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Biogeosciences Discussions

Interactive comment on "Ecophysiological modeling of the climate imprint on photosynthesis and carbon allocation to the tree stem in the North American boreal forest" *by* Fabio Gennaretti et al.

Anonymous Referee #1

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General comments:

Gennaretti et al describe modifications to the ecophysiological MAIDEN model for simulation of tree ring width and gross primary productivity, including parameter estimation, and compare results with observations for the purpose of validating the model. The work builds on the extensive development of MAIDEN over man years. The presentation is admirably compact and concise. I suggest revisions to expand and improve the diagnostic study of the simulations with respect to ecophysiological controls on the predicted variables, further discussion of the extent to which the results are explained by mechanistic processes operating within the model, multiple sensitivity of the results to parameters and variables; and improvement of the presentation to separate clearly

Results, Discussion and Conclusions. The discussion of the ecophysiological and environmental controls should be central to the abstract. The publication of the comparator dataset in the supplement should be accompanied by publication of MAIDEN code in the public domain such that others may experiment with this well-studied and highly valuable model.

Specific comments:

0. Abstract:

0.1. clarify/revise results statement, what is meant by 'full spectrum', give specifics.

0.2. use of the word robust means you have done validation or out-of-sample tests of the model. Have the authors done so?

Introduction:

1. pg 2, l. 1: define 'secondary growth' at first use.

2. pg 2, l. 5: also roots; e.g. Moorcroft (2006) and description in section 2.1.

3. pg 2, l. 25: I think you mean to say that these models should be able to simulate the following observed phenomena: (i,ii,iii).

4. pg 2, l. 31-32: briefly explain for those unfamiliar with its development, why MAIDEN is an ideal model with which to work for the purposes of this study. For instance, in the sentence prior, you've noted that it was developed for Mediterranean and temperate climates; why should it be suitable for simulations in boreal climates? Here you can borrow from section 2.1 the salient descriptive points, saving for section 2.1 the description of the modifications and the experiments performed for this study. But make the argument why the model should be suitable for the present study (noting also your point that it has never been applied in 'environments mostly sensitive to cold temperatures'.

Materials and Methods:

5. pg 3, l. 21: I think you mean here: model has not been used to simulated forest growth in boreal conditions. See also note 4.

6. pg 3, l. 19-21: "Drought and water stresses are well take into account": support this statement with citations and references, but otherwise I suggest to save such statements for the Results section.

7. pg. 3, l. 29: describe how the parameter estimates are cross-validated. To determine 6 or 12 parameters simultaneously, conditioned on two variables, must require a lot of data but also out-of-sample testing [to revisit after reading Supplement 1].

[to delete? 8. pg. 4, l. 1-30: please revise to better distinguish prior formulation of MAIDEN and the modifications introduced here. What is existing, what is new here?]

9. pg 4, l. 30: Euler's method might not be suitable in the case of a large time step, large change in the rate of change, or both; consider a Runge-Kutta solver, relatively straightforward to implement.

10. pg 5, l. 10-15, 16-20, 21-25: the determination of phenological phases seems highly specified for a modeling striving to be more ecophysiologically based (Introduction). Instead of basing these phases on correlation studies (empirical), could they be estimate from other properties of the environment, or prognostic variables within the existing model? In addition, please justify all choices of hard parameters, for instance, pg 5, l. 19, pg 6, l. 22.

Results/Discussion:

11. General remark, section 2.3: if the simulation produces a good out-of-sample or independent fit to observed predictors, then it would be good to diagnose the model: what factors are most important controls on the fidelity of the simulations? Because this is an ecophysiological modeling study, this would be much more instructive than the statistical regression analysis, although the latter may be used to support the intepretation with respect to modeled variables. Therefore, please add ecophysiological

diagnostics to this section or a new section 2.4.

12. Section 3.1, pg 9, l. 3: explain here and/or in the Table 1 caption the definition and how to interpret the series of numbers that are in the last column of the Table. What exactly do you mean by "sharp" here and on pg 11 (I think I know, but give a more objective description of what you mean for the reader).

