**Biogeosciences
Discussions**

Interactive comment on "Process based model sheds light on climate signal of mediterranean tree rings" *by* R. Touchan et al.

R. Touchan et al.

rtouchan@ltrr.arizona.edu

Received and published: 26 January 2012

Dear Dr. F. Campelo,

We would like to thank you for your suggestions and comments. We believe your comments and suggestions will improve and enhance our manuscript. In the paper we demonstrate the use of a process-based tree-growth model, the VS-model, for eco-physiological research in Mediterranean environments. The results are encouraging in adequate simulation of integral tree-ring growth in the sample application. Most of published papers concerning the VS model using are devoted to simulation of tree-ring growth in boreal zones. Our paper goes beyond this in demonstrating the potential of the model for semiarid regions of the Mediterranean. Authors' response to specific comments:

C5482

Comment #1. "Although the manuscript is clearly organized, the description of the model might be slightly improved. I would suggest that the Authors give some bibliographic support for the choice of some parameters, since this selection is a crucial step to obtain a robust model. For example, the value of "Minimum cambial cell growth rate" (Vcr) is of extreme importance to understand the behavior of tree growth throughout the year: Vcr = 0.2 -> One short growing season (in spring) with winter dormancy in cambial activity (temperate Mediterranean type). Vcr = 0.1 (the value selected by the Authors) -> Tree with a double stop (in winter and summer) and two growth peaks (spring and autumn), that is the typical bimodal growth of some trees in the Mediterranean areas (adapted Mediterranean type). Vcr = 0.05 -> Only one stop (in summer) and a bimodal growth pattern. Vcr = 0.005 -> Tree growth all year round. Curiously, these values of Vcr identify the four types of tree-growth proposed by Cherubini et al. (2003) for tree-ring formation in Mediterranean environments (after, Liphschitz and Lev-Yadun 1986). Additionally, the duration of the growing season is extremely dependent on the parameters of the model (e.g. Vcr)."

Authors' Response

Yes, Dr. F. Campelo is absolutely right, simulations by the VS-model are sensitive to settings of parameters. The basic simulation criterion in the paper was to obtain a realistically modeled tree-ring index for an "average tree" (tree-ring chronology) by step-by-step value changing of each parameter. In fact, with the VS-model we have a multidimensional parameterization problem. Attempt to resolve this problem have been made (Anchukaitis et al., 2006; Ivanovsky, Shishov, 2010). The authors of the papers cited used a modification of VS-model. In this paper we used the basic algorithm published by Vaganov et al. (2006) without any modification . We tried to show perspectives of VS-modeling for a new region (Mediterranean area).

Of course, the four types of tree-growth proposed by Liphschitz and Lev-Yadun (1986), Cherubini et al. (2003) for tree-ring formation took place in some Mediterranean regions and demand special detailed research included VS-simulation of cambial activity.

C5483

But to actually check cambial activity for agreement with the model we need to analyze new data, particularly, direct cell measurements and cell dynamics during the growing season based on multiyear natural experiments. We plan to do it in the near future, but it is beyond the scope of the present paper.

Changing the value of Cr according to the reviewer's suggestion would lead to a lower correlation between the simulated growth curve and initial tree-ring chronology (excluded 0.1). So, what Cr value is correct? We have selected a value of Cr (Cr=0.1) which provides best fitting of initial tree-ring chronology for the calibration period. The step-by-step procedure of basic VS-model parameterization was conducted for the whole set of VS-parameters.

