



Dr. Gabriela Bielefeld Nardoto
Associate Editor
Journal of Ecology

Dr. Carlos A. Sierra
Research group leader
Tel.: +49-(0)3641-57-6133
csierra@bgc-jena.mpg.de

24th May 2021

Dear Editor,

Thank you for handling our manuscript and for giving us the opportunity to reply to reviewers' comments in a revised version. You will find below detailed answers to all comments by the reviewers. Also, we are providing a version that highlights all changes made.

Below you can find in [blue](#) our answers to all comments.

Comments from associate editor:

This is an interesting manuscript dealing with the fate of carbon in a tropical forest in Colombia. However there are several issues that should be addressed accordingly with the revision made. While one the reviewer had only two major concerns with this manuscript, the other reviewer pointed out a series of issues especially related to the description of the methodology and that some assumptions and statements throughout the text are quite overwhelming. Besides, the reviewer found that the discussion on carbon use efficiency should be better managed. The reviewer gives a very detailed explanation about the issues raised that should be carefully taken into account. Whether the author will be able to answer those, explaining in detail every single concern raised, and providing a new version of the manuscript that incorporates all the concerns they will have an improved version of it potentially being well worth to be published in Journal of Ecology.

[We followed the reviewers' suggestions for the most part. In particular, we eliminated the section on CO₂ fertilization effects and expanded the discussion on CUE. We also give more details about the computation of autotrophic and heterotrophic respiration, and added relevant information about the data and parameters in tabular form.](#)

Here are some specific points stressed below that should be seriously taken into account:

1. Please give a better explanation regarding the recycle time of soil carbon based on reviewer 2 concern.

[We provide now a more detailed explanation of the results obtained for soil](#)



carbon, and show that our results are consistent with previous results from Amazon forests.

2. Some important feedbacks are missing in the discussion of the manuscript, especially related to the uncertainties across the fluxes estimates.

We give a more comprehensive description of how uncertainty was addressed in this study.

3. Improve the descriptions of the methodology to make them easier to be followed.

Based on reviewers' suggestions, we added a new table describing the data, and expanded the existing table describing model parameters. We also included a description of the species most common in the study site.

4. I would like to see the calculations about transit times with different GPP products as suggested by reviewer 1.

We provide a better explanation on how we used the GPP products in our uncertainty analysis. We actually used 1000 different values of GPP based on an uncertainty analysis of the available products. This uncertainty is latter propagated to the uncertainty in the transit times and reported in the manuscript.

5. Please find more estimates regarding biomass as suggested by reviewer 2.

This was a misunderstanding, probably based on a poor description of the biomass data in the previous version of the manuscript. We used only local data in this manuscript, and we didn't use a literature reported value as suggested by reviewer 1. We explain the issue in detail in the response to reviewer 1.

Comments from Reviewer 1:

The authors present an approach for assimilating field observations and remote sensing predictions into a matrix model of carbon movement between ecosystem components. The authors find that a simple mean statistic is insufficient to characterize the "transit time", and that it is better to focus on the distribution of potential carbon transit times. Specifically the authors draw attention to fast and slow processes which are better characterized by simulating a distribution of transit times.

Thanks, this is a good summary of our manuscript.

First I applaud the authors on approaching this very difficult, but interesting topic and their proposed methodology is interesting and could be of potential use for developing future studies. Although I think this study at present (1) requires a more explicit table of model assumptions, parameters, and constraining observations, and (2) a more thorough description for me to understand it.

We added a description of all model parameters in Table 1, which includes now a description of each parameter so it is easier for readers to understand their meaning and the corresponding assumption within the model. We also added a new table describing the data used for model-data assimilation.

I am somewhat dubious of the partitioning between heterotrophic and autotrophic respiration.

