Response to Referees Comments (RCs)

Ingrid Chanca on behalf of co-authors

Referee #1 – Karis McFarlane

We want to thank Karis McFarlane for the insightful remarks and suggestions that helped to improve this manuscript. The responses and changes to each query are detailed below in blue and italics. Additionally, the changes are highlighted in the pdf attached.

In this paper, the authors present 14C values for ecosystem respiration in a tropical rainforest in the Amazon, which they derive from a series of 14C measurements of CO2 from 2 field campaigns with samples collected at multiple heights above the ground surface. They estimate the 14C and 13C end-members of ecosystem respiration using both the Keeling Plot and Miller-Trans approach and compare the results from these two approaches to one another. They further compare their 14C values for ecosystem respiration to atmospheric values to derive mean transit times and compare these mean transit times to similar variables (mean turnover time) from the literature determined using different approaches. The approach used in this paper is sound and the data novel and interesting. The manuscript itself could use some additional editing prior to publication. Suggestions for a revised manuscript are outlined below with specific locations of these issues marked in the manuscript pdf with highlight or strikethrough.

In some cases, it seems like things are misplaced or not consistently called out in the appropriate places. It reads as if perhaps it was first written as a short-format paper with the Methods last so some things that should be in the Methods are instead in the Results and Discussion.

Response: Thank you very much for this and the further comments. We thoroughly revised those sections to increase readability. Specific changes are described below and highlighted in the pdf attached.

At the end of the introduction, you state that you discuss your results in comparison to model predictions. How interesting – why not include this in the Abstract? The section of the Results that describes this comparison needs some reworking. It is quite confusing and is not followed up with sufficient discussion of what the reader should take away from these comparisons.

Response: Thank you! We have included the comparison to model-based estimates in the Abstract, as suggested. Additionally, we reworked the Results and Discussion sections. As there were several changes in the order of paragraphs and wording, the detailed changes are better followed in the pdf attached.

The use of "14C/C" is confusing, since we do not discuss 14C/C data and 14C is such a small fraction of total C (1 out of 1 trillion C atoms!) You're also reporting Delta values, which are an isotopic ratio not 14C/C ratios. Either use Delta 14C values or 14C isotopic signatures or something similar throughout.

Response: Thank you for the comment. We've now edited the text to refer to 14C isotopic signatures (and variants such as 14C content) throughout the manuscript consistently.

I found the methods section a bit awkward in that the introduction to the Keeling and Miller-Trans methods came first, but then there are not very clear connections to how the data you collected are used in these approaches (which would justify explaining how the approaches work before discussing how you collected samples and generated the data). Consider describing the study site and field approaches first, then analytical methods, then introduce the approaches for deriving the end-member 14C and 13C for ecosystem respiration. You might add more in-text citations to the Miller-Trans 2003 paper (not just the Phillips et al paper). It's a little awkward that you don't cite the older papers when discussing the approach in the methods.

Response: Thank you, we agree that the suggested order improves the reading. Following the suggestion, the subsections of the Materials and Methods now read as: 2.1. Study site; 2.2. Sampling; 2.3. Analytical methods and data analyses; 2.4. End-member mixing analysis; 2.5. Conversion to mean transit time and reference atmospheric radiocarbon; 2.6. Comparison with other approaches. We also added the citations as suggested. The reordering of the subsections implied also changes in some paragraphs. All these changes are highlighted in the pdf attached to avoid a lengthy response here.

It is not clear what exact approach was used to solve for the 14C of ER. There are different ways to fit linear regressions, which one did you use in R? e.g., Did you use lmodel2 or lm() or something else? See Pataki et al 2003 methods section for discussion and recommendation on dealing with minimizing both x and y errors (model II regression). It's cited elsewhere in the paper. Whatever you used, this should be in the methods. How are the reported errors, 95% confidence intervals, and ranges derived? This should also be in the methods. It was not clear until one looks at Figure 3 that the Keeling plot method was applied across the 4 tower heights to achieve the range in CO2 concentration. This should be clear from the methods as the Keeling plot approach can also be applied over time at night when CO2 from respiration accumulates. The height-based approach is indeed more sensitive to varying backgrounds across the 4 heights – this would be easier to follow if it were clear that this how the Keeling plot approach is applied here. Also, it is not clear from the methods that the two approaches are used for 13C also until you see the results. This should be clarified in the methods.