Comment #2. "In the discussion of the results, the Authors underline that the "high positive correlation (r = 0.63, p < 0.001)" found between the initial chronology and the estimated chronology for the verification period (1959-1981) "confirms that VS-model can be used to estimate a non-linear tree-growth response to climate changes in the past for specific local drought conditions". However, no comparison with the traditional "response functions" was performed. Could the Authors compare the VS-model with the "response functions"? This kind of comparison could support the idea that the VS-model performs better than "response functions", when both temperature and precipitation could be the limiting factor (see Fig. 3). Throughout the discussion the Authors compare their results with only two previous studies, those from Kleeberg and Koplitz-Weissgerber (1983) and Vaganov et al. (2006). Please relate your findings with those of Anchukaitis et al. (2006), Evans et al. (2006), Vaganov et al. (2006), Touchan et al. (2008), de Luis et al. (2011), Nijland et al. (2011) and Touchan et al. (2011). Additionally, a reduced number of references was used (27), and some of them are quite old (Page 11096 Line 29, Page 11097 Lines 1, 19, 21, 31, Page 11098 Lines 15, from 1955 to 1989). The update of the literature should be done and some Mediterranean studies included in the Discussion section."

Dr. F. Campelo wrote: "This kind of comparison could support the idea that the VS-

model performs better than "response functions", when both temperature and precipitation could be the limiting factor (see Fig. 3)".

Authors' Response

The model allows us to trace tree growth and cell formation during the growing season based on daily data and to observe a changing of limiting factors and their influences on tree growth during the growing season. The "response function", on the other hand, does not provide any information on the actual period of cambial growth – nor was it designed to do that. The "response function" may indicate a correlation of some month's precipitation, say, with tree-ring index, but cannot discern or even provide an estimate of whether the cells were actually growing in that month.

The reviewer suggests that we should test whether the VS model is an improvement over the "response function" in simulating growth as a function of climate. This is an interesting question, but cannot be directly addressed by available response-function software. To do so, it would be necessary to estimate a response function using monthly precipitation and temperature from a designated "calibration" period, and then apply the response function to grow a ring in an independent verification period. Such an analysis would be useful for proving the superiority of the VS model over response functions, but cannot be done with the available software for response function analysis.

We can only speculate on what such a comparison might yield. Clearly the linear regression process inherent in response functions is not suitable for handling a non-linear response. It is known also that sometimes the multidimensional regression technique (as in response functions) can produce artificial correlations between climate variables and tree-ring chronologies (Cropper 1983, Cropper 1985, Cook, Kairiukstis, 1990; Briffa et al., 2008; Shishov et al., 2007). The correlations from response functions can be inexplicable from biological or physiological reasoning. In the VS-model a simple interpretation of possible interactions between climate and cambial growth is

realized; what is revealed, or sought, is the biological mechanism of wood formation controlled by the principal climatic factors.

The reviewer suggests some additional references. We argue that some of the suggested references (Touchan et al. 2008, 2011) are not directly relevant. Moreover, Touchan et al. (2011) used gridded climate data and principal component reduction of tree-ring data: differences in data treatment preclude any meaningful comparison with results presented in the present paper. We note that references Anchukaitis et al. (2006) and Vaganov et al. (2006), two suggested references, were actually included in our manuscript.

Dr F. Campelo wrote: "Additionally, a reduced number of references was used (27), and some of them are quite old".

For example, he suggested we delete basics of dendrochronology published by H. Fritts (1976) from the reference list. We note that in that book Fritts suggests the use of multidimensional regression analysis (particularly, orthogonal regression) in dendrochronology – the so-called "response functions analysis" to which Dr F. Campelo refers several times in his comments. We argue that the urgency of papers is not defined by the date of publication, and disagree that substituting more recent references for those to original and still relevant work will improve the paper.

Comment #3. "The Authors considered that the growing season "is assumed to end [in June-July] when the integral growth rate Gr(t) falls below 0.1". They explained that the cause why "growth does not resume is day-length (or solar irradiance), which decreases in September", "despite partial growth rates gT(t) and gw(t) being favorable for resumption of growth again after August (Fig. 4)". However it is now largely recognized, or at least suggested, that cambial activity in Pinus species could occur all year round. According to de Luis et al. (2011) "the duration of the growing season might be extended up to 11 months in P. halepensis in warm semiarid conditions of Spain". It should be discussed why it is not the case here."

