Clim. Past Discuss., 4, S365–S369, 2008
www.clim-past-discuss.net/4/S365/2008/

Climate of the Past Discussions

CPD
4, S365–S369, 2008

Interactive Comment

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

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Received and published: 5 August 2008

The anonymous review by Referee 1 contains a succinct summary of the content of the Wilmking and Singh submission and I see no purpose in repeating it. Similarly, Referee 1 makes a number of points, virtually all of which I agree with. I will, therefore, lay stress a few similar conclusions but attempt to make a number of points that I believe have not been covered or sufficiently stressed.

My general opinion of the manuscript is that any new light that it might cast on the ongoing discussion of the "divergence issue" is more than offset by the additional confusion it introduces. Certainly the use of the phrase "eliminating the divergence problem..." in the title is wholly misleading. The text reiterates evidence that the radial growth of a large number of trees at the Alaskan tree line appears not to increase in line with the relative magnitude of summer warming observed over the second half of the 20th century. Certainly there is nothing in the work described here that "eliminates" the problem and the somewhat semantic argument that the observation of divergence is not a "problem" but an "effect" serves only to distract from the importance of the issue for interpreting some dendroclimatic-based estimates of the magnitude of earlier warm periods, as has been well signalled in several prior publications (e.g. Briffa *et al.*, 1998b; D'Arrigo *et al.*, 2008; National Research Council Report, 2006; and IPCC Working Group 1 AR4, Chapter 6 – all but the last cited by the authors). These all make clear that it is the context of assessing the precedence of 20th-century warm (regionally or for large spatial averages) that the apparent phenomenon of tree-ring growth/temperature divergence should indeed be considered a "problem". As an aside, it is important to stress that not all high-latitude trees used for such a purpose are affected by divergence e.g. the RCS –processed data that make up long chronologies in Fennoscandia, and different locations on Siberia. This is not immediately clear from the introduction of the paper.

The authors show that by selecting roughly 17 per cent out of a large sample of Alaskan tree index series, on the basis of a comparison with a specified temperature target series, they can then construct a chronology from these selected data that displays an improved match with the same target series than a chronology constructed by including all of the samples. This obvious expected result does little to advance our understanding of the nature or likely cause of the poor association with temperature shown by the 83 per cent of series not selected. Little is provided in the way of details about how the authors' use of RCS performs for specific trees and sites and whether any biases are introduced in their application.

They make a number of statements about the processes of chronology construction and climatic interpretation that are not supported by their results, or at least in the extent that they are detailed here.

The authors state that "factors acting on an individual tree basis are the primary causes" of divergence and because of this they dismiss the likelihood of standardisation issues, global dimming or possible UVB effects etc. The do not give any details about the *particular* trees that do not show divergence, especially details of their climate associations and ecological context (at high- and lower-frequency timescales, through time, and as compared to the other trees). Nor do they say whether these trees have been used in previous publications and how these new results might affect the conclusions of those papers.

All growth limitations must act on “an individual tree” basis and with no details of the specific trees apparently affected, and no details of the specific character and magnitude of the effect in those trees, it is impossible to assess the basis upon which the authors dismiss several possible causes. Will not all tree-growth limiting factors vary in the extent to which they exert their influence on individual trees? Why should the practical application of a specific standardisation technique not produce varying biases when applied to a non-homogenous population of sample cores? – especially a technique like RCS that inherently assumes that one ‘common’ statistical model of expected non-climate growth as a function of tree age is appropriate for processing all trees in a sample. Why should individual trees not vary in their susceptibility to increased UVB radiation as much as drought stress?

There are also inherent difficulties in the selection of trees as advocated by the authors, though I can see why, like others before them, have used this approach purely for demonstrative purposes.

However, their extension of this largely circular argument to imply a requirement to abandon the chronology processing methods currently in use seems to me to be over interpreting the significance of their results. The tree-to-tree variation in response to external forcing is *why* we build, what we hope are robust “chronologies”, where the nature, strength and associated uncertainties in forcing signal, its expression and any associations with external forcings can all be measured, and the latter used as the basis for producing reconstructions with quantified confidence limits.

It is well appreciated by tree-ring researchers that individual trees (and even parts of the same tree) grow at different rates and display different relationships with climate time series. The logic behind the construction of ‘chronologies’, however, is that these better represent the expression of common influences on tree growth at an overall site level. By careful site selection, or selection of subsets of trees within a site or region, we can maximise the potential to express the signal we are seeking to study but the selection of trees must be objective and made independently of the target data. It is axiomatic that many things other than climate can influence tree-ring growth and no simple average of monthly temperatures would be expected to display an entirely time-stable stable association with any ring-width or chronology series, but selecting the tree-ring input data on the sole basis of coherence with an *a priori* selected climate series, of itself is not practical dendroclimatology.

This paper concludes with a clear recommendation for selecting individual trees on ‘a standard test of each tree’s reaction to climate over time’. In my opinion this will produce wholly biased chronologies where the “selected” chronology statistics are unrepresentative of the strength of changing chronology confidence and where the strength of the apparent association with recent climate will be biased and unrepresentative of any climate signal expressed in the chronology prior to the existence of available climate records.

There are already numerous techniques that are routinely employed to judge the strength of common growth forcing inherent in tree-ring sample data sets. They can and are frequently examined in time-dependent fashion to gauge the changing strength of common forcing or as an objective basis for sub-dividing the data into separate sub-chronologies. Similarly time-dependent applications of response functions are also used to explore the stability of identified or pre-specified climate associations. This is not clear from the authors’ text and they provide no guidance on how to implement the recommendations for a new approach in a practical way for dendroclimatic purposes.

The issues associated with “divergence” include: whether the trees are displaying the

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effects of a significant recent change in the nature of the growth limitation that largely controlled their growth in previous times; whether this is likely to be an unprecedented change; what implications this has for interpreting current temperature (and other climate) reconstructions; what the implications are for dendroclimatology and the future ecology of northern tree growth.

It is very interesting that so few Alaskan trees apparently do not show (or show reduced) divergence but we need much more detail about these specific trees, their geographical and ecological context, before invoking "temperature induced drought stress" and even more so "emergent sub-population behaviour". The former has, of course been widely suggested, but we are given no information here about how the specific divergent (and non-divergent) trees grow in accord with changing drought conditions.

In summary, first let me say that there should be no misunderstanding – The 'divergence issue' is important – for all the reasons listed in the IPCC AR4. This paper, despite its title, does not 'eliminate' it. Unfortunately, it conflates a number of issues and ultimately confuses them. The paper does not provide enough detailed results and information about the non-divergent population of trees and how robust their identification is, or why the other trees are divergent. The results that are presented are not clearly described in the specific context of earlier results on Alaskan trees. Recommendations to use dendroclimatic methods applied on a time-dependent basis and on an 'individual tree basis' are either not sufficiently new or sufficiently developed or defensible. I would not recommend publication of this manuscript.

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4th August 2008

Interactive comment on Clim. Past Discuss., 4, 741, 2008.

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