Response to referees

Manuscript ID: JEcol-2023-0296, "Starch storage strategy in the stem wood influences carbon dynamics and storage-growth trade-offs in tropical trees"

Dear Editor

Thank you for considering our paper timely and interesting. We really appreciate your consideration for publication in Journal of Ecology. The two reviewers are clearly experts on the subject and have made many important comments and raised reasonable concerns. We have done our best to answer their concerns and incorporate all their suggestions into our manuscript, as well as to rewrite many parts of the manuscript to increase clarity and readability. Below, we provide point-by-point responses to each comment. We cite in italics all the comments from the reviewers and our responses in normal font with an indentation to the right immediately after the comment. We hope you consider our replies and editions satisfactory and responsive - certainly we feel this has improved the paper.

Sincerely,

David Herrera-Ramriez, on behalf of all coauthors

Comments from reviewer 1:

*Overall comments:*

*The authors address a timely topic in tree physiological ecology by aiming to understand how life history traits (starch storage in parenchyma vs. living fibers, and evergreen vs. semi-deciduous leaf habit) influence seasonal tradeoffs between starch storage and growth in tropical tree species.The conceptual figure they present (Fig 1) is very helpful in interpreting their results, which are not always straightforward. In general, the manuscript could benefit from some added clarity. See my suggestions below for doing so.*

*Figure 1 is a very nice figure that is useful both for understanding the motivation for the study and interpreting the results. However, the evergreen/fiber storing species combination is not explored in this study, which is a weakness. Please explain why this combination was not tested in this study, as it appears to have been considered in a previous study (Herrera-Ramirez et al 2021). Make sure that this is also clear in the discussion, as it constrains the interpretation of the results a bit.*

We thank the reviewer for the encouragement and all the helpful comments and recommendations that helped our manuscript gain clarity and we hope that reviewers find the new version easier to read and understand.

We agree with the reviewer that all the combinations in the conceptual framework should have been tested. Unfortunately, we did not consider evergreen/fiber-storing species since the beginning of the study and this combination stayed out of our observations. The decision to consider only the three combinations came from the results from our previous study Herrera-Ramirez et al., 2021. In this study we did not observe an influence of the leaf habit in the NSC storage, growth and mortality for the fiber-storing species (Evergreen and semi-deciduous) and therefore we expected them to behave similarly in terms of carbon fluxes. In that study we also observed that evergreen/parenchyma-storing species and semi-deciduous/parenchyma-storing species, despite showing similar mortality rates, differed in terms of starch storage and growth, and therefore we considered these two groups in our analysis. We tried to make this clearer in the text: in the introduction (lines 141-146) and In the discussion (Lines 486-492).

*Although the authors state in the discussion that their results support the conceptual framework, this is not always true, except for D. microcarpa. The other two species do not always match up with expectations, particularly for S. guianensis. For example, expectations of NSC accumulation for S. guianensis do not match with results, which show highest levels of starch during the wet season. I think it could be helpful to either revisit (in the discussion) Figure 1 based on the observed results, or include a table showing expectations and actual outcomes for each of the variables in Figure 1 that were measured in the study. This would help clarify the results and structure discussion.*

We really found this suggestion useful. We realized that we were not clear in our writing of the results and the way in which we explained them in the conceptual figure. We have also thought that the way we presented the framework could lead to misinterpretations, so we adjusted our framework to provide more clarity. The conceptual framework is presented to explain in a combined way the results and it was not built before the analysis as a set for our hypothesis. We made that clear in the discussion in the new version (lines 458-495) . We also restructured the discussion to explain the differences in the carbon fluxes for each species and we explained clearly why they may not match some behaviors conceptualized in Fig 1.

*While the tradeoff between radial growth and starch storage in the sapwood is important to understand, it should be noted somewhere in the discussion that growth-storage tradeoffs could be clearer or change when considering other forms of growth that may be prioritized over radial growth, and other starch storage tissues (e.g., secondary phloem – see Rosell et al. 2020, Inner bark as crucial tissue for non-structural carbohydrate storage across three tropical woody plant communities, Plant, Cell & Environment).*

We agree with the reviewer and now make it clear in the text that we refer only to stem wood growth and not other types of growth (lines 536-539).

*Lastly, while I recognize that there could be a link between differences in mortality rates across species and the results of this study, these relationships were not explicitly tested. Thus, the conclusions drawn in the discussion don’t have much strength to them. I think it is okay to discuss the results in the context of the differences in mortality rates, but linking them (e.g., L 393-394) may be a bit of a stretch.*

We agree that we did not specifically test mortality. We did our best now to not directly link our results with mortality, but instead to use our results as a potential explanation for the previously observed differences in mortality rates for the studied species. Further experiments to test mortality are taking place and we will soon have data that will allow us to relate mortality to the carbon fluxes.

