**Response to referees**

NPH-MS-2019-31661 Non-structural carbon ages and transit times provide insights in carbon allocation dynamics of mature trees

by Herrera, David; Muhr, Jan; Hartmann, Henrik; Römermann, Christine; Trumbore, Susan; Sierra, Carlos

Dear editor

Thank you for your positive evaluation of our manuscript. We also want to thank the referees for the positive comments, critical comments, and the suggestions made to improve the manuscript. We feel that responding to these comments and suggestions has significantly improved the manuscript. Below we provide a point-by-point list of our detailed answers to the concerns raised by each reviewer.

The new improved version of the manuscript main text has 7019 words, 519 words more than requested by New Phytologist in the author guidelines. We considered all the suggestions made by the reviewers to improve the quality of the manuscript. Unfortunately, we could not shorten the manuscript without losing important information. We hope you understand and consider our manuscript for publication.

Sincerely

David Herrera, on behalf of all co authors

**Comments from referee 1:**

*I still think this manuscript has the potential to be a useful synthetic representation of our understanding of tree carbon cycling, and thus a valuable contribution to our knowledge. The authors have made some minor revisions to this manuscript and added a partial uncertainty analysis. I like a lot of the discussion points and think this ties in nicely with the current literature on the topic. Certain of my previous major concerns arose from my own misinterpretation of the previous draft, for which I apologize. Certain others, namely the presentation of the uncertainty and overly high NSC ages, are partially addressed or not addressed at all in the new manuscript text. I am also more concerned than previously with the overall clarity and presentation of the text. These comments are outlined in more detail below.*

1) *I noted language and clarity issues on the previous version, but they haven’t really improved, and I realized that some of my own misconceptions about the paper arise from a lack of clarity, and the continuing presence of many grammatical and/or language errors as well as simple typos. Moreover there are also just issues with consistency and presentation (text results not matching those in tables, mislabeled parameters, different names for things in the main text vs. in the figure captions etc.), and somewhat poorly described methods, figures, and tables. As such the results are somewhat difficult to follow and the main points of the manuscript are sometimes obscure.*

We apologise for the consistency and presentation issues in the manuscript that led to the lack of clarity. We thoroughly revised the text and considered all the instance of the language and related issues mentioned below, and one of the native-english speaking authors has thoroughly gone through the manuscript. These many changes can be seen in the track changes version of the document. When we refer to the lines where changes were made in our answers below, they refer to the line numbers in the track changes version of the document.

2) *I think the presentation could be more compelling for New Phytologist. Furthermore, the motivation for the study could be clarified. In reading many of the provided motivations for the study, I can’t help but think we really just need more measurements. I find half of the figures to be tangential, while some of the most important results are presented in tables. Figs. 5, 7, and maybe 8 could be cut from the manuscript without a loss of much information. Meanwhile many of the quantitative results provided are not exactly/literally shown by the figures, even though Fig. 3 is often referenced.*

*Presentation could be improved if readers are to be able to “see” the results that are discussed. Annotations or modifications to Fig. 3/4 are a possible avenue.*

Thank you for the recommendations. We revised the introduction section and made major changes to improve the presentation. We think that the introduction now better highlights the utility of estimating NSC age and transit time distributions to understand NSC dynamics, and clearly defines the modeling approach as a very useful tool to obtain these distributions. We also made numerous modifications to the Results and Discussion sections to improve the clarity of the message and the explanation of the results.

We moved Fig. 7 to the supporting information and referred to it in the text as Fig. S2. We would not like to cut out this figure as it shows the results from the sensitivity analysis. This analysis answer our third question: what are the principal carbon fluxes that influence the NSC mean ages and mean transit times?

We agree that Fig. 8 is tangential to the central message of the manuscript. We therefore moved this figure to the supporting information and referred to it in the text as Fig. S3.

We consider Fig. 5 important as it is the only figure where the transit time distribution of each tree species is shown, and we refer to these distributions frequently in the text. This figure also shows how the transit time distributions change with time after the simulation of carbon limitation starts, which gives rise to the distributions of the mean transit time values plotted in Fig. 6.

