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Interactive comment on "A model analysis of climate and CO_2 controls on tree growth in a semi-arid woodland" *by* G. Li et al.

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The manuscript compares predictions of a vegetation model to observed tree ring data for Callitris columellaris in Western Australia.

The main message is that a time-dependent calibration of the model supports a shift of carbon allocation to fine roots with time and/or increasing CO2 over the last 100 yrs.

The evidence provided for this assertion is fairly indirect. Various uncertainties remain in the approach, specially because only a small subset of the model parameters is calibrated over time. It may well be that a completely different process shifts over time, and the analysis compensates for this through a change in fine root allocation. Nevertheless, a change in fine root allocation is ecologically plausible and interesting,

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and the idea of using the vegetation model to fit time-dependent parameters quite innovative. I therefore think that this is an interesting study for biogeosciences.

I had a number of questions / comments that should be addressed in the revision of the ms. I provide a few general points first, followed by detailed comments later.

1) FOCUS OF THIS STUDY

The presentation of the ms is in general very clear, but I was missing the "big picture". What was the objective for doing this analysis? I can't find anything about this in abstract / info (apart from the information about what you do). It seems to me that the most interesting result of this study is the CO2-dependent allocation, but this message is absent in the title, and in the analysis you afford comparatively little space to it. Instead, you analyze in detail the model fit under constant CO2, i.e. under conditions that never existed in reality. Of course you can do this analysis, but you should explain why. What can we learn from evaluating the model with wrong drivers (i.e. constant CO2)? After all, you don't do the same with constant precipitation either. In general I would suggest shortening results for which a purpose cannot logically be constructed.

2) HOW CERTAIN ARE WE THAT ALLOCATION SHIFTS AND WHY

Connected to point 1: the interesting question is analyzed very briefly: how strong is the evidence for time-dependent allocation to fine roots from your results?

A big concern here is that the time-variable parameters are simply compensating some other structural error of the model (i.e. that the model is not able to create constant growth with increasing CO2). As very few parameters are under calibration, it may well be that there was no other option to get the predictions close to the data than increasing allocation to roots. The most convincing thing for me would be to put more parameters under calibration and show that you couldn't get a proper fit by changing other parameters (within reasonable bounds for these parameters of course). Morevoer, I didn't understand why you didn't validate the time dependent model in the same way as the

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other model versions (Figs. 5,7).

Assuming that the time-dependent root allocation is truly real and doesn't fudge away another problem, can we find out which driver is responsible for the change? Time can't be the reason, so it should be CO2, some other climatic variable, or something else. The references to literature in the discussion are good, but can you do anything more here? For example, you could try to make the dependence to CO2 or some other variable explicit in the model through a parameter and optimize this, or analyze how the inferred time-dependent parameters correlate to CO2 and other environmental variables (in a way, the latter is in Fig.8, but only for CO2. And here I was wondering if you want to suggest with this figure that CO2 is causal - if you look at the decrease of fine root allocation in the 20s - 50? with increasing CO2, is it sensible that he roots go down and the up again?)

3) TECHNICAL ISSUES

I had a number of technical questions. I want to highlight the most important ones here. Details in the comments below

a) Can we exclude problems with the allometric relationships and multiple stems?

b) The description of the model assumptions and parameters is quite short. It's clear that you can't describe everything in detail, but things that could be an issue for the main conclusions should be better explained. In particular, could changes in other parameters that were not under calibration cause a similar reaction as changes in the fine root allocation parameter? As there are uncertainties also on other model parameters, why didn't you put all parameters under calibration? If you have estimates from the literature, you could code this as a prior in a Bayesian analysis.

c) The calibration method is not described at all. What was the calibration objective, sum of squares? How did you assess convergence? Why use this somewhat exotic method instead of a standard optimizer?

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d) I liked the analysis of the climate-reaction of the model and the data, but I had some questions 1) Are you really using a GLM? It seems this is a LM problem 2) It seems you don't account for a) temporal autocorrelation b) random effect structure due to the individuals. An analysis via a linear mixed model with a lag for temporal autocorrelation would seem more appropriate to me (use e.g. function lme in R) 3) As you seem to have strong collinearity, you should always have ALL climatic variables in the analysis. E.g. move temperature back into Fig.7. To account for a possible temporal trend caused by non-climatic factors, you could also consider taking time in as an additional predictor.

DETAILED COMMENTS

Title: if the main story of this paper is that an increase in fine-root biomass explains a lack of CO2 effects on growth, why doesn't this message appear in the title?

#4769

1 a) start abstract with an introductory sentence b) what is the knowledge gap and the motivation for this study?

10 Simulated and observed consistent - is this basically the same statement as the previous sentence? The next sentence seems to be another repetition of the previous statement. Condense?

#4770

2 why despite?

17 not sure if everyone is familiar with the supersites

18 and following statements about temperature: where is this data coming from?

26 what do you mean by anthropogenic? Due to climate change, or do you question whether CC is anthropogenic?

27 however -> in any case

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#4771

10ff to show what? What were your hypotheses?

17 provide precipitation values again, or move them from the intro. You could also highlight Fig.3, which is very useful to see the climatic characteristics of the site.

4772

7 showed no obvious, or no? It should be pretty clear if there is variation or not.