Line-by-line comments:

*L 68-70: What about secondary phloem’s role in transport and as an NSC pool see Rosell et al. 2020, Inner bark as crucial tissue for non-structural carbohydrate storage across three tropical woody plant communities, Plant, Cell & Environment)? Some more nuance here would be valuable.*

Our original phrase was poorly written, we did not want to ignore the role of secondary phloem in transport and storage of NSC. We rephrase and clarify this information to be more precise about what we wanted to say and to acknowledge the role of secondary phloem in NSC dynamics in the stem wood (lines 70-73)

*L 92-94: I’m not sure this should be phrased as a hypothesis since this isn’t exactly what is being tested in this study. Could re-phrase to “Thus, it is possible that trees with larger plasticity…”*

Thank you for the suggestion, we implemented it and we think it fits better what we want to say (lines 99-103).

*L 95: Needs some clarification. After store starch in, add “parenchyma or living fibers of”.*

We implemented it (line 100).

*L 94-97: This sentence should be rephrased as it leads the reader to believe that there is a disturbance component to this paper, when storage dynamics and carbon sink activity are not being examined over a period when disturbance occurred. Perhaps, rephrase the latter part as “…in tree species that often experience multiple disturbances in a tropical forest.” In addition, it would add clarity to describe what is specifically meant by storage dynamics.*

We made the phrase clearer and more specific, and added the suggestions made by the reviewer (lines 99-103). Our measurements were taken for a period of one year, and during that year trees experienced stress during the dry season so we could analyze how storage dynamics and carbon sink activity responded to this disturbance for each group of species.

*L 105: Missing word after NSC? Storage.*

Yes, our mistake, added (line 111)

*L 108: has been reduced*

Thanks, this was corrected.

*L 116-121: It is not clear how investigating the relationship between tradeoffs and storage strategies will help to clarify the problem addressed in L 114-117, which I think is what is being suggested. Additionally, again, lines 120-121 suggest a disturbance component to the research which was not addressed. Remove, or rephrase.*

We rephrased the lines to give more clarity to what we wanted to say (lines 124-129). We were introducing the way such trade offs are regularly measured, e.g. as the relationship between annual growth and maximum NSC storage. However, as the work of Blumstein et al., 2022 pointed out, this comparison may be biased and alternative methods to quantify trade-offs should be adopted. In our paper we propose to use the change in starch between seasons and observe tradeoff between starch storage and growth on a seasonal basis. We removed the disturbance component from the lines 120-121.

*L 125-126: Explain this further. It is not clear (at least to me) how leaf habit is a proxy for carbon source variation throughout the year, and how this applies to the objectives of the study. Do you mean that evergreen and semi-deciduous trees just have different seasonal patterns of photosynthesis, and thus, evergreen species will have less variability in carbon accumulation than semi-deciduous species? If so, I think this could be phrased more clearly.*

We have rephrased the entire paragraph to give more clarity to our ideas (lines 130-146).

*L 132: Expected*

Changed.

*L 133-134: Explain somewhere why greater demand for carbon from respiration is expected for fiber-storing species.*

We shortly included the explanation in this phrase (Lines 147-151).

“i) semi-deciduous/fiber-storing species would have larger amplitude in the seasonal changes in wood NSC mass due to greater seasonal variation in photosynthesis, greater demand for carbon for respiration (due to a larger amount of living cells in the stem wood) and a greater seasonal variation in growth rates than the parenchyma-storing species”.

*L 135: Add a quick explanation for why an increase in carbon sink activity during the wet season is expected.*

We rephrased the entire hypothesis to make it clearer (lines 151-155).

*L 137-140: Add further explanation here as to why strong seasonality in growth rates would lead to a negative correlation between growth and storage during the wet season, and why the expectation is that parenchyma-storing species would not have a negative growth-storage correlation.*

We rephrased and simplified the hypothesis and made it more general (lines 155-157).

*Figure 1: It is not entirely clear why the growth-storage tradeoff is expected for the semi-deciduous/fiber storing species and not the semi-deciduous parenchyma storing species that have similar expected seasonal patterns for photosynthesis and NSC/NSC consumption. Please explain this explicitly in the introductory text.*

Indeed, the purpose of Figure 1 was to explain this in a more graphical and clear way. Maybe our representation was not entirely precise about the behavior of the carbon fluxes in the wet season. We readjusted the fluxes a bit to show more clearly that there is less starch consumption during the wet season by the parenchyma-storing species, mainly because respiration is lower and growth rates do not increase significantly during this season. These small changes in growth rates and low consumption of starch result in non- distinguishable seasonal trade-offs. We included this explanation in the discussion as we consider it is more appropriate there (lines 458-466).

