Climate
of the Past

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Discussions

Interactive comment on "Forward modelling of tree-ring width and comparison with a global network of tree-ring chronologies" *by* P. Breitenmoser et al.

P. Breitenmoser et al.

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We are very grateful for the review provided by the referee. We address the comments and questions point for point and modify the text of the manuscript to take these comments into account.

Summary and General Comments

The analysis presented in the paper is well-founded. However, with just a small extension to the work already presented, the authors could make a much more novel, interesting, and impactful contribution by providing explicit direction about building an observation operator from VSL for DA-based paleoclimate reconstructions. The method

by which they aggregate temperature and moisture-related tree-ring data separately up to the scale of the CRU grid is already strongly suggestive of a way to construct such an observation operator. At the very least, the authors should explicitly discuss how the comparison they do between simulated and observed aggregated TRW could be expanded or extended for such a purpose. [...]

Reply: We fully agree with the reviewer that the focus on the DA aspects could and should be strengthened and made more specific. We are currently preparing further papers that actually present DA using tree rings, and hence the goal for this first paper was to assess VSL. Our views of how we can use forward models in paleoclimate DA have recently been published (Brönnimann et al. 2013). There are various approaches, which we group into the three categories "covariance based approaches", "analogue approaches", and "nudging approaches". VSL can be used in each of them, but the way it is used differs depending on the approach. All approaches try to minimize the cost function $J(x) = (x\_b - x) \, B^{-1} \, (x\_b - x)' + (y-H[x]) \, R^{-1} \, (y-H[x])'$ The first group encompasses the DA methods used in NWP and for reanalyses such as the Ensemble Kalman Filter of 4D-VAR. Here VSL may serve as a starting point for formulating a well-behaving linear observation operator. Work is underway by other groups, and we do not want to duplicate this. Maybe one aspect is that our assessment will help to quantify the error variances (the diagonal of R, since in these approaches R is often taken diagonal). Another aspect is the assessment of biases relative to observations (which may be introduced as an additional term in the observation operator). In contrast, analogue approaches such as the Particle Filter choose among existing simulations and hence the first term drops out of the cost function. In these approaches, VSL (or also VS) can be used directly as $H(x)$ without further adaptations. Moreover, in contrast to many implementations of the Ensemble Kalman Filter, the observation error covariance matrix can easily be non-diagonal (see comment below). In our example given in Brönnimann et al. (2013), R was in fact non-diagonal, and the estimation of R was based on the paper under discussion here. One of the important points in our paper thus is to allow estimations of the full R matrix. In the third approach, nudging,

the cost function is not solved explicitly but rather small increments are added to the tendency equations. These increments depend on a target, which might be a "classical" climate reconstruction. In this case, VSL may guide the aggregation of tree rings in order to increase the signal-to-noise ratio in classical reconstruction approaches. We have included this information in the manuscript

Whether or not the authors choose to shift the focus to paleoclimatic data assimilation, the paper requires reorganization to clarify the main points of the study. The goals of the work as outlined in the abstract and introduction should be clearly revisited in the Results, Discussion, and Conclusion with the related findings for each goal. The methods, results, and discussion are also interleaved at present, which adds to the confusion and blurring of the main findings. I suggest separating the "Results and Discussion" sections (where "Results" should be an objective report of the findings, while "Discussion" should contain interpretation and speculation, and discuss potential applications and connections to existing literature). The aggregation procedure described in 3.4.1 is quite important, and should be described in the "Methods" section. Most of the specific comments below are suggestions for re-structuring and focusing the paper. Additionally, in many paragraphs throughout the paper, the analysis is described in the order it was performed, rather than in the most logical presentation order. More specifically, often a key result, number or method comes only at the very end of a paragraph, rather than at the top where it would be more clear for the reader. Reply: Many thanks for this comment. We agree that the structure could be done better and restructure our paper accordingly.

Specific Comments Abstract

line 9, Goal a): suggest making this goal more specific. The authors want to examine the relations between simulated and observed series for what purpose?