The quantitative results provided in the text derive from the figures. For example, the mean values for the ages and transit times and the quantiles presented in the text are based on the distributions in Fig. 3. We think that annotating these values in the plot (Fig. 3) would make the plot more confusing and difficult to understand, especially as we are already plotting many distributions per species in one single graph. Instead we made several modifications to the text, that now explicitly states that the values are descriptors of the distributions shown in Fig. 3. In addition, we indicated the mean age values for the whole tree in the Table 3.

3) Presentation of the methods is still somewhat opaque. Yes, the authors draw upon a previously published method, but the relevant details regarding interpretation of the results of this paper must be clearly described. In present form, the details of the origin of some results is obscure. In particular it’s difficult to link the results to the description of the models. Some examples: 80-90% consumption of NSC, preferential mobilization of young NSC, length of starvation simulation? There are others. Put another way: it’s well discussed that probabilistic consumption of a well-mixed pool that is mostly young will lead to respiration that is mostly young. But it could be better described how the model actually “achieves” this. And it’s also unclear how the active/slow partition plays into this result. Clear explanations of these results, and how the model is getting them, could strengthen the utility of this manuscript for a broader audience (i.e. not just modelers!).

We consider this suggestion very important for improving the quality of the paper. We carefully revised the methods description and made many modifications to improve the clarity of this section. Most of the modifications were made in the subsection “Estimation of NSC ages and transit time of trees under carbon limitation (out of steady state)”. In the revised subsection we specified the length of the starvation simulation, and we described the method for calculating the distributions and their mean values at each time step without giving explicit formulas as they are relatively complex. To increase clarity in this respect we refer to Metzler et al 2018 where the mathematical framework is described in great detail. Also, we now define the consumption level more clearly and describe how we calculated it (lines 302-306).

In addition, we now provide a link to a public repository where the code that we used to obtain these results is publicly available.

4) *Some of my major concerns from initial review are not particularly well-addressed:*

*-While an additional uncertainty analysis is performed, resulting uncertainties are small and it’s not clear what that “uncertainty” really represents. O.K. But none of the figures include uncertainty estimates, just some new text in Table 3 (and for some reason Table 4 includes no uncertainty).*

In this new version we have improved the description of the uncertainty analysis (lines 335-350). We did not include uncertainty estimates in many of the figures of the manuscript because they are density distributions, and it is not standard practice to add error bars to density distributions. In other words, calculation of uncertainty usually relies on an assumption of an underlying probability distribution. Here, we present the actual distributions, not their approximations. An alternative to error bars in the density distributions would be to plot all the density distributions of each model realization that resulted from the Monte Carlo Simulations, but we believe it would neither change or improve the main message and would make the figures extremely difficult to interpret. Further, as the reviewer notes, the uncertainty in the model output is smaller than the expected differences in the mean age and transit time between species. Thus, adding extra density distributions for each species does not add extra information. Instead, we now include uncertainty estimates in Table 4.

*-The authors provided arguments in response to my previous concern of overly high ages: In their argument (a) they acknowledge ages are high compared to previous work, but have not included this in the main text. Their argument (b) is a useful clarification, and the additional table 4 is more in line with experimental results (great!), but total mean NSC ages in leaves are still older than 1 year in a deciduous angiosperm. Perhaps this is within the margin of error… but there is no uncertainty provided. Their argument (d) somewhat undermines this previous argument as they state the storage pool in leaves is 2.5 years old. I know of no evidence for the presence of significant amounts of 2.5 year old NSC in deciduous leaves. This was my original major concern. Lots of studies showing carbon autonomy of leaves, limited early spring dependence on reserves, etc. And here the provided uncertainties do not provide much room. I understand the distinction in their argument (c), but do not see the relevance. My point with stump sprouts was that these hold some of the oldest empirical measurements of NSC, because they represent extreme cases. Yet these are still younger than the reported mean stored NSC in stems in this study. Even the deepest rings measured in (Richardson et al. 2015 New Phytologist) are younger than the mean here.*