*L 191: It would be helpful to see a few example images of the slides. This could be added to the supplementary info.*

As similar images have been published previously, we added new images for our three species in the supplementary information (Fig S1).

*L 216: Remove comma. Or re-word.*

Reworded (lines 241-243).

*L 247: Should be 25 °C.*

Changed.

*L 260-266: Please list level of used for significance for these tests.*

We restructured this section and distributed it throughout the methods section. After describing each set of measurements we now mention what statistical tests and what confidence level we used to compare the data. We also improved the reporting of the statistical results in the results and discussion section and we now refer to the statistical results in each figure.

*L 262-265: How were these differences evaluated? Statistically? This is not clear in the figure (Fig 6) either.*

We improve our figures to be clear about the statistical differences between groups. We eliminated Figure 6 as it was just showing the association pattern between starch mass and environmental variables, but it was not adding important information to the story.

*L 270-272: Move to methods (line 230), otherwise it is not clear when reading the methods why sugars were only measured for 2018.*

This part was moved to methods (lines 253-259). Nevertheless, we still consider it important to repeat in the discussion (lines 513-516).

*L 276: Un-italicize 0.055*

Done.

*Figures 3-8: It would be helpful to clearly label which trees species had which combination of leaf habit and starch storage strategy. Also, please note in the figure captions when there are variable scales for the axes.*

We added labels of the species traits to most of the figures. We also rewrote all the captions and modified figures to increase clarity.

*Figure 3: Add level of significance used.*

We added the level of significance in the caption (now this is Figure 4).

*Figure 4: Make wider to help with crowding of the x-axis labels. Describe what red vs. blue coloring means in caption (this is needed in other figures too, except Figure 6 where it is included).*

We added in each caption the meaning of the colors (now Figure 5. We also edited the figure, unfortunately making it wider did not improve the presentation.

*L 327: Is it known that the trees are consuming starch for growth? Or could their starch resources be consumed by other sinks during this time period?*

Starch can be used for other carbon sinks and not only for growth, but starch accumulation/consumption was correlated with growth in this season. We made an effort to describe this better in the text (lines 395-397).

*L 331: Add a comma after clear.*

We modified the second part of this statement to increase clarity (lines 401-409).

*L 332-335: Which figure shows this? I am having a hard time understanding how the lag is shown in Figure 7.*

The correlation shown in Fig 7 for the parenchyma-storing species are starch fluxes vs growth with a time lag of three months, while for fiber storing species they are simultaneous correlations. We rewrote this in the main text and added more clarity to the definition of the seasons and the starch changes observed between seasons. We also modified Figure 7 to make it clear what is being compared in each panel and we also mention it now in the caption. We hope that in this version the time lags are easier to see.

*L 338: Replace NSC with starch.*

We replaced NSC with starch in every instance where it was necessary. .

*L 343: Replace upcoming with “following”.*

Done.

*L 347: Starch mass is from the whole tree core, correct? Or perhaps I misunderstood in the methods. Either way, it could help to state that here.*

The reviewer is correct, the starch mass is from the whole tree core. We stated that more clearly in the text (line 410).

*L 354-356: Only for S. guianensis according to Figure 8; rephrase.*

Correct, we rephrased it to be clearer (lines 414-421).

*L 359: S. guianensis also had a positive relationship between annual growth and starch during the wet season, but that isn’t mentioned here.*

Right, we only mentioned the stronger correlation we observed. Now we mention both (lines 414-418).

*Figures 7 and 8: Since many of these relationships are not significant, it might be clearer to remove the regression lines where they are not significant.*

We partially incorporated the reviewer comment here. We changed the solid line for a dashed line to show the non-significant relationships. We still think it is important to show the trend even if it is not statistically significant with 95% confidence. We also increase the size of the dots (individual data) to add clarity to the plot.

*L 378: These results only partially agree with the conceptual framework (see my overall comments), so I’m not sure you can say this. Also, specific hypothesis were not explicitly tested.*

This is correct, and we apologize for the lack of clarity in our writing. We rephrased most of this section to bring more clarity about our results and how they are explained by the conceptual framework. We are also more explicit now about the differences between our finding and the conceptual framework (lines 458-495).

*L 381: What other carbon sources are being referred to besides photosynthesis? Clarify.*

We do not refer to any other carbon source, we now made clear that we are considering photosynthesis as the only carbon source here (lines 458-466).

*L 382: What about storage?*

Storage is included in the NSC dynamics, it is considered as the accumulation of NSC as storage. We rewrote most of this part of the discussion to make it clearer.

