EGUsphere, author comment AC1
https://doi.org/10.5194/egusphere-2022-883-AC1, 2022

Reply on RC1

Ivan Cornut et al.

Author comment on "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" by Ivan Cornut et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-883-AC1, 2022

Cornut et al present a description paper of a process-oriented model of an Eucalypt plantation with the major novelty of accounting for potassium cycling in an explicit way. The model is calibrated and evaluated based on data from a fertiliser trial in Brazil, and model predictions for potassium fluxes are described.

This is a timely and important endeavour and presents a challenging exercise. While the work is important and could provide an important step forward, there is a lack of attention paid to the description of the model calibration, separation of model evaluation from pure predictions, and the writing. Besides, there are some questions about the appropriateness of model assumptions.

We thank the reviewers one for their thorough review of our article, the detailed comments were useful or the clarification of key points in the manuscript. In the following comments we will address the main comments that refer to hypotheses, theory or interpretation of results.

Major points:

Appropriate of model assumption:

It is surprising that potassium leaching is observed to be negligible (L296) while potassium is assumed to be a highly water transported element in your model. How can there be no leaching of the K+ experiment if potassium is added such that plants are non K limited?

Doesn't the modelled accumulation of soil K during the experiment (Fig 4) suggest the assumption is invalid? Are there site observations available which could indicate such an accumulation is realistic?

The absence of deep soil leaching of K at our site is indeed counter-intuitive since it is a highly mobile nutrient and that large amount of K are applied. However, the very deep soils combined with the capacity of the soil to retain K+ ions (table 22 in Maquere, 2008), *and the storage in tree trunk and bark led to an absence of measurable leaching fluxes below a depth of 3m (which is understood in the model as soil accessible for plant uptake of K). This is the case both at the Itatinga site (Maquere, 2008) and at the Eucflux site (Caldeira Filho et al., 2022). Even after the clear cutting of the plantation, no K leaching fluxes below 3m were measured (Caldeira Filho et al., 2022).*

Table 22 Estimation of the capacity of the soil (in kg ha^{-1}) to retain cation or anion by non specific adsorption (CEC and AEC) or specific adsorption. For each element, the capacity is calculated for each soil layer from the bulk density of Table 11 and the CEC or AEC of Table 20 fully saturated with the studied element. The charge attributed to each element is +1 for Na, K and N-NH_4, +2 for Ca and Mg, +3 for Al, -1 for N-NO_3, Cl and P-H_2PO_4, and -2 for S-SO_4. Specific adsorptions are calculated from Table 21. n.d.=not determined.

	CEC						AEC				Specific adsorption	
	Na	K	Ca	Mg	Al	N-NH_4	N-NO_3	Cl	S-SO_4	P-PO_4	S-SO_4	P-PO_4
							kg ha^{-1}					
0-5 cm	511	869	446	270	200	311	9	23	10	20	0	232
5-15 cm	378	642	329	200	148	230	15	38	17	33	n.d.	n.d.
15-30 cm	443	753	386	234	173	270	27	69	31	61	n.d.	n.d.
30-50 cm	628	1067	547	332	246	382	53	134	61	117	n.d.	n.d.
50-100 cm	1394	2371	1215	737	545	849	132	334	151	292	n.d.	n.d.
100-200 cm	2270	3861	1979	1200	888	1382	578	1464	662	1279	n.d.	n.d.
200-300 cm	1852	3150	1614	979	725	1128	1031	2611	118	228	2996	6405

The accumulation of soil K during the experiment is consistent with the very high levels of fertilization at both the Itatinga and Eucflux sites. These fertilization levels are above the necessary levels for optimal plant growth since they were chosen to make sure that K is non-limiting. This accumulation is also consistent with the CEC measured at the Itatinga site (Maquere, 2008).

I could imagine that potassium might be efficiently adsorbed to organic matter preventing leaching losses? But if this is the case, why is it omitted in the model? If so, you should explain why this was omitted, and what the implications for the result are.