Response: Thank you for the comment. We cited Pataki et al. (2003) for the height of reference background (~ 2x canopy level). Indeed, the technique for obtaining the slope and intercepts from the linear regressions was not stated in the Methods, but in the Results. We used ordinary least squares as recommended by Zobitz et al. (2006) after reanalysis of Pataki et al. (2003), with the argument that the Model II regression can lead to biased isotopic values for the source of the end-member mixing analysis. The biases on the end values (to justify using Model II instead Model I regression) are particularly important when CO2 differences are not large enough (about 75ppm according to Pataki et al. (2003)). Given we have much larger CO2 differences in our dataset, we opted for the Model I regression and refer to Zobitz et al. (2006)'s paper. We added it in the Methods alongside the clarification that we used all heights and all times of the day in each approach. We also added how we report the errors and ranges. It reads now:

"Isotopic carbon signature of ecosystem respiration ($\delta^{13}C_{ER}$ and $\Delta^{14}C_{ER}$) were estimated with both Keeling and Miller-Tans approaches. Both end-member mixing models considered all the heights below and above the canopy, i.e. 4, 24, 79 and 321 m and were not separated according to time of day. The results of the analyses were estimated by linear regressions fit with ordinary least squares (Model I regression) (Zobitz et al., 2006). We report the mean values with one standard error (σ) of the intercept obtained by the regressions in the Keeling approach and of the slope of the regression in the Miller-Tans approach. In both cases, we also report the 95% confidence interval (CI, ranging between percentiles 2.5 and 97.5)."

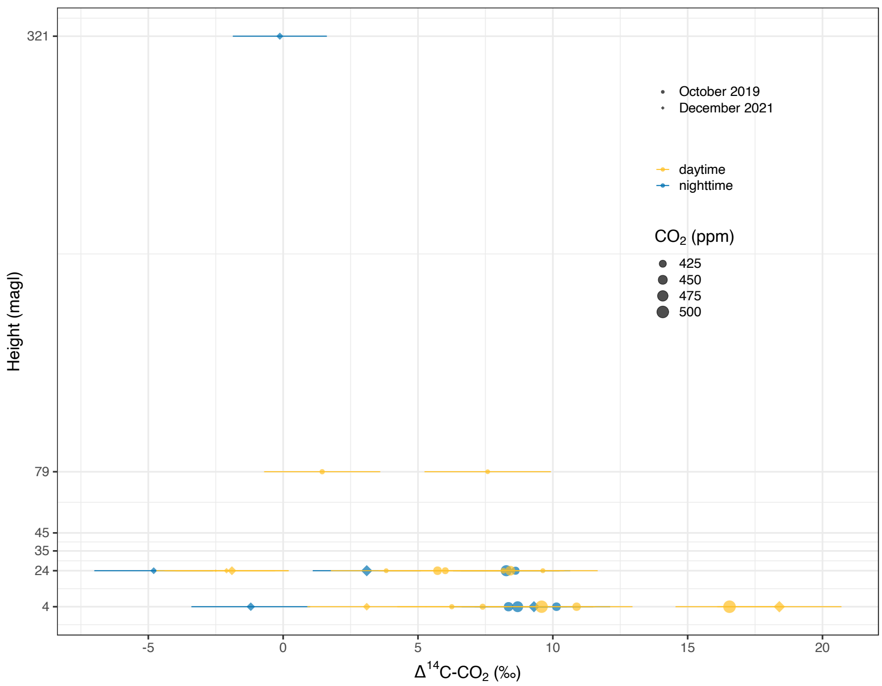
How were Delta 14C-ER values converted to mean transit time? The abstract and methods (L206) and results (L273) are vague, please be more specific.

Response: Thank you for the comment. The conversion was described in the last paragraph of the previous section 2.1 'End-member mixing analysis' (which is now section 2.4, following the above recommendation). This paragraph was moved to section 2.5 (now called 'Conversion to mean transit time and reference atmospheric radiocarbon'). It reads:

"To estimate the time between C assimilation and release from the ecosystem (mean transit time), the $\Delta^{14}C_{ER}$ obtained from the intercept of the Keeling plot and slope of the Miller-Tans plot was compared to the subset of the CORSO data described above. The difference between the year of collection of the samples and the equivalent calendar years where $\Delta^{14}C_{ER}$ = at- mospheric $\Delta^{14}C$-CO_2, translates into an estimate of mean transit time in units of years (yr). When $\Delta^{14}C_{ER}$ is not equal to atmospheric $\Delta^{14}C$-CO_2 of a given year, the calendar year with the closest atmospheric $\Delta^{14}C$-CO_2 to $\Delta^{14}C_{ER}$ is taken. Estimates of mean transit time are based on the variability of the mean $\Delta^{14}C_{ER} \pm \sigma$ (standard error of the linear regression), with the 95% confidence interval of the mean reported within parentheses."