13. pg 9, l. 7-12: "However, the ensembles of daily and annual time series retained by the MCMC sampling were not always centered on the observed time series (Fig. 5)..." Revise and expand to reflect that the simulated annual GPP values overestimate the actual GPP at low observed GPP. This will better reflect the excellent information content of this figure.

14. pg. 9, l. 13-17: Put uncertainty estimates on Fig 6 and use them in the description of results and in discussion later.

15. pg 9, l. 13-17: "The model explained about 20-30% of the 15 observed yearly RWhighF variability corresponding to correlations of 0.58-0.66 (Fig. 4b). This is a good result because simulated detrended annual GPP values (i.e. photosynthetic assimilation before any carbon allocation) had only negative R2 with RWhighF (Fig. 4c; meaning performance worse than a straight line centered on RWhighF). This suggests that the modified MAIDEN daily partition of carbon in the plant compartments significantly improved the concordance with treering observations." Although I am not sure I understand this result (and its discussion; please clarify, in mechanistic terms, why we see the results in fig 4c?): If the correlations in Fig 4c are statistically significant (estimate p-values), then this is an even more important result than described, because not only are the model improvements an important advance, but they correct a result that would otherwise produce the opposite correlation.

16. pg 9, l. 20-24: Fig 7b, here and elsewhere: it is an interesting result! But where r is given, also give effective degrees of freedom and p-value; interpret based on the p-value as statistically significant or nonsignificant.

17. General remarks on sections 2 and 3: Reorganize the content in these sections into separate Results (section 3) and Discussion Sections (new Section 4), with subsections as appropriate. Results are what was objectively found and will be discussed; Discussion is for interpretation of the results. As it is, Results and Discussion are entwined, but it would clarify for the reader to separate and distinguish them. I would suggest to focus the Results on the following items of interest: (1) sensitivity of the simulations to specified parameters; (2) mechanistic and regression-based diagnostics. I would then put in the Discussion the following argument: (1) The results are sensitive to parameter estimation in the following ways: .... but: (2) Comparison with independent observations suggest MAIDEN as revised produces more accurate simulation of GPP, TRW, intra-growing season dynamics ... which are (3) consistent with response function analysis, and (4?) here are some predictions made by the model/simulations, that could be tested with additional observations.

Once these revisions help to reorganize the essential content of the paper, it will be easier to evaluate the expanded ecophysiological interpretation, which I think should be more central to the main thrust of the paper than the response function analysis (Abstract; section 3.3).

18. Section 3.2: Revise the title for English; perhaps: Mechanistic diagnostics? And consolidate mechanistic results here, with their discussion in the Discussion section. Moving the Supplemental Figures that are most relevant for the central elements of the argument into the main text, and by expanding this part of the results, this may address my previous comment #11 on Section 2.3. This will help the reader understand what is going on in the simulation that explains the consistency with observations.

19. p. 9, l. 30: "..is sensitive to all..." Discuss how the sensitivity of the results to parameter estimates related to "Vcmax or thetaG, except soilb", complicate the diagnostic interpretation. Are there multiple controls that all could produce a similar result and good fit to observations?

Conclusions:

20. General reamrks on Section 4: some of the points are speculative (e.g. p 12, l. 24-26; paragraph starting at l. 30); these belong in Discussion rather than Conclusions (see note 17, last sentence).

21. I would appreciate more discussion of fig S2. Interpret the bimodal structure of the posterior probabilities; connect to the plausible value descriptions in the last column of Table 1.

22. Is MAIDEN publicly available (it was first introduced in 2004), and if not, could it be made so, to encourage experimentation in other environments, species, applications? This would be a great contribution and it would be consistent with the open data access policies of CP.

23. Fig S12: this is not isoMAIDEN as in the caption, correct?

24. Trivia: for future revisions if necessary, make line numbers cumulative rather than by page

25. Trivia: p. 9, l. 30: "..is sensitive to all..."

26. Trivia: Slight revisions for English grammar and usage: pg 3 l. 3-5; please go through entire manuscript to revise for grammar as well.

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