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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.

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Interactive Discussion

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Yours sincerely.

G. Gea-Izquierdo & coauthors

Anonymous Referee #2 Comments General comment. It's unclear how climatic drivers can limit carbon allocation. I think climatic drivers will change patterns of carbon allocation, but not limit allocation itself. In addition, secondary growth is considered, but what about primary growth. I know is difficult to have a record of annual growth of the overall parts of the tree (branches, secondary shoots, roots, etc. . . .). However, it's difficult to discard this important annual sink of carbon if a realistic model has to be elaborated. We change now “limit carbon allocation” to “change patterns in carbon allocation”, e.g. see line 44. Primary growth is included either in what we called “allocation to the canopy” or in “allocation to roots”. See e.g. line 313. We agree this is a critical point that merits further development when there is availability of such data.

Specific comments: Line 13 of abstract what kind of environmental changes are being considered by authors? Temperature increase in future? Concentration of CO2 in atmosphere? Drought? Evaporative demand? Recurrence of dry periods? All together? Authors must be more explicit. We now state “to enhancement in water stress”, which is driven by higher temperatures and also lower precipitation at one site (Puechabon). See new line 29.

Line 15 Details of how ecosystem WUE was estimated should be pointed out. Please, see lines 418-419 (the last in the M-M) where we specify how we calculate WUE and iWUE following Beer et al. (2009)

Line 16 It seems GPP followed a decrease according to a progressive lowering of rainfall in one of the sites. However, it's a little misleading for reader to what are referring authors, whether total annual rainfall or increase of variability in annual or monthly rainfall. We specify now “annual”, line 34.

Problems in using average annual values for LAI and SLA. We do not use an average value of LAI but let LAI vary during the year with leaf shedding and leaf flush (in Figure 1 we show an outline of leaf phenology). In figure A3 we show the maximum annual value used. Regarding SLA, we use an average value. It is known that there is certain variability in SLA in response to environmental forcing, we are aware of this (see references therein). However we did not have a way to estimate how this variability works at the stand level in space and time and preferred to simplify and use average values to avoid adding extra sources of complexity in the model that we could not accurately take into account.

Considering Rd as a direct function of An can include important bias in the model. Rd changes with temperature following an exponential function with a change in the sensitivity of parameters as Q10 with water stress. In Mediterranean systems carbon losses are as important as carbon uptake. Thus, modelling respiration should not be oversimplified by a mere linear dependence with An. Maybe, modifying exponential response to temperature of Rd, according to water stress, would improve the models in a more realistic way that a mere linear dependence of Rd with An. On other hand the linear dependence of Rd-An assumes implicitly a constancy in the An/Rd that is well known from ecophysiology not true. A similar shortcoming arises from the linear dependence of Jmax with Vcmax (line 22, page 2752). It's true both are highly coupled, but it's unclear how the Jcoef is inferred. We agree with the statements presented here. But as we state in lines 196-202 we tried different formulations when developing the model including also explicitly SWC but decided to leave the direct function as the best option for our application. Regarding Jmax/Vcmax, we tried also several formulations as from De Pury and Farquhar 1997 and Masseyk et al. 2008, but decided to leave a simpler relationship because the model proofed better and more stable (when compared to the data used for calibration). Even if the processes themselves have been shown to be non-linear functions of climate, performance of our model was better through further simplifications of these relationships. This is because there is a less complex parameterization to be done and also because of the nature of our data-driven approach. We acknowledge that there are always simplifications in models and try to state them clearly in the text (M-M).

Minor comment authors change abbreviator ï£ijfrom An to Ac without a clear rationale. In addition, I do not see necessary to include the sub-index (i) in the formulations. It's clear most parameters are variables which value depend of some constants or other functional variables. It can be inferred from lines 196-199 that An=Ac-Rd, and that Rd=f(Ac). We prefere to leave the subindices to show more clearly when different variables are a function of different time steps to avoid misinterpretation of equations.

