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Biogeosciences Discussions

Interactive comment on "Evolution of the potential distribution area of french mediterranean forests under global warming" *by* C. Gaucherel et al.

C. Gaucherel et al.

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Anonymous Referee #2 Received and published: 20 February 2008 General comments: The manuscript is an interesting contribution to the discussion on species responses to global warming and fits within the scope of BG. The authors present data on two Mediterranean tree species and parameterize the tree growth model MAIDEN for these two species. They use tree ring data and transpiration measurements to parameterize and validate the model. The main conclusion is that the model can be used to model the future development of the two species. Furthermore, the future tree growth is highly impacted by the CO2 concentrations. However, as the author model a large region and do not include the growth limitation at higher elevation (due to frost or other limitation), some

of their results and conclusions might not be valid. The authors need to convince

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the potential reader, why there results are valid and why they are relevant, e.g. what does increased (decreased) importance of CO2-fertilization helps, if frost kills the trees anyway. Maybe the focus should be more on the disappearance, worsening growth conditions in the region the forests are growing today. There are a number of other inconsistencies, which need to be removed, before the paper can be accepted for publication. Therefore, I suggest to reject the manuscript at this stage. However, as the topic of the paper is very interesting and highly relevant, I want to encourage the authors, to make the appropriate changes and resubmit a revised manuscript. In case of a major revision, please provide improved figures and appropriated changes in the text, and detailed explanations and answers to my comments below.

———————— Specific comments Title: Should be rephrased. "Evolution" has another meaning in biology, but also generally. Distribution areas do not evolve.

-> Done.

Abstract What are "complementary" growth mechanisms? I do not understand and why is that an argument?

-> True, we changed it by “different”.

Introduction p. 575, L. 14: "these changes", specify! Ciais et al. 2005 look only at summer drought 2003. So you mean "the increase in drought risk"?

-> Done.

p. 575, L. 15-28: you cite extremely frequently your own work, which is fine in the methods part. For these general approaches, there should be other papers, you could cite and broaden the view.

-> You are totally right, except that there exists very few dendrochronological modelling in literature.

p. 576, L. 3: in this context, Luoto et al. 2005 might not be the appropriate reference,

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as he does butterfly modeling, you are dealing with vegetation / forests / trees. Please find a reference to vegetation type modeling, using statistical patterns.

-> Associated with Thuiller et al. 2004.

p. 576, L. 5-20: Here you mention already a lot of details, which should be moved to the method part and this part should be shortened.

-> Done.

Methods You have tree ring data from 21 sites for pine. You show the spatial variability in Figure 2 and analyse it later on. You also have climate information for each site (more or less, I assume). Why do you not use each stands tree ring growth for you parameterization, instead of the "average chronology"?

-> The idea of this approach was to avoid over-parameterisation and to do a rigorous calibration.

Only in this way, you can use the spatial information as you intend it, and might be able to filter out effects of climate and CO2 in the observed data. Please explain why you are using the "average chronology" and loose information on the spatial variability. And, if you only could use the average chronology, why you then in turn can make comparison of spatial patterns like Figure 2?

-> The basic idea was to characterize the global species response instead of the local responses. The model is finally calibrated for each species and not for each site.

p. 577, L.19: How do you calculate a R50 value (e.g. 50 years), if you have a 34 year chronology only?

-> The 34 years (actually only 32 shown) are common years for all sites, each of them having more than 50 years.

p. 577, L. 18: years 1956-1958 has been removed from the data set. Your simulations start in 1960 or? You do not have to remove those years. Please explain or remove.

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-> Done. As it is now explained in the text, bole increments have been averaged into a 32-year regional chronology (1963-1994) concerning 21 sites. The 38-year oak series (1968-2005) is more local as it is based on a single site.

p. 578, L. 17: Do you mean temporal autocorrelation?

-> Yes.

p. 578, L. 20: replace "it" by "the model"

-> Done. ———————— Results: P. 582, L. 1-2: You use the transpiration data for the model calibration (step 1). Then by definition simulated and observed transpiration should not be very different! Do not place this as a result or explain in your method, why this is an independent results, independent of your calibration. Alternatively, re-formulate, that it become clear to the reader, that this is the outcome of the calibration exercise.

-> We are afraid not having understood your remark. Indeed, two calibrations have been successively performed.

P. 582, L. 10-12: For the oak: is there any specific reason, why 1990-1993, 1995-1998 fits worse? What about the two dry years 2003, 2005, they seem not be found in the tree ring series. Do you have an explanation?

-> This is an important point indeed. Model and data did not agree well all the time. First, the model agreed better in the first part of the period, from 1942 to roughly 1997, then the fit degraded in the second part. As said above, it is possible that canopy closure after 1990 influenced the decoupling between tree-growth and climate after 1990.

Second, the model overestimated the growth in 1996 and 1997. These 2 years appeared after 1995, which was extremely dry. It is possible that some after effects of this drought were not well simulated by the model. Especially, trees might have shed some leaves during this extreme drought and the reconstruction of the canopy might

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have necessitated carbohydrates reserves through processes that are not modelled.