Reply: Rephrased: "(a) to assess the VSL model performance by examining the relations between simulated and observed growth at 2287 globally distributed sites"

C2871

line 10, Goal b): as VS-Lite is a forward model, it has no intrinsic potential for reconstructing past climate on its own, as paleoclimatic estimation is an inverse problem. Again, a more specific phrasing may be all that is needed here, eg. " . . . to evaluate the potential of the VSL model as an observation operator for data-assimilation based reconstructions of climate from tree-ring width."

Reply: Sentence is changed according to the suggestion.

line 11-13: Is the result about the parameter estimates a key finding of the study that belongs in the abstract? Constraining the model parameters is not listed as a main goal of the study. If it is, add it to the list of goals. If not, eliminate this sentence (and consider condensing section 3.2).

Reply: It is a main aim, and it is now added as "(b) evaluate optimal growth parameters found during the model calibration"

Introduction

pp. 4067, line 16: see also work by N. Steiger for relevant paleo-DA research, http://www.atmos.washington.edu/nathanjs/

Reply: We add the reference to N. Steiger (Steiger. N. J., Hakim, G. J., Steig, E. J., Battisti, D. S., and Roe, G. H.: Climate field reconstruction via data assimilation. J. Climate, 26, submitted, 2013.)

pp. 4068, line 10-11: As discussed above, the VSL model is NOT a candidate in itself for paleoclimate reconstruction, as the reconstruction problem is an inverse problem and VSL is a forward model. As above, revise for specificity and accuracy (eg. make it clear that the authors mean as an observation operator in DA contexts, or more generally as a link between the proxy data and climate variables in any reconstruction methodology that can support use of a forward model).

Reply: Sentence is rephrased according to the suggestion.

C2872

pp. 4068: line 12-15: Here 3 main goals of the paper are listed, which conflicts with only two main goals listed in the abstract. Decide what the main goals of the paper are and outline them clearly and unambiguously.

Reply: We repeat here the aims from the abstract

Data and Methods

pp. 4069, line 11-16/Figure 1: To support the discussion of the relative density of the TRW and instrumental networks here, it might be useful to include a subpanel or subpanels in figure 1 showing the locations or density of instrumental coverage at one or more points in time.

Reply: We appreciate this comment and will provide additional information / references to clarify the locations of the instrumental data.

pp. 4069, line 19: Version 2.3 (not 2.2) of the VSL model is accessible from the link given.

Reply: Noted and corrected.

pp. 4071, line 4-6: Describe the Bayesian parameter approach in detail here, unless it is the estimation procedure of Tolwinski-Ward et al (2013), in which case cite this reference at first mention.

Reply: We did use the Tolwinski-Ward et al (2013) parameter estimation. This has now been clarified.

pp. 4071, line 14: unclear what correlation constraint is imposed and referenced here.

Reply: This refers to the fact that the parameters of the VSL model were tuned to the observed tree-ring data, which may contain some noise and thus result in model overfitting. We have attempted to make this more clear.

pp. 4072, line 14: Start the first paragraph by describing the number of chronologies

in the network that were actually used in the study, and the state in which they were actually used (eg. 2287 standardised chronologies). Explain the criteria that were used to filter from the ITRDB after (perhaps more briefly than is presently done).

Reply: This has been revised.

Section 2 should also include a description of how aggregated chronologies were constructed from the individual site chronologies;

Reply: The description is moved to the Methods Section.

Section 2 should also include a brief but comprehensive description of the structure of the study or experimental design. List the sets of analyses run, and explain that these were run on simulations of two sets of data: the individual-site chronologies, and the aggregated chronologies.

Reply: A section on the experimental design is added to the revised manuscript.

Results and Discussion

Strongly suggest splitting this section into two sections (Results and Discussion) for clarity.

Reply: Thanks for the suggestion; the result Section is now split into "Results" and "Discussion"

subsection 3.1: It might be interesting to show the VSL skill as a function of some of these different indicators of climatic signal (EPS, Rbar)

Reply: Fig. 1 shows for each site the Pearson correlation coefficient between TRWVSL and TRWITRDB with the corresponding values for a) EPS and b) Rbar.

pp. 4074, line 20-27: Was independent noise assumed, or AR(1) noise structure at each site? Analysis and reporting of estimated noise variance or signal-to noise ratio also seems conspicuously absent.