*L 382: Since starch was measured and not total NSC, I think it would be clearer to just say “starch” rather than NSC throughout the discussion.*

This is correct. We modified most of the instances where we used NSC instead of starch and made it clear that we are talking about starch accumulation and consumption. Nevertheless, as we point out in the text, starch represents the majority of NSC in our species.

*L 383: Do you mean differences in background mortality? Isn’t that what is shown in Table 1? Or was there some mortality event?*

Yes, for now we only have background mortality. Although mortality was not tested directly in our measurements, we used background mortality rates in the control forest to support our hypothesis.

*L 387-389: It is not clear why lower and more seasonal allocation of carbon to growth should benefit competition or survival. Include an example and citation here to explain this.*

We have simplified most of this part of the discussion to be more clear about our results (lines 467-475). Nevertheless, we added this explanation later in the text were we thought was better expressed (lines 545-547)

*L 390: Add “seasonal” before plasticity.*

Done

*L 391-394: It is also not clear why no seasonal variability in starch storage or growth show a priority of growth over storage; please clarify.*

We apologize again for the lack of clarity. We have modified the text to make clear that we refer not only to the lack of seasonal variability of starch to storage of growth in *O. leucoxylon* trees, but also to all the results from parenchyma-storing species. These maintain nearly constant growth rates during the entire year, have low wood respiration rates and less seasonality in starch storage, lower allocation of carbon to storage on a seasonal basis. *S. guianensis*, even though it stores a lot of NSC in deeper layers of wood, still grows faster than the other two species. Its growth is fast even during the dry season despite a strong decrease in carbon fixation, so under this scenario they may give priority to growth over storage of starch to fuel future respiration and defense. *O. leucoxylon* does not store much starch and seems to invest almost all the carbon that they fix in growth and metabolism.

*L 403-404: Is there evidence for this from another study? Include citation.*

We moved this part of the discussion to the subsection 4.4 and we now include references and the supportive figure S3 (lines 590 -594). .

*L 404-406: It is important to note that the conceptual framework is not perfectly supported by the results (see overall comments).*

Yes, we rephrased to clarify this (lines 492-495) .

*L 423-424: Which figure is this referring to?*

We did not include a specific figure, but we now include Figure S4 in the supplementary information where we compare the seasonal changes in starch for each species and we compare species using non parametric 95% confidence intervals for the mean. Here it is shown that *D. microcarpa* has the largest relative changes in starch content during the months May19 to Aug19 than the other species.

*L 424: Delete “so far”.*

Done.

*L 429-431: But figure 3 suggests that the parenchyma-storing species S. guianensis had the highest starch content. Perhaps I am not understanding something?*

The reviewer is correct, Figure 3 (now Figure 4) suggests that *S. guianensis* has the highest starch content. Nevertheless, we wanted to point out that per unit of wood volume, fiber storing species have greater storage capacity. We rephrased these statements to make this point clearer (lines 524 -526). We also included a new Figure 3 where we show the radial distribution of starch in the stem wood. In this figure it can be seen that *D. microcarpa* has larger storage capacity per unit of wood volume, but *S. guianensis* stores more starch deeper into the stem.

*L 439: After storage strategies add “and respiratory demands”.*

Thank you, we included the suggested modification (line 534).

*L 445: Not be able to? Or not need to? Might they just not need to regulate their growth rates because they have lower respiratory demands?*

This is a good point. We rephrase this part of the discussion to be more objective and clear about our findings lines (530-539). Nevertheless, we may expect that those trees would face a negative carbon balance during the dry or the wet season and we did not detect significant reductions in any carbon sink for the two parenchyma-storing species, although we see a small reduction in growth during the dry season, which indicated that these species may risk running out of reserves before reducing carbon sink fluxes. We agree with the reviewer that we cannot establish here if this is due to a lack of capacity to regulate carbon sinks or simply to a lower necessity of regulation.

*L 468-471: This is a really interesting conclusion from the study and should be highlighted.*

Thank you, we highlighted this idea by modifying the entire paragraph and mentioning it earlier in the discussion.

*L 480-483: Is there evidence for this from other studies?*

There are several studies that point out that consumption of starch is allocated to metabolic needs and when the carbon balance is negative the stocks of starch in the stem wood decrease. We cite studies providing support for this in the introduction.

*L 492: Change “in” to “on”.*

We rephrase this part of the text.

*L 504: Change “die” to “death”.*

We believe die is the right word.

*L 505: Remove “a” from “in a previous work”.*

We rephrase this entire paragraph (lines 577 -586).

*L 526: Figure citation?*

We rephrase this section and included all the figures in the citation.

