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Climate of the Past Discussions

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Interactive Comment

Interactive comment on “Eliminating the “divergence problem" at Alaska’s northern treeline” *by* M. Wilmking and J. Singh

M. Wilmking and J. Singh

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The authors thank the reviewer for his/her input to the review process. Several remarks, especially specific comments on wording, sentence structure and a better description of the methods used are welcome and will definitely help strengthen the MS. Some other remarks by the reviewer however, are not as helpful and probably due to some profound misunderstanding of the goal of the MS. Here, we preliminarily address the concerns of the reviewer. We feel that is is best to do so during the open discussion to allow for further input from readers and reviewer. Our replies are in bold located below the corresponding reviewers comment.

Review of the manuscript entitled: Eliminating the divergence problem at Alaska s northern treeline submitted by M. Wilmking and J. Singh for publication in Climate of the Past This manuscript presents a compilation of individual tree-ring width (TRW) measurement series from seven sites (partly downloaded from the ITRDB) across the northern treeline roughly at 68 N and 140-160 W in Alaska. Data were corrected for agetrend biases using the Regional Curve Standardization and classified into subsets of individual series that correlate (i) significantly or (ii) non-significantly with instrumental summer temperatures. Based on this stringent pre-selection, the authors created two mean chronologies of different late 20th century trends: increasing (i) and stationary(ii) indices after 732;1980, with the later subsequently indicating some offset with rising temperatures.

Some remarks: We create two chronologies of differing temperature sensitivity, not different trends. One chronology was built of trees with a consistently significant positive relationship of growth with temperature over time, the other was built of all trees sampled. This division resulted in different trends of the two chronologies

Methodological details provided for the rather imprecise analysis are not sufficient and the proxy plus target database considered is limited.

We would like to mention that the proxy database is probably not as limited as the reviewer suggests. It includes all available tree ring data from northern Alaskan treeline which extends into the 21st century. As the title indicates, our analysis is centred only on northern treeline in Alaska and over 500 trees of the treeline building species White spruce are used from seven locations across the Brooks Range. We are not aware of any other study with this much data analysing the climate-growth relationships and resulting divergence at the northern Alaskan treeline. In addition, all the data was sampled by the authors and we are thus confident about the quality of the data as well as the ecological setting of every individual sample. Nearly all of the data has been submitted to the ITRDB. We have concentrated on Alaska, because our ecological knowledge is centred in this area and all results from our study indicate that the divergence problem

there (again, we are talking about northern Alaska) is the simple result of non-stationary climate growth responses of a subset of trees. The mechanism for this non-stationarity operates on an individual tree basis. This is the main contribution of this paper, since it offers and alternative explanation to the often repeated large-scale mechanisms such as Global dimming or UV-B increase.

Therefore, I consider this brief analysis to be not a suitable input towards a better understanding of the currently debated Divergence Problem in tree-ring research.

We are unable to follow the reviewer in this statement. In this MS we successfully show a method to eliminate the divergence problem in an area which has been used intensively for climate reconstruction in the past. Even though our analysis might be regional in scale (1000km transect), it offers several points to better understand the 8220;divergence problem8221;. Above all that at all sites the divergence is affecting only individual trees and not their neighbours only tens of meters away growing in similar environments. This leads us to the important conclusion that if we want to understand the divergence problem better, research should be intensified at exactly that scale, the individual tree and its environment. We fully agree that other factors or combinations thereof might also be contributing to the divergence problem, however, at our sites, all of these other hypotheses (e.g. global dimming, increases in UV-B radiation, pollution, differential responses to min/max temperatures) fail to explain our results.

This is of particular importance, since the apparent inability of formerly temperature sensitive TRW chronologies to track global warming since the mid 20th century, in combination with their failure to reflect high-frequency temperature signals forms a serious problem for tree-ring research and thus deserves dedicated scientific research - everything else merely confuses the issue.

We would like to refrain from commenting on the apparent implications about the dedication of our research. Instead, we would just like to point out the fol-

lowing. We were able to mirror Fig 1 from the highly cited D Arrigo et al (2008) paper, where Fig 1 served as the example to show the divergence problem. It is an example from an Alaskan network. Our Fig 2 shows our northern Alaskan network. There is no data overlap with D Arrigo et al. 2008. Our northern Alaska network captures the main behaviour of Fig 1 of D Arrigo 2008, note especially the similar decrease in temperature sensitivity (our Fig 2 A/B). Our Fig 2 C/D then shows our northern Alaskan network AFTER we have screened the sample for trees with consistent positive relationships with temperature (please note that our running correlations are plotted at the end of the sliding window length, i.e. the correlation from 1910 to 1941 is plotted in 1941). Since we are able to eliminate the divergence problem using this screening approach, we feel confident that strong explanatory power can be drawn from this analysis, which might help to understand the divergence problem and its causes. We fully agree with the reviewer on the issue that the divergence problem is a serious problem for tree-ring research and we feel confident that our analysis offers important insights into exactly that problem.