In the last years it's beginning to be clear the need to consider Cc instead of Ci in the model of Farqhuar in order to take into account effect of some functional parameters as mesophyll conductance to CO2. This seems not to be relevant for authors, though a comment is included in passing when coupling stomatal conductance with photosynthesis from a modified version of Leuning (1995) equation (line 4 page 2754). At least a brief comment on the matter should be included to justify the use of Ci instead of Cc in the Farqhuar model. Yes, this is also true. Please, see new lines 208-213 and 245-248 where we further explain how this is address in the model and acknowledge the effect of possible simplifications applied.

It's unclear how authors split total LAI in sun and shade components. If a co- efficient of extinction is used to model in continuous LAI though the crown by following the Beer–Lambert law, how it's established the threshold to consider leaves of sun or shade type. Total LAI is divided on sunny and shaded leaves. The amount of sunny leaves follows the expression in line 253 and then shaded leaves are just the total LAI minus LAIsunny (line 254, it was not properly expressed before). Photosynthesis is calculated for both leaves (sunny, shaded) and then integrated to the amount (i.e. proportion of LAI) of sunny and shaded leaves considered. There is no threshold, just LAI is divided in sunny and shaded components and integrated to get canopy values.

The model considers different allocation of carbon canopy, stem, roots or storage of non-structural carbohydrates (NSC), but losses as respiration are consider at the overall tree without any consideration of the specific respiratory patterns of the different carbon sinks (equation 7 in page 2754). Yes, this is rigth. It is one of the assumptions of the model, for the same reasons explained above when we discuss why calculations of some parameters are simplified

Again the ratio root/leaf is considered constant to 1.5 whether it's well-known it changes with site, time and species. This kind of limitations, and those previously mentioned, should be addressed by authors at least with a brief comment. Please, see lines 314-315 where we explained this point and that we use 1.5 since this is the value estimated for the studied places as in the references mentioned (and unpublished data available from coauthors of this paper)

Results The increase in iWUE but not in WUE could be explained only from an increase in LAI if interdecadal GPP did not change significantly. However, this not seems to be the case. How authors explain this mismatch between the two proxies of water use efficiency. We have rewritten this point because it was not clear: there is a close match in the high-frequency annual response in both sites but there is a long-term mistmatch particularly in last years at the site where there is a decline in GPP (e.g. see lines 32-35, 474-480). The combination of gs, E+soil evaporation (=ET) and GPP determine this relationship, and just the last three are influenced by changes in LAI simulated.

Discussion In line 20 page 2761 What are authors meaning when they refer to leaf activity? Photosynthetic activity? Respiration? Phenological phase? Please make a more precise use of physiological concepts. Stomatal conductance is coupled to other diffusional and biochemical processes that affect carbon uptake. We add "photosynthetic capacity" to be more precise (please, see line 488)

In line 21 page 2761, the model does not simulate carbohydrate storage. At the most, it simulates carbon allocation. Yes, the model calculates carbon allocated to storage dynamically as a function of environmental variability and in relation to carbon allocation to other plant components. Please, see M-M

In line page 2762, growth is considered as the only carbon sink for trees, however in many ecosystems and especially Mediterranean ones carbon losses from respiration and VOC emissions are importantï£ijï£ijï£ijï£ij carbon sinks. Again, authors should consider this issue briefly. We now consider this point on lines 508-509

In page 2763 line 12- 14. It's valuable the work of authors in improving previous models. In my opinion, the endeavour for modelling in the future should be focussed to evaluate effects of intense perturbations over impact of average climatic values. In fact, variability in climate could be as important as changes in total precipitation or average temperature. Yes, we agree this is an interesting point to address in models

To finish this review, I would have liked to see any comment on the changes in potential competitiveness of the species. The model addresses performance of two very different species at one of the study sites: Q. ilex and P. halepensis. Maybe, it would be interesting for reader to include a brief comment about the expected differential performance of both species in terms of carbon allocation and GPP. We add now a statement on this subject, lines 627-630

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C1663/2015/bgd-12-C1663-2015-supplement.pdf

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