This is the case at our site since a large part of the cationic exchange capacity was due to the organic matter in the soils (Maquere, 2008). This was omitted in the model since no deep leaching fluxes of K were measured even at very high (higher than practiced in commercial plantations) levels of fertilization (Caldeira Filho et al., 2022), and therefore this mechanism could not be calibrated. Furthermore, the model did not consider this level of details with K echanges between the soil and the soil solution. In the future, an improvement in model genericity would require a model of K flux and exchange in the soil since other sites could present deep leaching loss of K. This has very few implications for our sites but could lead to unrealistic simulated accumulation of K in the soil at sites with shallower soils or soils with less cationic exchange capacity.

Model description: Not all fluxes are described with equations (e**.g.** Kleaf☐litter Is missing) and not all changes in K pools are described **(**e.g. Ksoil or K in roots). Make sure all fluxes and pools are described. The overview figure is very hard to follow (see minor points below). The coupling of the water cycles is not described (see minor points).

We thank reviewer one for pointing out these inconsistencies and adresse these points below in our answers to the comments. Kleaf to litter had no specific equation in the manuscript since it is the result of leaf senescence. K pools are not described here *but are described in the companion paper (Cornut et al., 2022)*

Description of model calibration: There is hardly any information on how the calibration of parameters was achieved. e.g. what method was used, what data was used for a given parameter. Where does the data origin from, etc. It is not clear if Fig2 shows the results of model calibration or an evaluation (as suggested on Line 555).

Most of the processes were parameterized based on dedicated experiments, as described throughout the text. When calibration was necessary, it was done at the process level and not at stand level, as is generally done with process-based models. For example, leaf expansion parameter models were fitted on leaf expension data measured on this site. However, we agree that some descriptions were lacking and we will change the text to detail explicitely how the parameters were obtained, for each model process. When calibration of parameters was necessary, it was achieved using a linear exploration of the parameter space and evaluating model fit using RMSE. Figure 2 is used mainly to show the theoretical functioning of the leaf expansion model without the rest of the model. This figure shows a calibration of this sub-model independently of the rest of the model and the sentence on line 555 will be modified accordingly ("The leaf sub-model took into account both the influence of K on both the dynamics and maximum value of the individual leaf area (Fig.2d).").

Lack of model evaluation. The results are mostly describing model results with little confrontation with observation, etc. There are comparisons of model predictions and observations but they fail to identify and highlight predictions which are apparent results of the model and which are calibrated. The discussion would benefit from the restructuring into distinct parts for evaluation and for prediction. Besides, all datasets and their purpose (evaluation, calibration) should be described in the method section, e.g. only in the discussion the Christina et al 2015 model data is explained.

In this manuscript, which consists of part 1 of a two-paper article series, we focused on paramterising the model using data from both study sites. This was done since the data is incomplete at each site. Carbon and water flux data were only acquired at the Eucflux site while the response of an eucalypt plantation to K omission was only measured at the Itatinga site. Calibrations were done at the scale of processes and not the whole stand. For example, leaf production in the fully fertilised condtion was calibrated by using LAI, biomass and litterfall data. The calibration of model processes was only done in the +K condition since the responses of different processes to K deficiency were derived from measured parameters (except for the leaf expansion process which was calibrated in both +K and oK conditions). This meant that oK simulations were meant to act as tests for the model as a whole by seeing of the model was able to replicate the response of the canopy or fluxes to K deficiency.

Thank you for these suggestions, we will describe parameter sources and calibration in a more detailed manner in the manuscript.

minor

Section 2.3

This section is mostly focused on the motivation of revising the water cycle in CASTANEA than in describing what has been actually done, i.e. the new model structure of CASTANEA-MAESPA. It is not clear how the coupling has been achieved. I would suggest explicitly stating the modifications done to the underlying equation of CASTANEA given the scope of the paper as a model description and reference paper.