Authors' Response

To check the reviewer's suggestions (e.g. concerning duration of growing season in certain investigated regions) it would be necessary to conduct multiyear natural experiments with cambial activity. After such experiments we could check all hypotheses concerning growing process in that region. Our recent paper is just a first step in this direction.

De Luis et al. (2011) have used results based on annual natural experiments for two sites (Guardamar del Segura (GUA) (38o6' N, 0o40' W; 5 m a.s.l.); Maigmo (MAI) (38o3' N, 0o38' W; 844m a.s.l.)). For each site the authors observed cell dynamics during one year (GUA – 2004; MAI - 2005). In total, this provides data for two different years at two different sites. The limitations of such a small sample are obvious. Of course, robust statistics on cell dynamics are not easy to obtain because of the difficulties in conducting continuous multiyear natural experiments. Based on results for those special locations and specific years the authors mentioned above suggested "that cambial activity in Pinus species could occur all year round." We note these unique data and results are extremely important for testing of the VS-model and adequate simulation by the model especially for cambium activity simulation and cell size formation, and would strongly support the idea of conducting longer experiments and extending them to different environments. In recent times there have been a number of papers concerning the influence of day length on tree growth and the ability of this factor to limit tree-ring growth even if other principal climatic factors (temperature and precipitation) are suitable for growth (e.g. Rossi et al., 2006).

In our paper we obtained average characteristics for the growing season. But the model also yields estimated growing seasons year by year. If we consider the annual duration of the growing season during last 23 years (1982-2004) we see that in a specific year season-duration could reach 320 days (or about 11 months). In our paper: "For our example, the average duration of the growing season is 191 days, and the standard deviation of the growing season is 82 days." By statistical theory, average

plus (minus) standard deviation (191-82; 191+82) means that 70% of values should be in that interval for a Gaussian distribution. But 30% of season-durations would be expected to be out of that interval. Therefore, based just on statistical theory, we can find extremely short or long seasons (e.g. 11 months). And this was confirmed by more detailed analysis of simulated annual growing seasons. The results suggest that in some favorable years trees could possibly change their strategy of growth. They could change their uni-modal growth pattern to bi-modal. But this is a preliminary hypothesis which should be checked.

Comment # 4. "Moreover, how many missing rings or intra-annual density fluctuations (IADFs) were found? Trees from Mediterranean environments with recurrent summer drought easily produce IADFs. According to de Luis et al. (2011) "in summer, the cambial cell production occurred at a very low rate, but an increased production of xylem cells took place in September when the amount of precipitation was twice as high as the long term average". This kind of relation between late summer precipitation and the IADFs formation was found in Pinus species elsewhere (Campelo et al. 2007; Vieira et al. 2009; de Luis et al. 2011). Therefore I suggest that IADFs occurrence and the second growth peak (see Fig. 4) should be discussed. Please add more comments on the capacity of this mechanistic model to detect intra-annual changes in the lumen area of tracheids (IADFs). These points are crucial to the whole study. Please reassure the Readers about all of these points."

Authors' Response

These factors could be checked if we had direct measurements of cell sizes during the growing season and multiyear seasonal observations of cell size formation. In the paper our objectives are more limited. We demonstrate the possibility of using the VS-model for eco-physiological research in the Mediterranean environment. Yes, the VS-model allows us to simulate cambial activity in tree rings and estimate cell size in different zones (see previous references). With the VS-model we can trace the cell formation process during the growing season. But checking, or "ground truthing" a

simulation is possible only if we have direct measurements of cell sizes during several growing seasons. This work is not within the scope of our paper, but should be the next step in our research.

Comment #5. "Several studies have found two periods of activity (the so-called bimodal pattern) coinciding with periods of favorable temperatures and precipitation (see Camarero et al. 2010; Gutiérrez et al. 2011). Additionally, Touchan et al. (2007) have shown that precipitation in May-June, when temperatures are close to optimum, is controlling treering growth (in a linear fashion). According to Fig. 3 there are 4 important phases as a function of the day of the year (DOY): i) During the first 4 months of the year temperature is the limiting factor. ii) In May and June tree-growth is limited by soil moisture and temperatures are nearly optimal. iii) During July and August both factors could be the limiting factor for tree-growth. iv) After the summer soil moisture become again the main limiting factor. These 4 phases suggest a bimodal growth pattern and are clearly separated in time suggesting that multiple linear regressions using month climate variables will produce good estimates of tree-growth. Therefore, I suggest that the Authors compare the mechanistic model used in the present manuscript against simple/multiple linear regressions."

