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Comment

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.

M. Campioli et al.

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The reviewer provides many constructive comments and questions, to which we respond in detail in the list below. The reviewer's recommendations were carefully accounted for in producing the new version of the manuscript. In general, in the new version (i) we emphasised the importance to simulate the impact of drought on C allocation, (ii) we added new results and reviewed relevant analyses reported in recent references, (iii) we explained the modelling approach in more detail, (iv) we extended the discussion section (e.g. adding comments on possible model applications), and (v) we further increased the clarity of the manuscript and improved its structure (e.g. by adding a conclusion).

Synthesis

(R) Are substantial conclusions reached? Not exactly since the improvement of the model by the use of the new allocation model is not proved.

(A) This is true. However, our objective was not the comparison between a fixed allocation model vs. an improved allocation model, rather (1) to develop and test a model based on source-sink relationships and growth rules for C allocation among aboveground wood, belowground wood and fine roots and (2) to analyse the temporal variations in C allocation among years with different environmental conditions, particularly drought stress. In the new version, the substantial conclusions of the study are clearly reported in a dedicated section (Sect. 5). On the other hand, our study accurately shows that the C allocation pattern in a temperate deciduous forest presents variability between years and in particular between years with various degree of drought stress. For instance, in 1999 (a wet year) the C invested in fine roots and wood was 16% and 67%, respectively, of the total C invested in biomass production, whereas these proportions were 34% and 45% in 2004 (a moderately dry year) and 14% and 52% in 2003 (a severely dry year). Empirical models with fixed allocation pattern (e.g. deriving increment in fine root biomass from increment in wood biomass, Le Goff and Ottorini 2001) will definitely fail to simulate C allocation dynamics at interannual scale.

(R) Are the results sufficient to support the interpretations and conclusions? Yes, but some improvements can be done.

(A) A comparison of the modelled and measured ratio between C partitioned to wood production and GPP was added to test the ability of the model to simulate the C partitioning pattern (P13, L3-10)

(R) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes, but a

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clarification of data used for calibration and validation should be added

(A) We clarified that datasets used for parameterization/calibration were not used for validation (P11, L14-16; P11-12, L30-2)

(R) 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

(A) Following the reviewer's recommendations, we added several topics in the discussion and new references (see specific comments below).

(R) Does the title clearly reflect the contents of the paper? Yes but too long.

(A) We shortened it.

(R) Does the abstract provide a concise and complete summary? Yes, but the functional basis of the carbon allocation model is missing

(A) A mention to functional balance is present in the new version of the abstract.

General comments

(R) 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.

(A) The papers of Litton et al. (2007) and Davi et al. (2009) were carefully analyzed and incorporated in the new version of the manuscript. In particular, the paper of Litton et al. (an extensive review of experimental studies on C allocation in forest) was comprehensively used e.g. to define the terminology employed (Sect. 2.1), corroborate modelling assumptions (P8, L12-14) and openly discuss the model limitations (P20-21, L14-9). This latter section reports and discusses also the impact of competition on allocation, the reason why it was not considered in our study and its possible modelling

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implementation.

(R) 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.

(A) Datasets used for parameterization and calibration were not used for validation. This important point was clarified in both the parameterization (P11, L14-16) and corroboration section (P11-12, L30-2).

(R) 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 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).

(A) As suggested by the reviewer, the C partitioning to wood production (i.e. the ratio between C partitioned to wood production and GPP) was tested comparing measurements and simulations. This test revealed that (i) errors in C partitioning were low (<15%), (ii) the level of variations is indeed underestimated by the model but (iii) errors in simulating C partitioning to wood production are not correlated to errors in simulating GPP (P13, L3-10). This shows that our model is not biased in a systematic way.

(R) 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

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clearly separated. Transposition on other sites should be tested, at least discussed in the discussion.

(A) This weak point was strengthened by (i) emphasising the relevance of modelling the impact of drought on allocation (e.g. P4, L15-26; P17-18, L31-25), (ii) discussing the applicability (or possible adaptation) of the model to simulate other beech sites, stands of different species and at different climatic conditions (P18-19, L31-11), and (iii) clearly stating that datasets used for parameterization were not used for validation (P11, L14-16; P11-12, L30-2).

Specific comments

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

(A) We propose "Modelling C allocation in a deciduous forest and its implication to ecosystem C cycling"

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

(A) We add a concise but clear sentence to the abstract: "CAF is based on source-sink relationships and growth rules, as functional balances and allometric ratios".

(R) 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.

(A) We agree that using simulations of soil water content into a vegetation model which is not coupled to the soil water model can cause inconsistencies. The Granier et al. (1999) model uses climate data, and simulates transpiration (big leaf approach) to calculate the soil water balance and REW values. However, this model was validated in detail for several temperate forests, including the study site. So we can assume that the REW values we are using are correct measures of the observed soil water status

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for the study site and exclude inconsistencies for this study. We consider REW purely as a model input. Nevertheless, in future, it would be very interesting to expand our model with a soil water balance model, in order to prognostically simulate the REW.

