Clim. Past Discuss., 9, C1822–C1833, 2013
www.clim-past-discuss.net/9/C1822/2013/

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

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Received and published: 25 August 2013

General Comments

Through their paper, "Forward modeling of tree-ring width and comparison with a global network of tree-ring chronologies", Breitenmoser et al. make what will be a substantial contribution to the dendrochronological literature. Their global comparison of tree-ring width simulations via the process-based VSL model with actual tree-ring width chronologies represents the first step in moving towards development of transfer functions for climate reconstructions based on a process-based model. Although a number of previous papers have explored the value of the Vaganov-Shashkin or Vaganov-Shashkin Lite models for specific regions, this is the first to do so at a global scale and therefore provides information that is potentially useful to dendrochronologists

worldwide. The analysis done is scientifically valid and, in general, the main issues surrounding the methodology concern the description/discussion of it rather than the methodology itself.

In order to maximise the value of the paper, however, two major issues require attention. The first of these relates to the structure of the paper. The authors need to clearly separate methods, results and discussion. The current structure of the paper means that it is difficult to read, some important methodological information is omitted, information is out of order, and there is repetition. In addition, the reader is not left with a clear impression of the importance or relevance of the results. Improving the structure of the paper would go a long way to correcting these issues. The single Results and Discussion section should be split into separate Results and Discussion sections.

The second issue is the focus of the paper. If the Discussion were much more clearly and closely aligned with the questions raised in the Abstract and Introduction, this would help. A tightened focus of the paper and deeper exploration of the results would provide the basis for a much more insightful discussion. This would make the paper a considerably more useful reference source for others wishing to explore the use of process-based models for simulation purposes, and/or the subsequent development of transfer functions for specific species/regions. One particular aspect that seems to appear as 'an extra' is the discussion concerning threshold temperatures for growth. While this may be an interesting point to make in the discussion, it does not really constitute – in my opinion – a major point of the paper and is probably best either omitted or considerably de-emphasised. The abstract also indicates that investigation of the climate reconstruction potential of the VSL is a goal of the study. There are at least two issues with this goal. Firstly, as also pointed out by Reviewer 1, the model as it stands is not a model for reconstruction, but a forward model. Second, there is no real attempt to discuss whether the VSL model could be a basis for the development of transfer functions, and, if so, how. In this regard, the Discussion could be considerably enriched by greater consideration of where the model does and does not perform well.

For what species groups, site types, geographical regions might the VSL provide a good basis for transfer function development? What issues with climate data should be considered in some depth when discussing the results? (A number of the comments below relate to these aspects). If such a discussion were to be included and goal b) in the Abstract revised somewhat, this would help better focus the paper. Having assessed how well the VSL performed with respect to various species/regions, the authors could then discuss with greater clarity what the next step towards development of transfer functions should be. This would give the paper not only a very clear direction as a paper but perhaps broaden discussion within the dendrochronological community concerning more wholistic ways to develop transfer functions that make use of more of the available climatic information. More rigorous discussion and investigation along these lines is sorely needed, and this paper has the potential to become a catalyst for accelerating this discussion.

Greater consideration of model priors and assumptions implicit in the model itself is required: how applicable will these priors be to non-European or non-American species? It is very clear that the model (especially in relation to the aggregated data), performs particularly well in the southwestern United States and across northern Europe/Russia, but why doesn't it perform well in other regions? Is it related to model priors and implicit assumptions? Are these assumptions/priors more suitable for some environments/species/regions than others? In the case of the VS model, for eg., Vaganov et al. (2011) indicate that further work examining the appropriate model inputs for specific species is required. One example that relates to evapotranspiration are important differences that exist in the stomatal response of various species to environmental conditions which will affect responses to temperature and precipitation (see, for e.g. Brodribb and Cochard 2009).

Another possibility that needs to be considered is that, in some cases, poorer model performance may be related to the climate data. While the CRU data set is an obvious and entirely valid choice of global climate data to use in this study, greater attention

needs to be given to possible issues associated with the CRU data when discussing results, not just when introducing the data set (p. 4069). The 0.50 x 0.50 resolution of the data set is likely to be problematic in areas of highly variable topography and may therefore affect the results of the model. Tasmania (Australia) and southern New Zealand are cases in point – correlations between the TRWVSL and TRWITRDB are relatively weak. Significant rain-shadow effects (wet west coasts, dry east coasts) for both small land masses mean that a number of CRU cells will include both wetter parts and drier parts of the respective land masses and therefore possibly affect the ability of the VSL (using CRU input data) to simulate tree-ring chronologies in regions like these. In addition, shorter and fewer records in the early parts of the twentieth century are likely to affect a number of regions, including Tasmania, southern New Zealand and parts of Asia in particular. This may be important when considering consistency of results across the identified calibration/verification periods in some regions.