We recognize that the approach to split total respiration between autotrophic and heterotrophic was not included in the previous version of the manuscript. We included this description in the new version in the methods section. We would like to point out that our quantification of these two respiration fluxes is the result of splitting pools in the model between live vegetation pools and dead biomass and soil carbon pools. Therefore, our estimates depend largely on the model structure and on the parameters obtained through data assimilation. Other methods to quantify autotrophic and heterotrophic respiration also have to make critical assumptions as there is no direct method to obtain both quantities from observations alone.

(3) I think the method also has problems around its assumptions of the hypothetical tropical forest ecosystem being in a steady-state equilibrium, but perhaps these simplifying approximations can be justified.

This particular topic was addressed in Sierra et al. (2007, *Global Change Biology* 13:838) for these particular forests, and we found that given the level of variability of carbon fluxes among different years, there is no evidence that would suggest that the forests are outside a range of dynamic equilibrium. We have conducted extensive studies in these forests, and so far we have not found conclusive evidence that would suggest that they are a major sink or source of carbon to the atmosphere.

In addition, it is important to keep in mind that almost all computations of residence time of carbon in the literature are based also on an equilibrium assumption, dividing a stock over a flux of carbon. This applies to the influential studies of Galbraith et al. (2013, *Plant Ecology & Diversity* 6:139), Malhi et al. (2015, *Global Change Biology* 21:2283), and Doughty et al. (*Biotropica* 20:16), as well as global scale analyses such as those of Carvalhais et al. (2014, *Nature* 514:213). If our results are questioned on the basis of the equilibrium assumption, similar concerns should be raised to these other previous studies.

I appreciate the focus on going beyond the mean and to estimate the distribution of carbon compartment transit times. It is also great to see the open git repository with not only the code for the analysis, but for making the figures and compiling the text. Also I think the authors are making an interesting point about the distribution of carbon transit times, and they potentially have an interesting but relatively simple modeling approach to estimate it. However, at present I don't think this is at the stage where it should be published. The connection to CO₂ fertilization is at odds with the (hard to justify) assumptions of the forest being in some kind of steady-state carbon equilibrium. I have highlighted several issues throughout this manuscript, which I think can be addressed. I understand some of the issues may well just be analytically infeasible. I would happily read a revision of the MS as there are many interesting aspects of this study. Most of my comments are focused on the methods, but some are targeted towards the discussion around carbon use efficiency and the relevance of this approach to CO₂ fertilization.

We recognize that there are uncertainties and limitations in our modeling approach, but the idea of an underlying transit time distribution is an important contribution that we want to put forward with this analysis. We provide here the conceptual framework and the mathematical methods to compute these distributions, and the specific estimates for this and other forests is something that can

be improved in the future with more comprehensive datasets, but we believe the manuscript and the idea is in a mature stage that deserves rapid publication, so other investigations can be based on this conceptual approach and computational method. Nevertheless, we acknowledge that some aspects of the discussions were too general and needed a more in depth treatment. Since we are mostly interested in publishing the approach and the idea of an underlying probability distribution of transit times of carbon, we decided to remove the discussion on the implications of the transit time distribution for understanding CO₂ fertilization effects.

General Comments:

It will be key to point out in the abstract that this regards a submontane tropical forest - not a lowland forest, so this may be worth making more explicit in the text.

We added this information to the abstract. However, it is also important to mention that the species composition in these forests is more similar to the composition of typical lowland forests than the composition of montane forests. We added in the methods section a description of the most important species in the area according to the Importance Value Index, so readers can have a better idea of the characteristics of the site and its similarities or dissimilarities to lowland forests.

It is not entirely clear to me why this study is focused on the El Porc region in Colombia. The satellite products are global, and there is data from other regions some of which have corresponding flux towers. I think this could be more clear if a table was presented with all the corresponding field observation data from El Porc.

This study is focused on the Porc region of Colombia because this is the site where the lead author has developed most of his tropical forest work. We have access to all data from the site, and know very well its ecological characteristics. It is simply natural to model the data that you have collected yourself!