Reply: Independent noise was assumed here as described in Tolwinski-Ward et al. 2013.

pp. 4075, line 6: note that consistency across intervals is also indicative of stability in TRW response to climate over time

Reply: Sentence is changed to reflect this.

p. 4075, lines 10-18: Note that there is some dependence between results here. Tolwinski-Ward et al (2013) drew on the conclusions of Körner (2012), Korner and Paulsen (2004), and Rossi et al (2007) to develop priors for their study. If the uniform priors used in this study are identical to the defaults for uniform priors in the algorithm of Tolwinski-Ward et al (2013), then the choice of prior support has also been influenced by these studies.

Reply: We did not use the exact same priors as Tolwinski-Ward et al. (2013) but do note that our choice of priors have been influenced (as they should) by the previous knowledge. We will clarify the significance of the fact that results converge particularly in light of the wide range of priors considered.

pp. 4075, lines 1926: if the differences are really insignificant in the statistical sense, then the authors should follow the procedure that makes the most sense before seeing the results, and the discrepancy need not be discussed. However, why not correct site-by-site rather than making an average correction, which will improve things at some sites but degrade the realism at others? Note too that the parameter estimation should be carried out after any corrections to temperature inputs for elevation.

Reply: We did test this correction on a site by site basis, and have made this more clear. We are not convinced that makes more sense to assume some lapse rate also given uncertainties in the reporting of site elevation. Hence we did test this and reported upon the results.

pp 4076, line 1-10: These species-specific results are very interesting, especially since

the effect of species on parameter choice was neglected by Tolwinski-Ward et al (2013). Can the differences in parameter estimates by species be accounted for by the differing distributions of species at different elevations? Do statistical tests of differences between empirical CDFs show significant differences between the marginal posteriors across species?

Reply: Fig. 2 shows for each site values of the threshold temperature T1 at the corresponding elevation for different tree genera: a) pine, b) spruce, c) fir, d) hemlock, e) larch, f) cedar, g) juniper, h) oak, and i) nothofagus.

Since the authors have used a Bayesian approach to carry out this analysis, statements about the difference in threshold parameters between species (eg. line 6) can and should be made more quantitative by reporting posterior median and credible intervals for the difference.

Reply: We have included the mean posterior parameter values for T1 in the manuscript.

The statements about joint posterior relationships between parameters (discussed pp. 4076, lines 16-24) should also be supported graphically; the authors might consider including 2D scatterplots or contours of posterior probability for all pairs of parameters being estimated.

Reply: Fig. 3 shows for each site the posterior relationships between parameters a) T1 and T2, b) M1 and M2, c) T2 and M2, and d) T1 and T2.

pp. 4077, line 6-7: the parameter tuning at each site should take care of this problem (if the correction for elevation was correctly applied before the parameter estimation)

Reply: Yes, it does. However, for these very few sites, parameter tuning was not possible due to very different climatic conditions influencing real tree growth and at the climatic station where tree growth was simulated.

pp. 4078, line 18-20: This comment about simulations under projections of future climate change is an example of a discussion point that properly belongs in a "Discussion"

section.

Reply: The sentence is shifted to the Discussion.

pp. 4079, line 10-11: Fair assumption, but note that an interpolation of the CRU field to the particular location of the TRW site might be an even more defensible assumption, especially for TRW sites that fall near the boundaries of a CRU gridcell.

Reply: We stick to the closest grid point in order to preserve variance.

pp. 4079, line 14-16: "We further demonstrated that joint influences of temperature and precipitation on tree-ring growth is implemented within the VS-Lite model, and hence, no separation of the climatic influences on growth is needed"– unclear what this means; sounds like a reference to a conclusion that has not yet been drawn on the basis of analysis performed within the paper.

Reply: The sentence is now omitted.

First paragraph of section 3.4.1: This is another example where what the authors are about to do/explain should be stated in broad-brush terms at the top of the paragraph (eg. "We perform aggregation of temperature and moisture sensitive chronologies up to grid-scale in order to reduce the signal-to-noise ratio") before describing details. The details of the aggregation (pp. 4079, line 24 pp. 4081, line 19) should go in the "Methods" section.

