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Climate of the Past Discussions

Interactive comment on "Extracting a common high frequency signal from northern Quebec black spruce tree-rings with a Bayesian hierarchical model" *by* J.-J. Boreux et al.

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Reviewer 2 (Clim. Past Discuss, 5, C62-C63,2009, by Boreux *et al.*)
We would like to thank Reviewer 2 for his/her interesting suggestions that have been very helpful to clarify and improve our manuscript. We have answered to his/her specific issues below.

"A non parametric transformation is performed in order to make the series stationary

but this leads to remove the low fre- quency part of the common signal. Hence only the high frequency part of the signal is described by the model. The authors do not indicate how relevant is this information for climate study".
The referee raises an important point and the following elements will clarify our views on this issue and they will be added in the new version of the article. As stated in the second paragraph of Section 1, our goal is neither to reconstruct precipitation and/or temperatures time series nor to climatically interpret our extracted signal. In this context we have made sure that the word "climate" never appears in sections 2, 3 and 4 (the main body of our work) and in the title. Contrary to Vincent's statement in the first paragraph of his review, we do not assume that the common signal is "climatic". It is very likely but, for some tree specie on some specific site, the main part of the extracted signal may not be connected to climate but to other environmental aspects (soils, competition, etc). Hence, the strength of our modeling strategy is that no prior assumption on possible covariates is made (we let the data "speak"). Of course, one motivation of our research is to derive (in future work) how extracted signals from various species could be connected to climate. By developing a new "blind extraction" method for dendrochronologists, the objective of the present article is **methodological**. The proposed techniques could be applied to other research domains like for upscaling problems (ie. how to extract a common regional signal from different weather stations). The question asked by the referee ("how relevant is this (interannual extracted) information for climate study") can only be answered by saying that, as written in the conclusion, "our present work should ... be viewed as an addition of a simple statistical procedure to the mathematical toolbox of dendroclimatologists". As stated by the reviewer, one important remaining point is to "propose reliable ways to fulfill climate reconstruction work". We believe that this fundamental and very complex statistical question is beyond the scope of this work. During the next few years, we are planning to reach this goal but we were not satisfied with classical average-based approaches to extract common signal in trees. Hence this work can be viewed as a first step in our reconstruction scheme. We understand that our present methodological objective could be currently viewed

as very limited in terms of climatological interpretation. We prefer to be honest and not oversell our method. Our future goals are to improve this model to deal with both high and low frequencies and to make an exhaustive study to identify connections with climate variables and the extracted signals.

"As emphasized by the authors, only the high frequency part of the common signal is described by the model. Hence it is not surprising that the autoregressive coefficients are found negative and I wonder what is the interpretation that can be given on the signal. This is not pointed out.
The referee is correct. The interpretation should have been more exhaustive. The following ideas will be added in the new version of the article.

Concerning the interpretation of Y_{ts} defined as a log-difference between two consecutive ring area values, the following simple facts need to be recalled. Whenever the relative ratio of two inter-annual consecutive ring areas from the same tree is close to one, then Y_{ts} is close to zero. If this relative ratio is very large (ie the ring area from year t is much larger than the one formed during year $t-1$), then Y_{ts} has to strongly positive. Conversely, a negative Y_{ts} represents a large decrease in ring areas between two consecutive years. As exemplified by Figure 3, working with Y_{ts} instead of the raw ring areas X_{ts} allows us to remove long-term trends, to focus on the inter-annual relative variability and to work with time series that can be assumed to be stationary and Gaussian. One drawback is that we have lost the absolute value of X_{ts}, ie working with the couple $(X_{ts}, X_{t-1,s})$ is equivalent to analyzing the couple $(aX_{ts}, aX_{t-1,s})$ for any $a > 0$, independently of the value of a.

Keeping in mind this drawback and those advantages, the correlation meaning in Y_{ts} and Z_t can be viewed as the short term memory in the relative log-transfom rate between two consecutive ring areas.

"There is a significant discrepancy between the classical tree-growth index and the common signal given by the BHM, this should be analyzed further, coming back to

the data. Just telling that the classical method shows its limitations by not providing confidence intervals is somewhat simplistic."
Concerning the discrepancy between the classical tree-growth index and the common signal given by the BHM and its climatological interpretation, we are a little bit reluctant to draw conclusions from our example. This may due to the fact that we are statisticians by training. Consequently, we don't want to interpret (over-interpret??) our results about the northern Quebec climate from only a single site. In addition, none of us are dendrochronologists or climatologists. So we prefer to leave the interpretation to the experts in those fields. Finally, we want to insist that our goal in this article is to propose a new statistical method and not to reconstruct the northern Quebec climate. In this context, we prefer to limit our criticism to the methods (instead of interpreting differences). This may be "simplistic" but it could also be "wiser" than over-interpreting the difference between the classical tree-growth index and the common signal with only one single site .

"I do not agree when the authors expect the relationships between the observed Y_{ts} and $\hat{Y}_{ts}$ to be linear. If the fit is perfect, the scatter plot is on the diagonal, and that kind of graph shows how well the model fits the data. But the relationship that is expected to be linear is the relationship between Y_{ts} and Z_t and this one would have been shown."
We are not sure that we understand this comment. We agree with the referee that the scatter plot, ideally, should be diagonal. Figure 6 shows such a type of diagonal behavior and this tends to indicate that the model adequately fits the data (as stated by the reviewer). Concerning the relationship between Y_{ts} and Z_t, we agree that it should be linear. The variable $\hat{Y}_{ts}$ is defined as $\hat{Y}_{ts} = \hat{\mu}_{ts} + \hat{\lambda}_s \hat{Z}_t$. Hence, if the relationship between Y_{ts} and Z_t was not linear for our data, then the plot between Y_{ts} and $\hat{\mu}_{ts} + \hat{\lambda}_s \hat{Z}_t$ in Figure 6 could not show a diagonal. In other words, the linear relationship between Y_{ts} and the estimated of Z_t is also indirectly captured by Figure 6. We could also provide the raw plots between Y_{ts} and the estimated of Z_t. Instead of a diagonal, we simply have a linear relationship whose slope and intercept will depend on $\hat{\mu}_{ts}$ and $\hat{\lambda}_s$, the coefficients being shown in Figure 5.