To give a better idea of the data used for the study, which is entirely available in the supplementary material, we created a new table summarizing the observations used for the study with the corresponding field method.

Several statements throughout the text are stated far too strongly, which I urge the authors to acknowledge there is massive uncertainty across nearly all of the fluxes described here. For example, the section on CUE in the discussion is stated with certainty ~ but CUE is from well understood in the tropics and the literature here is lacking several more recent studies on the topic in the tropics.

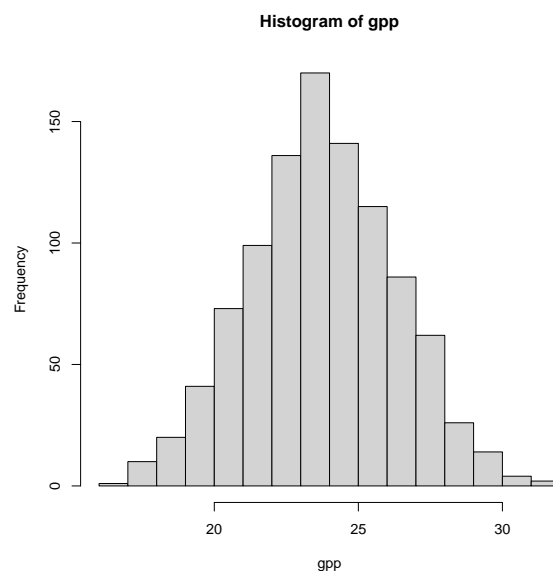
The discussion on CUE was edited to avoid statements that are too strong. We also included references to more recent studies on CUE in tropical forests.

Methods Comments:

The BESS (Jiang and Ryu) satellite derived GPP is very much a modeled product, and likely to have considerable error. Ideally this should be coupled with some comparison of the error to some kind of field measurement estimate of GPP or Eddy covariance flux tower estimate of GPP. I am especially skeptical of these modeled GPP estimates over closed canopy tropical forests where the optical signature is already saturated. The BESS product has a fairly low goodness of fit ($R^2 \approx 0.25$)

and RMSE of $3 \text{ g m}^{-2} \text{ d}^{-1}$). The BESS and MPI-BGC GPP products differ greatly from the standard MODIS GPP. I appreciate that at least two estimates of GPP were used, but these are the most similar. I suggest adding more GPP estimates. Essentially none of them are constrained by data over this region in Colombia, or really even everwet Neotropical forests.

We completely agree with the reviewer regarding the uncertainty of these estimates of GPP. This is exactly why we decided to do the fitting of the model using 1000 random variates of values of GPP, sampled from a probability distribution that propagates the uncertainty of each of the estimates. More explicitly, we took the two mean GPP values reported by Jian & Ryu and by Jung et al., propagating their standard deviation to a combined distribution of potential values of GPP. We sampled 1000 random numbers from this normal distribution and assumed each is a probable value for the site. A frequency distribution of these random variates is presented below. You can see that the spread is large, and that we are using many different possible values of GPP, taking into account that the original estimates are uncertain, but we used these Monte Carlo procedure to explicitly address the uncertainty.



It is also important to point out that the GPP product from MPI-BGC includes most (if not all) of the available GPP data from eddy-covariance towers. The product is based on a machine learning approach that combines satellite information with biophysical variables from the site and available eddy-covariance data. We believe that this is one of the most advanced approaches to obtain estimates of GPP for any site.

Some of the linear algebra parts of the methods (eq 5-8) were beyond my understanding to evaluate, so I hope the other reviewer(s) can more competently evaluate this.

This estimate of “transit time” is dependent upon several very strong assumptions, many of which I think are difficult to justify. (1) Linearity and steady state - this is already violated by using a sequence of secondary forests recovering from land

clearing. All forests are essentially recovering from some disturbance. I don't believe the transit of carbon through the compartments is actually linear, but I can understand the practicality of making this assumption for the purposes of the model. I think the authors might add some text exploring the implications of this assumption, and evidence for and against it. Basically, it should be made clear that this is a pragmatic approach meant to work as an approximation.