*In short, high NSC ages must at least be discussed in the main text as they suggest that these models are not adequately representing reality, and limit generality and scope of the implications. Again, I still think this is a valuable paper. That models fail to accurately represent what we know from measurements is not surprising. In fact I think it would be most interesting if the analysis could speak to why these errors arise from this type of compartmental model, and what the implications are of this mismatch for things like, say, dynamic global vegetation models, and, perhaps more interestingly, how we could improve them. Likely a topic for a different study, but in my opinion the magnitudes/directions of the discrepancies must be clearly discussed in the main text.*

1. We now mention explicitly in the main text the fact that estimated NSC ages in leaves are higher than expected (line 375). We also mention it in the discussion in the line 458 and discuss possible reasons why this might have happened and how it might be improved in lines 531-549, as well as in the limitations and conclusions section. In addition, we clarify that none of the carbon fluxes related to the foliage compartments had an important impact on the mean age and mean transit time of the trees’ NSC. We now also include the uncertainties related to the NSC mean ages of each organ specific compartment in Table 4 to underscore their range of variation.
2. Probably our original (*d*) argument was not well expressed, and that is why it may be perceived as undermining our previous argument. In our previous explanation we essentially wanted to express the same idea as in argument b, the mean NSC age generally measured in an organ such as leaves is the combination of the ages of all NSC contained in that organ, gathering fast and slow carbon pools. Hence, we think that 2.5 years (the age of the NSC in the slow carbon pool in leaves) is not the right estimated age of the NSC to compare with measurements. For the purpose of comparison it was necessary to combine all the pools in the organ specific compartment (leaves, stem and roots) and calculate the age of the NSC for each organ. We now stated in the text that 1.98 years old NSC is still an older mean age for leaves than expected, and we discuss some possible explanations for this in the discussion section (lines 531-549) and in the limitations and conclusions section. Despite this, we consider that our models and modeling approach produce adequate estimates of the NSC age and transit time distributions and their mean values for the other carbon pools.
3. We understand the comparison between the oldest measured age of the NSC of stump sprouts (17 year old) of the trees studied in Carbone *et al* 2013 and the mean age of the slow NSC pool in the wood (21 years old), and the concern of the reviewer for the big discrepancy between these two values. Nevertheless, with all due respect and consideration, we think this comparison should not be made. The use of the mean age of the slow carbon pool, for this comparison, would imply that NSC in other carbon pools do not participate in the regrowth after a major stress. However, our conceptual framework indicates that the transit time of the NSC is a better measure for the carbon used for growth (i.e. the age of the carbon leaving the NSC pool), and this is expected to be younger. Furthermore, the simulations with stress are in best agreements with data when they are assumed not to be able to access all available NSC reserves, but only about 50-60%. Under this assumption the estimate of the mean transit time is 10 years old for *A. rubrum* (Fig. 6). This estimate suggests that 10 years may be the age of the accessible carbon if trees have a consumption threshold that ranges between 50 and 60% of their reserves.
4. We now mention the possible reasons for the discrepancies between our model estimates and the reported ages of NSC by empirical studies (lines 531-549). Also, we now discuss in the limitation and conclusion section how these allocation models could be improved to increase the precision in the estimates.

*Minor comments:*

*Perhaps a minor point, please include line numbers in future responses to review so that reviewers are able to locate relevant revisions.*

*Title: I agree with another reviewer that the title is misleading and suggests this was an empirical study. I suggest to include the word “simulation”, but recognize I am always annoyed when reviewers suggest changes to my own titles :).*

As suggested, we modified the title of the manuscript for “Probability distributions of non-structural carbon ages and transit times provide insights in carbon allocation dynamics of mature trees”.

*Abstract: Why is the result that last-in, first-out patterns can be represented by probabilistic mobilization from well-mixed pools not noted in the abstract? This is (to my mind) one of the most important results here. Should also emphasize in abstract that the starvation simulations are somewhat “extreme.”*

As suggested, we included the probabilistic interpretation of the last-in first-out hypothesis in the abstract.

*L25: Odd phrasing.*

We modified the phrase in order to improve clarity (lines 24-25).

*L27: “and transit times across three species”… . Also, necessary to state species here? Awkward.*

We rephrased this to gain clarity (lines 26-29).