Thank you for this suggestion, we will add details as to how this coupling was made. The coupling was made by integrating MAESPA sub-routines in the CASTANEA code. The sub-routines were those related to soil water and photosynthesis. Radiation and water interception were simulated by CASTANEA and the integrated sub routines from MAESPA simulated photosynthesis, transpiration and leaf water potential for each canopy layer. Soil water fluxes and water potential were calculated using sub-routines from MAESPA.

You should indicate units of all variables. Use a consistent format for units, e.g. there is am ic of /year and year-1

Thank you for your attentive review, we will check and correct all the variables units.

Figure 1: An overview figure is an excellent idea but the current figure is hard to follow.

- What does the broken line stand for? What do the different colours stand for?
- Caption indicates all K fluxes are based on Ohm's law which isn't the case. Rephrase.
- The figure is a mix of process, fluxes, relationships, pools. E.g .you could produce separate figures/panel: one for pool & fluxes, and one for the process linkage

Thank you for the useful suggestions, that will be addressed in the final revised version of the manuscript.

Line 5: large-scale - specify what 'large' means here

We will modify the text to "at the stand scale" instead.

Line 10: internal/external is not clear unless you define the boundary of your system. I would suggest to rephrase

We will rephrase this as abiotic/biotic sources since this is a more relevant way of separating these fluxes.

Line 68: It is not clear why it is a prerequisite one could also theoretically start modelling with the sinks than with the source.

This is indeed possible, but modelling C-sources is well documented thanks to the good theoretical framework surrounding photosynthesis (Farquhar model) and stomatal *response (Ball and Berry model, Tuzet model, etc.). Driving the C-source activity and stomatal functioning by C-sinks has been attempted with some success (Hölttä et al., 2018) but is more computationally complex and had never been calibrated on eucalypts. We will add these arguments to the corrected version of the manuscript*

Line 108: specify to what extent this clone is comparable to the other one?

Most of the clones planted in this regions are very similar, because they were all selected locally for the climate. For instance, the wood production is similar, leaf area index evolves in the same ranges of values, photosynthetical parameters are similar (unpublished data). However, they also differ for some other aspects such as branches and litter turnover, stomatal conductance, etc. The parameter set of both genotypes give an idea of their main differences. However, differences between clones can be hard to investigate at our sites since they were not planted at the same time and thus did not experience the exact same climatic/edaphic conditions at the same developmental stage. We will calrify this in the manuscript.

Line 126: is this a novel technique ? Give references or additional information on how you derived the damaged leaf area.

The technique was developed in the frame of the present study. It is based on simple color threshold on leaf scans. Indeed, symptoms areas are clearly different in colours in the visible range and observable by photointerpretation. Color thresholds were therefore adjusted manually. We will add some more description: "… based on a colour threshold calibrated by photointerpretation and automatized in a Matlab ® script". If it is necessary the scripts can be deposited on a dataverse repository.

L167: does this mean you have (365 days *6 years ->) 21190 leaf cohorts at the end of a 6 year rotation ? Is this really needed?

This would not be needed to simulate leaf dynamics but is useful for the simulation of K fluxes between leaves and the other tree components. Once all leaves of the cohort have fallen the cohort is no longer simulated. So there a no more than 400 cohorts (since leaves have a 400 day theoretical liffespan) at the same time. While this seems a lot, this is in fact easier to simulate (at the expense of some memory space – but nothing critical) than grouping leaf emergence and growth every x days experiencing various weather and soil conditions. Simulating daily cohorts also brings stability to the model since all processes are simulated at a daily scale (carbon and K fluxes). The added computation brought by these cohorts is also negligeable compared to the half-hourly calculation of photosynthesis and transpiration for each canopy layer (since it results from a computationnally intensive minimum search).

L185: m2 of ground ? leaf?

Of ground, thank you.

L 187: what is P_leaf ? ; units of k are missing

This is a writing error, P_leaf should be written as N (as is the case in the rest of the manuscript).