*L 535: Clarify what is meant by higher carbon supply. Is this higher rates of photosynthesis? Or differences in carbon supply related to leaf habit*?

We changed carbon supply to carbon acquisition in the text. We think this term may be more accurate as it can result either from higher photosynthetic rates or from more presence of mature leaves throughout the year.

Comments from reviewer 2:

Overview:

*This paper seems to describe a follow-on study to an earlier paper, and describes growth rates, starch concentrations, and wood respiration in three tree species in a tropical forest in Brazil. The study measures these quantities in different time periods across about 1 year. The study sets up a nice conceptual framework in Fig. 1 to describe how seasonal variation in these measured variables may tell us something about tradeoffs between storage and growth. The major results of the paper to me seem to be that one species that is semi-deciduous and has living fibers stores quite a bit more starch and this starch pool varies more during the single year of the study.*

Major comments

1. *Overall the paper is a little difficult to follow. There are errors, incomplete description of statistics, and unclear or incompletely described figures. For example, a number of the results seem free of statistical support (e.g. Fig. 6). Description of statistical methods is incomplete (e.g. no mention of normality assumptions for ANOVA). Reporting is also somewhat informal, with a few marginal relationships reported and discussed as significant. It’s also not always clear where statistical results are coming from or what figure they refer to.*

We appreciate the comments of the reviewer and apologize for the lack of rigor in reporting our statistical results. We have done many changes throughout the manuscript to address the issues raised on this point by all reviewers. We hope now the paper is easier to read and clearer about the aims and findings of our study. We made the description of statistical analyses clearer in the methods section and we mentioned all the assumptions made when evaluating statistical differences between data groups. We improved our writing and mentioned the marginal relationships where they existed and identified the figures that show these results correctly in the text.

*2) I suggest to note traits related to both leaf habit and wood anatomy on figures. This is done a little already (e.g. wood anatomy on fig. 6, despite that the difference seems to be among leaf habits). It would make the paper a much easier read. There are also undescribed traits related to reproduction, which are noted in purple on the conceptual diagram but only tangentially discussed.*

We added the association of the species to the traits in each figure as suggested. We described better the traits related to reproduction in the figure caption and in the introductory text (line 137). They are only tangentially discussed because, while we have information on the phenology of the fruits and flowers, we do not have actual data on the carbon being allocated to these sinks. We thus have an indication that reproduction is important, and quantifying allocation to this sink should be considered in future research.

*3) Sugars were not measured in the study. The authors describe this later, but sugars are the major pool in lots of species so this is confusing and the justification seems a little underpowered to me. The title is about starch, but the abstract then goes on to describe difference in “NSC” in point 4, despite that only starch was measured. This is somewhat minor but should be revised.*

Sugars were measured in a preliminary study in 2018. We now describe better when they were measured and why they were not measured in 2019 (lines 241-259). We found that soluble sugar concentrations are very low and did not change seasonally. For our three species starch is the larger NSC pool, therefore we focused on starch dynamics. We made this point clear in the methods and discussion. We also changed NSC in many instances of the manuscript to only refer to starch, because it was what we measured and we also do the clarification in the abstract that starch is the major component of NSC for these species.

*4) Section 3.3 of the results was largely unintelligible during my read through. Perhaps the text was just unclear, but it appears results given are either without statistical support (changes in starch amount between seasons), refer to specific months (December) while the figure 7 illustrates time periods (Nov-Feb), or refer to patterns not shown on the figure (I think!). The horizontal labels on the figure are also upside down and backwards. Please see line comments for examples. I gave up after a bit. I think perhaps one issue is the figure shows starch change, but it is not clear what the comparison timeline is (i.e. change from when); this is not explained in the caption. Also in the caption starch not “scratch”. Also on the figure, what does “(cm, after)” mean? Again not described.*

We largely modified section 3.3 in order to make it clearer and more readable, lines 381- 421. We adjusted the text to be clearer about what periods we were comparing and to be more consistent with our definitions of the seasons evaluated. We also made several changes to figure 7 to make it clearer and we specified in the panel labels what periods of starch change and growth are being compared. We also modified the figure caption 7 to include all information necessary to understand the figure and relate effectively to what is being reported in the text. We really appreciate pointing out all these details that we did not see before.

*5) The discussion makes a lot of unsupported claims about mortality risk. It also relies a lot on a number of weakly supported statistical results with marginal p-values and low sample size. As such it was difficult for me to really evaluate how much stock to put in the various discussion points.*

We agree that we cannot directly relate mortality with our findings. Although our results suggest that a link may exist between mortality risk for individuals of a species and their strategies for storing starch, our findings provide us an idea about the potential mechanism behind this relationship. We revised our text to be more precise about this difference.

