

General comments

The manuscript submitted to Biogeosciences by Cornut et al. studies the impact of a potassium limitation on wood productivity and in particular on the allocation of carbon and potassium towards trunk, branches and bark, through the modelling and evaluation of the CASTENEA-MAESPA-K model. This is the Part2 of a two-part paper, Part1 being dedicated to the impact of K limitation on the C-source activity (GPP). The split in the 2 parts is relatively well done, and the present manuscript (Part 2) is sufficiently self-supporting, without the need of reading the Part 1 first.

The overall objectives of the manuscript are relevant; and the “Results” and “Discussion” sections report fairly on the model capacities at simulating biomass of each compartment (organ) for different K availability scenarios.

However, the manuscript needs a strong revision of the Methods section and some restructuration. Currently, this section contains too many inconsistencies in several equations, variable units, ... There is not a particular major deficiency but an accumulation of inaccuracies, which really prevents to access at the content of the manuscript. I address many of them in the Technical comments below.

We thank Reviewer one for his/her thorough and relevant comments. They were very useful when revising our manuscript. This has fed into the restructuration/clarification of the methods section in the revised manuscript. We have explicitly answered to all reviewer concerns. The minor points were directly addressed in the revised manuscript. We take notice that our two-part approach is acknowledged and accepted here.

Technical comments

I would suggest using the present tense in the Methods section when describing the model features, instead of the past tense.

We have found no specific guidelines regarding the tense of model description in BG. The co-authors agree that the present tense could be interesting but, if the Editor agrees, we would prefer to keep the past tense since developments presented here correspond to a specific version of the model.

Line 100-108: As far as I understood, the allocation coefficients presented in subsections 2.3.2 and after are applied over “NPP - C allocated in leaves” not NPP. If this is correct, this should be clarified here. To my understanding, the sentence “The growth of all organs was a fraction of the daily NPP” does not reflect the way it is modelled. If “all organs” include leaves, it is in contradiction with the sentence “the generation of leaves ... was not directly dependant on NPP”. If “all organs” means “all organs except leaves”, their growth is a fraction of NPPorg, not NPP. To my opinion, NPPorg should be defined here and not line 200 of the current manuscript.

Thank you for pointing this presentation issue. Indeed, as identified by Reviewer1, the allocation coefficients apply to “NPP minus C allocated to leaves”. In other words, the production of leaves had priority over the production of all other organs. The production of leaves can be limited by NPP in our model but this was very rarely the case in our simulations. In the revised manuscript at line 106 the “section 2.3.1 Leaves” was removed and the following paragraph was added for clarification: “In the model, leaves had priority over other organs as regards the allocation of carbon. This means that carbon was firstly allocated to leaves and what was left could be allocated to the other organs. Leaf production in the model was driven by the increase in tree height (see

Cornut et al., 2023 and eq.8 below). The parameters used here were fitted using experimental data from the +K stand. Leaf growth could however be limited if demand for growth was higher than the available C in the SSs compartment.

The growth of all other organs was a fraction of the daily NPP minus leaf biomass production (Fig.1). The allocation coefficients of each organ except leaves (i.e. fine roots, coarse roots, woody organs) were calculated at a daily time-step and were the result of NPP and allometric relationships among organs.”

Line 151-152: Could you give slightly more information on how the growth respiration is computed?

Growth respiration was computed by using parameters measured in eucalypt plantations (Ryan et al., 2009) and the growth respiration was modelled as in the original CASTANEA model (Dufrêne et al., 2005). In the revised version of the manuscript two paragraphs and two equations were added that explain the modelling of growth respiration at lines 155-165:

Organ growth was calculated at a daily time-step. In the base CASTANEA model, growth respiration was calculated using the daily growth of the organ and the associated construction cost (Dufrêne et al., 2005). In this study, the construction cost of trunk wood was based on age of the plantation following Ryan et al. (2009):

$$GRC_{trunk} = \max(0.076, 0.368 - 0.0343 \times age) \quad (9)$$

where GRC_{trunk} ($\text{gC}_{CO_2} \cdot \text{gC}^{-1}$) was the growth respiration cost associated to 1 gC of trunk wood increment and age (days) was the age of the plantation. Due to lack of available data, the growth respiration for other organs used values identical to those used in Dufrêne et al. (2005).