It is important to clarify here that by linearity we meant that there are no interactions among carbon-pool variables, which leads to first order linear differential equations. This is different than linearity in the context of linear functions as in linear regression. We used a linear dynamical system of differential equations, based mostly on a parsimony principle. There is indeed a potential of some process to interact nonlinearly such as CO₂ uptake based on foliage status, but we have very little information to derived nonlinear differential equations based on the available data, therefore we resort on parsimony in choosing our linear model structure. Furthermore, research by the group of Prof. Yiqi Luo (Northern Arizona University) has shown in a number of publications that the structure of most ecosystem models follows a linear structure similar to the structure we adopted in this study. Even complex land surface models such as CLM4.5 (Huang 2018, Global Change Biology 24:1394) have a linear structure, and one main reason for this is that the underlying nonlinear equations with their respective parameters are not well known.

(2) Using a singular literature derived fractional estimate of canopy biomass to woody biomass is quite a stretch. Also the estimate seems focused on primary forests, when other data in this study pertains to secondary forests. Tropical forests are quite variables, and I don't think it can be justified that a singular value could accommodate the varied secondary and old-growth forests used for the biomass data. I urge the authors to find more estimates, and especially consider the variability. They could even try estimating them from LAI and LMA. There are many caveats behind LAI products, but perhaps these could be accounted for in the uncertainty propagation.

The value used to partition foliage to wood biomass was based on measured data from the site, sampled at the time the local biomass equations were derived. It is not a 'literature derived' value as the reviewer suggests, but rather a measured value obtained from the destructive sampling of 144 trees for which their foliage and stem biomass was measured. However, this value was never reported in previous papers we have from this site, and for this reason we reported it as a citation from the thesis in which the value was actually computed. To avoid misunderstandings, we provide more details on the derivation of this number in the methods section.

Suggestions:

Make the assumptions loud and clear, and write more about the violations of the assumptions and why they must be made. Calculate the 'transit times' with different GPP products. Make a table of the transfer coefficients and cycling rates (α , k) with their respective sources. A sensitivity analysis might be warranted. Or at least, some analysis of how much do the parameters need to be perturbed in order for the uncertainty range to encapsulate the observations?

Our Monte Carlo method does exactly this. We provide now more details on the approach so others that are not familiar with the method can better understand.

It's not entirely clear how mortality is factored into this. I assume through $\alpha_{6,2}$, yet how does this estimate rate compare with field derived estimates of mortality, necromass production, and coarse woody debris accumulation?

The coefficient $\alpha_{6,2}$ is the proportion of carbon transferred from the wood pool to coarse woody debris. It includes the flux due to mortality, but also branch fall, and therefore it is difficult to compare directly with tree mortality data alone. The coefficient is a best estimate of the transfer of carbon based on the stocks of these two pools, but it is not derived from common measurements of mortality and CWD production from forest inventories.

More detail is needed into the separation of heterotrophic and autotrophic respiration. These are enormously difficult to separate, so much more detail is warranted here.

We added a section describing how autotrophic and heterotrophic respiration were obtained from the model.

I urge that a table is greatly needed of all the observations used to constrain the model.

We added a table describing the data used for the model-data assimilation exercise.

Soil carbon has many pools itself. I understand the authors might not want to overcomplicate this model - but this is an approximation that is surely at odds with the soil biogeochemical modelers who explicitly try to model the fast and slow cycling C pools. Perhaps the authors could try to write some justification for this simplification?

We only have observations of total soil organic carbon, but not data on different fractions. If we would have radiocarbon or fraction data we would be able to separate SOC into different pools as we have done in the past for other tropical forests (Sierra et al. 2013, Biogeosciences 10:3455). However, we don't have additional data to constraint a more complex soil carbon model for this site.

