

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

J. Guiot (Editor)

guiot@cerege.fr

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After careful reading of the manuscript, the two comments and the reply of the authors, I have two major arguments to reject the paper. These are in the same mind that several given by reviewer #1.

- Even if the authors acknowledge that establishing a climate reconstruction on selected series is often a problem, they do it and the fact that 17% of the chronologies is still a large number (133 series) does not justify the approach. Indeed, one explanation of the divergence problem is that another factor may explain the growth when temperature is higher. This factor could be water stress and in this case, we can accept the selection of a subset of trees as they are located in “favourable” places. In doing this (a posteriori) selection, the authors have implicitly accepted this hypothe-

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sis and the reasoning is circular (as mentioned by the first reviewer). To be robust, the reasoning must be based on a *a priori* selection: e.g. by looking at the ecological conditions and selecting the less stressed trees. If those have a trend significantly different from the remaining, then the hypothesis can be accepted. I agree with the first reviewer that this descriptive and circular analysis does not bring much to the general discussion of divergence problem. The replies of the authors (large dataset, objective is to simulate the DP) are not convincing.

- Figure 3 does show a convincing relationship between tree-ring indices and climate (the high frequencies are not at all reconstructed), tending to show that the only responsible of the correlation is the trend. When we know that the series have been selected for their trends, it is hard to be convinced by the purpose. It is likely that all curve (e.g. world demography) with a positive trend could have a correlation with the tree-growth curve as good as 0.50. To bring something new to the general discussion, the minimum is to work with trees sensitive to summer temperature in the whole frequency spectrum.

About the reply of the authors:

- I cannot receive the reply of the authors that it is a common practice in dendrochronology to select trees. Trees are indeed selected on the field for their age and position to enhance the climatic signal. But they are not selected a posteriori on the basis of the correlation with climate (it is true that sometimes a small proportion is rejected but never 83%, it should be a statistical non-sense). If the data registered in the ITRDB are not enough to make a selection on the ecological situation, the only way to investigate the problem is then to work on well known data.

- A sentence as *Our analysis shows that there are tree with consistent growth relationship to climate and there are trees with a non-consistent relationship with climate*; (reply, end of S331) shows that everything rests on a prescribed definition of the the climate (as seen by the trees): summer temperature. If this factor is

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only a part of the signal, all the reasoning falls. And again Fig 3 shows that the high frequency variations cannot be explained by this summer temperature.

A few specific comments

- Generally the paper is written as for a dendrochronological journal. Climate of the Past is an interdisciplinary journal and the methodology need to be described for not tree-ring people.

- Fig 2: legend caption does not explain several points: correlation between what and what, where are A, B, C , D? how are chosen the series: slope (low-frequencies) or correlation (high frequencies)? In the first cas, the reasoning is more circular than in the second case, as we may have a positive slope even with no correlation with the summer temperature.

- P745: the principle (principal) component regression with one PC (eigenvalue = 4.05, % variance explained?) is used. I have understood that there is only one predictor, the regional series, then the method is just a simple regression. The reply of the authors is not clearer.

- From a statistical point of view, when we do a hypothesis test and we reject the null-H (no slope) at the 0.10 significance level, we accept that up to 10% of the tree have a positive trend. Here the percentage is 17%. It is too much to reject the hypothesis of no trend, but is it not a too small number to accept this hypothesis?

- At the contrary of what is written in p.747 (l. 17-21), the RCS method may induce large biases because relatively large variations of high productivity trees contribute more than relatively large variations of low productivity trees in the regional curve (this is due to the fact that ring width is a positive variable). Apparently the authors have controlled that fact with alternative detrending method. Why not show the results in the paper?

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S353

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