*6) Why not just measure starch concentrations? The authors present a novel (and neat!) visual approach to quantifying starch in tree rings, but there is no justification of why the authors chose to use this method as opposed to established concentration measurements. Some reporting of uncertainty in this method, and any bias as compared to established methods would be nice. Some of this information may be described in the previous paper, but it also seems to be in the supplement of that paper, and I am only willing to go so far down the rabbit hole.*

We used the histological approach to quantify starch because it offers more detailed information about the distribution and dynamics of starch in the stem wood. It also gives more precise information about how much and how deep trees can store starch, and where it is stored, which is a central part of our study. Differences in the cells used to store starch lead to differences in carbon dynamics and may help to explain differences in mortality. Unfortunately, we still do not have standards to evaluate the uncertainty of our histological measurements, but in a previous paper we calibrated these measurements using established methods to measure soluble sugars and starch concentrations. We added some more information about this validation of the histological method to give more confidence in our measurements.

Minor comments.

*L23. I would say that stemwood NSC is an outcome of carbon source-sink balance rather than influencing it?*

This is true if we consider storage solely as a passive process, but as shown here and in other studies, NSC storage and use can also be partially an active process regulated by gene expression.

*L25. Why starch only?*

Because starch represented more than 70% of the NSC in all three species and they showed significant seasonal variability compared to the less abundant soluble sugars. We now made it clear in the abstract (line 26) and in the main text () why we decided to focus only on starch, lines (lines 252 -259).

*L33. Implies parenchyma storage is the main trait rather than deciduousness*

Yes, we expected the strategy of storing starch in the stem wood to have more impact on the NSC dynamics of the stem wood than leaf habit. This is based on our previously reported results that showed little difference in NSC dynamics with leaf habit (see response to Reviewer 1 also).

*L44. “Starch storage strategy” seems to imply that starch concentrations are the optimization target, when in fact starch patterns may simply emerge from the other traits?*

We did not want to mean that starch concentrations were the optimisation target. Starch storage strategies do not refer to the concentration of starch in the stem wood but rather to where trees are able to store starch either in the parenchyma cells alone or in the living fibers as well. We think this is clear in the main text now. We added a figure in the supplementary information (Fig. S1) where we show the starch in the wood tissue and it can be observed if the starch is stored in living fibers or parenchyma.

*L46. What about “more seasonally dynamic starch concentrations”?*

We incorporated the suggested change (line 48).

*L49. But you have no mortality data?*

We have background mortality data from inventories taken every two years during the last 8 years.

*L58. Richardson 2013 shows sugars are of similar age to starch, suggesting sugars also “locally accumulate”.*

We agree that soluble sugars can also be locally accumulated. Nevertheless, they are osmotically active, which limits their long term accumulation. Starches are not osmotically active and therefore they are a more efficient way to accumulate sugars in the long run. The similarity in ages reported by Richardson et al 2013 can also indicate a rapid interchange between starch and soluble sugars, or a high contribution of starch to the soluble carbon extracted for 14C analysis.

*L69. What about phloem, which may accumulate for many years, and has concentrations typically 5x to an order of magnitude greater than sapwood?*

This is a good point. We modified the text to add more clarity and acknowledge the role of the phloem in the NSC dynamics in the stem-wood (lines 70-73).

*L172. Reproduction is included in Fig. 1, could the authors provide reproductive traits in this table 1 as well? For example, I see one or more of these species produces large fruit, potentially representing a large sink?*

*In general, some more description of the species would be very helpful to readers like myself unfamiliar with these species. A one sentence definition of a living fiber would be also useful.*

*-Also on table 1. Where are growth and mortality rates from? Please explain in caption. Is %leaf loss annual? Or?*

We included a more detailed description of the species in this section of the methods (lines 186-192). We added a short definition of living fibers in the introduction (lines 78-79). The reproductive fluxes included in Figure 1 were inferred from the phenology of fruits and flowers reported in Fig. S3. At the moment we do not have more reproductive traits that help us to constrain these carbon fluxes. While reproduction seems to have an important role in the carbon dynamics of these trees we did not quantify it specifically and it was not the central scope of our study.

*L176. What about heartwood? Was it sampled? Omitted? Identified?*

We did not differentiate between heartwood or sapwood, we sampled as deep as we observed starch. In the species where heartwood was identifiable, starch always was completely absent before the heartwood/sapwood transition. Now we included a new figure (Fig. 3) with the radial distribution of starch where the observed starch concentrations decrease with depth until reaching zero, and this limits changes between seasons.

