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Biogeosciences
Discussions

Interactive comment on "An inverse modeling approach for tree-ring-based climate reconstructions under changing atmospheric CO_2 concentrations" *by* É. Boucher et al.

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The manuscript by Boucher et al. describes the application of an ecophysiological model to the problem of reconstructing past climate parameters. The authors test whether using an inverse approach can yield reliable reconstructions and how this methodology compares to simpler transfer function approaches based on multiple linear regressions. They further use the model to test possible effects of CO2 fertilization. I think the presented work is a notable contribution at the intersection of paleoclimatology and dendroecology.

General comments

The authors describe two topics: First, they show conclusively that the inverse approach is able to produce local reconstructions of past climate parameters and that the approach appears to have a number of advantages compared to simpler transfer functions. Second, they provide evidence from their modelling-study that indeed CO2 can have an effect on the reconstruction results and they present one hypothesis how the increased CO2 might have affected the plant.

The first topic is handled convincingly and I have only minor comments on the presentation. However, their discussion of the CO2 effect is vague on how the differences in water uptake efficiency (WUE) link to the plant growth or in terms of the model to the considered parameters (latewood widths, d18O and d13C). Furthermore, this part is missing any discussion of how the quality of the reconstruction is affected.

Overall I would appreciate if the authors presented more clearly the uncertainty of their results and the range of their probability densities. This would allow the reader to assess the differences between the different approaches. I detail this and my further minor concerns below. Further, I fully endorse the review comments of the anonymous referee #1.

Specific comments

1. Reviewer 1 already noted that the assumption of a significant "CO2 fertilization" is central to the manuscript. I think it is also the least convincing part of the work. Thus, I would like to encourage the authors to (i) more thoroughly discuss the link between WUE and plant growth and (ii) especially the effect of the found relation between CO2 and WUE on the three "proxy"-parameters latewood widths, d18O and d13C. So far the manuscript does not show whether this effect reduces the quality of the reconstruction.

2. On a side note, I agree with the authors that simple transfer function approaches have various caveats. However, I am uncertain whether it is correct to call the simple linear transfer functions black box approaches. The algorithm, the internal workings, the transfer characteristics are known in principle. They may not be fully appropriate,

but it's not generally a black-box from my point of view. Kirchhefer (2000, Dendroclimatology on Scots pine (Pinus sylvestris L.) in northern Norway, Doctoral thesis, University of Tromsø) even describes the tree itself as black box between climate and tree-ring.

If one calls the simple approaches black boxes, I am tempted to see MAIDENiso also as a black box.

Furthermore, I do wonder whether increasing complexity of a model does necessarily improve its fitness for the intended purpose. Therefore I remain unconvinced that it is generally necessary to go for the most complete and complex model.

Computationally cheap models have their purposes and should not be neglected. The authors imply added value due to complexity, but they don't provide evidence. It is a possibility, but it isn't necessary to reduce the value of simple approaches to motivate the presented study.

3. Similarly, I am not sure whether the single- vs. multi-proxy argument is really valid in the introduction on page 18482. Single vs. multi and tree-ring vs. multi-parameter approaches are not mutually exclusive, they both have a value.

4. I'd like to ask the authors to extend on the complementary signals and different sources of noise and also to change the sentence-construction "that present complementary (different) signals (sources of noise)".

5. The reader would benefit from a short note on the computational costs since part of the paper is a proof of concept.

6. It is worth noting that to obtain a regional or large scale reconstruction we still need a transfer function approach subsequent to the use of an inverse ecophysiological model. This transformation may be as complex as the Bayesian Hierarchichal Modelling approach. I think including references to the work by Martin Tingley on BARCAST (e.g. http://journals.ametsoc.org/doi/abs/10.1175/2009JCLI3015.1 or http://www.nature.com/nature/journal/v496/n7444/abs/nature11969.html) is appropriate to show how broad the discussions on the topic are.

7. I think it is appropriate for the paper to also reference Tolwinski-Ward, S. E., Anchukaitis, K. J., and Evans, M. N.: Bayesian parameter estimation and interpretation for an intermediate model of tree-ring width, Clim. Past, 9, 1481-1493, doi:10.5194/cp-9-1481-2013, 2013. http://www.clim-past.net/9/1481/2013/cp-9-1481-2013.html

8. I am surprised by the "below 250 ppm" phrase on page 18481 line 28. The NOAA reference is 280 ppm for pre-industrial times?

9. As said by reviewer 1: The authors should try to give explicit answers to the three questions posed in the introduction on page 18483.

10. A question: The definition of the alternative meteorological scenarios uses constant deltas. Wouldn't that allow a reduction of the complexity and the computational costs of the model by using only seasonal or monthly mean data instead of daily input. That is, if I use a constant offset anyway, the daily intra-annual variability loses part of its value.

11. Is the Euclidean distance relative to the average annual cycle or relative to the annual mean?

12. Is a uniform prior reasonable? It is an apparent choice but, as far as I remember, there have been a number of discussions on better non-informative priors. I wonder whether especially for precipitation the range is too narrow with the minimum still being 25% of the mean? Could this exclude potential extremes?

13. Do I understand it correctly that RemoISO was used in the settings of Sturm et al. (2005)?

14. The authors generally omit the probable bounds for their results. This complicates assessing, for example, the difference between the A1-ensemble and A2-ensemble results but also reduces the value of the comparison with the observations. Only Figures 6 and 7 present a suitable quantile range.

I think it is absolutely necessary to show the "uncertainty" range in Figures 4, 8 and 9.