Specific comments

Abstract

In general, the Abstract needs to be more focused on the main objectives, results and implications of those results.

Is the finding in regard to temperature threshold for growth onset a key one? Does it belong in the abstract? It does not seem to be a central focus of the paper and should probably be removed from the Abstract and Conclusions.

Introduction

p. 4067, l. 8. Uniformitarianism. The point here is not that the calibration of these empirical models is bound by uniformitarianism per se, rather, it is that there is an assumption that the relationship to the SINGLE climate variable of interest (e.g. temperature) remains the same, regardless of available moisture or other factors. This is not the same as an assumption of uniformitarianism. In contrast, the process-based model does not examine temperature (for example) “while holding all else constant”, and is thus able to produce a non-linear response to a single variable. A simple re-wording is all that is needed here. Similarly, p. 4067, l.29 – p. 4068, l.2, requires some careful rewording.

p. 4067, l. 10 – 14 and l. 20 – 24. Both of these points can be built upon in the Discussion as part of ‘where to next?’

p. 4068, l. 18. “All suitable...” What does suitable mean? This is really a matter to be addressed in the Methods section rather than in the Introduction.

p. 4068, l. 19 – 20. “Results of “...information in climate reconstructions”. This would be better placed in the Discussion along with the required discussion of evidence from this paper.

p. 4068, l.25. Suggestions for future analysis are not really made in this paper but are certainly something that should be discussed.

Methods

p. 4068, l. 9 – 16. Some comments are made here about spatial interpolation and density of the tree-ring and instrumental records. Explicit consideration of temporal variability is also required - e.g. how many Southern Hemisphere instrumental stations extend as far back as 1900 compared to the number that extend back to 1960? (and compared to the NH?) Temporal changes in density of instrumental stations may be more important in some regions than others. This deserves some comment.

p. 4069 l. 18 – p. 4070, l.7. This section largely repeats what has been said in the Introduction about the VS and VSL models.

p. 4071, l.10 – l. 16. Is it possible to show some of this information in the supplementary material? Also, please explain WHAT the possible advantages for the Bayesian parameter estimation are rather than forcing the reader to visit Tolwinski-Ward et al. 2011).

p.4072, l.6 – 11. Choice of growth period window. The 16-month window does not capture the full previous season. September (NH) and March (SH) represent the end of the previous growing season. To incorporate the previous growing season surely the window should extend back to May/June (NH) and Nov/Dec (SH)? A simple statistical selection of ‘optimal season’ does not necessarily match reality. As an aside, what subset of regions was used to test this window? It is not necessary to simply mimic what was done by Tolwinski et al. (2011).

p. 4072, l. 12. Some detail about how the TRWVSL chronologies were standardised would be useful.

Results and Discussion

p. 4073, l. 21. “Various chronology characteristics...” Be more specific – ie. RBar and EPS provide some information about chronology quality. They are also only TWO statistics.

p. 4074, l.8-9. “Typical tree ring chronologies...” This sentence should be rewritten such that it is clear it applies to the chronologies used in this study. Does ‘typical’ imply ‘average’, or perhaps ‘median’? Be more explicit.

p. 4074, l.12. Did the ability to test modelling of TRW at the interannual and multi-decadal scales partly drive the choice of selected detrending option? If so, this should be mentioned as part of the methodology.

p. 4074, l. 18 – 22. See comments above regarding use of CRU data.

p. 4075, l. 5. “...were calculated independently...” It should be made obvious from the text preceding this sentence that the response parameters were calculated separately (and independently) for each of the two periods.

p. 4075, l. 6. What does “skill” mean? Not statistically different across the two periods? Would consistency better describe what is meant here? Also, the statistics shown in Table 2 are just summary statistics, but if results were compared on a site-by-site basis would they be as apparently consistent across the two periods? This needs to be drawn out. The inclusion of some additional information about inter-period variability at the site level would be useful here and would better support a statement about “skill” or “consistency”.

p. 4075, l.11. Were there any ‘significant’ differences by region and/or species? If so, these should be mentioned if a discussion of growth-onset temperature is considered an important part of this paper.

p. 4075, l.19 – 24. If a correction for elevation is considered appropriate, then it should be made. A statistic should not be the final arbiter of what is done here. Potential reasons for poorer results after a correction for elevation could be non-constant lapse rates and/or interaction between topography and changing climatic conditions.