Authors' Response

Dr. F. Campelo wrote: "These 4 phases suggest a bimodal growth pattern and are clearly separated in time suggesting that multiple linear regressions using month [sic] climate variables will produce good estimates of tree-growth. Therefore, I suggest that the Authors compare the mechanistic model used in the present manuscript against simple/multiple linear regressions." Yes, this observation is correct. But good results from linear models based on monthly resolved data can be expected only when the periods of uni- or bimodal growing season estimated with daily data happen to coincide with the start and end of specific months. More generally, such coincidence cannot be expected, and if the start and end dates of the growth periods happen to shift, correlations based on monthly data will change. Daily resolution is more accurate and

more reliable from biological point of view.

Comment # 6: "According to the Authors of the manuscript the "optimal parameters have been chosen manually by trial and error using the model and average daily variation of model soil moisture from 1959–2004". However, the mechanistic model is guaranteed to fit well in the calibration period by definition, because the model's parameters are tuned using data from this period. The true test of the robustness of a given model is obtained in the verification period, for which results are independent of statistical model development; the verification period should not ever be "inside" of the calibration period. Therefore, I suggest that the Authors clarify this point if their objective with the "split sample procedure" was to evaluate the strength of the obtained model, as suggested in abstract (Page 11090, Line 6)."

Authors' Response

This comment is confusing, as indeed the model was tuned to set the parameters using data for a calibration period and then the model was applied to "grow" rings for a completely separate verification period.

Technical corrections

Page 11089, Title. "Process based model sheds light on climate sensitivity of Mediterranean tree-ring width".

It is done.

Page 11090, Line 3. "halapensis" -> "halepensis"

It is done.

Page 11090, Line 6. "We use two periods to calibrate (1982–2004) and verify (1959–1981) the model." -> "In order to test the strength of the model, we use two independent periods to calibrate (1982–2004) and verify (1959–1981) the model." Please keep this sentence consistent with the methods section (Page 11093, Line1–5).

C5490

Modified the sentence.

Page 11090, Line 8. "(r =0.76 p<0.001)" -> (r = 0.76 p < 0.0001, n = 23 years).

It is done.

Page 11090, Line 25–26. I am not sure that the word "practically" is valid in this context.

Changed the word to adequately.

Page 11091, Line 1. "halapensis" -> "halepensis"

It is done. Page 11091, Line 1–3. Please add the reference Anchukaitis et al. (2006). It is done.

Page 11091, Line 20–21. "(Touchan et al., 2008)" -> "(Touchan et al., 2008b)".

It is done.

Page 11091, Line 20-23. Please give the coordinates and altitude of the study area and provide the altitude of the weather station. It is done Page 11091, Line 22. "Jandouba" -> "Jendouba"

It is done..

Page 11092, Line 8. Function gl(t) should be defined.

It is done.

Page 11092, Line 13. Function f(P) should be defined, since a constant (k1) is associated to this function (see Table 1).

It is done.

Page 11093, Line 4. "(AD 1959–2004)" -> "(1959–2004)"

It is done

Page 11094, Line 4. I am not sure that the expression "limiting influence of climatic

C5491

variables" is valid in this context. Our sentence in the paper is correct Page 11094, Line 15–16. "Accordingly, our results suggest that cambial activity stops in the middle of July and does not resume until next year (Fig. 4), (: : :)". By "reading" the Fig. 4 I suggest the following change: "Accordingly, our results suggest that cambial activity stops in the middle of July and could not resume until next year (Fig. 4), (: : :)".

It is done.