This is asking too much, but I suggest testing the data assimilation approach by sampling from a more expansive process-based ecosystem model that is being forced with a fixed climate and stationary CO₂ concentration. If this more simplified matrix model can recover the parameters (carbon allocation for example) of the ecosystem model, I think this would do a lot to convince the reader of the utility of this approach. I understand there is already a growing literature using matrix models to accelerate the spin-up of the biogeochemical soil components. CLM, CABLE, etc.

We appreciate this suggestion, but this is beyond the scope of the present manuscript and would probably add little to the main message we want to convey here. A more complex model with zero constraints from field data would actually add more uncertainty to our estimates for our field site. Nevertheless it is good that the reviewer can see that if can take a more complex model such as CLM or CABLE written in matrix form we can compute age and transit time distributions as we do here with a simple model. These complex models are valuable in the sense that they can predict spatial patterns, and one can compute transit time distributions across regions, continents or at the global scale. We are indeed working on a related manuscript doing exactly that, but again this is a completely different topic outside the scope of the present manuscript.

Table 1: This table needs a label of each transfer coefficient, also units. I know this can be pieced together from the text, but it's inconvenient to the reader.

Changes made in the table as suggested.

Figure 1. Why not label the arrows with the corresponding transfer coefficients? The figure gets too crowded if we add all transfer coefficients as suggested.

Figure 2. The sub-panels are not described (a,b,c,d) therefore I don't really understand what this is. Some of the colors are a bit difficult to differentiate, so labels are really needed. The observations are well outside of the range of random variates, which makes me think there are problems with the structure of the model and/or data assimilation approach.

We added descriptions of the different panels. There are indeed observations that fall outside the prediction value, as in any regression exercise. However, the important aspect to see in this figure is that the model can predict an average trend of carbon recovery of the different pools, and in some pools with more uncertainty than others. The model is fitted simultaneously to all data, therefore it tries to balance over- and under-fitting the different pools. This adds uncertainty, but we recognize that our predictions have a large degree of uncertainty, nevertheless the obtained distributions can tell us something useful about the age dynamics of the pools.

Figure 3. Is any carbon going to fine roots? In the bottom panel, it looks like the coarse woody debris is immediately decomposing, which seems at odds with reality.

The proportion transferred from foliage to roots is small, only 0.9% of all transfers from the foliage compartment. The coarse woody debris C does not decompose immediately. On the contrary it decomposes slowly at a rate of 0.5 yr^{-1} .

Line comments:

L5: The fate of most carbon sequestered in biomass is eventual microbial decomposition/respiration to the atmosphere, but this manuscript focuses upon within-tree carbon transport.

We actually provide in L5 a formal definition of *fate* for the purposes of this manuscript. We define it as *the trajectory of photosynthetically fixed carbon through a network of ecosystem compartments*. By providing this definition, we aim at being specific and avoid other interpretations of the term fate. The reviewer suggests a slightly different interpretation of the word fate, but this differs from the definition we provide in the manuscript.

L7: 'estimated' might be more accurate than 'quantified' since there are no field measurements of NSCs, etc.

Changed as suggested.

L15-17: I am not convinced this statement is relevant. Carbon turnover from biomass estimates is a very different thing that is somewhat disconnected from GPP and carbon use efficiency.

Sentence deleted.

L18-24: not sure about this section...

This comment is not specific enough for us to make any change.

L40: "radiative effects"? Unclear what is meant here.

This sentence was reworded to convey the idea that during the time carbon is stored in an ecosystem, it does not contribute to the greenhouse effect.

L45: I am not sure the global GPP flux is what is relevant here. It's the global NPP flux that's relevant to the carbon sink.

This is a study is about the amount of time the annual GPP flux stays in an ecosystem. Therefore, we consider relevant to mention the global GPP flux here.