Figure 2 is not very helpful. Can you replot in a way that distinguishes between day and night? The text states that day and night values differed and that they differed between sampling dates, but it is not very easy to compare in this plot. If it can't be improved to highlight your results more effectively, move it to the supplement.

Response: Thank you for the comment. Given that we don't use the distinction between day and night to make the calculations and plots, we preferred to leave the figure as it currently is to avoid confusion. But we had a figure in the supporting material that has the distinction between day and night. We, therefore, decided to update it to include the Delta14C values, so the differences can be easily observed also by height (see below). We refer now to the figure in the supporting material in this part.



Section 3.3 is difficult to follow. The header should be something more like "Estimates of mean transit time and comparison to other values from the literature". I am not convinced this should be in the results, especially considering there is no explanation of how the 14C data were used with the atmospheric data to determine the mean ages or meant transit times. Was this just comparing the end-member values to the atmospheric data and matching them to years in the atmospheric data set or did you use a one pool model? I do not understand where the ranges reported here for the Miller-Trans approach come from. The reported ranges in the text and those in the table are not the same – but appear to be from this study. The paragraph does not describe the results from the Keeling plot approach, but they are in the table. These results are reported as single values in the section 3.2 then as very large ranges in 3.3. All of this needs to be better explained. I do not understand why there are decimal places for everything when the ranges and uncertainties are magnitudes higher. It makes this section more difficult to read and conveys a false level of precision and accuracy. This section needs to be rewritten. I think the comparison to the other methods is useful, but perhaps in the discussion is better. Table 1 also needs footnotes or other clarification of where these values came from (with the references in the table).

Response: Thank you! We revised this section and corrected the reporting of ranges that was confusing because we reported mean value plus-minus standard error and the 0.95 confidence interval. Additionally, the Methods section was revised, so the information on how we do the calculations and how we report the results are more clearly described there.

There are numerous sentences and phrases that are awkward and could benefit from editing. In some cases, these are word choice, in others they are long sentences that could be easier to read if split into multiple statements. There are several single sentence paragraphs – these need to be restructured into cohesive paragraphs. Some of these are highlighted in the PDF preprint. I have also marked unnecessary text with a strikethrough in the PDF preprint. Specifics are below:

L15: what do you mean by "combining"? Please be more specific. If you are up against the word count, you can reduce the text in the lines above reporting the Keeling plot and Miller-Trans methods – they're close enough you can provide ranges. And no need to provide decimal places when the errors are greater than 1.

Response: Thank you for this comment. We meant that the radiocarbon isotopic signature of ecosystem respiration, estimated by end-member mixing models, was <u>compared</u> to the radiocarbon signatures of atmospheric CO2 of previous years. We changed this in the Abstract and rephrased the reporting of Delta14CER estimates as suggested.

L16-19: The final sentence of the Abstract is problematic. The relatively young mean transit time of ecosystem respiration does not suggest anything about the size of the fraction that is assimilated for decades or longer. Arguably the fact that respired C is so young is a good sign that these systems are not losing older C. Revise this. I understand why the authors might not want to conclude that the presumed increase in mean transit time in 2019 from 6 years to 18 years in 2019 suggests this forest is losing older C but it seems that a stronger statement could be made about what the observed variation might mean and the possible implications should this be a real trend. At minimum it points to something we might want to watch as these forests become increasingly impacted by climate change.

Response: Thank you for the comment. While we agree that our results alone do not provide an estimate of the size of the fraction that is assimilated for decades or longer, previous studies in the *central Amazon (e.g. Chambers et al., 2004) indicated that about 70% of the assimilated C is respired back to the atmosphere as autotrophic respiration. Therefore, we argue that only a fraction of the assimilated C can be stored in those forests. We added this in the Abstract. It reads:*

"We discuss these results in the context of previous model-based estimates of mean transit time for tropical forests and the Amazon region. In addition, we discuss previous studies that indicate that approximately 70% of assimilated carbon is respired as autotrophic respiration in the central Amazon. Our results suggest that newly fixed carbon in this terra-firme tropical forest is respired within one to two decades, implying that only a fraction of assimilated C can act as a sink for decades or longer."