(R) M&M: 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.

(A) Crucial phenological events as the onset of the growing season (budburst) and its end (leaf senescence) are modelled in a process oriented way. On the other hand, the duration of the phenological stages is indeed described as fixed number of days. This approximation was necessary because the lack of datasets to properly develop a fully process-based phenology module. Anyway, this simplified approach relies on sound assumptions, based on phenological observations for beech and other deciduous species (P6, L16-31). We realize that a process oriented modelling of leaf phenology (based on environmental conditions and C reserve) will be important in further research on C allocation. We discuss this in the new version of the paper e.g. stressing that along with budburst and leaf senescence, the transition between heterotrophic and autotrophic growth for leaves is likely to be a relevant phenological event for C allocation dynamics and that research is needed on this field (P19, L16-27).

(R) M&M: 3. Why using level of NPPLY (p3788, l10) since a level of reserve will be more suitable.

(A) We believe an approach based on a threshold in net photosynthetic gain is more appropriate than one based on the level of reserve to model the onset of leaf senescence. This was supported by experimental evidences ("decreased sugar concentrations are unlikely to be a primary factor in triggering the leaf senescence", Yoshida 2003). Other models adopt similar conceptual approaches (Medlyn et al. 1999, Arora and Boer 2005).

(R) M&M: 4. The model of functional balance needs to be described (p3787, l25).

(A) The model was described in detail (P7, L18-29)

(R) M&M: 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.

(A) The sentence was awkward and it was reformulated. The C reserve were partitioned among fine roots, coarse roots, stems, and branches using proportions assumed equal to the proportion of standing C reserve reported for beech by Barbaroux et al.

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

(A) Fine roots have higher rank than wood because of their key role in uptaking limiting resources as water and nutrients (Bossel 1996). This assumption is indirectly confirmed by the observed increased allocation to wood in case the sink strength of the fine roots reduces e.g. under fertilization or irrigation (Litton et al. 2007).

(R) M&M: 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.

(A) We emphasized the importance of modelling the impact of drought on tree growth and C allocation throughout the manuscript (abstract P2, L10, introduction P4, L15-26; material and method P8-9, L33-20, results Sect. 3.2.2) and particularly in the discussion (P17-18, L31-25; P21-22, L31-15).

(R) M&M: 8. Why calibrating leaf fall parameters on NPP dynamics since leaf fall measurements should exist in this fluxnet site?

(A) The leaf senescence is a key phenological event (end of the growing season) and its simulation with a process oriented approach is of relevance. The hypothesis that leaf senescence initiates when the photosynthetic gain decreases below a threshold agrees with basic leaf physiological principle (Gan and Amasino 1997). Calibration was

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done to have an estimation of such threshold. Other models adopt similar conceptual approaches (Medlyn et al. 1999, Arora and Boer 2005).

(R) 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.

(A) We compared our simulated date of senescence onset with simulations presented for the same study sites and years (1999, 2000, 2001) by Wang et al. (2004). The two models perform similarly (approximated maximal difference in senescence onset of about 10 days). Furthermore, a more stringent test was performed by regressing modelled LAI values vs. measurements of autumn LAI (years 1999, 2000, 2001; n=7; Wang et al. 2004). The test revealed our model behaved very well (slope=1.01; R2=0.88). We did not add such information in the manuscript because it focuses on C allocation dynamics between non-leafy compartments. Further publications will focus on that. Datasets used for parameterization and calibration were not used for validation P11, L14-16; P11-12, L30-2).

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

(A) This section was re-written, reporting in detail which seasonal variations were mere consequences of the modelling assumptions.

(R) Results: 3. The discussion of interannual C pool variations is a judicious analysis.

(A) Also this part was re-written. Subjective analyses were avoided.

(R) 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.

(A) As reported above, we followed the reviewer recommendation and the partitioning to wood production (i.e. the ratio between C partitioned to wood production and GPP)

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was tested comparing measurements and simulations. This test revealed that errors in simulating C partitioning to wood production are not correlated to errors in simulating GPP and that our model is not biased in a systematic way (P13, L3-10).

(R) Discussion: 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)

(A) The sentence to which the reviewer refers to was skipped. A more general note was added instead ("the substantial post-drought C investment in fine root production simulated in 2004 might be of relevance to elucidate the experimentally observed time lag effect of the severe drought of 2003 on the 2004 wood growth").

(R) Discussion: 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.

(A) We considered this topic in more detail (P16, L10-18).

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

(A) This awkward sentence was re-formulated (P16, L22-23).

(R) Discussion: 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 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) This hint was added in the discussion (P16-17, L33-4).

(R) A conclusion will be welcome.

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(A) Present in the new version of the manuscript.

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