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Interactive comment on "Modelling the climatic drivers determining photosynthesis and carbon allocation in evergreen Mediterranean forests using multiproxy long time series" *by* G. Gea-Izquierdo et al.

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Dear reviewer,

thank you for your comments. The responses are just behind each of your comments in the same paragraph. If you find that they are not easy to follow in the text I copy-pasted below, you can see them also attached in a supplement where our responses are in bold for clarity. The new manuscript with edits needs to be uploaded elsewhere in the review process.

Yours sincerely.

G. Gea-Izquierdo & coauthors

Anonymous Referee #3 General comments I generally find the changes to the model well described, however, it would be very helpful if the authors could give the physical meanings of the numerous parameters where applicable (I am aware that this will likely not be possible for all), or at least give some indication why the particular form of this equation was chosen. Otherwise the form of e.g. Eqs 8-11 can seem rather arbitrary. Following from this, why is allocation to the stem set as a function of climatic forcing in [P4], but not in [P3]? Some explanation of why this change in equation is made would be appropriate. When possible we now describe more in detail some of the parameters with a physical interpretation (see M-M, e.g. lines 317-318). We refer to Gea-Izquierdo et al. 2013 to justify selection of allocation functions (lines 293-294). Regarding P3, please note that this is also set explicitly as a function of climatic forcing, see [E10] in line 320.

There are a few aspects of the results where it seems like the authors could be more definitive in their interpretations. For instance, on pg. 2761 it is stated that "the model simulated a decrease in GPP, which was likely driven by the prevailing decrease in precipitation". It should be possibly to definitively attribute this decrease in GPP to precipitation by also running the model with fixed precipitation data throughout (e.g. repeated 1960 precipitation cycles). Given that the model does not seem computationally heavy to run, I think this would be easily done. We now state more clearly that it is a consequence of precipitation (see e.g. lines 462-463)

Likewise, on pg. 2761 it is suggested that differences in GPP between the two sites could also be explained by less limitation of carbon assimilation during the winter at due to higher winter temperatures at ï£ijFontblanche. Surely, using the model outputs, it is possible to be more definitive on this? We state that temperature is likely the factor explaining this, however we think it is better to acknowledge that other factors (forest composition, for instance) can also be co-responsible

The model is intentionally formulated so that growth can be scaled down independently of photosynthesis, but as far as I can tell there is no scaling down of photosynthesis as a result of a reduced sink of carbon (this is a contentious point, I know). The result of this could be, however, that under certain conditions very high levels of NSC accumulate within the plants, perhaps even to levels that are physiologically unrealistic. Given the importance of NSC within this model, and the little we know about its allocation, it would be appropriate for the authors to display the evolution of NSC throughout the experiment (ideally both inter and intraannually), and also provide some discussion about what they observe, whether it is realistic, or whether it points to some deficiency and/or missing process in the model (e.g root exudates, down-regulation of photosynthesis in response to a reduced sink; Millard et al., 2007, New phytologist 175, 11-28; KoÌĹrner, 2013, Nova Acta Leopoldina NF 114 , Nr. 391, 273 –283). We describe further how NSC are addressed in the model (M-M) and show their dynamics (intra and interannual) in new Figure A5. We lack detailed knowledge on their dynamics and how they are actually distributed in the studied forests, see lines 515-516. We add now the suggested references, which are helpful to address this point. Photosynthesis is indeed downscaled as a function of sink carbon: if there is not sufficient carbon stored to build the canopy each year this results on a decrease in LAI (which is also downscaled in the case of protracted drought). See M-M (e.g. lines 303-305) where we explain how we do this and also the results and discussion (e.g. lines 504-…). Further possible downscaling of photosynthesis is not addressed in the current model formulation.

The authors present the interesting result of differing trends of WUE between individual plants and the canopy-scale, however they do not discuss why this comes about in the model. I would guess that the reduced LAI at the Puechabon site leads to more radiation reaching the ground, and thus a strong increase in soil evaporation? Given that this difference in WUE is emphasised in the abstract, there should at very least be some discussion over why this difference occurs - better a definitive answer based

on model outputs. We add now several lines discussing this issue both in the results and discussion sections. Please, see lines 30-34 in the abstract or 475-480 where we further discuss these results and the rationale behind

The results regarding the decoupling of photosynthesis and growth are highly relevant for global environmental change studies, often carried out at large scale with models that only consider a carbon-source view of vegetation growth (e.g. Friend et al., 2013, PNAS 111(9), 3280-3285). The results herein might thus be highly relevant for such models (as described in Fatichi et al., 2014). It would be good for the authors to spend a few sentences in the discussion/conclusion highlighting the relevance of their results in this light. We now address more thoroughly this point in the discussion; please see e.g. lines 503-518