L29: "in land photosynthesis" is awkward phrasing. Consider "among terrestrial ecosystems" or something else.

Response: Thank you for the comment. We changed it to "terrestrial ecosystems".

L30: "might compensate most" is missing a word but I suggest more specific, if not quantitative, language here.

Response: Thank you for the comment. We modified this sentence and it now reads:

"Although the rates of C uptake in Amazon forests are among the largest in terrestrial ecosystems (Malhi et al., 1999), C losses through respiration are also very high and autotrophic respiration is estimated in around two-thirds of assimilated C in the central Amazon, compensating most of the C uptake (Chambers et al., 2004; Sierra et al., 2007; Malhi et al., 2011; Chambers et al., 2013)."

L49-50: This is very awkward and confusing because you've not yet explained that the 14C in the atmosphere is oxidized to CO2 – you might specify this in the previous sentence.

Response: Thank you for the comment. We edited this paragraph and it now reads:

"Radiocarbon (^{14}C) can be used as a tracer of C dynamics in ecosystems and to track how C moves across different ecosystem C pools. Measurements of radiocarbon in respiration can also be used to quantify the transit time of C through ecosystems (Trumbore and De Camargo, 2009). Radiocarbon is produced naturally in the upper atmosphere by the interaction of thermal neutrons from cosmic rays with ^{14}N in the atmosphere. Additionally, nuclear weapon tests in the atmosphere during the late 1950s and early 1960s produced a large number of thermal neutrons that led to the production of excess ^{14}C. After natural and anthropogenic production, ^{14}C is oxidized into CO_2. After the Limited Test Ban Treaty in 1963, the concentration of $^{14}CO_2$ in the atmosphere started to decline due to its incorporation in the biosphere and surface ocean (Levin et al., 2022). Atmospheric CO_2 containing amounts of ^{14}C that change over time since the 1960s is assimilated by terrestrial ecosystems in the same manner as natural isotopes of C."

L-53-54: Revise this it's very awkwardly written. The first part of the sentence is about respiration but the end is about organic matter ages. Maybe the age of the organic matter is several years should come first.

Response: Thank you for the comment. We changed the order of the sentences, making the whole statement more coherent. Now it reads:

"For instance, C in freshly fixed plant metabolites (e.g. leaf sugars) will have the same ratio of ^{14}C content as the atmosphere at the time they were assimilated. Yet ^{14}C respired from organic matter decomposition would reflect the age of C used to grow plant tissue plus the time it takes for decomposition, leading to C ages of respiration from organic matter generally higher than one year. CO_2 respired by fast-cycling pools (e.g. canopy leaves) should have ^{14}C isotopic signature close to the contemporaneous atmospheric ^{14}C signal. Thus, the age of C in ecosystem respiration is a mix of ages of C respired from different compartments with distinct isotopic signatures and integrates the timescales of different processes such as production, allocation, and decomposition (Trumbore and De Camargo, 2009; Chanca et al., 2022)."

L55: integrates rather than reflects seems like a better word choice.

Response: Thank you. The suggestion was incorporated.

L58: Do not use 14C/C – you are not deriving the 14C/C ratio you are deriving the isotopic ratio and they are not the same thing.

Response: Thank you, you are right. We changed it to "14C isotopic ratio".

L68: use "and" instead of "/"

Response: Thank you for the comment. We changed it from "/" to "and".

L116: I do not understand what "also counts with two more towers" means.

Response: Good point. I meant that there are in total three towers in the ATTO site, two more towers alongside the 80-m walk-up tower described in the previous sentence. This sentence was changed to: "In addition, the site includes two other towers: (...)"

L135: flasks come up here but not earlier – when would flasks from the tall tower not be available? Perhaps it would be better to describe the tall tower data in the section above where the other towers are introduced? Otherwise rather than starting with the tall tower start with the lower altitude samples, then say you used the tall tower as the background (for Miller-Trans approach, I assume – you could specify that here too) and that when you needed to you filled in missing tall tower data with measurements from the 80m walk up tower.