*L38: “The outflow of the NSC stored in the wood and the root fluxes”.-> Confusing. Why not, “Fluxes of NSC from wood and roots”?*

This sentence was modified to make the message clearer: “Mean NSC ages and transit times were sensitive to carbon fluxes in roots and allocation of carbon from wood storage” (line 37).

*Intro*

*L 86. Still a very confusing sentence. I think the phrase “mixture of NSC” is used to describe the NSC pools present in deep vs. shallow sapwood, while acknowledging the NSC in these two physical compartments is a mixture of NSC of different ages. However, the phrase suggests you are talking about the actual process of mixing. I tried to suggest an alternative phrase but I can’t understand the desired argument.*

We think that the confusion was due to the poor linkage of this phrase with the previous one. Now we have made several modifications to the introduction to make this point clearer and improve the linkage between the paragraphs. We now present the two different approaches to explain the differences in the NSC ages: one using different pools with different cycling speed (which we used in this paper); and an alternative modeling approach presented by Trumbore et al 2015 where the NSC is considered as one single pool and the differences in ages are explained by a radial diffusion of NSC where the NSC close to the cambion is younger than the NSC inward the wood.

*L 88. Delete “proper”*

Done

*L 101. Perhaps: “because the precise measurement of these quantities remains elusive” ?*

We split the phrase to make the point clearer.

*L 115: Perhaps: “about how NSC age distributions differ between tree organs and species, particularly under carbon limitation.”*

The first part of the phrase was changed as suggested, but in the second part we still would like to emphasise that the differences in the NSC distribution reflects the differences in the NSC use (line 135).

*L118. I don’t see a clear case for why more measurements are not the way to address these questions. What does this type of modelling approach uniquely contribute that we could not get from more measurements? Perhaps a good place to mention the cost of AMS measurements …*

In the improved version of the introduction we highlight better the importance and necessity of the modeling approach. We also think that more measurements are absolutely necessary to understand this question but it is still challenging to estimate NSC distributions from AMS measurements, we mention this reason in the line 118.

*L 121. I suggest emphasizing the utility of the additional information contained in an “NSC distribution” compared with a measurement (i.e. a mean age) for a given pool. (For example is the distribution skewed more towards younger NSC in different pools/species?). I am just “getting” this, and this is the second time I have read the manuscript…*

As suggested, we emphasized the utility of estimating NSC distributions by improving the connection between the third and fourth paragraphs of the introduction stating at the beginning of the third paragraph that the age distribution of carbon is a consequence of the NSC dynamics (line 106). Later in the fourth paragraph we refer to the extra information that an age distribution provides: “These distributions describe the relative abundance of carbon of different ages in each NSC pool” (line 144).

*L 130. “and for how long can these reserves last?” – Should be rephrased or qualified as the analysis cannot address this as trees never fully exhaust NSC reserves without succumbing to other physiological limitations. This part of the question is not clearly addressed in the discussion.*

Following the advice, we modified the question adding the word “theoretically” to emphasize that we are not representing mortality explicitly. Our analysis allows us to estimate the time that trees take to reach any level of consumption up to 99% (because under mass conservation principles trees never reach 100% of consumption) and the corresponding age structure of the carbon remaining in and leaving the system. In Fig. 6, we present the trajectory that the mean transit time follows while trees consume their reserves under the carbon limitation simulation. This figure also indicates how long an average tree of each species would take to reach any level of consumption from 1% to 99% assuming that there are not other physiological limitations. With this information we now discuss the time that trees would take to consume their reserves assuming theoretical consumption thresholds ranging from 50 to 60% and from 80 to 90 % (lines 419-424 and 550-554).

*L 137-139. But how is consumption level determined by the modelling approach?*

We included this description in the methods (Line 302).

*Methods*

*Line 167. Perhaps emphasize that the assumption that starch=slow and sugars=fast is built into the original model. Or was this added by the authors to their parameterizations of these models?*

Yes, Added in line 193.