*L185. Please justify why not just directly measure concentrations?*

Histological methods give more detailed information about starch storage, for instance it shows where trees store the starch (which we refer to as starch storage strategy) and the distribution of starch in the radial profile, allowing us to be more precise in the estimation of the starch mass and temporal changes. We also wanted to evaluate if we identify a pattern in the mobilization of starch between seasons which is not possible with the standard (extraction) methods. For instance, we saw in some cases a complete seasonal remobilization of starch from the deeper layers of wood. We include this information now in the manuscript (lines 331-335).

*L205. In same species? Also please report the uncertainty here as compared to a concentration measurement.*

Yes, the comparisons were done in the same species and even in the same trees. We included in the text the R2=0.80 of the regression line that compares both methods.

*L219. Any justification for no temporal overlap in sampling of sugars and starch?*

During 2018 we conducted a preliminary study where we measured soluble sugars and starch only in the dry season (July 2018) and in the wet season month (January 2018). From this preliminary study we realized that soluble sugars had very low concentrations (compared to starch) in these species and they did not vary seasonally (Fig. S2).

*L220. Please describe respiration halting step and timing (i.e. freezing or microwaving, time from collection to processing, etc.).*

We kept the wood cores frozen until drying them at 60˚C in an oven for two days. We state this now in the text (lines 204-206).

*L229. Please state the method only quantifies GFS sugars and total sugars could be higher.*

Yes, we made it clearer sooner that only GFS were measured (line 241).

*L227. Could report long term coefficient of variation on repeat sample measurements as a metric of lab precision.*

Yes, our coefficient of variation of the mean concentration of starch in repeated samples using the histological technique was very low (0.2). Alternatively, in our calibration tests, where we compared the histological measurements of starch with the ones made by the established chemical methods, we used standards with known starch concentrations and blanks. The measured concentrations did not differ from the expected concentrations. Our previous paper Herrera-Ramirez et al. 2021 showed a significant correlation with an r2=0.8 and a slope =0.88, which give us enough reasons to trust the measurements done with the histologic techniques.

*L242. What is the purpose of this measurement ?*

We are interested in understanding how NSC contributes to balance the carbon sources and sinks activities. As shown in our conceptual framework respiration is a key carbon sink which is bigger in fiber storing species and therefore important to understand its magnitude and temporal variability for each species.

*L248. What does “volumetrically” mean here?’*

We removed the word “volumetrically” from the text.

*L264. I am not really sure what this means. Just comparing means? Or a regression?*

We distributed the data analysis in the methods section. We mention right after describing the data acquisition what statistical methods we used to compare the data. We hope this makes the statistical approach more easy to understand.

The comparison between starch mass and climatic variables was eliminated because it was not adding important information to the story.

*L272. Maybe move to methods. And, how does this compare to average starch concentrations (starch figures have different units than %)?*

Done, moved to methods (lines 253-259). Now in Figure 3 we provide the percentage of starch at each wood depth. More info can be found in Herrera Ramirez et al., 2021.

*Fig. 23 Could the author note traits on the figures? So rather than, or additional to species name, add “SD” (semideciduous) or “EG” (evergreen) and “par” (parenchyma) or “fib”? or similar? Also note statistical test used in caption?*

We added the traits in the figures labels and captions.

*L277. Unless differences are directly compared between species statistically I do not think this is a valid statement, that is, it appears authors are just comparing p-values which do not measure ‘strength’ of changes.*

We agree with the reviewer, we were using the wrong statistical evidence to back this statement. We now provide a comparison of the differences between species in the starch changes between months in Fig S4. The differences between seasonal change and between species were assessed with non-paramentric confidence intervals and are also shown in Fig S4. We now used Fig S4 and the seasonal changes in starch mass to back up our original statement.

*L279. But for S. guianensis lowest appears to be May19*

In Figure 4, for *S. guianensis*, the median seems to be slightly lower than August 19, but the mean is actually lower in August 19. Nevertheless the differences are not statistically significant.

*Fig. 3 why different line colors? Not described in caption.*

We modified the figure to make it more informative and we now included in the captions the information about the color differences.

*L281. What is the statistical support for this statement and test used?*

We now provide the adjusted p value for the multiple comparison between months with a Wilcoxon signed-rank test.

*L285. What was the post-hoc test. Not described.*

We now describe in the Methods section the test used to compare between groups and species. We also include the comparisons in the figures and describe the results in the figures caption.

*L313. There are no panel labels/letters. (Nor on other figures)*

We decided to eliminate this figure as its contribution to the main story was very marginal.

*L317. Any statistics here or just pattern description?*

Originally it was only a pattern description, nevertheless we now eliminated that figure from the text.

*L326. Please label subpanels with letters and refer to them. There is no “December 2019” on the figure (which is also upside down/reversed).*

We apologize for the bad presentation of the figure. We have made several changes to Figure 7 to increase clarity. We also rewrote the text to be consistent with the nomenclature used in the figure 7.

