**Earth System
Dynamics
Discussions**

Interactive
Comment

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

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We thank P. A. Robledo and the two anonymous referees for their careful reading of the manuscript and the detailed constructive comments which have helped to improve the manuscript.

Anonymous Referee #2

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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

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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."

In fact this evolution study was done and that is not yet so clear. We could and should developed more this part of the study even because it represents a nice piece of results specifically for this region. Only in a second stage we want to focus in the relationship between climate and NPP, but based on results from the first stage.

Such ambiguity in the study's objectives is reflected throughout the manuscript: a claimed model calibration and accuracy is not shown (see below). The model was calibrated and validated for this area by Lopes et al. (2009) and that was not efficiently reported.

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).

We agree with you. The results section could be expanded and in the end the balance between the different parts of the manuscript could be much better. This mixture between parts is only related with the fact that some previous results have influence on the following methodological procedures.

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

We completely agree with you.

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].

And it is not a calibration process. That stage was previously done, results were already published and this is now using that part of the study. we agree that was not clear in this paper but we would like to turn it more clear and improve it.

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.

We will do it. We did not do it before because we were worried with repetitions. Lots of previous papers have already done it. But we also consider that a more intensive explanation of the model should be done.

Anonymous Referee #1

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The authors present an application of Forest-BGC to estimate NPP in three different forested areas in Portugal trying to assess the effect of the changes in temperature and precipitation on the model results. Although the topic is interesting I found the paper not clear and with major lacks and flaws and for this reason I recommend to reject it.

We would like to improve our paper, based on the comments received and we only can do it if the paper would not be rejected.

The main concerns are:

1) Methodology is unclear and without detailed description. The model calibration procedure is not explained. Model calibration is not only the use of specific meteorological data or LAI values. Has been the model calibrated at the site?

We agree with you. The previous referre has already reported that. The model was previously calibrated and validated for the site. results were published by Lopes et. al.(2009). We can clearly improve this part of the methodology. The model can describe quite accurately the photosynthesis of these ecosystems, in this region. It is a powerful tool for monitoring NPP, thus carbon sequestration. In this part of the shudly we were trying to go a little further and to understand if different climatic characteristics would have implications on carbon sequestration dynamics. That was not so clear but we propose to improve it.

3Add references to figures and tables in the text!

We will do it.

Figures 2 to 7 are not very relevant for the paper, could be summarized in one or two.

Typo in the Y axis (Rainfall instead of Rainfall)

We will merge it and clear the unimportant figures.

P50-L23-24: define PPL and Pb

Sorry, instead of PPL we should write NPP. It is the same (only written in Portuguese). Pb is the abreviation of Pinus pinaster (in Portuguese means Pinheiro bravo). We will correct it.

P46-L5: leaf carbon or leaf dry matter? In the methods explained in 3.2 the reference seems to be dry matter weight.

It is dry matter.

All you specific considerations will be very helpfull for a better improvement of this paper. We will have all in mind.

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CO2 analysis and measurements is so complex and the results are very difficult to interpret. In general, the paper is a good propose for regional study of the ecosystem roll in the CO2 balance and exchange, as well as to understand better which is the roll that plays ecosystem in the climate change and global warming.

We completely agree with you. It is a very complex subject. These each small step we can follow can be extremely important for a better understand with implications in forest management, we hope!

However, the study area is very small for support general conclusions and is not rigorous affirm that forests are acting as major sink of CO_2 because oceans and internal Earth system can trap or release much more volume of CO_2.

The study area is obviously too short but this is only a first step. At this time we mainly want to understand if the methodology will allows to understanding these processes. If yes we are planning to apply it to the national scale. And forests are important as carbon sinks (the Kyoto protocol agrees with us). We are not saying it is the most important sink.

However, in other hand, have not been referenced same important articles related, like Martin Heimann & Markus Reichstein, (2008), (Nature. 451/17. 289-292 pp) where they discussed the evidence of how the ecosystems will provide a positive feedback in a warming world albeit of uncertain magnitude, according CO_2 Earth models.

We will read carefully this paper. Thank you so much for reporting it.

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