*L247. Constant B under zero assimilation is very unrealistic. Carbon supply and environmental conditions alter carbon allocation. I see this is discussed briefly in L 498, but I feel this is a major weakness of the approach.*

We agree that constant B is unrealistic and that may be one of the causes of the discrepancies between our mean age estimates and the ones reported before. This is discussed in the second subsection of the discussion, under point 3 of the possible reasons that may explain the discrepancies between our estimates and empirical measurements (line 565). This would be a topic for further study where we may be able to incorporate in our models specific dependencies of the transfer carbon coefficients (B entries) with the environmental conditions or stressful situations. Nevertheless, with the sensitivity analysis we have evaluated how variations in the B entries affect the mean age and transit time. This is discussed in lines 579 - 587 and presented in Fig. S2 in the new version of the manuscript (Fig. 8 in the former version).

*Line 261. Methods should at least be described in a supplement.*

We now provide a general description of the method we followed to obtain our results. The description and derivation of the formulas used in the simulations for our systems out of steady state are exhaustively described in Metzler et al (2018), and they are implemented in the public python packages referenced in the manuscript. We also include information for where to find the code that we used for our calculation, including how we used the formulas in the python packages. We therefore would prefer not to duplicate information already published in Metzler et al 2018, especially as this would significantly lengthen the manuscript.

*Line 268. Note that this is at steady state (?).*

Yes, included.

*Line 276. “feasible range” is extremely vague, giving no real indication of values used.*

We now improved the description of the uncertainty analysis. Now we refer directly to the range of variation of the parameters that we used in the uncertainty analysis for each model, and we provide the range of variability as a standard deviation for each parameter in Table 2.

*L277. p+1 is not defined*

We now defined p (parameters) in the text and did several modifications to this part of the manuscript to be clearer with the procedure followed to run the sensitivity analysis.

*L 280. So p is 99?*

No, this is due to our lack of clarity in the definition of *p.* Indeed *p* is the number of parameters (19 for *P. halepensis* and 20 for *A. rubrum and P. taeda*).

*L 290. MCS not well described. How many iterations? Also again referring to the “assumed parameter space” without reference to something interpretable like a variance or standard deviation.*

We made major changes to the description of the uncertainty analysis, including improving the description of the Monte Carlo Simulations carried out to propagate the uncertainties from the model parameters to the model outputs (lines 335-350).

*Results*

*Line 307-315. What figure/table associated with these results? And where are the uncertainties coming from/ what are they? Standard deviation of monte carlo simulations? Something else?*

These are the mean values of the NSC age distributions for the whole tree presented in Fig. 3, now we added these values to Table 3. The mean transit times correspond to the mean values of the transit time distributions of trees in non limited conditions, black lines in Fig. 5. The Uncertainties are the standard deviation of the Monte Carlo Simulations. We made several changes to the description of the uncertainty analysis and the results section to increase clarity.

*L 320. P halepensis result does not match Table 3.*

We apologise for the mistake, it was corrected.

*L 318. Delete “slightly”*

Done

*L 365. No figure, table, or uncertainty associated with this result.*

We apologise for neglecting the uncertainties in these results, we now corrected the error and added the respective uncertainty to each result. We also made a major change to the description of the results (lines 419 to 424) to improve clarity. And we now referenced the Fig. 6 as the figure where those results can be found.

*L 367. What is consumption level and where is it defined? If a rate, is it annual?*

We now defined consumption in the methods section and we also included the information how this level of consumption was calculated (lines 302-306).

*L 376-387. Letter the subpanels in Fig. 7-8 and refer to them in the text.*

Fig. 8 and Fig. 7 were moved to supporting information. In each figure we named the subpanels with the species name and the corresponding model output (mean age or mean transit time). In the text we reference these results in a general way given the fact that each model output was sensitive to the same fluxes in all the three species.

*L 388-390. If not giving a figure or table, give numeric values in text.*

We modified the structure of the phrase to increase clarity. We also included the numeric values for the mean differences of the NSC age between species and the mean uncertainties in the mean age for the three species.

*L 391. Perhaps an example?*

We made a minor change to the phrase to indicate that these values were obtained from different combinations of the model parameters. Some rare combinations of these parameters may result in exceptionally high mean ages that can be seen in Fig. S1 (System Mean Age plots).

*Discussion*

*Subheadings (associated with the questions stated in the introduction) might be helpful.*

We welcome this suggestion and added three subheadings to the discussion section. they correspond to the discussion of each main question presented in the introduction.