ï£ijMinor amendments pg. 2747, l9: It would be good to be more specific in the abstract that you are acclimatising photosynthesis and allocation to water stress over the previous year, to immediately make clear to the reader that this study is not address the temperature acclimation of photosynthesis (a different problem). Added, see line 22

pg. 2748, l7: [CO2] should be defined properly the first time it is used, presumably as atmospheric CO2 mixing ratio. Done, line 61

pg. 2748, l16-19: It would help the uninitiated reader to make some short introduction of what is meant by the C-source and C-sink hypothesis. It would only require a couple of sentences to make this completely clear. We provide now a brief explanation in the introduction, plus some extra references as kindly suggested by the reviewer. Please, see new lines 72-75 in the introduction and also further discussion later in the text

pg. 2748, l28 and throughout: "CO2" is often written with referring as to whether a flux or a mixing ratio is being considered. Presumably in this instance you mean flux, but this should be explicit every time you use it. Added, see line 87 pg. 2749, l17: What is "at a greater scale" referring to? Spatial? Temporal? How big? Spatial, at the regional scale (line 105)

Section 2.2: Given the importance of WUE calculations to the overall conclusions, I think it would be appropriate to include a small summary of how plant transpiration and soil evaporation are calculated, so that the reader is not required to read a second paper. This need not be as detailed as for the processes which are newly presented here, but just give the salient aspects. See M-M where we describe now more in detail this point, lines 181-185

pg. 2753, l1: Based on what criteria did it behave better? Please see lines 219-220

pg. 2753, l17: Which surface does Cs refer to? Leaf surface? OR ground surface at some reference height? Line 238, "leaf surface"

pg. 2754, l1: The daily soil water content is given in mm. Does this take into account the space taken up by soil structure, or is it a normalised value? It is total SWC (in mm) taken into account the soil structure, not a relative or normalized value

pg. 2754, l9: Please give value of Kb used. C917 Line 254

ï£ijpg. 2758, l20, l25 and Fig. 2: I think the units of stem biomass increment should be g m-2 year-1 ("-" missing)? Yes, that was a mistake that now has been corrected along the manuscript

pg. 2759, l21: What is meant by "well-coupled"? We delete now "well-coupled" for clarity

pg. 2760, l20: On page 2758 it was specified that carbon allocation was calibrated to stand-specific measurements. How then does the model assume species-specific carbon allocation responses? The model analyses carbon allocation at the stand level together for both species (lines 389-392) at Fontblanche

pg. 2764, l8: I don't think it is possible using Fig. A4 to separate a pure CO2 effect

on gs, from the effect of [CO2] on temperature? But it would be easy to make such a separation using factorial experiments (e.g. fixed [CO2] or fixed climate). Yes, we agree, that is what we state in that sentence "that they apply simultaneously", line 592

Fig. 2: At which level of confidence are the confidence intervals displayed? 95 % (now specified)

Fig. 3: Grey dots are almost invisible. Perhaps used coloured dots instead? Modified

Grammatical and typographical corrections We appreciate these corrections, they have all been modified

There is a scattering of grammatical errors throughout, but I do not believe these sufficient to require copy-editing, instead I list them below. pg. 2747, l15: "translated into a parallel increase" Done

pg. 2747, l16-19: These sentences are confusingly phrased. Suggest, "In contrast, at the other site where long-term precipitation remained stable, GPP did not show a negative trend and the trees buffered the climatic variability." Done

pg. 2748: "...such data are applied at..." Done

pg. 2749, l17-20: The meaning of this sentence is unclear. Please rephrase. Done

pg. 2750, l3: Are you trying to say that that these relationships differ between phenophases? That they can be opposite, now rephrased pg. 2750, l18: "...dense coppice in which..." Done

ï£ijï£ijï£ijï£ijpg. 2751, l25: Better to say "eddy co-variance fluxes", rather than data. Done

pg. 2752, Eq. 1: I think the first instance of Ko in this equation should in fact be Kc? Yes, that was a mistake, now corrected

pg. 2752, l15: "compensation point" Corrected pg. 2753, l8 and throughout: "leave" should be "leaf". Corrected

pg. 2753, l9: Presumably this means "reduced lower leaf replacement rates in response to long-term water stress"? Yes, “leave” should be “leaf” instead

pg. 2754, l21: "...phenological phases during the year..." Modified

pg. 2757, l10: Presumably you mean "half-hourly net CO2 flux measurements"? Yes, added “flux”

pg. 2757, l11: NEP is not yet defined. Now defined just before

pg. 2757, l16: "In a second step..:" Modified (line 347)

pg. 2764, l3: "...co-responsible for active acclimation of plant physiological processes..." Modified (line 583)

pg. 2764, l11: I think you simulated increase WUE, rather than observed it? We state that is simulated WUE “we observed an increase in simulated annual WUE”

pg. 2765, l6: Bouchard et al. (2014) is not in the reference list. That should be “Boucher”, now corrected

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C1669/2015/bgd-12-C1669-2015-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 12, 2745, 2015.