Biogeosciences Discuss., 12, C701–C704, 2015
www.biogeosciences-discuss.net/12/C701/2015/

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.

Anonymous Referee #2

Received and published: 20 March 2015

General comment The paper of Gea-Izquierdo et al. is an interesting exercise of calibrating a process-based model to forecast uptake and allocation of carbon by using a combination of eddy covariance CO2 flux data, dendrochronological time series of secondary growth and forest inventory data as raw data. I recognize process-based models are very complex, and of difficult implementation because the numerous parameters to take into account. But, I consider that despite the shortcomings I add below, the paper is a good contribution to the advance of this kind of proxies in the analysis of forest ecosystems carbon balance. I include some comments I'm aware

may be a little difficult to implement at present work, but that I think could be interesting for future studies and in any case I hope will help authors to improve the final version of this paper.

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.

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. Line 15 Details of how ecosystem WUE was estimated should be pointed out. 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. Problems in using average annual values for LAI and SLA. 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. Minor comment authors change abbreviator from 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. 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. It's unclear how authors split total LAI in sun and shade components. If a coefficient 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. 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). 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. Results The increase in iWUE but not in WUE could be explained only from an increase in LAI if inter-decadal 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. 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. In line 21 page 2761, the model does not simulate carbohydrate storage. At the most, it simulates carbon allocation. 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 important

carbon sinks. Again, authors should consider this issue briefly. 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. 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.

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