**Earth System
Dynamics
Discussions**

Interactive comment on "Analyzing the carbon dynamics in north western Portugal: calibration and application of Forest-BGC" by M. A. Rodrigues et al.

Anonymous Referee #2

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The current manuscript relies on the Forest-BGC model to evaluate the impacts of temperature and precipitation in NPP for a region in NW Portugal. Although the subject seems to match the scope of ESD, the manuscript is in need of significant attention. In general I find that:

1. The objectives of the study should be clearly defined: the title "Analyzing the carbon dynamics in north western Portugal: calibration and application of Forest-BGC" does not reflect the goal introduced in the abstract "this study aims to analyze the climate evolution at the Vila Real administrative district during the last decades" nor the one expressed in the conclusions "The principal focus of this study has been on the important

role of the NPP in relation climate change, biodiversity and forest management." Such ambiguity in the study's objectives is reflected throughout the manuscript: a claimed model calibration and accuracy is not shown (see below) and the modelling component of the manuscript is quite small, while most of the results refer to observed trends in temperature and precipitation time series. Upon the clarification of these points, the manuscript's structure should be revised accordingly. In addition, to aim for consistency between the several parts of the manuscript, I would also suggest searching for balance between the different components (e.g. the results section is extremely short, which seems to result from the fact that many results are mixed with previous methodological and descriptive sections).

2. The methodological part needs significant improvements, both in terms of (a) organization and (b) conceptualization:

a. Some of the shown results seem to yield from non-explained methods (see below);

b. The authors do not show a model calibration. Instead, it seems – by reading section 4.1. – that LAI is considered as a model input; but this is not explicit and the authors should clarify. This cannot be considered a calibration exercise, since no adjustments in model parameters were performed based on the comparison between any model outputs and measured variables [c.f. Wang et al., 2009].

Also, the model description could be improved and explicit why the other dependencies from temperature or precipitation, or other factors controlling primary production, were not considered here.

3. The results section seems to be spread through section 3 - although explicitly only in section 5 - and is quite unbalanced, since most results report trends in the climatic variables and insufficient modelling results are shown and discussed. More importantly, the results shown seem to be based in methodological analyses that are not described in the previous sections (namely the results in Table 1). Non appropriate concluding remarks are also shown here.

The authors should consider: harmonizing the results with the methodological section: breaking down the results section into subsections; showing modelling results and model performance prior and posterior to the changes in the model inputs; revise the conclusions according to it.

4. The conclusions should be supported by the results shown in the manuscript: throughout the paper there is no notion on the model's "applicability and accuracy" to this case study; the sole model results provided are in Table 1 and seem associated to a sensitivity analysis and not to a comparison with observational data; there is no reference to any simulations performed for climate change scenarios except in the abstract and the conclusions.

Overall, the subject approached here is relevant and the novelty of this research would come out of the results from a specific case study and the addition of an observational dataset. However, there is a significant need to improve the organization and methodological structure of the manuscript in order to show robust conclusions supported by a clear and sound methodology based on observed results. Ultimately, remains the idea that the current work relies on a valuable in situ LAI dataset to support a modelling exercise. However, in the current status, this work reveals some flaws requiring significant efforts for integration in a modelling framework.

Some particular comments:

The title could be more specific and reflect the focus on the vegetation carbon dynamics.

The references present in the introduction could be more appropriate (for example, the reference used for P42 L23-26 is improper) and many should be preceded with "e.g.". The paper focuses strictly the vegetation dynamics of forests (which could be explicit in the title), although the soil component should be acknowledged as a significant contributor in the carbon dynamics in forests, and ecosystems in general.

I find the need for a profound English revision. I list some examples of improvements in terms of syntax, but also find the need to revise the phrase construction:

- P44 L4: "simulation modelling" -> "modelling"

- P45 L9-10: "It works simulating water"... -> ..."simulates water"...

- "precipitations" -> "precipitation"

- "figures" -> "values"

P42 L14-17: the total of 45 sampling plots does not match the total of 19 (Pinus) + 17 (Quercus) + 10 (mixed). This is also repeated later in the manuscript.

P46 L14: add reference.

P47 L6: "repartition of the precipitation"... do the authors mean "distribution of precipitation"...?

P47 L7-8: "how precipitation works in"... -> "how precipitation influences"?

"3. Study area and data": the characterization of the study area would gain from the introduction of a temperature map. Also, the differentiation of the three temperature regions is only distinguished by a 1°C difference between them? Is this significant? Also, there is a significant variability in the tree structure populations – according to the description of the field sampled data – how was this variability considered in the simulations?

"3.1.1 Data quality": this section highlights the importance of the distribution and amount data gaps in the time series of climate variables but it is no clear how the authors handled this problem. The method referred to in P48 L8-10 should be explained or a citation added. This section would gain much from an English revision as well.

"3.1.2. Data analysis": the significance level of the trends found is not provided. I would

suggest the condensation of all this information in a table – and maybe add just one example figure. It would be important to understand if the issues related to the data gaps identified in the previous section influence these trends significance.

P49 L18: what is "phase 1 and 2"?

"3.2. Ground data": adding a synthesis table with relevant information would greatly help understand the data at hand.

"4.1. Calibration of FOREST-BGC": this section covers mostly technical details on how to deal with the model's input-output in an informatics perspective – at least apparently – which is not appropriate. Conceptually, this description does not translate a calibration process (see above).

Regarding model simulations there is no information about: temporal range of model simulations; which years do the datasets refer to; comparison between model outputs and in situ observations...

P50 L23-25: define "PPL" and "Pb". This sentence is awkward.

Table 1: a better description of the differences between each simulation is required (what do the changes in temperature and precipitation refer to?). This seems to report a sensitivity analysis on the modelled NPP that is not explicitly explained in the methodological section of the manuscript.

References:

Wang, Y. P., C. M. Trudinger, and I. G. Enting (2009), A review of applications of model-data fusion to studies of terrestrial carbon fluxes at different scales, Agr Forest Meteorol, 149(11), 1829-1842.

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