The daily growth respiration associated to each respective organ was the following:

$$GR_{organ} = GRC_{organ} \times \Delta B_{organ} \quad (10)$$

where GR_{organ} ($\text{gC}_{CO_2} \cdot \text{m}^{-2} \cdot \text{day}^{-1}$) was the growth respiration of the organ, GRC_{organ} ($\text{gC}_{CO_2} \cdot \text{gC}^{-1}$) was the growth respiration cost of the organ and ΔB_{organ} ($\text{gC} \cdot \text{m}^{-2} \cdot \text{day}^{-1}$) was the daily growth in biomass of the organ.

Line 153 and after: Description of the maintenance respiration modelling is quite difficult to follow. This section needs clarifications (see below).

Indeed, the description of the simulation of maintenance respiration lacked the necessary explanations. We have answered the questions below and amended the manuscript to clarify this aspect of the model.

Line 155: maintenance respiration is a “function of their respective respiration rate per nitrogen unit, nitrogen content and surface temperature”. Equation (9), in which we divide by N_{trunk} , defines the respiration rate per nitrogen unit (MRN_{trunk}). Equation (10) defines the maintenance respiration from MRN_{trunk} by multiplying it by N_{trunk} . So, RM_{trunk} (or more generally RM_{organ}) does not seem to be a function of the nitrogen content.

This is true (for the trunk only). By including N_{trunk} we made an error since this was a fixed value (0.001) that was used to convert the respiration measures of Ryan et al. 2009 to CASTANEA maintenance respiration per unit nitrogen formalism. In the revised manuscript the term (1/0.001) was included to the parameters in the max term of the equation:

$$MRN_{trunk} = \max(0.47, 7.3 - 1.16 \times 10^{-3} \times B_{trunk}) \times 10^{-6} \times 3600 \times \frac{1}{12}$$

Line 159, equation (9): There is a problem with the 2 terms of the max functions (in brackets). Both terms are constants without any variable.

Yes, there was an error in the transcription of this equation. Thank you for seeing this. The equation now reads in equation 11 of the revised manuscript:

$$MRN_{trunk} = \max(0.47, 7.3 - 1.16 \times 10^{-3} \times B_{trunk}) \times 10^{-6} \times 3600 \times \frac{1}{12} \quad (11)$$

where MRN_{trunk} ($\text{mol}_{CO_2} \cdot \text{gN}^{-1} \cdot \text{hr}^{-1}$) was the respiration rate of the trunk per nitrogen unit at a reference temperature of 25°C, B_{trunk} the trunk biomass in $\text{gC} \cdot \text{m}^{-2}$, 3600 the number of seconds per hour, 12 the molar mass of carbon. The respiration rate per nitrogen unit of roots (MRN_R) and branches (MRN_{Br}) was assumed to be equal to twice MRN_{trunk} .

Line 168: The terms “0.256”, “-0.00854” and “0.0759” don’t match with the values reported on Figure S2b while those of the equation for $N_{branches}$ do match with values of on Figure S2a.

These values were changed since there was an error in the value of parameters used. The values were replaced in equation 12 of the revised manuscript:

$$\begin{aligned} N_{trunk} &= \min(0.004, 0.00496 \times e^{-0.00413 \cdot B_{trunk}} + 0.00081) \\ N_{branches} &= \min(0.005, 0.00256 \times e^{-0.00966 \cdot B_{branches}} + 0.00260) \end{aligned} \quad (12)$$

Line 183: “The realised lifespan of leaves was influenced by their K status (see Part 1)”. I could not find in Part 1 any information of the equation that relates $LLS_{realised}$ to K status.

This is presented in the companion paper Part 1 (Cornut et al., 2022), as referred to in the revised version of the manuscript (L. 195). Since leaves fall when their K concentration is below a threshold value, their “true” lifespan can be strongly reduced by K availability.

Line 214 and after, section 2.6.1 Wood: it is the only section where you deal with the cohort level. Is it really needed ? If so, you should better explain the cohorts principle. For instance, line 217-218: “trunk NPP was allocated daily to a cohort of wood”. How is this cohort selected among all ?