Response: Thank you for the comment. Flasks from 321m at the tall tower started to be collected in September 2021, thus they were not available in 2019. This information was mentioned in another section of the manuscript. In the revised version we moved this information to the Methods. We also incorporated the suggestions in the order of paragraphs and mentioned the Miller-Tans method here to explain the need for the higher altitude samples. Now it reads:

"Forest air samples were collected from two heights within the canopy at 4 and 24 m agl, in two campaigns during the dry and transition of dry-to-wet seasons. The first campaign took place in October 2019, and the second campaign in December 2021. In both campaigns, a few samples were collected from the top level of the 80 m walk-up tower (79 m agl) to be used as a reference of the above-canopy air for the Miller-Tans plots, which consists of an approach where the values ($\Delta^{14}C$-CO_2, CO_2 concentrations) observed within the canopy are plotted after subtraction of the values

observed in the tropospheric background (Miller and Tans, 2003). The canopy level at the study plot is around 35 m high, making the 79 m level reasonably appropriate as a background (Pataki et al., 2003). At the ATTO tall tower, since September 2021, air samples have been collected into flasks from 321 m agl. Additionally, since February 2019, one-month-integrated samples have been collected by absorption of CO_2 in NaOH solution for radiocarbon analysis at 321 m through the method detailed by Levin et al. (1980)."

L153: Clarify why then you bothered to remove the water or omit the statement before that you don't need to remove the water – or both.

Response: Thank you for elucidating the confusion on this point. Even though the eventual water vapour in the air of the flasks does not interfere with the analytical results, it can interfere with the collection of the samples and damage the sampler because of water condensation on the pieces of the sampler. During the dry season that is not critical, however, the second campaign happened in the dry-to-wet season and water drops can even be seen in the flowmeter and connections. Thus, we added the water trap to avoid excessive water condensation. We modified the paragraph to:

"Additionally, a drying agent can be attached to the system; the drying agent is particularly relevant when one is interested in the $\delta^{18}O$-CO_2 (Steur et al., 2023), which was not our case. Nevertheless, for the campaign in 2021, when the air relative humidity is higher (dry-to-wet season) we decided to use anhydrous magnesium-perchlorate inside a cartridge before the flask to trap the water vapour from the air and avoid interferences on the airflow and eventual damage to the sampler due to water condensation on pieces of the equipment."

L185: just "graphite" the underscore and italics are unnecessary and make it harder to read.

Response: Thank you, we've made this change.

L189: It is difficult to tell for sure if all of the 14C data were corrected for 13C measured on the AMS or if this only happened at MPI. It is also confusing that 13C measurements via IRMS are described between two paragraphs describing 14C measurements. Can you reorganize this section and make this more clear?

Response: Thank you, we agree. We reorganized this part and made it more clear that the AMS delta13C was measured both at CEZA and MPI-BGC. It now reads:

"$\Delta^{14}C$ from CO_2 in air samples collected in flasks was determined after cryogenic extraction of CO_2 in a vacuum line and conversion to graphite, which is the target of the Cs sputtering in the AMS both at CEZA and MPI-BGC. At the ICOS-CRL facility, CO_2 extraction is performed using a dedicated automated Extraction and Graphitization Line (EGL) (Lux, 2018). At MPI-BGC the extraction of CO_2 for radiocarbon analysis follows the same principles of EGL. ^{14}C-to-C ratios at both CEZA and MPI-BGC are corrected for mass-dependent fractionation by $\delta^{13}C$ measurements in the AMS and calibrated against oxalic acid standard material (Ox-II)."

L195: Yes, we are lacking in monitoring atmospheric 14C data for the tropics but I think it's better to state what you did and point out later (perhaps in the Discussion) if you think this lack of data is important for your interpretation of your results.

Response: Thank you, we agree. We modified the sentence to remove the previous statement and it now reads:

"As a reference for the atmospheric radiocarbon data in the study region, we used a compilation of recently reported data by the CORSO project, which includes time series of atmospheric radiocarbon data measured in research stations in the tropical region and surroundings."

L233-235: Cut, this type of statement belongs in the discussion, but probably not worth discussing. Be sure to put the previous sentence with the preceding paragraph.

Response: Thank you, the change was made as suggested.

