Biogeosciences Discuss., 12, C914–C919, 2015
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Biogeosciences
Discussions

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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 #3

Received and published: 30 March 2015

Gea-Izquierdo et al. present a landscape-scale biogeochemical model for the exchange of carbon over forest ecosystems. They describe adaptations to the basic model formulation to try and better represent the assimilation of carbon and its allocation within the tree. Key aspects of their formulation are the incorporation of non-structural carbohydrate storage and the medium-term acclimation of photosynthesis and growth rates to temperature and water availability. They calibrate the model against observations from two forest sites, and use it to demonstrate that the processes of growth and photosynthesis are decoupled at these sites, in accordance with ecophysiological theory. They further find an increase in the water use efficiency (WUE) of individual plants over the last half century, but little change or a decrease in the canopy-scale WUE. I find the analysis well conceived and well presented, providing further evidence that the recent arguments put forward in the scientific literature that emphasise the importance of a carbon-sink, rather than just a carbon-source, perspective of vegetation modelling is relevant at the canopy scale.

There are several places in the manuscript where I believe it would be appropriate to give a little more information, as detailed below, along with several minor suggestions/corrections. If these are addressed, I would absolutely recommend the analysis for publication in Biogeosciences.

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.

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. 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

BGD

12, C914–C919, 2015

Interactive Comment

Fontblanche. Surely, using the model outputs, it is possible to be more definitive on this?

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; Körner, 2013, Nova Acta Leopoldina NF 114 , Nr. 391, 273 –283).

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.

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.

BGD

12, C914–C919, 2015

Minor 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).

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

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.

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.

pg. 2749, l17: What is "at a greater scale" referring to? Spatial? Temporal? How big?

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.

pg. 2753, l1: Based on what criteria did it behave better?

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

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?

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

pg. 2758, l20, l25 and Fig. 2: I think the units of stem biomass increment should be g m-2 year-1 ("-" missing)?

pg. 2759, l21: What is meant by "well-coupled"?

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?

pg. 2764, l8: I don't think it is possible using Fig. A4 to separate a pure CO_2 effect on gs, from the effect of [CO_2] on temperature? But it would be easy to make such a separation using factorial experiments (e.g. fixed [CO_2] or fixed climate).

Fig. 2: At which level of confidence are the confidence intervals displayed?

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

Grammatical and typographical corrections

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"

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."

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

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

pg. 2750, l3: Are you trying to say that that these relationships differ between phenophases?

pg. 2750, l18: "...dense coppice in which..."

pg. 2751, l25: Better to say "eddy co-variance fluxes", rather than data.

pg. 2752, Eq. 1: I think the first instance of Ko in this equation should in fact be Kc?

pg. 2752, l15: "compensation point"

pg. 2753, l8 and throughout: "leave" should be "leaf".

pg. 2753, l9: Presumably this means "reduced lower leaf replacement rates in response to long-term water stress"?

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

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

pg. 2757, l11: NEP is not yet defined.

pg. 2757, l16: "In a second step..:"

pg. 2764, l3: "...co-responsible for active acclimation of plant physiological processes..."

pg. 2764, l11: I think you simulated increase WUE, rather than observed it?

pg. 2765, l6: Bouchard et al. (2014) is not in the reference list.

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