L65-67: This is a very large assumption that is hard to justify, especially because it ignores the large contribution of canopy leaf NPP to ecosystem carbon dynamics. Yes, we agree, and for this reason we explore this assumption in this manuscript.

L107: Linear transfer rate assumptions seem like a reasonable starting point, but I doubt it's actually reflective of reality. I wonder if the authors could cite some justification for this assumption, or at least make it very clear this is a hard assumption being made for analytical tractability.

We added some reference in support of this assumption. In particular, most ecosystem carbon models make an assumption of linear transfer rates, and this has been shown by the group of Prof. Yiqi Luo, who has analyzed a large number of ecosystem carbon models and have shown that all of them represent carbon dynamics among compartments using linear first order rates. It may be that there are other more realistic representations, but we are not making here an assumption that is different from common practice in ecosystem-level carbon modeling.

L147: Perhaps the 'Theory' section should be a subheading within the Methods section.

We prefer to keep it as a separate section because this information is intermediate between the introduction and the methods. On the one hand, we simply introduce some equations and concepts that have been developed before, and on the other hand we show some particular equations that serve as the conceptual basis for the calculations on the paper. For this reason we believe this is a separate section, but we are flexible and can imbed it in the Methods if the editor considers this is more appropriate.

L181: I think more than one estimate of the canopy leaf biomass fraction is needed. As mentioned previously, this is not a literature derived number, but an actual measurement from the site. It was obtained in the process of developing the local biomass equations used for the computation of biomass, and was based on data from the destructive sampling of 144 trees. We used the citation here because the actual number of the leaf biomass fraction was not published before, but this does not imply that it is a literature derived value. To clarify we provide additional details in this section.

L186-191: More detail is needed in this section. I am unclear how the 1000 parameter values derived using the optimization algorithm. What was the optimization criteria? What was being maximized (or minimized) and was it a singular or joint optimization?

The optimization criterion was the difference between model predictions and observation. It finds the set of parameter values that minimizes this difference, and does it with the entire set of observations. We added these additional details to this section.

L198: Were these parameter sets just sampled randomly from a uniform distribution, or were they sampled from an observation informed prior distribution? Approximate Bayesian Computation methods might help constrain the sampling

procedure.

The sampling was over normal distributions. This information is in the section that describes the optimization algorithm.

L210: rapid accumulation of carbon in which compartment(s)?

All compartments except soil carbon. This sentence was reworded to make this point clearly.

L215: If GPP is 23.98 MgC ha⁻¹ yr⁻¹ (from L166), and total respiration is 23.7 MgC then the CUE is 1%? This seems wrong, even if the forest is at 'equilibrium'. Perhaps I am not understanding something. . .

The standard definition of CUE is the ratio NPP:GPP, not Re:GPP. We used the same definition of CUE as in well established contributions such as those of de Lucia, Vicca, Malhi, Doughty, among others. At equilibrium, GPP is equal to Re, therefore the ratio Re:GPP is 1 as the reviewer have confirmed, but this is not CUE. We present details about the CUE computation in equation 10, noting that these are standard equations and were not derived by us directly, but rather used in previous publications by different authors.

L245: The age of carbon in the ecosystems is not normally distributed , so why not present the median and the 5-95% percentiles?

The 95% quantile is already presented in the following sentence. We added the median to the text as suggested.

L282: Perhaps it would be better to focus on the transit times through each component in addition to the overall ecosystem transit time.

The contribution of each component to the transit time distribution is presented in Fig. 5, and presented at the end of the Results section. We added here a reference to Figure 5.

L293: We also measure fine roots and coarse woody debris to estimate NPP.

Added these additional measurements to text.

L300: For a more thorough exploration between the field measured and ECC discrepancies, I suggest looking at Campioli et al 2016 Nature Communications.