Title: is misleading as the so-called DP is created rather than eliminated. The authors to some extent generated offset between the instrumental target and proxy reconstruction artificially .

We completely disagree with the reviewer. We use >500 tree samples, use established dendrochronological methods, analyze the change of the climate growth relationship over time and plot the resulting chronology with the temperature data. The divergence effect simply appears, it is an inherent phenomenon of that data set. We then however, add an additional step to the analysis and are able to show that the divergence effect in our complete data set is not present for every member (individual tree) of that dataset. Instead, we are able to eliminate the effect by a rigorous screening approach.

Two aspects are critical in this regard: the applied approach of simple site differentia-

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tion into responders and non-responders (those data that do or do not correlate with climate) without considering independent calibration/verification periods can result in a circular reasoning.

This comment does not apply to the current analysis. Here we just use 1) all trees sampled (no differentiation into any groups) and 2) only trees with a CONSISTENT positive relationship with the climate target (P744, l20-26). It is a common practise in dendrochronology to select series or trees which show the signal the investigators are looking for. For example, based on the principle of site selection we sampled at northern treeline, as we expected to find trees, which would be temperature limited and thus increase in growth under increasing temperatures. It is just an unfortunate fact that the ecological situation is not as easy as we (and many others) thought. However, even though the majority of sampled trees does not show the expected increase in growth (resulting in a divergence effect), there are individual trees which completely comply with the basic ecological theory. They grow at a temperature limited site and increase in growth when temperatures increase. The challenge is just to explain why their neighbours do not. This is the main point of this paper, that the reasons for the divergence at least in northern Alaska are individual tree based.

Moreover, the post-1980s offset appears to be as large as divergence at the beginning of the time-series

We would like to point to the reviewer s attention to P 748, l22-27 where we discuss this exact observation. We also would like to mention that to further substantiate this point, we have used only the CRU data from 1906 onwards in the Fig 2 of this reply. That was the date when the first continuous climate station in Interior Alaska (Fairbanks) started recording, prior to 1906 the CRU data is likely of very low quality, see P 748, l 26-27. There is no divergence in the early part of the 20th century for the data period considered with a contributing regional climate station.

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P 743, l3: the frequently cited D Arrigo et al. paper was published in Volume 60, 2008 (289-305) and not in2007

Thanks to the reviewer for pointing this out. The D Arrigo paper was published online in 2007 but should be cited as published in 2008.

P 743, l11: the northern latitudinal band described by Büntgen et al. (2008) must be 44-48

Correct

P 743, l22: northern Alaska is a fairly unreliable region in terms of instrumental station quality and quantity, and thus most likely not the best area for an in-depth study of (temporal) relationships between tree growth and climate. This argument is mainly valid along the entire northern circumpolar ecotone

Well, if that should be the limiting factor for dendrochronology, investigation on tree growth climate relationships would probably be limited to central Europe and some parts of the U.S.. However, since many climate reconstructions have used exactly that area (northern treeline) we feel that it is important to address the recent challenge to dendroclimatic reconstructions, the divergence effect, there as well. For a study on the divergence effect in more southern area, see Büntgen et al., 2008.

743, l23: the expression physiological growth threshold could be more precisely explained (summer warming at the latitudinal and altitudinal treeline should shift growth conditions from formerly temperature limited to now optimal conditions without exceeding some threshold towards drought limitations, for example)

As the reviewer points out, warming should shift growth conditions from formerly T limited sites to better conditions8230;However, this just does not hold true for large parts of the Alaskan treeline ecotone (see e.g. Lloyd and Bunn 2007, Lloyd 2005, Wilmking et al. 2004, 2005, Wilmking and Juday 2005, Lloyd

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and Fastie 2002). We are also happy to expand this sentence if required.

P744, l4-10: what is novel about the current manuscript and how does it transcend existing work: detrending, data splitting, temporal analysis, alternative reconstruction? All of these arguments have been tested before

In the context of the single tree analysis as performed here, these arguments have not been tested in the northern Alaskan region (nor anywhere else to our knowledge).