12 A single stem of given diameter would have another allometry than three stems of the same aggregated diameter. In the data that you used to create the allometries, was the fraction of multiple stems similar? It seems important to me that you discuss the potential issues that could arise through this decision in more detail. Is your model sensitive to the error created by this assumption?

4773

20 at some point here you seem to move from the P to the T model, but it's not clear to me exactly when

4774

11 You say your model "has no free parameters", by which I understand that you want to say that the parameters should be chosen identically for all C3 species. There is clear evidence for differences in photosynthesis of C3 plants, just to pull some random citations that don't need to be included:

Evans, J. (1989) Photosynthesis and nitrogen relationships in leaves of C3 plants. Oecologia, 78, 9-19-.

Flexas, J. & Medrano, H. (2002) Drought-inhibition of Photosynthesis in C3 Plants: Stomatal and Non-stomatal Limitations Revisited. Annals of Botany, 89, 183-189.

Even if the P-model does not include the proposed mechanisms that lead to these dif-

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ferences, it seems unlikely to me that one couldn't get a better description of individual species' photosynthesis if one would adjust them.

Moreover, with respect to the question that come up later - can you exclude the possibility that parameters in the P-model should be adjusted if CO2 changes, or would you say that this is impossible?

13 150 trees of what sizes?? How selected?

21 I'm a bit confused as to which parameters are in what model. If the P-model simulates GPP, then shouldn't quantum efficiency be in there? Here it seems it's in the T model. Yield factor as well?

4775

1 I appreciate that one can probably get this information from the cited publications, but as this is pretty central to the further story, it would be useful if you could explain shortly why these parameters are so influential, and what the underlying assumptions behind that are (physiological / ecological), and if the model response caused by these parameters (specifically fine root allocation) could also originate from some other parameter that wasn't varied in the analysis

1 Looking at Table.1, there seems uncertainty also in the other parameters. So why didn't you put them all under calibration, potentially with constraints given by the uncertainty ranges that you had here?

3 Why this algorithm, and not a simple optimizer? What was the objective function for the optimization?

11 I'm not sure if I get it correctly - when you say linear interpolation, do you mean that you take the monthly precipitation, and distribute it evenly across all days of a month? Would your model not not react differently when you compare two scenarios 1) evenly distributed rain 2) unevenly, according to typical precipitation patterns for this region, which should have a mix of rain and dry days. Same problem for other variables

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potentially

4776

1 About this whole section: it was unclear to me why you do this, and how the growing season enters your model/analysis. Is the growing season a parameter of the T model?

15 1) Why do you use a GLM? The response seems normal. 2) What kind of GLM did you use, i.e. which distribution 3) OK, you regress climate against growth, vary the interval to do this, and look at p-values and Rˆ2. But what is the argument that tells you that the best Rˆ2 is the optimal growing season for your analysis? I see absolutely no reason for this. Couldn't it just be that you average away noise on larger time scales, hence the better Rˆ2 for a longer period, independent of what one would really think of ecologically as the growing season? Also, for sure you will get much better predictions for shorter time scales if you include nonlinear relationships and lags. Or let's put it the other way around: is your definition of growing season: the time span that allows the best prediction of growth with a regression using only linear terms of climate data as predictors?

4778

4 You state that the model captures the dynamics realistically (Fig.4) ... well, the mean growth is fine all right, but if we look at the variation of the mean, I doesn't look so great to be honest. This is a bit surprising because the univariate responses to climate seem indeed fine. Do you have an explanation for why the observed and predicted time series are so seemingly unrelated?

6 1) to state that r=0.37 is high is quite optimistic. A simple linear regression of 2-year PAR, MAT and alpha had an R2 of 0.3, not so much worse (Table2). 2) See main comments: I wonder how sensible it is to give p-values on this because data is not independent (individuals + temporal) 3) supplement Fig.4 by a predicted vs. observed plot

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8 so, you exclude the possibility that it could be a problem of the model?

11 again, I wonder what kind of GLM you are using here

Fig. 5 separation of a / b is very easy to spot. Separate the panels visibly

18ff I would say that the fact that the temperature effect disappears or even changes sign (observation) due to the apparent collinearity of temp with VDP basically suggest that we should disregard Fig.5a, because the temp correlations are spurious, and rather concentrate on Fig.5b. Here, however, the model / data comparison doesn't look so convincing any more. There seems to be a slightly positive temperature reaction in the data, but not in the model, and there seems to be a difference in the VDP reaction as well. I would guess that the former is not significant, but the latter seems to be. What is your interpretation here - is there a discrepancy between model and data, and if so, could int be that some parameters in the P-model would need to be adjusted?

4779

6 What was unclear to me - with the varying CO2, Fig. 6/7, did you recalibrate the model, or did you use the calibration done for Fig.4/5

8 For the Regression, why did you exclude temperature? We see in Fig. 3 that they are collinear, it seem crucial to include this in the regression

4780

18 Are you explaining the model results or the data?

4781

4 As the climate was changing at the same time, I would formulate this a bit more carefully.

6 it may be that tree-ring studies find that, but on the other hand inventory and satellite data does seem to support effects of CO2 fertilisation. What is your take on this?

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12 wouldn't we expect increase in WUE also without fine root changes? –> so what evidence does this add to fine root changes?

// Figs.

Figs 4,6,9: I assume the model error bars originate from the different conditions at the different sites? Clarify.

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