*Paragraph line 400: Second part of this paragraph in general is vague and poorly supported.*

We made several changes in the paragraph to improve clarity. We also added more references to support the discussion.

*L 402-403. There was no simulation of different environments.*

It is true that we did not simulate explicitly different environments. We rephrased this sentence to increase clarity (lines 463-467). We still refer to different environments in the discussion because the *P. halepensis* trees used to parametrized our model *P. halepensis* model grew in an extremely limited mediterranean environment, in contrast to *A. rubrum* and *P. taeda* that represented average trees of each species in temperate environments.

*L 414. “However, it may also be possible that long-term storage pools where neglected by the assumptions made in this model.” Vague. Which assumptions? How? More typos.*

We now improved clarity and form in this phrase. We specified the assumptions that may lead to this very low mean ages and transit time (lines 480).

*L415. “These results are consistent with the difficulties of separating and measuring NSC pools”. Unclear what this is referring to (and no reference to help clarify).*

We made several changes to these phrases to clarify the message and added a reference to support our statement (lines 485).

*L454. An issue of style: This might be stronger if this material was moved directly into the topic sentence in line 423.*

We thank you for the suggestion, but with the inclusion of subheadings we consider that this paragraph now provides a good conclusion for this subsection of the discussion.

*L 458. “highlight the utility of obtaining the NSC transit time distribution in mature trees for understanding carbon source/sink imbalances” I wonder how this could be done empirically?*

Measuring NSC age distribution is still very challenging. It may be possible that by comparing the NSC mean age and mean transit time with carbon isotopes of key pools, and by measuring the key fluxes reported in the manuscripts, we may be able to empirically estimate NSC transit time distributions. Then, following how these distributions change may help to understand carbon source/sink imbalance. Also, when trees are carbon limited, such as girdled trees, the age structure of NSC used for survival can give some idea of age distribution if this is combined with estimates of NSC reserves consumed as discussed in Muhr *et al* 2018.

*L475-477. Where does 80-90% come from?*

We refer to this range of exhaustion of the NSC reserves as an observation where the increasing trend of the transit time starts to change from an exponential increase to a linear increase, which reflects the flattening of the NSC age distributions in the tree.

*L 478. “the end of this period” -> what period? (Length of starvation simulation is not specified in methods. What are the stopping criteria?)*

We rephrased this and clarify that we are referring to the end of the exponential increase in the transit time.

*L 484-485. These might be better to note on one of the figures.*

We now refer to Fig. 6 in the text.

*L 486-488. How is this actually represented/produced by the model?*

We now provide extra information in the methods section (lines 289-308). In these lines a general description of the method to estimate the distributions and consumption level at each time step is provided,

In general the method consists in solving the system described in Equation 1 for each time step of the simulation. This allows us to map the movement of the carbon particles through the system in each time step and therefore to calculate how many years it takes for the system to exhaust certain percentage of the NSC reserves. This is plotted in Fig. 6.

*L 493. ii) Or steady state is not a reasonable assumption.*

We also agree that steady state conditions are a very special condition and may be rare in reality. But we have used this assumption for mature trees in order to understand this complex process.

*L 509. What is “source variability”?*

We added carbon to the phrase to make it clear that we refer to the carbon fixation variability

*L 539. “but it is still unclear what physiological and environmental factors alter this carbon allocation parameters”. I may be misunderstanding (“this carbon allocation parameters”?), but again, lots of research on variation and controls upon carbon allocation:*

*Comas, L. H., L. J. Anderson, R. M. Dunst, A. N. Lakso, and D. M. Eissenstat. 2005. Canopy and environmental control of root dynamics in a long-term study of Concord grape. New Phytologist 167:829–840.*

*Giardina, C. P., M. G. Ryan, D. Binkley, and J. H. Fownes. 2003. Primary production and carbon allocation in relation to nutrient supply in a tropical experimental forest. Global Change Biology 9:1438–1450.*

*Guillemot, J., C. Francois, G. Hmimina, E. Dufrêne, N. K. Martin‐StPaul, K. Soudani, G. Marie, J.-M. Ourcival, and N. Delpierre. 2017. Environmental control of carbon allocation matters for modelling forest growth. New Phytologist 214:180–193.*