P744, l12: add the between from northern **possible change** P744, l13: change year 2000 to 21st century **possible change** P744, l15: change treeline building species to dominant treeline species **possible change**

P744, l20-21: there are various approaches to developing a mean chronology after RCS detrending, and RCS is certainly not a silver bullet in terms of proper standardization. Most critical issues are related to the fact that the authors don t utilize composite (i.e., including living and relict wood samples) data, which can potentially bias the overall long-term evolution of the resulting time-series

We agree with the reviewer that RCS is definitely not suitable in all the cases and certainly not a silver bullet, but in case of our data, most of the series contain pith information and only in few cases pith offset. The RCS chronology captures low frequency variability and since our aim was to capture twentieth century warming, we used the RCS detrending method. We also used conservative method of detrending using negative exponential curves or straight-line curves to develop ring-width chronologies but standardisation method did not much affect the results. Our analysis shows that there are tree with consistent growth relationship to climate and there are trees with a non-consistent relationship with climate. While there might be a potential for a bias in long-term evolutions when using composite or non-composite time series, this does not affect our analysis.

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P744, l22: the authors should provide details about their methodological application of ARSTAN: was any variance stabilization considered on the series level (e.g., power transformation) and later on for chronology development (using a bi-weight or arithmetic mean for example)

We applied power transformation and later developed a robust bi-weight mean chronology. We will add this information to the methods section.

P745, l8: more information on the CRU grid-box data could be helpful: were min, mean, max temperatures compared, which ones were finally used, was the CRUTS2.1 version used, and did the authors reflect on potential trend issues as stated in the original publication

We used average mean monthly temperature data (CRUTS2.1 version) of six grid points closest to the sampling sites.

P745, l9-10: the performed selection is based on a subjective rather than objective decision

Correct, we did not want to attempt a reconstruction with a proxy dataset which showed a non-stable relationship with climate.

P745, l10-12: this sentence should be moved to the results chapter **Possible change**

P745, l14-18: it remains unclear which calibration technique was performed in the end. Such information, however, is relevant, as calibration impacts the reconstructed amplitude

Please see P745 l 18-18. We tested several calibration verification models to check the fidelity between the tree-ring data and June-July temperature data and finally to capture low frequency variability, we used the entire period (1901-2000) in the calibration model for the June-July temperature reconstruction.

P745, l22: climate parameters should be temperature **Accepted, should be temper-**

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ature

P745, l23: how high, respectively significant are positive correlations . The authors should be consistent throughout the entire manuscript

Please see Fig 2a of the original MS.

P745, l27: change mostly to most **will change to 8230;over time, most of them recently negative8230;..**

P746, l1: see P745, l22: climate parameters should be temperature **In this case it actually should read temperature and precipitation, since in the cited studies relationships with precipitation were also recorded.**

P746, l7: change increase in temperature to warming **We actually chose the wording carefully and would like to keep it that way, since increase in temperature and warming seem synonyms to us, is that incorrect?**

P748, l22-29: the authors stress a serious limitation of their study. Not only are carefully selected and well-replicated tree-ring data necessary to detail potential instability in growth responses to climate, additional uncertainty may also be the result of instrumental station and methodological grid bias. The homogenization procedures applied, micro-site effects, influences of the urban heat island , and interpolation can all influence the calibration procedure. See also my earlier comment on P 743, l22

P 749, l12-15: this is just not true, as both commonly applied Rbar and EPS statistics are usually obtained via moving window approaches.

Correct, we are fully aware of this. However, a calculated sliding Rbar or EPS with e.g. the default option in ARSTAN (a window length of 50 year with 25 years overlap) does still not capture the divergent trends present in our sites, since they are only apparent in the last 25-30 years. We are more than happy to clarify the text in this regard, if necessary, or add a figure showing a consistently strong EPS value above 0.85 for our northern Alaskan tree ring network (all trees).

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As a side point, such statistics solely provide information on common signal strength within a data-set/chronology, but don t allow conclusions on (changing) external forcing parameters to be drawn

We are also aware of this and agree that we might have worded the text not clearly enough. We will address this issue.

P 749, l21-24: the ITRDB is a unique databank and represents a great opportunity for the entire paleo-community including climatologists, ecologists, and biologists. This archive should not be treated as a black box with any misuse relying upon individual users

We again fully agree. It was not our goal to attack the ITRDB, we are contributing and using it as well. However, we feel that certain additions should be made when the scientific evidence for their need becomes available, to ensure the quality of the ITRDB in the long run.

In short: this manuscript should not be considered for publication in Climate of the Past. The authors, however, could alternatively perform a more in-depth analysis of an extended tree-ring network and use various versions of instrumental target data. Methodological improvements with respect to chronology development and calibration technique would further assess temporal stability/instability of growth responses to climate variability.

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