Reply: The details of the aggregation have been moved to the Methods section and the Results section was correspondingly restructured.

pp. 4079, last paragraph: Another paragraph that would benefit from re-ordering. Before launching into details of search radius, explain overall goals: searching for what within each radius? How will the results of radial search be combined? The reader should have a sense of how the authors plan to "aggregate" before the term is used on line 10 of pp. 4080, as well as a broad-brush idea of how the "principle climatic drivers on growth" will be determined.

Reply: See above - the details of the aggregation have been moved to the Methods section and the sentence is rephrased.

pp. 4080, line 16-17: Suggest accounting for the weighting by the relative daylength (g_E) within VSL when "counting" the moisture vs. temperature-limited months. At most sites in the Northern Hemisphere, for example, January will be "temperature-limited", but the growth response from January will contribute very little to the overall signal variability because it gets downweighted by the relatively short daylength. If this is not accounted for, the scheme you suggest will likely bias locations to be classified as temperature-controlled.

Reply: We only considered months during the growing seasons (i.e., $g>0$) for temperature and precipitation classification. This has now been clarified in the text.

pp. 4081, equations 6-9: Yet another example where re-ordering is necessary. On first read, it was unclear what the weights would ultimately be used for, as the authors do not simply state up front that the aggregation would be performed by a weighted average of chronologies within the given radius. Suggest moving the last sentence of this subsection to the top of the paragraph (before the equations for the weights). Reply: Ordering changed.

pp. 4082: Lines 46 ("the 600 km search radius improves. . . ") is a result that belongs in a "Results" section, while the information in the following sentence ("We. . . introduced the condition that at least one chronology. . . ") belongs in the separate "Methods" section.

Reply: We have moved the sentence to the "Methods".

The main result of this paragraph is again stated in the last sentence, but should be moved to the top ("ATRW VSL and ATRW ITRDB show spatial coherency and capture the main climate signals. . . "). Additionally the presentation of the result in its current form is unconvincing, in part because the figure is not constructed to show the results

clearly. I suggest rearranging figure 5 (perhaps 2x2 subpanels, rather than 4x1, so that the size can be increased and the reader can see what is being discussed without squinting; a change in color scale is also strongly suggested so that negative and positive correlation coefficients can be easily distinguished by eye; perhaps statistically insignificant correlations can be shown as open circles with no color; also all color axis labeling and titles should be in larger font.) In a revised figure I would hope to be able to easily see the stated result that the increased search radius improves the comparison between simulation and observation, as well as the other spatial features discussed pp. 4082 lines 9-20. Consider adding additional figures as well to support the claims in this paragraph.

Reply: The sentence is moved to the beginning of the paragraph. We agree with the referee that in the CPD version this figure was exceptionally tiny. We increased the size of the text and the figure size to increase visibility. We hope that the main results are now (and will be in the final typeset manuscript) more clearly visible for the readers.

Conclusions

pp. 4083, line 22: "performs well" should be replaced with a more specific finding, perhaps "produces parameter estimates that are stable with respect to the choice of calibration interval."

Reply: Changed.

Again, the authors should consider whether parameter estimates of T 1 from this study should be a focal point. If not, and the result is actually more of a discussion point, then it should be excluded from the abstract and conclusions.

Reply: We find this is a sufficiently important point to emphasize in the abstract and conclusions. This was also indicated by our changes to the abstract as indicated in the the response to lines 11-13.

pp. 4084, line 5-6: not clear that any figures or results presented support the claim that

VSL shows "notable skill at locations with a less extreme climate due to the model's explicit consideration of joint temperature and moisture controls on modelled tree growth." If the authors want to keep this statement in the conclusions, it should be supported with evidence and discussed in the body of the paper.

Reply: Sentence omitted.

Figures

Figure 3: It is generally preferable to show a kernel smoothing of the numerical output to visualize the posterior probability densities than to joint the tops of a histogram (as through the function ksdensity in MATLAB, for example). As stated above, since the joint posterior relationships are also discussed though (pp. 4076, lines 16-24), the authors might consider including 2D scatterplots or contours of posterior probability for all pairs of parameters being estimated.