Third, the model seems to have overestimated the drought effect in 2003 and 2005. In fact, tree-ring width during the 1999-2005 showed a decreasing trend probably due the accumulation of stress such as increasing drought in the nineties, high temperature in 2003 and insect defoliation in 2004 and 2005. In these circumstance, it seems that the model show too much interannual variability and is unable to solve for low frequency growth cycle.

p. 582, L. 10-12: in Misson et al. 2004, you found a correlation for pine of r2=0.67. Why do the results differ so much in this paper, although you are using more advanced calibration?

-> True. Two reasons explain this difference: i) the model has been improved, and ii) the calibration is different (see Gaucherel, C., Campillo, F., Misson, L., Guiot, J., and Boreux, J. J.: Parameterization of a process-based tree-growth model: Comparison of optimization, mcmc and particle filtering algorithms, Environmental Modelling & Software, In press, 2008.).

p. 582, L. 12: "details.. given in (Gaucherel..)" , this does not belong in the result section, but might be part of the methods.

-> Ok.

P. 582, L. 17: "confirm its climate origin". I cannot follow that argument? Why?

-> This is because intermediate processes (correctly modelled) are not able to compensate for this trend already present within the input parameters.

P. 582, L. 20: Why do you not compare the results for the same time period, for which you have your growth index? These maps are difficult to interpret, as the location of the studied stands and modeled stands are missing, so I cannot evaluate, whether your argument is reasonable or not. Show stand location and productivity difference with symbol size or colour.

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-> Done. Thank you for the advice.

p. 582, L. 16: Could the "negative growth trend" be an age or site effect rather than a climate effect? How do you know, if you have one site only? Whole paragraph 4.2 is in my opinion a part of the methods and should be moved there.

-> This is not our opinion.

Discussion How can you extrapolate with your model outside the area, where the trees are growing today. This might be difficult.

-> Actually, this is not because our model is process-based. Obviously, this point does not mean that extrapolations will be quantitatively exact. We might be careful anyway.

You need to discuss that. How do you know, that the trees would grow better at higher elevations? In fact they do not grow there at all, you say this is due to frost, but how do you know how they would grow there in case of no frost? The interpretation of many of your results are based on areas, where trees are not growing today. As long as you would estimate the shift in distribution, this would be fine. But you interpret a lot about CO2 importance outside the actual range? Why? Is that meaningful?

-> This point is crucial. Climate is changing, with frost frequencies potentially changing too. It is possible (probable ?) that frosts will be rarer and will favour colonization at higher elevations. With this respect, it is important to our opinion to keep as much information as possible (especially at higher elevations).

The tree might not grow there due to frost anyway? What is the importance of a CO2 fertilization effect high up in the mountains if no trees are growing there?

-> Same comment. It is easy to remove some parts of the studied area, but which one? Without any additional information about this process, we made here the hypothesis of similar CO2 effects at high elevations.

CO2 - effect was found to be very strong -> how can you ensure that this is not an

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artifact of the model? Did you consider the CO2 increase in the last decades or any experiments? If not, please discuss the implications in more details. Especially, as your talk about regions, where the trees potentially will not grow, as I mentioned above.

-> Yes, CO2 increase has been considered at all time steps. It means at the end of the XXth century (measures) and during the XXIst century (simulated). Increasing CO2 has been used to calibrate the model so that we can ensure that CO2 effect is not over-estimated.

p. 585, L. 23. "cold winters that could have damaged the cambrium ...". This argument holds for pine. But how do you explain the differences of the oak trees, they have a higher observed tree ring width compared to the model! Which factor does the model miss? Might that be an important one? For oak other periods do not fit as well, any explanation for that, see comment above in the result section?

-> See above.

p. 586, L. 6-8: "climate changes not beneficiate to these species in the future". You conclude that from your simulation for the whole region, but some in some region they might grow better, at higher elevations? Please be careful with your conclusion, as it depends on the size of the region you discuss.

-> Yes.

p. 586, line 22: Is Sitch et al. 2003 the correct reference? This is a modeling paper, not including dispersion? He might have speculated, that it is important, but not done any tests.

-> This is true. Actually, we (and other teams) are working on that point but, to our opinion, no paper has explored dispersion effect in this context.

P. 588, L 12: This should be mentioned in the methods and maybe even in the results, where appropriate.

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-> Done.

P. 587, L. 7: Why can a species only grow at its optimum? It obviously grow below its optimum at lower elevations, why should it grow until its optimum? Strange argumentation. You state yourself that other factors not included in the model might limit growth at higher elevations! Additionally, if oak would not die of some adverse conditions, it would grow extremely well at very high elevations? Is that really realistic? Can you verification this, are there any observational evidence? At least, you should discuss that and be more careful with your conclusions/predictions.

-> This is right. We are not saying that species only grow at their optimum rather than they may progressively reach them in case of favourable climate changes. What we call an optimum is rather a higher bound/limit of possible productivity. Corrected.

p. 587, L.15-20: How can you be sure, that there really is such a strong fertilization trend? For oak you have only one tree ring series, this can be climate as much as CO2 influencing the growth? How can you distinguish that?