This is a nice reference, but it deals with a different type of comparison, GPP or NEP from biomass inventories versus eddy covariance. What we are referring to in this paragraph is the difference discussed in Clark et al. (2001, Ecological Applications 11:356), which is NPP obtained as the sum of different biomass increment measurements versus the theoretical definition of NPP as GPP minus Ra. For our study site we can compute both, because we have published estimates of NPP based on biomass increment data (plus litterfall, root growth, and herbivory), and a model-based estimate of NPP. For this reason, we abstain to cite the paper by Campioli et al. (2016) and stay with the more focused discussion promoted by Clark et al. (2001).

L308: I disagree, the CUE is known to be relatively low in tropical forests yet it is still quite variable. One can not simply state it has a fixed value of 0.3. It is far more complex. For example, see Doughty et al., 2018 Biotropica.

We agree with the reviewer that CUE is highly variable among different tropical forests. However, we were referring here to the value we obtained from our model, and not to all the different values reported in the literature. We rephrased this

sentence to avoid misunderstandings.

L323-326: I find this hard to understand. I assume the authors are attempting to state that conditional upon the assumption of steady state GPP, NPP, Ra, Rh, that the NPP flux will converge with the Rh flux.

Yes, but this is a very old concept, not our own idea. One of the first to introduce this idea and to use it for computations with forest data was Raich and Nadelhoffer (1989, *Ecology* 70:1346).

That may be so when integrated over the course of decades to centuries, but fluxes of NPP to the necromass pool are highly stochastic (mass mortality events) so I think it could be an overreach to make this approximation with a limited dataset. Further, even intact, old-growth tropical forests are by and large, undergoing massive deviations from steady-state equilibrium because of climate change and natural variability (e.g. El Niño). Also, is there not some (small) component of NPP that is effectively sequestered in a recalcitrant soil carbon pool?

The steady-state assumption we use here is the same assumption currently used by anyone who computes wood residence time as the ratio of Biomass to NPP_{wood} . We agree with the reviewer that old-growth forests are likely being pushed out of equilibrium, and there is a lot of variability from year to year; still investigators find useful to compute wood residence times, which have an implicit steady-state assumption. This is similar in our approach, these forests may be being push out of equilibrium by a number of factors, but we still think it is useful to compute the ratios of NPP to GPP, and provide an alternative interpretation of this ratio in the context of the transit time distribution.

L345-347: The link to the supplement on radiocarbon feels tacked on and poorly connected. The units in figure S1 are delta C14 per mill, which makes it difficult for me to compare to the “transit time” in years (Fig 4). It would be great to see the radiocarbon approach better connected (as in compared) to Figure 4 in the main text.

We expanded this section to make a better connection to the radiocarbon results. The main point here is that we are providing predictions of radiocarbon as a method to validate our model predictions of age and transit time. The standard unit to report radiocarbon values is as a ratio of ^{14}C to ^{12}C in relation to a known standard in units of per mil. In other words, the $\Delta^{14}\text{C}$ nomenclature is the standard way to report and compare radiocarbon values.

L361-377: I think this section gets pretty speculative and the connection to the analysis here is an overreach. Everything in this manuscript so far relies upon assumptions around steady state and equilibrium. I agree that data assimilation approaches are potentially very useful, but it seems at odds (contradictory to the steady state assumptions) to only now invoke the utility of this approach for examining consequences of global change (CO_2 fertilization). This is also missing any mention of the other aspects of elevated CO_2 , such as rising temperatures, potentially increased atmospheric aridity, and more variable precipitation. Each of these is changing GPP, respiration, and allocation. CO_2 fert is also changing biomass stoichiometry, leading to differences in photosynthetic capacity and microbial degradability. I could go on, but simply focusing on CO_2 fertilization is an oversight. In short, I suggest shortening the discussion to what is immediately relevant to this analysis.

We agree with the reviewer in that the discussion can be shortened, and therefore

we removed this section from the discussion. We still believe that the transit time distribution can be very useful to infer effects of CO₂ fertilization in old-growth forests, but such a discussion would need to be expanded. This is a topic that deserves a whole separate paper, and it does not need to be mixed with the more specific results of this manuscript.