L188: indicate how the calibration was performed (which obs variable did you target, time step, method of calibration, etc)

The calibration was a linear exploration of parameter space using multiple RMSE as a goodness-of-fit indicator. The data used for calibration were destructive leaf biomasses, *leaf area, leaf biomass and leaf fall measurements. We used mainly cumulative leaf production and leaffall as the points to fit since simulating fine weekly variation in leaf production or leaf fall were not the objectives here. The time-step between these measurements were dissimilar (yearly, monthly).*

L190cc: equation/description for leaf fall is missing

There is no specific equation for leaf fall. Leaf fall is the result of a decrease in the leaves' K content by the leaf K content and expansion sub-model. Otherwise leaf fall occurs in function of leaf lifespan. We will clarify this in the manuscript.

Section 2.4.4. : explain and show how this equation was fitted.

This equation was fitted using leaf expansion measurements on trees in both fully fertilised and K omission stands (see Battie-Laclau et al., 2013). These mesures were conducted on 70 tagged leaves from creation to full-expansion.

L245 : indicate how is alpha computed. Is it a fixed input parameter?

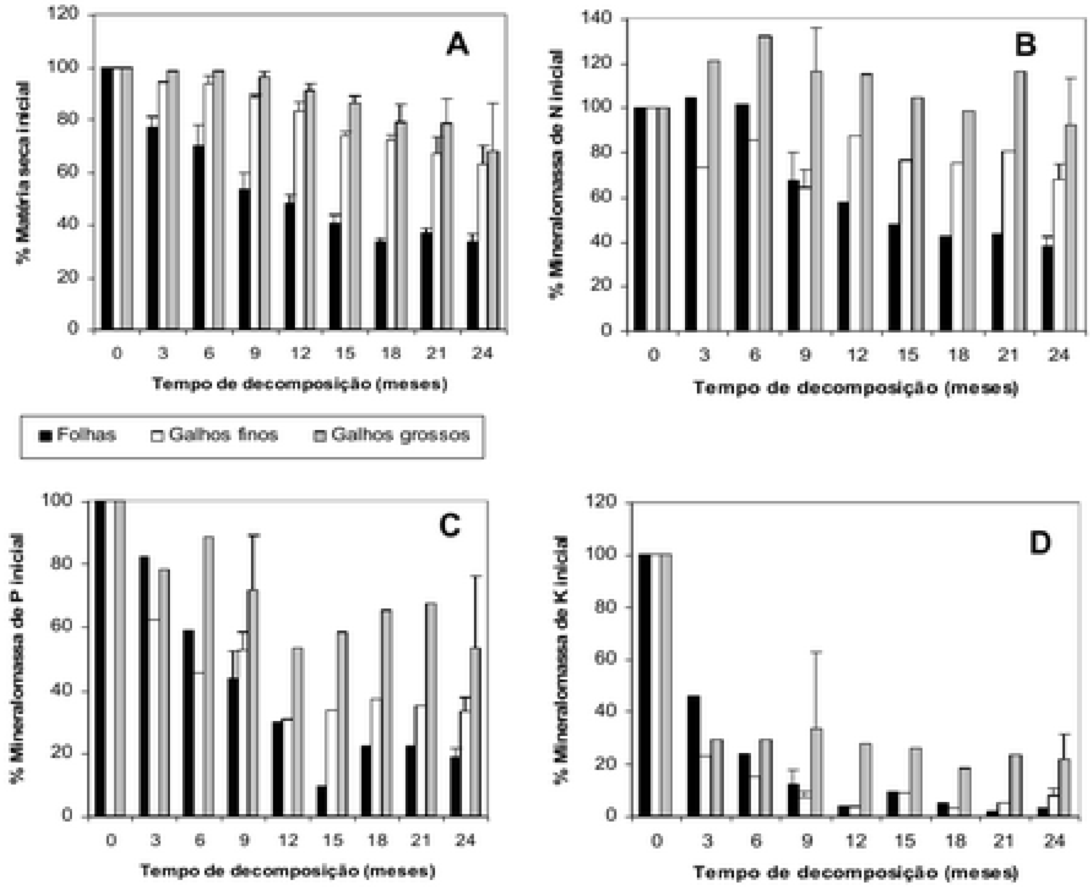
Yes alpha is fixed input parameter and was calibrated using fine scaled leaf K concentration measurements (Laclau et al., 2009).