-> No. This is one of the advantages of a mechanistic model to help dissociating several influences/forcings. Climate AND CO2 effects have been calibrated simultaneously by Bayesian approaches, meaning that their influences and interactions have been correctly taken into account.

For pine you do have more data points (locations), but you do not analyse the effect of climate (e.g. location) vs. CO2 importance (CO2 should be within a year similar at all locations, whereas climate will be different due to different elevations.)

-> Yes, these remarks concern the previous paragraph about topography.

Too strong conclusion, which is not necessarily supported by the data, although supported by the model outcome, but only valid if model is correct! What if your model overestimate the CO2-effect?

-> You are right. Yet, conclusions are supported by the model AND numerous data.

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Tables: Table 1: I do not understand that table. Why does pcp APG/CRU, which is supposed to be the corrected values differ so much from the CRU data, like May-July? It seemed that the uncorrected APG data were much better? Why do you present T (mean) CRU, but afterwards max and min values? Difficult to understand and compare.

-> Checked. CRU available temperatures only concern mean temperature, while APG available temperatures are min and max.

Table 2: what does 21/20 mean?, Can you really use same coefficient for T max and T min correction?

-> 21/20 mean the difference or ratio between the two studied centuries (corrected in the caption). Yes, we are not certain that Tmax and Tmin may be corrected by the same values, this is a (weak) approximation.

Figures: In General: the Quality of the figures was poor, lines were too thin and therefore not always visible/distinguishable on printout or enlargements. This made it difficult to evaluate the arguments/conclusions from those figures.

-> Done, yet not perfect! We apologize; this is not easy, due to Matlab outputs.

Figure 1a: I found a mismatch in the numbers of years, you mention in the methods versus showing in this figure. For pine you have a 34-year regional chronology, but shown are 32 years. Please explain the difference.

-> True, corrected (the two first years have been removed due to various modelling constraints).

Figure 1: The dotted line is hardly visible! Figure 1: Years 1985-1987 has been removed, please indicate in diagram these years.

-> Arrows have been added.

Figure 2: Please indicate, where you region is situated within a map of France. You use different scales for Figure 2 and Figure 3-5, 8,9. This is confusing, if no common

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map indicates where these maps are situated in relation to each other. Maybe add it within Figure 2 b: Please provide a map, where the location of the pine stands can be seen. Otherwise, it is difficult for me and a future reader to evaluate, whether your interpolation is meaningful or not. In Figure 4 you show the modern distribution area of pine, which is only 15

-> Done. We have located figure borders between each others; we have superimposed study sites; we have shown a map of France and the coastline when missing.

Figure 3a): Quality was not sufficient. The scale of temperature anomalies is difficult to interpret. Lines are too thin, maybe thicker lines, or even fill out the areas? Figure 3c): dots and stars were difficult to distinguish (quality!). The elevation lines where rather thin and difficult to see. Do you need the negative values at all?

-> No.

Figure 4b) Should this figure not belong to figure 5, very confusing to have it as a separate figure. Figure 4a): the limit you draw is the limit of the "actual distribution", not the potential, or? If it is the potential, how do you know that, what causes it?

-> Yes.

Figure 5b should be moved to figure 6 otherwise confusing, then figure 5 would be todays distribution and 6 would be future with and without CO2 and difference between. Figure 6: b, d: If you know, what causes the elevation range, in which the species do not occur, can you estimate that for the future as well, and draw equivalent lines? Can you really extrapolate growth beyond todays occupancies? I would rather say not.

-> We would rather say yes (because of the use of a mechanistic model), otherwise what would be the interest to try modelling new area distributions?

Figure 7: I cannot distinguish between the dotted and the solid line, (quality!). Figure 8b should be connected to figure 9, otherwise confusing. Maybe have Figure 5a and 8a together as todays distributions.

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-> To our opinion, Figs 5a and 5B on one hand, Figs 8a and 8B on the other hand should not be separated. We admit that pine figures (4 and 5) may be gathered into the same figure, but they would be too small and would not respect the order of apparition in the text (same remark for oak figures 8 and 9).

Technical corrections

-> Most of them corrected. Thank you very much for suggesting them.

As I think the paper need a major revision, I take only up some of the technical corrections: p. 575, L. 19: "time and space scales" , replace with "temporal and spatial scales". Please check the rest of the document and always use spatial scale and temporal scale, where needed. p. 577, L. 5: What is the PACA region? Spell out. p. 577, L. 20: "fertility index" in Figure 2 you call it "productivity index", please choose one term and use it consistently. p. 577, L. 25: "for one year", specify the year. Replace "plain line" with "solid line" in whole paper. If you use 20th century and 21st century you should use either a dot (20. or 21.) or the "th" and "st". Please change in whole document. p. 577: if you do not present the equation, how the index is calculated, you do not need the abbreviations. Please add the equation of index calculation (or remove the abbreviations). p. 584: "evolution" as in header, this is not an appropriate term, please replace in the whole manuscript. Interactive comment on Biogeosciences Discuss., 5, 573, 2008. S61

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