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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

Reviewer #1: If I understood correctly, the authors attribute the decline in LAI since âĹij1990 to a decline in precip. Why did E (and gs) remain constant while LAI and Precip declined (and water stress presumably increased, as the authors say)? Yes, we attribute the decline in LAI (Figure A3) in the Q. ilex woodland to the decline in precipitation observed at that site (Figure A1). However, please see that gs and E did not remain constant: E shows a decline similar to that in GPP (Figure A3) while gs decreases in the late 1970s and then remains constant. We show Figure A3 with another scale to depict clearer these effects and rewrite the text to be clearer in the discussion (see lines 30-34, 462-480 of the revised version). Similar acclimation processes at the canopy level (LAI) have been observed in different drought experiments (e.g. Limousin et al. Misson et al. in the text).

Since E is so much smaller than precip (âĹij50% in Fontblanche), one could think that the site is not water limited. What's the potential ET? Where does the difference between P-ET go? (I presume that's a flat site, no run-off) Yes it is a flat site, but both sites are on a calcareous substrate, which means much water will be percolated into the soil and not available for plants. Additionally in the studied site precipitation generally happens on stormy episodes. We set maximum water holding capacity to field measurements carried out within the studied sites (see references within the ms). E values shown in the ms are of similar magnitude to those in Gaucherel et al. 2008 for a similar ecosystem.

The authors claim to have addressed the source vs sink debate. I was wondering as to whether the effects of eCO2 could be discussed under this angle (sink, and not source, controls). The model really is one driven by assimilation and, as the authors point out, the results are driven by the equations used. However, we don't know, for instance, whether Leuning's model is or not an accurate representation of responses to eCO2

(potentially affecting the interpretation of iWUE) and, if growth is sink driven, then it could affect the statement of: “Thus, the absence of a long- term increase in GPP and growth would not mean that enhanced [CO2] was not beneficial for model outputs: growth and photosynthesis would have been lower had we used constant [CO2] with the same model parameters.“ Yes, we agree. Please, see the discussion particularly in lines 602-607 where we add “. . ., particularly in the case of C-source limitation”.

We have a few models that link assimilation with growth. Why another one? What’s the justification for deriving such a new model? How does it compare with other models? Could the results obtained from this model not have been obtained by using the existing ones? The model we used is not new. It was based on an existing one (MAIDEN; Misson 2004) in which we have made a number of developments in order to improve model performance in the light of the data used and hypotheses assessed. We have cited other models in the discussion to show how they perform in terms of goodness-of-fit (lines 570-574). To check how they perform more in detail compared to our results they should need to be analysed on “ad-hoc” modeling comparison. Likely, our results of GPP should be similar to those process-based forest models based on similar photosynthesis equations, but differ in the allocation pattern particularly with those models considering allocation constant.

I find Fig. 5 quite puzzling as it seems to indicate that an extreme drought is the most favorable environment for growth (at least, for allocation to growth), yet water scarcity is likely to limit growth under those conditions. I presume AN under those conditions will be close to zero anyway because of respiration, but what’s the theoretical basis for assuming that growth (driven by turgor) is less sensitive than allocation to the hot and dry? Please, note that in Fig 5 we show the opposite, namely that more humid conditions lead to more carbon allocated to stem growth and less to storage. Therefore, we agree with your comment.

A minor comment: I found the nomenclature a bit confusing. For instance, the authors refer to allocation to stem and allocation to storage as different things, yet a large

part of storage happens in the stem. I presume the authors mean allocation to stem growth? Yes, this is true. We modify this now for the sake of clarity, please, see e.g. new lines 23, 89, 329

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C1653/2015/bgd-12-C1653-2015-supplement.pdf

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