*L326-328. Not really results (seems discussion-y)? Or rephrase? Also, there is no evidence for this statement?*

True, as it was written there was no evidence for the statement. We rephrased it and referenced the figure that supports our claim, lines 396-397.

*L329. Why June? May is on the figure? Also please clarify the comparison time point for starch change, last month or last 3 month period?*

We rewrote the text to be consistent with figure 7. Also we did some edits to figure 7 to make it clearer what comparisons are being shown.

*L336. I think the authors are reporting the p-value from the Aug19-Nov19 period but stating it as if it refers to December 19 -Feb 20? Which again, December is not shown on the figure.*

Yes, we now corrected our wording to be more consistent (lines 393-409).

*L341. How can we tell it accumulated? Is this shown? I don’t think so. And, no statistical support.*

We thank the reviewer for letting us know this point. Now we provide evidence of significant accumulation of starch across seasons in Figure S4. We also rephrase this statement acknowledging the lack of statistical significance for the expected accumulation of starch in some seasons (Lines 387-392).

*L351. Again, p-value does not indicate effect strength I think. That could be estimated by calculating an effect size or looking at the actual parameter estimate of the slopes (i.e. steeper for one). I think the authors mean to say there is greater evidence for a significant relationship between starch and growth. (I might call this weak evidence): Muff, S., Nilsen, E. B., O’Hara, R. B., & Nater, C. R. (2022). Rewriting results sections in the language of evidence. Trends in ecology & evolution, 37(3), 203-210.*

The reviewer is right, we made this mistake. We now have rephrased the sentence as the reviewer suggested to be more consistent with the evidence. Thanks for the recommended reference .

*L356. Not significant.*

We consider this p value as marginally significant and now we state it clearly in the text.

*L358. Suggest perhaps leading off the paragraph with this one relationship that is significant and also substantially explanatory?*

We gave more relevance to the relationship between annual growth and starch mass during the dry season for *S. guianensis* in the text. Nevertheless, we kept the structure of reporting first the results about the fiber storing species and then the parenchyma storing species for consistency along the Results section.

*L403. “some months of delay” ->  I saw no evidence for this?*

We apologize for the lack of clarity in our original Figure 7. Now we added labels to clearly indicate what are the time lags being considered, as well as a proper description in the captions that we hope makes this point clear.

*L404. Providing some reproductive traits could help make this point*

Yes, we provide the phenological behavior of fruits and flowers in Fig S3.

*L423. Given statistical evidence instead of “we show” I would encourage “we observe some support for our idea that”*

Thank you, totally agree, we rephrase the entire paragraph to be more clear and cautious with our interpretations, lines 512.

*L432. Unless their demands scale with supply, which is another interpretation of these results.*

Thanks, we rewrote this part of the discussion to be more clear about our interpretation of the results. Here we try to point out that starch storage scales up with the demands of carbon to be able to cover metabolism in case of low carbon supply (lines 512-527 )

*L444. Or, latewood?*

Yes, a reduction in the size of the cells towards the end of the tree ring is latewood. Nevertheless, we have eliminated this part of the discussion and wrote something more specific (lines 530-539).

*L449. Again, not really a reduction in growth if cells are lignifiying?*

Even if carbon investment does not change between latewood and earlywood cells, generally latewood cells take longer time to be formed and this results in slower growth rates (Silvestro et al 2023). As we mentioned before, we thought this was distracting and eliminated this part from the text.

Silvestro, R., Zeng, Q., Buttò, V. *et al.* A longer wood growing season does not lead to higher carbon sequestration. *Sci Rep* 13, 4059 (2023). https://doi.org/10.1038/s41598-023-31336-x

*L454-456. This is an example of a number of similar claims in the discussion for which I see no support.*

We recognise our results do not give strong support for a relationship between mortality and carbon dynamics and more specific experiments should be done in order to test the hypothesis that we state here. We have rewritten most of these instances to differentiate more clearly between our interpretations of the results and the related hypothesis linking our results and the mortality data.

*L466. Seems like starch depth should be reported somewhere? Did this differ between species? Is that what is being implied here?*

We added the starch concentration radial profiles for each species at each sampling date in Figure 3. Nevertheless, here we wanted to point out that fiber storing species have a larger proportion of living cells per unit of wood volume and therefore their wood respiration rates are higher.

*L505. But was any relevant damage observed on the focal study trees?*

We did not observe heavy damage to any of our trees while we were conducting the study. Nevertheless, damages due to strong winds and blowdowns are very common in the forest evaluated.

*L533. A good point.*

Thank you!