I guess scenario lines are medians (for example in Figure 4), please clarify for all Figures.

The probability range is important to assess whether the two scenarios really are significantly different. The difference in the median is only part of the story.

Indeed even the observational uncertainty is of interest if it is easily available.

15. My impression is there are quite large differences between observations and scenario in Figure 4 for the latewood widths which could result in large uncertainties in the reconstructions.

16. The caption of Figure 4 suggests that A1 is the stable CO_2 scenario. Is that correct? This should be clarified between the various Figures and the text.

17. The temperature fit appears to be worse than the precipitation fit in Figure 6. Thus: is the site and are the trees primarily precipitation dependent; or is the model implementation of the temperature response possibly worse than for precipitation; or may this be due to the calibration of the parameters? Please discuss the different quality of the reconstructions.

18. I suggest that the discussion at the end of section 5.1 (P 18492 l 5-19) could be better presented in a later section together with other thorough discussions of uncertainty and results. Generally, I think, it is necessary to give a more thorough presentation of the shortcomings and uncertainties of the approach (e.g. parameters, daily input uncertainty, the assumption of a constant annual offset). For example: the authors mention the parameters to be calibrated and refer to the previous publications on MAIDENiso; I would appreciate at least a short discussion on the uncertainty due to these parameters.

19. Which version of the CRU data is used? A reference should be given. Please discuss the differences between CRU and the observations more clearly. The original

methods of producing CRU may also lead to differences.

20. Are the Ns given in the text really of interest, what about the effective degrees of freedom?

21. The reconstruction range/uncertainty should be displayed in Figure 8. It would also help if the target/the observations were plotted.

Considering the discussed precipitation trends in the results for Figure 8:

- It would help if the trend-lines were plotted and the uncertainty of the trend was discussed.

- How large is the trend?

- How sensitive is the trend calculation to the end points?

22. The authors appear to consider only the trend of the median and ignore the full uncertainty range.

23. To expand on the previous point: Shouldn't the reconstructions via inversion be considered as a probabilistic ensemble of possibilities instead of as a "best estimate plus uncertainties".

24. Page 18494: Is more complex really always better? Isn't the question rather whether a model is fit for the purpose?

Is it really "clear that each and every improvement made within the plant ecophysiology science community will directly translate to better reconstruction performances" (p 18494 l 13-14)?

25. In line 25 on page 18494: It's Fig. 9, isn't it?

The range should be plotted not only the median.

The time series in Figure 9 look quite different from previous plots (e.g. Fig. 8)? Please check that all Figures are correct?

Is the trend of the median meaningful if we interpret the reconstruction probabilistically?

How sensitive are the trends to the end points? What is the uncertainty of the trends? Couldn't the extreme variations in the A2-reconstruction alone result in different linear trends (especially for temperature)?

Please include the target/the observations in the comparison.

26. Which reconstruction fits better the observations?

A possible fertilization effect is shown it is implied that it possibly affects the reconstruction-result but it isn't discussed whether it has a relevant effect in making the reconstruction biased or less reliable.

27. Are the results especially dependent on one of the measures LW, d18O, d13C?

28. I think one can disagree on the "strongly related" on P 18495 line 13.

29. Page 18495 line 24. It's Fig. 10, isn't it?

It appears as if the black line is some years shorter than the red line in the end. There also seems to be an offset between both lines already in the early portion of the data? Please check the plot and the underlying data.

30. Page 18496 line 10ff: Could this be clarified and possibly extended on? I.e. what are the implications a) for the growth and b) for the reconstruction. Does this affect all parameters LW, d18O and d13C, or are some less affected?

I am not sure I understand what is discussed here: a) mechanisms that may have occurred over the last 150 years at the location, b) problems in the code, c) sources of uncertainty in reconstructing temperatures using a less complex model or d) general tree-local-climate interactions, . . .?

31. I would appreciate if the authors discussed the potential effect of the fertilization not only on the inverse reconstruction but also on the simple transfer function recon-

struction.

32. P 18496, line 25: Please discuss how the less than "fully integration" of processes affects the reconstruction and your conclusions. Is the dependence on CO2 not fully (thus only reasonably) reliable?

33. Please extend on the "divergence in climate-to-tree-growth".

34. Is the inversion really the best suited approach or just a well suited one?

35. P 18497 L26: The results show a fertilization effect but at the same time indicate that the inverse reconstruction captures the observations quite well. So far, I take away that the fertilization basically has no relevant effect on the reconstruction.

Further, the bias of the transfer reconstructions appears to be to a warmer and wetter climate than the inverse reconstruction. However that's hard to assess since the observations and the general uncertainties of both are not included in the plots.

36. Please add (where appropriate) legends to the plots according to the recommendations by the journal.

37. Finally I would like to note that I am looking forward to a possible application of the inversion approach to a millennial scale regional or large scale reconstruction.

Annotations

P 18481 l 5: infra-annual -> intra-annual

P 18484 l 15: is an another -> is another

P 18484 line 19/20: (Danis et al., 2012) -> Danis et al. (2012)

P 18489 line 11: Each tree rings were -> Tree rings were ?

P 18490 line 26: as well a -> as well as

P 18490 line 26: incorporated to -> incorporated into?

P 18491 line 24: temperature signal -> the temperature signal

P 18495 line 2: point view of -> point of view of

P 18497 l 10: nonstationnary -> non-stationary

Interactive comment on Biogeosciences Discuss., 10, 18479, 2013.