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Biogeosciences Discussions

Interactive comment on "C allocation among fine roots, above-, and belowground wood in a deciduous forest and its implication to ecosystem C cycling: a modelling analysis" *by* M. Campioli et al.

Anonymous Referee #2

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1) Does the paper address relevant scientific questions within the scope of BG?

yes

2) Does the paper present novel concepts, ideas, tools, or data?

The allocation model is not completely new, but the integration of the direct effect of stress is seldom done and the coupled model is interesting

3) Are substantial conclusions reached?

Not exactly since the improvement of the model by the use of the new allocation model is not proved.

4) Are the scientific methods and assumptions valid and clearly outlined?

yes

5) Are the results sufficient to support the interpretations and conclusions?

Yes, but some improvements can be done.

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Yes, but a clarification of data used for calibration and validation should be added

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes, but not enough discussed in the discussion section

8) Does the title clearly reflect the contents of the paper?

Yes but too long.

9) Does the abstract provide a concise and complete summary?

Yes, but the functional basis of the carbon allocation model is missing

10) Is the overall presentation well structured and clear?

Yes very clear it is a quality of the paper.

11) Is the language fluent and precise? yes

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

yes

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13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

no

14) Are the number and quality of references appropriate?

yes

15) Is the amount and quality of supplementary material appropriate?

yes

General comments

This paper deals with an interesting subject: the carbon allocation in forests. It is well written, I have almost no comments on the form of the paper. I have four major critics:

- Firstly, the state of art is not complete and the authors do not compare their approaches with others ones in the discussion. Two recent references are missing: Litton et al. (2008) and Davi et al (2008). Moreover, the use of average tree model to simulate the effects of carbon allocation needs to be discussed since competition, not taking account in this kind of model, is an important factor acting on allocation.

- Secondly, the model is well described, but the reader does not know clearly, if the data used for parameterization (and calibration) are not also used for validation.

- Thirdly, the demonstration is not totally satisfactory, since authors do not separate the carbon allocation in terms of quantity of carbon and allocation in terms of ratio between carbon allocated to a sink and carbon assimilated. Indeed, when they compared wood production measured versus simulated, the probably main effect is the variability of GPP. To separate the direct effect of GPP variation and the effects of variability of allocation, authors needs either to divide wood growth by carbon allocation or to compare two versions of the model: with fixed allocation and with complete allocation scheme as in Davi et al. (2008). I used the data from table 3 to compute the ratio between wood

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growth and GPP. The result is satisfactory and instructive: there is a discrepancy in 2000, and the level of variations is underestimated by the model. I advise the authors to include it (I can not give the figure here, but I can send it to the authors).

- The only variability between years is assessed, and therefore essentially the effect of drought. No analysis concerning the variability between sites and climates of the allocation schema is done. This point needs to be discussed, since the generality of the model can be questioned, all the more since calibration and validation are not clearly separated. Transposition on other sites should be tested, at least discussed in the discussion.

Specific comments Title: The title should be shorter: For example Carbon allocation in a deciduous forest: which effect on ecosystem carbon cycling?

Introduction: No comments

Abstract: Authors should add the functional basis of the carbon allocation model.

M&M: 1. The water budget is not included in the model. If I well understood, authors used outputs from Granier model (p3786, l8). It puts some problems since water stress acts on stomata conductance and it acts on evapotranspiration and then on water balance. Using two models, not coupled, can cause inconsistent.

2. Why using fixed dates for phenology (p3788, l3) knowing the important variability due to (i) temperature, (ii) link with carbohydrates levels; Some improvements of this point need at least to be discussed.

3. Why using level of NPPLY (p3788, l10) since a level of reserve will be more suitable.
4. The model of functional balance needs to be described (p3787, l25).

5. the C reserves are then distributed in fixed proportions (p3788, l5). It is false, in Barbaroux et al, it is the proportion of carbon allocated to wood from assimilates and not reserves, the carbon allocated to compartments from the reserves can only be calculated using isotope marker.

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6. Explain better why there is a priority to fine roots (p3788, l9)?

7. Including the direct effect of water stress is a real advance of this paper (p3789). Authors should better focus on that point in discussion.

8. Why calibrating leaf fall parameters on NPP dynamics since leaf fall measurements should exist in this fluxnet site?

Results

1. The leaf fall is simulated to early; it is amazing since this part of the model is calibrated on measurements. The separation between calibration and validation is not clear.

2. Paragraph 3.2: the link between results and hypotheses of the model are not clear enough.

3. The discussion of interannual C pool variations is a judicious analysis.

Discussion

1. p3796 l25: table 3 does not prove that C allocation is well simulated since GPP variations could over-determines the wood growth pattern. You need work on ratios.

2. p 3797 l1: The time lag effect of 2003 is not completely captured, since the decrease of wood growth measurement in 2004 is much stronger in measurements than in simulation (see table 3)

3. p3797, l8: Wood growth dynamics exists in Bouriaud thesis, ant the question of the time lag between wood growth and budburst needs to be further studied.

4. p3797, l16: "First the proportion..." The sentence is unclear since the the proportion of reserves changes in your model.

5. Other explanations can be found to explain the underestimation of reserves: the way to model the SLA/LMA dynamics. The sink of leaves depends of their mass, and

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maximum surface is reached in 15 days, maximum of mass is reached in 30 days. If we incorporate this delayed growth in mass and the fact that carbon used to product buds comes from the previous season, you also decrease the sink constituted by leaves.

A conclusion will be welcome.

two recent works on that topic

Davi, H., Barbaroux, C., Francois, C., le Dantec, V., Dufrêne, E. The fundamental role of reserves and hydraulic constraints in predicting LAI and carbon allocation in forests. In press Agricultural and Forest Meteorology. doi:10.1016/j.agrformet.2008.08.014

Litton, C.M., Raichw J.W., Ryan, M.G., (2007). Carbon allocation in forest ecosystems. Global Change Biology, 13, 2089–2109

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