p. 4075, l. 27 – p. 4076, l.9. This is somewhat confusing - can these specific examples be incorporated into the more general discussion about Figure 3 (p. 4075, l. 7 – 18)?

p. 4075 l. 7 - p. 4076 l. 28. This section appears as an interjection between a discussion of calibration/verification periods and the value of the VSL model for simulation of tree-ring series. The section needs to be re-ordered (or perhaps omitted) in order to better present the findings to the reader.

p. 4076, l. 9-10. The Babst 2013 paper only deals with tree ring sites in Europe, and a restricted number of species, and yet 163 species were potentially used in this study (p. 4072, l. 14). To use the phrase “. . .a wide range of species. . .” is inaccurate. Please rephrase this.

p. 4076, l. 11 – 13. As mentioned above, with respect to stomatal control, some acknowledgement of physiological difference amongst species is important because it may have important implications for performance of a model using assumptions not suitable for specific species, and point to the need for fine-tuning of the model in some cases.

p. 4076, l. 25 – 27. This point should be expanded upon in the discussion.

p. 4077, l.11. Some discussion of success by region/species groups would be useful for future studies. For example, it would appear that sites in southern New Zealand and Australia were relatively poorly simulated, although the correlation between them may have been statistically significant (it is difficult to tell because the Figure is so small).

p. 4077, l. 16 – 19. What is the calibration period here? It sounds as if there is only a single calibration period used, and yet three lines later, the two calibration periods are mentioned. The methodology used needs to be more explicit. Splitting methods, results and discussion into separate sections would help clarify what was done.

p. 4077, l.19. “. . .significant relationships are globally fairly well distributed. . ..”. There are also very clear areas where there are (generally) relatively poor relationships (e.g. Australia/NZ, Alaska, a NW/SE band from central Canada to NE USA, the northern part of the British Isles and parts of southern Europe. The statement made here needs to be tempered.

p. 4077, l. 24.“($r > \sim 0.44$). It would be useful to include the relevant p-value here also to remind readers what ‘significant’ means in this study.

p.4077, l.26-27. Can a table/figure be added to show model stability by geographic region/species?

p. 4077, l. 20 – 25 cf. p. 4075, l. 1 – 6. Both these sections refer to calibration/verification. Some re-organisation into separate sections for methods, results and discussion would help clarify what is being done to what data.

p. 4078, l. 11 – 15. Please rephrase this for clarity (e.g. it is not necessarily clear in text what the correlation values relate to).

p.4078, l.15 – 16. “. . .for sites in Switzerland. . .” should refer to the TWO sites examined here and not imply all sites in Switzerland. In addition, are these differences also typical of other regions besides SW USA and for other species? (e.g. more extreme/less extreme climates, Asia/Oceania, South America?) Some comment on this would be useful.

p.4078, l.21 – 27. – See comments above regarding selection of 'climate window'.

p.4079, l.5 – 6. This needs rephrasing.

p. 4079, l.9 – p. 4082, l. 2. The explanation of how the aggregation was done belongs in the Methods.

p. 4079, l.22. How important might changes in limitations over time be?

p. 4079, l. 26 – 27. "…and the latter reflects the large scale at spatially coherent precipitation and especially temperature patterns…" This is not entirely correct in highly topographically variable areas (e.g. New Zealand, southwestern South America). Also, some (even relatively small) regions subject to complex interaction of multiple broad-scale climate drivers may not demonstrate 'spatially coherent precipitation patterns'. Rephrase this paragraph.

p. 4080, l.11 – 13. See comments above regarding the CRU data.

p. 4081, l.4. "…we account for persistence…" See earlier comments on climate window/persistence.

p. 4081, l.5-6. "…and also account for…" This needs to be rephrased, differing growing seasons are not 'accounted for'; rather, the 'months used' are the same for all sites. If differing lengths of growing season were 'accounted for', a variable algorithm would be used to select the relevant portion of the Nov-Mar (SH) or May-Sep(NH) window for different sites.

p. 4081, l.7 – 19. This section needs to be reordered. Explain rationale first and then define weighting functions (but in a methods section).

p. 4081, l. 24 – p. 4082, l.2. Rather than say "For instance…" perhaps a table could be included to compare correlation coefficients for both temperature- and precipitation-

limited sites for both the TRW and ATRW (for both the 200 and 600km radii).