Comments from Reviewer 2:

The article by Sierra et al. deals with the fate of carbon in a tropical forest located in Colombia. The authors basically calculate the transit time of carbon from the moment of photosynthesis to that it is respired back to the atmosphere integrating multiple ecosystem processes occurring at a wide range of timescales. The approach they propose is novel. The article is very well structured. The Introduction section is very well focused and exhaustive. The material and method section, despite the difficulty of following the not so easy formulas presented in the text, is relatively clear. Discussion is as well very clear.

I have only two main concerns:

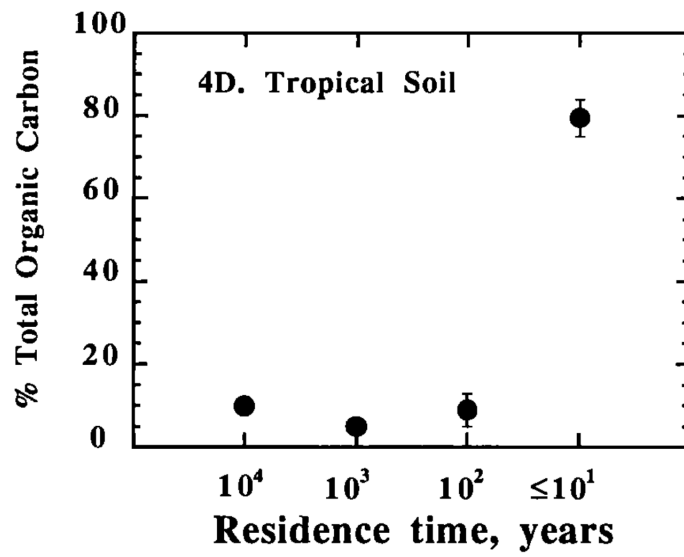
1) The first concern is about the description of the investigated forest. The authors state that are secondary forests growing on former agricultural and pastoral areas, but not so many other information are presented, despite they cite many papers that were performed in the same area. A sentence introducing the type of tropical forest investigated would be very welcome. For instance, at line 179, it is introduced the fact that palm trees are present in the considered area, not really a typical forest specie.

We expanded the description of the studied area following suggestions from both reviewers. We added a species list based on the Important Value Index (IVI). Regarding the presence of palms in our studied forests, we respectfully disagree here with the reviewer. The landmark study of ter Steege et al. (2013, Science 342:6156) showed that palms is one of the dominant families in neotropical forests. From the top 10 list of hyper-dominant species in Amazon forests, 6 are palms, two of which are common in our study site: *Oenocarpus bataua* and *Euterpe* sp.

2) The second concern is about the recycle time of soil carbon. In fact, I was quite impressed by the fact that soil carbon seems to reside in soil on average 63 years. Considering that the authors consider the soil compartment down to 30 cm depth, the recycle time of this compartment seems to be very fast. I do not have a direct experience with south American forests, but radiocarbon measurements from tropical African forests indicate much longer turnover times for such compartment when considering bulk radiocarbon measurements. Since the authors compare their data with radiocarbon data too, some more explanation for this fast recycle time of carbon in the soil would be very welcome. Apart from these two points it seems to me a very good article that certainly deserve to be published.

Our results are not in conflict with previous radiocarbon studies in tropical forests. Even though the mean age we obtained for the soil carbon pool was 63 years, Figure 4 in our manuscript shows that the underlying age distribution has a long tail. Carbon in the soil can still be hundreds of years old, but their contribution to total carbon is small. Similar results were found by Trumbore (1993, Global Biogeochem. Cycles 7: 275, see figure below), who found that about 80% of the total carbon in the soil (0-22 cm depth) had a residence time lower than 10 years,

and the very old carbon was just a very small proportion of the total.



Sincerely,

Carlos A. Sierra, PhD
On behalf of all authors