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Discussions

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.

Anonymous Referee #2

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The study combined two different approaches i.e. semi-mechanist model and statistical models (GLM), to study the relationships between climatic variables and global change and the stem growth (measured with tree-rings) of a tree species of a dry environment. The combination of the two approaches is valuable and the majority of the results are interesting. However, I find some technical issues, many interpretations questionable, the discussion quite poor and the structure not ideal. Overall, I think that a very heavy revision is needed to make it a smooth paper for BG.

1. pg 4775, L26 onwards. Key parameters of the allocation process have been derived from Bayesian optimization using only tree-rings. I'm not so familiar with Bayesian techniques, but how reliable is an 'allocation routine' (i.e. C partitioning between different tree organs and functions) when data for only 1 organ (stem) and 1 function (growth)

are used as constraints? Some processes are very difficult to measure and few data are available, but I would expect that a calibration of an allocation routine should comprise at least 'something' also about roots and leaves (root and leaves biomass for instance) and not only stem. If I'm wrong please explain why. Also I note that the tree-ring data used for calibration are also used for validation. Have you tried to split the dataset and used different datasets for calibration and validation? It might also be instructive (as your parameter variability shown in Fig. 8).

2. the performance of the model is only evaluated against tree-ring data. However, exact simulation of stem growth might be the results of different biases: e.g. overestimation of GPP and overestimation of respiration. An analysis of the overall modeled fluxes (GPP, respiration, stem growth, leaf growth, fine root growth) will say more about the model, even if the fluxes can not be validated because lack of measurements.

3. The fact that radial growth has not responded to increasing [CO2] is recent decades is interesting but the story about increased allocation to fine roost is highly speculative. It can be mentioned but I do not see any strong indication supporting this. The only support is that the parameter 'fine-root mass to foliage area' increased by about 14% from 1950 to the end of the period. . .. But this parameter was not calibrated against fine-roots or leaves but just stem (see above). I do not find this convincing . . .. Lines as the three ones at beginning of pg 4783 are not-supported. It is reasonable that trees with more carbon resources invest more in fine roots in infertile or dry sites to favor uptake of the limiting resources. But also all other possibilities should be mentioned and then eventually excluded: e.g. increase in respiration, increased allocation to leaves (are not your data of Fig 5 (ring vs PAR), indicating some light limitation?) or even increased allocation to belowground transfer to mycorrhiza/exudations . . .

4 I find results of Fig 5 interesting and clear. However, I do not find necessary to know how you arrive to understand that MAT is not important. For me, Fig5b is the only one you need to show . . . Similarly, all the section starting from L18 of discussion is a repetition of what already said . . .

5. you say in results that “T model captured the amplitude and interannual variability of Callitris tree growth in the GWW realistically (Fig. 4)”. However, there are many deviations and in many years growth is underestimated or overestimated by ca. 50%. Why some growth peaks are very well captured and others (as around 1970) are not captured? What about the deviations around 2005-2010? And what about the period 1920-1930? Similar (last line of Results) “. . .. In particular, this simulation does not produce an overestimation of ring widths in recent years compared to observations”: it is true that there is an improvement but still a large overestimation around year 2005. Please be accurate in results.

6. I think the study need a better structure: (i) what are your detailed objectives? For example, you mention “growth response to the shift in precipitation regimes in the 1960-70s” in both abstract and discussion; is this a goal? Definitions of goal might help you to have a clear discussion (ii) I would present first the GLM results and then the results of the other model (e.g. about Fig. 4 and similar). In doing so, maybe your GLM results can be used to understand and explain the issues I mentioned above in my point 5. (iii) Abstract should be re-written accordingly to what said above.

Minor remarks

Paragraph 2.6. I understand your point about “Growing season”; but many people define “growing season” as something else (period of measurable diameter growth); so your text is confusing. Actually you do not need whole this info about growing season. Delete?

Table 1: yield factor (y): if you re-ran the model you could use a better estimate derived from data (see Vicca et al 2012 Ecology Letter); I guess that you are using now a simulated value of y . . ..

4776 L23. And what about correlation with the other meteo stations?

Why not joining Fig 4, 6 and 9 in only one figure?

Interactive comment on Biogeosciences Discuss., 12, 4769, 2015.