p. 4082, l.28 – p. 4083, l. 4. Redefine the calibration period in brackets here as a reminder to the reader. This passage is confusing and needs to be rewritten and the meaning of Figure 6 needs to be more clearly explained –also see comments on Figure 6.

p. 4083, l.7 – 15. It is probably worth including a figure/table to define these regions more clearly. Be more explicit about what is meant by "model skill" being "stable" - this would also enable more specific observations about model performance. Be careful not to overstate model performance – it would be more useful to carefully describe where/for what species the model performs best and then also indicate regions/species where it performs poorly. Some discussion along these lines is likely to lead to important comments about model strengths/shortcomings.

p. 4083, l. 23 – 24. This statement needs to be qualified e.g. what about the northern UK/southern European/Australian/NZ sites? Also, some comment on the 200km vs. 600km radius ATRW should be made in the conclusions.

p. 4083, l. 24 – p. 4084, l. 1. Is this a major finding for this study? Should comment on this be confined to the Discussion only?

p. 4084, l. 1 – 3. This more or less just repeats the statement at p. 4083, l. 20 – 24.

p. 4084, l. 5 – 7. This finding is not drawn out, with examples, in the body of the paper.

p. 4084, l.14 – 15. This last sentence (and the conclusions in general) needs to be revised. The nature of the revision of the conclusions as a whole will obviously depend on the authors' decision on what the final focus of this paper should be.

Figures

Figure 1 – This figure needs to be larger.

Figure 2 – It is not clear that the horizontal axes represent classes (or bins).

Figure 5 – This figure is too small.

Figure 6. It is not entirely clear what the scatter plots show – i.e. is the first scatter plot showing correlations between ATRWVSL and ATRWITRDB for the 1901 – 1935 period or is it showing correlation between ATRWVSL 1901 – 1935 and ATRWVSL 1936 – 1970? Should there be 4 panels for this figure? The figure is also too small.

Technical notes/typographical

p. 4067, l. 20. "...including also..." change to "...also including..."

p. 4068, l. 3. "...yet applications...has..." Plural/singular.

p. 4069, lines 10 – 11. "...of the underlying observations...and interpolation over greater distances is routinely performed..."Greater than what/where/when? Although more detail is given in the following sentences, a slight re-arrangement/rewording of information here would be an improvement.

p. 4072, l. 3-4. "We tested different standardising..." should be "We tested different methods of standardisation...". "To infer the most applicable..."This also needs to be reworded.

p. 4073, l.14 & l. 15 – 16. Inappropriate tense change in same sentence: l. 14 "...required..." and l. 15/16 "..is..."

p. 4073, l.20 – 21. "...is essential to interpret..." Reword, eg. "...essential for the interpretation of..."

p. 4074, l. 11. Should "Mean segment..." be "Mean segment length..."?

p. 4074, l. 15. "amount" should be "number"

p. 4076, l. 11. The sentence beginning, "As to what concerns the moisture..." needs to be rephrased

p. 4077, l.7. "...remained..." should be"...remainder..."

p. 4077, l.9. Do the authors really mean that the Indonesian tree rings were 'identical'? This needs to be rephrased to explain exactly what is meant here.

p. 4080, l. 10. " ...aggregated separately..." change to"...separately aggregated..."

p. 4080, l. 25. "...weighted mean..." should be"...weighted means..."

p. 4083, l.11 – 14. Needs to be rephrased with attention to the English.

p. 4083, l. 18 – 21. Rephrase this sentence with attention to the English.

p. 4084, l. 5 – 6. "...due to the models..." should be "...due to the model's..."

p. 4084, l. 9. "...and resulted in an improved relationship..."

p. 4084, l. 10 – 15. This sentence is ambiguous.

References

Brodribb, TJ and Cochard H. 2009. Hydraulic failure defines the recovery and point of death in water-stressed conifers. Plant Physiology 149: 575-584. Tolwinski-Ward, S., Evans, M.N., Hughes, M.K., Anchukaitis, K.J. (2011) An efficient forward model of the climate controls on interannual variation in tree-ring growth. Climate Dynamics 36: 2419 - 2439 Vaganov, E.A., Anchukaitis, K.J., Evans, M.N. (2011) How well understood are the processes that create dendroclimatic records? A mechanistic model of the climatic control on conifer tree-ring growth dynamics. Ch. 3 in Hughes, M.K., Swetnam, T. and Diaz, H. (Eds.) Dendroclimatology progress and Prospects, Developments in Paleoenvironmental Research 11, Springer Science+Business Media, New York

Interactive comment on Clim. Past Discuss., 9, 4065, 2013.