Page 11095, Line 13–14.": : :from 0.0005 to 0.007 (with step 0.0005): : :" -> ": : : from 0 to 0.007 (with step 0.001): : :"

It is done.

Page 11095, Line 17–18. "Such a change of drainage coefficient was strongly reflected in the tree-ring growth response." -> "Such a change of drainage coefficient was strongly reflected in the estimated tree-ring growth."

It is done.

Page 11095, Line 23–24. "halapensis" -> "halepensis"

It is done.

Page 11096, Line 6-18. Please consider the present comments to re-phrase this sentence.

Modified the paragraph.

Page 11096, Line 20-21. This sentence should be change to better illustrate the results and the findings of the present manuscript.

Modified the sentence.

Page 11096, Line 21. "(: : :) how tree growth there is forced by climate" -> "(: : :) how tree growth is controlled by climate" It is done. Page 11098, Line 11–12. "Rossi, S., Deslauriers, A., Griar, J., and Seo, J.-W.: Critical temperatures for xylogenesis

C5492

in conifers of cold climates, Global Ecol. Biogeogr., 17, 696–707, 2008." -> "Rossi, S., Deslauriers, A., GriĚĞcar, J., Seo, J.-W., Rathgeber, C., Anfodillo, T., Morin, H., Levanic, T., Oven, P., Jalkanen, R.: Critical temperatures for xylogenesis in conifers of cold climates, Global Ecol. Biogeogr., 17, 696–707, 2008." It is done.

Page 11099, Table 1. Was the optimal parameters shown in this table used for the calibration period? According to the text (P11093, Line 1:3) these optimal parameters were manually selected for the period of 1959–2004. However, the calibration and verification period should be different (independent), and only the "calibration period" should be used to determine the optimal parameters, otherwise it does not make sense to verify the robustness of the model over an "independent period" or "verification period".

We have modified the paragraph

Page 11099, Table 1. "C1" -> "k1" "C2" -> "k2" "C3" -> "k3"

It is done

Page 11099, Table 2. The period used to calculate the descriptive statistics of simulated tree-ring curves obtained by different values of drainage coefficient was 1978-2004 (Fig.5). Please elucidate why was not used the calibration period (1982–2004)? Additionally, an explanation about the "valid N" should be given. The column "valid N" assume always the same value, therefore should be deleted and the "valid N" or the period (1978–2004) add to the figure legend. In my opinion a new column should be added to give the correlation between the initial chronology and the estimated curve using different values of drainage coefficient.

We have changed the period 1978-2004 to 1982-2004

Page 11101, Fig. 1. Add a new graph to plot gl(t) as a function of day of the year (DOY). This piece of information is extremely important to clearly evaluate the content of the manuscript, since the Authors stated that "gl(t) is the common limiting growth factor beginning in late August" (see Page 11094, Line 19–20). Please add a title to

C5493

the y-axis: "Growth rate, relative units".

It is done.

Page 11102, Fig. 2. Add the r2 for the verification period. For the calibration period the significance level is 0.0001.

It is done.

Page 1103, Fig. 3. Please see the below comments on Fig. 4. A curve illustrating the gI(t) should be added to the plot. Please indicate the title of the y-axis.

It is done.

Page 11104, Fig. 4. Explain why it was used a "negative exponential smoothing" rather than a LOWESS (locally weighted scatterplot smoothing). Please give the smoothing parameter (_). I suggest that a different smoothing parameter (_) should be used, in order to obtain a less rough curve, since the smooth curve should not detect sudden changes in the growth. Additionally, a smooth curve for each year should be plotted in order to identify when the growing season ends and to clarify a hypothetical bimodal nature of the tree-growth pattern in P. halepensis.

In comparison with the negative exponentially-weighted fitting (smoothing) LOWESS is more rough transformation and cannot take into account preventing points at remote subregions from biasing the curve. We have added an explanation to use negative exponentially-weighted fitting

Page 11105, Fig. 5. Please indicate the title of the y-axis.

It is done.