Wood cohorts correspond to daily productions of wood. Trunk NPP is allocated to the cohort created on that day. This was necessary for our potassium remobilisation model from the wood. The necessity of daily cohorts of wood was a reflection of the use of daily cohorts of leaves (which were the main K sink, Cornut et al. 2023). We modified line 231-232 in the revised manuscript to: “The trunk NPP was allocated daily to a newly formed cohort of wood.”

Line 218-219: “optimal K concentration of newly formed wood was constant and to the maximum trunk concentration measured”. In this respect, why equation (15) includes Limorg^K which tends to reduce K_{trunk}^{opti}

This is because “optimal” (when K is present in sufficient quantities) and “realized” (after computation of offer vs demand of K) concentrations are different. We added this sentence at lines 233-235 of the revised manuscript: “The realised concentration of newly formed wood was a function of the optimal K concentration of newly formed wood and the strength of K supply limitation.”

Line 295, equation (20): as it is defined, KUENPP seems to be a function of the length of the rotation. Is it really expected? It does not appear to be a very handy metric to compare experiments with different rotation length. You could use the mean daily C flux over the length of the rotation instead of the cumulated one. This would imply to express KUENPP in $\text{gC day}^{-1} (\text{gK})^{-1}$

Yes. It is true that this means that KUE is a function of rotation length. However $K^{\text{max_plant}}$ (the maximum amount of K immobilised in the trees) is also a function of the length of the rotation. Dividing by the length of the rotation does not change conclusion for comparisons between K supplies since simulated rotations were all of the same length. We also believe that rotation length is an important factor to consider when measuring consumption of K per unit C produced and the rotation length can vary depending on the type of forest management.

Line 393: “Potassium concentrations in trunk wood and branches are correctly simulated”. Could you provide a quantitative metric for this “correct simulation performance” ? In addition, you should probably highlight that for branches at least, there is a large spread in the measurements in particular for low biomass values.

It is true that this sentence was overly optimistic. We have deleted that paragraph in the revised version and replaced it with the more realistic paragraph below. We have no hypotheses regarding the large spread of values at low biomasses other than an effect due to the large proportion of very young branches.

References:

- Cornut, I., Delpierre, N., Laclau, J. P., Guillemot, J., Nouvellon, Y., Campoe, O., ... & le Maire, G. (2023). Potassium-limitation of forest productivity, part 1: A mechanistic model simulating the effects of potassium availability on canopy carbon and water fluxes in tropical eucalypt stands. *EGUsphere*, 1-37.
- Dufrêne, E., Davi, H., François, C., Le Maire, G., Le Dantec, V., & Granier, A. (2005). Modelling carbon and water cycles in a beech forest: Part I: Model description and uncertainty analysis on modelled NEE. *Ecological Modelling*, 185(2-4), 407-436.
- Ryan, M. G., Cavaleri, M. A., Almeida, A. C., Penchel, R., Senock, R. S., & Luiz Stape, J. (2009). Wood CO₂ efflux and foliar respiration for Eucalyptus in Hawaii and Brazil. *Tree Physiology*, 29(10), 1213-1222.

Here I review the 2nd part of the work submitted by Cornut et al., focusing on simulating growth limitation induced by K deficiency. Overall, I find this manuscript well-written. I have one comment regarding the model structure. Here, the authors indicated that the model structure accounts for additional processes relating to K allocation, remobilization and turnover. I do not understand why the model structure is different in this paper as compared to the model structure of the first paper. I know two papers have different focus, but it would be valuable to justify the reason as to why the authors decided to omit processes described here in the first paper. Particularly, does it mean that the results of the two papers are not directly comparable even if they were used to simulate processes for the same site? Apart from this comment, I think the manuscript is generally well presented.

We thank reviewer two for her/his positive remarks and the constructive criticisms that they bring to the manuscript. The model structure that we used is the same in the two papers. In the first paper, we chose to focus the description on the canopy and the processes related to leaves and C acquisition. In the second paper, we focus instead on the allocation of K and C in trees. The schematic (Fig. 1) attempted to show this but it is possible that our explanations lacked clarity. The results of the two papers have thus been produced by the same model. This was an attempt to avoid redundancy between the two papers. The new general model schematic of the model in the revised version of the companion paper Part 1 (Cornut et al., 2023) -makes the model structure more explicit.