L245-247 and elsewhere: it is a little odd to provide both F14C and Delta. You might explain why you do this in the methods and/or use one throughout but provide the other in parenthesis or in a data table. It's also not clear if you applied the Keeling plot to F14C and to Delta 14C or if you applied it to F14C and then converted the ecosystem respiration end members to Delta 14C using the year of sampling (2019 and 2021). This should be specified or just use one 14C notation throughout. Figure 3 shows F14C but the figure caption starts with Delta 14C. If you want to show the F14C figure, the caption should read: "Keeling plot of F14C…." Also here, no need to provide decimal places for the Delta values.

Response: We agree that it is not common to report both F14C and Delta14C in similar studies to ours, and it is particularly uncommon to calculate the radiocarbon isotopic signature through Miller-Tans (MT) and Keeling plots using the F14C values.

It is worth noticing that calculating the MT plot with F14C or Delta14C does not change the value of the slope. We have used Delta14C values for the regressions in Delta14C notation and F14C values for the regressions with F14C. We did not calculate in one notation and converted the slope value to the other notation. We did the conversion of the slopes from F14C to Delta14C to confirm that we obtain the same value as running the calculation directly with the Delta14C.

However, we agree that the way we reported the notations in the submitted manuscript might confuse readers. We decided to use the Delta14C notation in the main text, which makes it more familiar with previous studies in the literature, including Phillips et al. (2015), and we added a short appendix with the F14C plots and a short discussion on the physical understanding of the plot when using F14C. With that, we aim to keep the reader aware of this possibility, while also increasing readability and reducing the presentation of F14C values in the main text.

L252: It is odd to reference the small variations that have not been presented in the results. Also not clear what you mean by variation at 79 m – between dates? Cut this. I would rephrase that the delta 13C of ER are similar between the Keeling and Miller-Trans approaches, despite the explicit incorporation of background variation in the later method.

Response: Thank you for the comment. We modified it as suggested.

L255: "qualify as a violation" is a bit awkward. Perhaps you mean of the assumption of a stable background implicit in the Keeling plot method? I'd omit this statement here and save it for discussion, but you could rephrase that this suggests the small variations in CO2 and 13C at 79 m were small enough not to violate the assumption of a stable background made in the Keeling plot approach. Where are these results shown?

Response: Thank you. We modified the sentence and moved it to the Discussion. The variation of CO2 concentrations at 79m was already described in the Results. We added the missing information on the variation of delta13C at 79m.

L266: This and the next paragraph should come first seem to be about how you quantified the background for the Miller-Trans approach. This should come first in this section before the first paragraph that reports the results. There is also quite a lot of detail here that arguably could be in the methods or supplement.

Response: Thank you for the comment. We changed the order of the paragraphs, as suggested. Additionally, we moved details related to the sampling to the corresponding subsection in the 'Materials and Methods'. Note we changed the Figure too, as we are using the Delta14C notation in the main text and F14C in the appendix, following the earlier recommendation to focus on one notation throughout the manuscript.

L264: "we selected" sounds like you arbitrarily picked one. Better to say "we used" or you should explain how you selected this specifically and what implications this has vs choosing something else.

Response: Thank you! We changed it to "we used".

L287-268: This is unnecessary if all you did was convert Delta to back to Fraction. You must have rearranged Eq 4 to do so and so this is confusing as well as unnecessary.

Response: We agree with the comment. We removed this sentence.

L270: Why do you provide the dates here (days of month) but not elsewhere? I don't see why it is necessary.

Response: Thank you. We removed the specific dates and left only month and year as in the rest of the manuscript.

L275: Where did this large range come from? In L270-271 you report 32 permil for October 2019 from the Miller-Trans method. Same comment for December in the lines that follow. You can't even see the ribbon for the 95% confidence interval for October – how can it be so large? If these are the correct numbers they should be provided in the section above.

Response: That is an important point. Because the Delta14C has units permil, it is possible to see that the ribbon is indeed large. See the new figure in the attached pdf. Also, we reported the 95% CI in some parts of the text and mean values with standard error in other parts and Table 1. This made it hard to understand the ranges. We changed the text in different parts to make it more clear. You can follow the changes in the highlighted revised version of the manuscript.

L290: "We were able to obtain" sounds like someone gave them to you. Try "We estimated the mean transit time…"

Response: Thank you for the comment. We changed as suggested.