*Poorter, H., K. J. Niklas, P. B. Reich, J. Oleksyn, P. Poot, and L. Mommer. 2012. Biomass allocation to leaves, stems and roots: meta-analyses of interspecific variation and environmental control. New Phytologist 193:30–50.*

*Kannenberg, S. A., K. A. Novick, M. R. Alexander, J. T. Maxwell, D. J. Moore, R. P. Phillips, and W. R. Anderegg. 2019. Linking drought legacy effects across scales: From leaves to tree rings to ecosystems. Global change biology.*

Thank you for the suggestion and all the references. We also realized that this phrase was very confusing and vague, and that the idea we wanted to express was already included in lines 587-595 with supporting references, therefore we eliminated this phrase from the manuscript.

*L 567. “providing a plausible probabilistic interpretation about why trees consume primarily young carbon during healthy stages and why this shifts after a prolonged disturbance”. I would argue this is the most important result from this study, but did we need such a complex modelling approach just to show evidence of this?*

There may be simpler ways to prove this concept. Nevertheless our “complex” modelling approach (which was already very simplified) provides extra and useful information such as the NSC age and transit time distributions for different species and different carbon pools. With this information we were able to estimate the differences in carbon storage strategies between three species and the role of each tree NSC compartment in the overall NSC dynamics. Furthermore our modelling approach allowed us to estimate how trees use the NSC under limiting conditions and how the NSC age structures changes over time when demand of carbon exceed the supply. These results also allowed us to estimate the age structure of the carbon at any exhaustion level of NSC.

*Figures and tables.*

*Captions refer to the “disturbance” but this is more often referred to as “carbon limitation” or “simulated starvation” in the text. Please be consistent.*

We now homogenized this to “carbon limitation” in the text as well as in the captions.

*Table 3: uncertainty estimates are narrower than the precision with which we can measure del14c. I understand these are model uncertainties based on a sensitivity analysis, but this makes me question the uncertainty estimates and makes the results seem even more dubious.*

*Active pools are also mislabeled in this table (FLNSC instead of FANSC).*

We apologise for the misspelling of the pool names in Table 3 and we made the necessary corrections.

The estimation of the NSC age with 14C measurements is completely independent of the age estimation provided by our modeling approach. The uncertainties that we reported are the propagated variability of the model parameters (now provided in Table 2) to the mean ages and transit time (Table 3). To explain why some of these uncertainties are so small is difficult because these uncertainties depend on variables that we can not improve yet, such as:

i) the model parameter estimation done by Klein and Hoch (2015) for *P. halepensis* and Ogle and Pacala (2009) for *A. rubrum* and *P. taeda.* We acknowledged in the manuscript that these parameters can be improved;

ii) The assumptions related to the Monte Carlo Simulations such as the independence between parameters and their gaussian distribution. The estimation of the uncertainties may change if better information related to the parameters becomes available.

In addition, it should be considered that the uncertainties related to the NSC age estimation with 14C measurements are affected by the sampling design (number of trees, sample size, sampling point, etc) and the age of the trees.

*Table 4 includes no uncertainty estimates even though you report them in the text.*

Apologies for the mistake. We already include the uncertainties related to these calculations.

*Figure 6. The caption does not define key points of the figures. The subfigures are not lettered. “Productivity” rather than “assimilation” is used in the caption.*

We carefully revised the captions and added information that would improve the interpretation and reading of the figures. Instead of letters for naming the supanels we used the name of the species because we think this is simpler for the reader. We also changed the word “productivity” to “assimilation” in this caption.

*Fig 8. What is the point of plotting variance vs. mean? Bigger quantities always have larger variances, but this figure is only used to present which quantities are the most influential (mean) without reference to the variance. Nor is variance ever discussed. And none of the small mean quantities are legible in this format anyways.*

The referee refers to Fig. 7. The variance of the “elementary effects” reported in this figure is an indicator of the non-linearities in the response of the NSC mean age or mean transit time to changes in the carbon fluxes. This is now mentioned in the text and in the figure caption and supported better with the Fig. S2 (former Fig. 8).