*We tried to clarify that the two papers have comparable results by modifying the model presentation paragraph: “Here we focused on NPP (for GPP see Cornut et al., 2023), which led to concentrate efforts on the C and K allocation models (Fig.1). All the sub-models (leaf cohorts, external K cycling, uptake) described in Part 1 (Cornut et al., 2023) were re-used albeit sometimes with different parameter values (Tab.S1-2) since the simulated experimental sites were different. **While implicitly used in the modelling work of the companion paper (to be able to simulate K fluxes and maintain general model functioning)**, the new modules simulating the carbon allocation, the availability of K for organ growth and the remobilisation of K from organs developed for this work are presented below.”*

Specific comments:

L64: no hypothesis on sink limitation was previously introduced yet. It would be great to make it explicit. And, it would be useful to describe what you meant by parsimony principle.

We are not sure to understand the first comment. Sink limitation was introduced on line 49. By “sink limitation” we refer to processes that could limit wood production by affecting sugar transport from carbon source organs (i.e. leaves) to sink organs (or from sap to sink organs) or by directly limiting the functioning of sink organ through K deficiency (e.g. by affecting cell expansion, metabolism or lifespan).

As regards the “parsimony principle” we introduced the following sentence at line 67 of the revised manuscript: “In other words, we mean that we did not introduce more processes in the model, if those that were already included could explain the observed patterns.”

L 91 – 93: Why these modules on carbon allocation and K effect on organ growth not important in the first manuscript? In particular, why the model structure of the two papers different? Does it mean the results of the two papers are not directly comparable, even if simulated for the same site?

Results presented in both manuscripts have been produced by the very same model structure. In other words, only one set of simulations (same model structure and equations, same parameters, same forcing variables) was used to produce results presented in both manuscripts. The Part 1 (Cornut et al., 2022) focused mainly on the canopy, carbon assimilation and the calibration of these processes. Part 2 focuses mainly on the allocation of this carbon and the validation of the model using measures of biomass production. We therefore just change the focus on different processes in Part 1 and in Part 2. This was addressed at line 96 of the revised manuscript: “While used implicitly in the modelling work of the companion paper (to be able to simulate K fluxes), the new modules simulating the carbon allocation, the availability of K for organ growth and the remobilisation of K from organs developed for this work are presented below.”

Figure 4b: Why litter K content starts so high and declines over time?

This was due to the presence of forest floor and harvest residues on the soil following the clear-cutting of the previous stand (corresponding both to litter that was present on the ground at cutting and branches, leaves and bark of the cut trees that were added to the soil as litter). The following sentence was added in the figure 4 caption of the revised manuscript: “The large amount of K present in the litter compartment (solid grey line) at the beginning of the rotation was measured in the plots at planting (forest floor + aboveground harvest residues).”

Equation 8: Btrunk should be biomass, not in unit of g C m⁻², right?

All biomasses in the model are in grams of carbon per square meter of soil (gC.m⁻²)

Equation 12: Knpp in the unit of gK m⁻²? Shouldn't it be in the unit of gK m⁻² d⁻¹? But if this is the case, what's the correct unit for Kavailable in equation 14?

Thank you for your comment. Indeed, as pointed by Reviewer 2, the correct unit for K_NPP in equation 14 should be gK.m⁻².d⁻¹ The correct unit for K_available is also gK.m⁻².d⁻¹. This was corrected at lines 214 and 219 of the revised manuscript. Time (previously implicit) was introduced in the K available equation to maintain unit homogeneity. The equation for K_available now reads as follows:

$$K_{available} = \frac{K_{phloem} - K_{phloem}^{min}}{\Delta t} \quad (15)$$

where $K_{available}$ ($\text{gK.m}^{-2}.\text{day}^{-1}$) was the amount of K available for organ growth, K_{phloem} (gK.m^{-2}) the amount of K in the phloem sap, K_{phloem}^{min} (gK.m^{-2}) the minimal amount of K in the phloem sap (Cornut et al., 2023) and Δt (days) the timestep (1 day in our simulations).

References :

Cornut, I., Delpierre, N., Laclau, J. P., Guillemot, J., Nouvellon, Y., Campoe, O., ... & le Maire, G. (2022). Potassium-limitation of forest productivity, part 1: A mechanistic model simulating the effects of potassium availability on canopy carbon and water fluxes in tropical eucalypt stands. *EGUsphere*, 1-37.