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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.

Anonymous Referee #1

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I was a bit surprised to be asked to review this manuscript since I am neither a modeler nor an allocation guy, so this review should be understood under the light of these limitations.

I enjoyed reading this manuscript. Its main strength is the development of a new approach to model allocation. The manuscript does not present allocation to all the different compartments, but it is focused on allocation to stem growth. The results show an overall agreement between stem growth inferred from the model and measured using tree rings. The manuscript also touches on some of the issues discussed these days,

such as source vs sink controls, or CO2 effects. The manuscript is very nice and the experiment seems to have been performed well. I just have a few questions.

If I understood correctly, the authors attribute the decline in LAI since ∼1990 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)?

Since E is so much smaller than precip (∼50% 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)

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."

We have a few models that 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?

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?

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?

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