Reply: We have included the mean posterior parameter values for T1 in the manuscript.

Figures 1 and 5: Suggest change of color scale that makes negative/positive correlations more easily identified by eye.

Reply: We believe that the color scale from red to blue is clear enough.

Figures 3, 5 and 6 need larger font sizes. 3

Reply: Done.

Typographical and Stylistic Comments

pp. 4073, line 2: Suggest "These tests and Pearson correlation analysis. . . LEAD US TO favour a hierarchical approach. . . " so as not to anthropomorphize the tests and analysis.

Reply: Changed.

pp. 4074, line 8-9: "Typical tree-ring chronologies have a mean segment length of. . .

" How are the set of "typical" trees determined over which to take the mean? Or do the authors just mean the mean provides a measure of the typical segment length over all trees?

Reply: This has been revised.

pp. 4077, line 21 and elsewhere: suggest changing "verification" to "validation." The former word has its roots in the Latin word for "truth", and of course one can't expect a model to exactly represent the truth, but only to provide a valid approximation to the truth.

Reply: Changed.

pp. 4078, line 12: re-word "the high-elevation site is negatively correlated with. . . ", as it's not the site that is correlated with growth, but the simulated time series at the site in question.

Reply: Changed

pp. 4083, line 12: "promote to" should be "promotes the"?

Reply: Changed.

pp. 4084, line 1-3: "Moreover, our results demonstrate the VSL model skill to simulate in response to climate variation. . . " Revise for proper English.

Reply: Changed.

pp. 4084, line 14-15: ". . . by using in data assimilation approaches. . . " Revise for proper English.

Reply: Changed.

Reply: References Körner C (2012) Alpine treelines: functional ecology of the global high elevation tree limits. Switzerland: Springer Basel Korner C, Paulsen J (2004) A world-wide study of high altitude treeline temperatures. J Biogeogr 31:713–732,

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DOI 10.1111/j.1365-2699.2003.01043.x Rossi S, Deslauriers A, Anfodillo T, Carraro V (2007) Evidence of threshold temperatures for xylogenesis in conifers at high altitudes. Oecologia 152:1432–1939, DOI 10.1007/ s00442-006-0625-7 Tolwinski-Ward S, Anchukaitis K, Evans M (2013) Bayesian parameter estimation and interpretation for the vs-lite model of tree-ring width. Clim Past Discuss 9:615–645
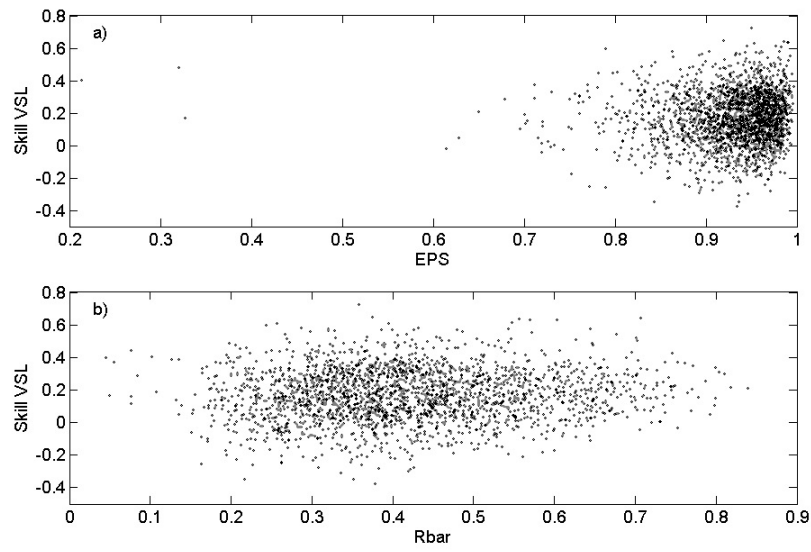
Fig. 1. Pearson correlation coefficient between TRW VSL and TRW ITRDB for each site with the corresponding values for a) EPS and b) Rbar

C2883

C2884

a)

24
22
20
18
16
14
12
10

T2

0   2   4   6   8   10

T1C2885

b)

0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1

M2

0.02   0.022   0

M1