L253: which cycle? You mean ecosystem?

Ecosystem cycle is better, thank you for the suggestion.

L264: is there no biological mediated K release from litter?

We found no evidence of biologically mediated K release from litter in the literature and the difference between the dynamics of K and N or P (which are known to be biologically mediated) in the litter suggest that this is not the case at our sites. Furthermore, K losses are similar in leaves (Folha) and branches (Galhos) contrary to N or P (Maquere, 2008) which suggests that litter leaching is the most parsimonious explanation for the dynamics of K in the litter. This will be clarified in the manuscript.



What about the unavailable soil K. indicate how this was represented.

Unavailable soil K was represented as a pool that progressively was added to the K accessible soil K using equations of horizontal root expansion (equation on line 310). Inputs were shared between availbale soil K and unavailable soil K depending on their respective relative surfaces.

What about root and wood litter production? Was this omitted?

Wood litter production was omitted since the model system are young eucalypt plantations with no mortality and wood exports at harvest Branches mortality and bark litter were however simulated. Root litter is simulated, but not described in this Part 1 but is described in the companion paper (Cornut et al. 2022) and was modelled in the simulations shown here. For root litter and branch litter we used measured turnover rates (using cameras for fine roots and biomass and litterfall data for branches).

Is K immobilisation by soil organisms really negligible? The initial loss from litter might be due to leaching, but the question is rather how much of all the K in litter is lost via leaching. Can you elaborate on this.

When looking at the K dynamics in litter (see the figure above from Maquere, 2008) it is clear that biologically driven decomposition processes are only responsible for a small fraction of the K losses. This is visible when comparing K losses to N and P losses (since N and P follow the same dynamic as dry matter). We didn't findy any actionnable information regarding immobilisation of K by soil organisms. This could be the result of the negligeable effect of soil micro-organisms on the cycle of K in the soil or a *measurement/publication bias.*

L326: why not call it maximum K conc instead of optimal K? Can you rule out that the optimal conc < max conc?

The difference is that there could be luxury consumption or storage of K in the leaves, therefore maximal K concentration is not necessary the optimal one. There is a difference (Walker et al., 1996) between K stored in vacuoles (very variable) and cytosol (les variable) but we cannot conclude that the variability of K in the vacuole is evidence of luxury consumption.

L354: which ‘part 2’?

This manuscript has a companion paper we called "Part 2", the full reference is: Cornut, I., le Maire, G., Laclau, J. P., Guillemot, J., Nouvellon, Y., & Delpierre, N. (2022). Potassium-limitation of forest productivity, part 2: CASTANEA-MAESPA-K shows a reduction in photosynthesis rather than a stoichiometric limitation of tissue formation. EGUsphere, 1-27.

L429: what is the significance of the speed of senescences for the equation?

The speed of resorption, is a measure of how fast the K in the leaf can be remobilized to the phloem at leaf senescence. This process is very fast and it is possible that K is necessary for the remobilisation of sugars from the senescing leaves.

L452: explain how K affects the wood production in this paper.

This question is fully answered in the companion paper "Part 2", dedicated to wood growth. Briefly, there is no direct impact of K on wood growth (no sink limitation is represented in this model).

L453: impact on what?

Impact on the generation of new leaves, we will change this in the text.

L472: you mean ‘was replaced with’?

We meant that leaf expansion was recalculated using an updated value for the expansion. We will clarify this in the manuscript.

