 536, 537, 538 and in the year of 541 for ALT. Opposite to other proxies and sites, the year of CE 537 in ALT was rather dry (Fig. 5)". P. 25, L. 468-471.

Reviewer: L452-453: "CE 1258 was dry in YAK and ALT, while drier than normal conditions occurred in 1259 for all studied sites." Dry? Then why do you have purple circles in Fig. 5 indicating low vapour pressure deficits? Sure low VPD means wet?

Answer: When VPD is increasing – drier, decreasing – wetter (by increasing air moisture).

We revised above-mentioned sentence according to corrected Fig.5: "Dry conditions prevailed in CE 1258 in TAY, in CE1259 in ALT, whereas wet anomalies were recorded in 1258 and 1259 in YAK" P. 25, L. 471-472.

Reviewer:L453: "CE 1641 was dry in TAY; 1642 in YAK and ALT" -- but 1641 is not shown in Fig. 5. Why not? And 1642 has no coloured symbols to mark moisture at all.

Answer: L453 revised to: "No anomalies were recorded for the CE 1642 event, irrespective of the sites." P. 25, L. 472-473.

Reviewer: I just realised that many of these were in my original review and have been ignored by the authors. They have not included any of these comments in their "author response" and now I have spent my time reporting the same errors for a second time!]

Answer: We are thankful for the Reviewer's time and appreciate the very valuable comments. We apologize for missing a full section in our previous revision. This was due to a mistake when merging several versions of co-authors revisions. We now considered all of the Reviewer's suggestions and comments, and corrected them accordingly.

Reviewer: Fig. 2: I would recommend adding a vertical black line at CE 540, as currently this eruption is not marked in Fig. 2 but all the others are.

Answer: We added a vertical black line at CE 540 as requested (revised Fig. 2, P. 18).

Reviewer: SM: Much clearer now. The figure caption has disappeared, however. Despite the authors' claim, they still don't show superposed epoch analyses since all years are shown separately rather than superposed to obtain a composite. But an SEA has been included in the main figure 3.

Answer: **We added a figure caption to Fig. S1 and revised Fig. 3 (P. 20) according to the Reviewer's suggestions.**

Fig. S1. Probability density function (Pdf) computed for each of the tree-ring parameter for northeastern Yakutia (YAK) (left panel), eastern Taimyr (TAY) (middle panel) and Russian Altai (ALT) (right panel), respectively. Tree-ring parameters (TRWi - black, MXD – violet, CWT – green, $\delta^{18}O$ - blue and $\delta^{13}C$ - red) in bold lines represent the probability density function. Dotted lines represent the anomalies (z-score, standard deviation) induced by the volcanic years: CE 536, 537, 538, 541, 542, 1258, 1259, 1453, 1458, 1601, 1602, 1642, 1643, 1816, 1817, 1992, 1993.