L535: indicare over which period. Does this refer to Table 2?

It is an annual period. We will clarify this in the manuscript. We think that rephrasing this to "Ecosystem K fluxes and stocks over one rotation" would be more relevant since this does not only refer to table 2.

L549: important for what ? you mean higher?

Yes, "higher". We will correct this in the manuscript.

L580-600: does the good agreement with Christina et al 2015 mean we don’t need a potassium model to capture GPP and transpiration? The motivation for comparing your results with the ones of Christina et al 2015 should be given in the methods. Also a description of the data from Christina et al 2015.

We compared our results to the results of the model in Christina et al. 2015 since the potassium effect that they simulate is not the result of a mechanistic modelling approach (which we use in CASTANEA-MAESPA-K) but two distinct parametrisation sets (one set for +K and another parameter set for oK). Our model only changes for the K fertilization amount parameter, all the processes included in the model now simulate the difference between the treatments. The advantage of our model is increased genericity, the feedback between K availability and growth, the capacity to simulate a fertilisation gradient (in the companion paper) and a decrease in computation time.

L598: why was it done for both? The K+ treatment effectively shuts off most of the model developments and is thus not really informative. It makes sense to report for traceability of impact of model developments, but might be better off in the SI as this is mostly relevant for MEASPE developers.

Indeed, we found it useful to show that the model was able to accurately replicate fluxes and behaviour of the eucalypt plantation with classical optimal fertilization. This seems logical, but many processes were added and need to be tested. However, this is clearly not enough, and it is the changes in the ecosystem after removing K fertilisation that is the targeted important validation.

L674-678: WUE: you never defined the modelled WUE. Avoid comparing apples with oranges. (e.g. https://hal.archives-ouvertes.fr/hal-01606915)

Thank you for this comment. Indeed the simulated WUE me mention in this paragraph is WUE_GPP (GPP/Transpiration). We agree that comparing this WUE to other WUE (intrinsic or wood) is not of the highest relevantce However, we do not have any direct measures for WUE_GPP and we wished to highlight the responses of different WUEs to K deficiency. We will calrify and modify this paragraph in the manuscript.

L678-679: K and GPP vs N and NEP - what is the connection?

Sorry, we do not understand this question.

References:

Caldeira Filho, A., Krushe, A. V., Mareschal, L., da Silva, P., Nouvellon, Y., Campoe, O., ... & Laclau, J. P. Very Low Nutrient Losses by Deep Leaching after Clearcutting Commercial Eucalyptus Plantations in Brazil. *Available at SSRN 4270148*.

Cornut, I., le Maire, G., Laclau, J. P., Guillemot, J., Nouvellon, Y., & Delpierre, N. (2022). Potassium-limitation of forest productivity, part 2: CASTANEA-MAESPA-K shows a reduction in photosynthesis rather than a stoichiometric limitation of tissue formation. *EGUsphere*, 1-27.

Hölttä, T., Lintunen, A., Chan, T., Mäkelä, A., & Nikinmaa, E. (2017). A steady-state stomatal model of balanced leaf gas exchange, hydraulics and maximal source–sink flux. *Tree physiology*, *37*(7), 851-868.

Laclau, J. P., Almeida, J. C., Goncalves, J. L. M., Saint-Andre, L., Ventura, M., Ranger, J., ... & Nouvellon, Y. (2009). Influence of nitrogen and potassium fertilization on leaf lifespan and allocation of above-ground growth in Eucalyptus plantations. *Tree physiology*, *29*(1), 111-124.

Maquere, V. (2008). *Dynamics of mineral elements under a fast-growing eucalyptus plantation in Brazil. Implications for soil sustainability* (Doctoral dissertation, AgroParisTech).

Walker, D. J., Leigh, R. A., & Miller, A. J. (1996). Potassium homeostasis in vacuolate plant cells. *Proceedings of the National Academy of Sciences*, *93*(19), 10510-10514.