L299: Why isn't 2 years included in the range in Table 1? I see 4-24 in Table 1, 2-28 in the text in Section 3.3 and 2-30 here. Why aren't these values consistent across text, table, and sections?

Response: Thank you, that is a good point. In the text, we were reporting the mean transit time based on the 95% CI, while in the table we included the values based on the mean values and its standard error. We revised the text to show the 95% CI values in parentheses and described it in the Methods. Additionally, we modified the table, also showing the 95% CI within parentheses.

L301: Delete "may" – "suggests" already hedges your statement. Elsewhere you make the point that this may be variation rather than a [linear trend] change from 2019 to 2021 – I think that it is worth pointing that out directly here when you discuss that variation is observed.

Response: Thank you for the comment. We modified as suggested.

L305: Use difference rather than variation. With 2 data points it is difficult to call this "variation" you just know the 2 values differ. Do you mean variability in mean transit times or do you mean in 13C? This paragraph is mostly about 13C so it seems out of place. Perhaps discuss 13C first and then 14C together or in separate paragraphs.

Response: Thank you for the comment. We changed from "variation" to "difference". We meant the variability was in mean transit times. We edited this paragraph to make the connection more clear. Now it reads:

"These changes in $\delta^{13}C$-CO_2 are known to occur in the Amazon region due to changes in precipitation (Ometto et al., 2002; Pataki et al., 2003). Assuming that the environmental factors altering $\delta^{13}C$-CO_2 are also responsible for the changes in the $\Delta^{14}C$-CO_2, the observed difference in $\delta^{13}C_{ER}$ may help to explain the differences in mean transit time we observed among the two field campaigns. Changes in other environmental factors such as soil moisture may have also contributed to this difference in mean transit times. Chambers et al. (2004) have demonstrated that, for example, high soil respiration fluxes correlate with low soil moisture levels in the central Amazon. Furthermore, changes in the composition of pools contributing to respired C can alter its C transit time (Lu et al., 2018). Meteorological data from the 80-m walk-up tower shows that precipitation and soil water content were higher during the campaign of December 2021 than in the campaign of October 2019 (Figures S4, and S6, supporting information). "

L306-310: this is very hard to follow with some grammatical issues. Please break this down into multiple sentences. It seems to say that you need to know the background conditions because you use them for the Miller-Trans estimate and because you need them for estimating mean transit times. This is confusing because the first part is about concentrations and isotopes but the second is really just about the 14C. Conflating the requirements of the approaches for the end-members with the mean transit time estimation is confusing. Start with one paragraph to discuss point i and a second paragraph for point ii.

Response: Good point. We changed the paragraph to follow the suggestion of referring to each point separately. To avoid a lengthy response here, please follow the changes in the highlighted pdf attached.

L313: Delete "nevertheless" as you have overcome this limitation – I don't know that you need to spend so much space on this. You did a good job filling in data gaps appropriately and you can address this in the methods and not belabor the point in the results and discussion. You make the point nicely in the next paragraph that these atmospheric background and calibration data are generally lacking for the tropics.

Response: Thank you, we edited it as suggested.

L330: I think you mean "steady annual decline". In the next sentence, be clear that you mean the end-member from the Keeling plot vs Miller-Trans approach. You still report a large range (e.g. in Table 1 and the results and beginning of the Discussion) and again I don't follow where this 2-30 year range (or 15-45 permil range) comes from when you provide single values from the Keeling and Miller-Trans approaches that are so close to one another.

Response: Thank you, we added "annual". We also clarified the end-member mixing models we refer to, i.e. Keeling and MT approaches. I do not understand what you mean by the large reported range. We are not reporting the transit time range in this part. However, I believe it may come from the confusion with the estimates reported in the text diverging from the ones on the table. We corrected this throughout the text following the previous comments, so this part should become clear now.

L333: Are you suggesting that the analytical errors on the raw 14C values are larger than the intercept errors from your linear fits? This is where it is very important to know how you fit the linear regressions as some approaches take into account the uncertainties on the data points, in both x and y, and others do not.

Response: We addressed this issue in a previous comment. In the revised version we describe the use of Model I regression instead of Model II based on the recommendation of Zobitz et al. (2006) after re-analysis of Pataki et al. (2003). This reference is also the one used by Phillips et al. (2015).

L335: Yes this has already been demonstrated, so it is promising. It is a nice result that it worked here but I don't understand why you say "especially for the tropical regions". You might rephrase that you demonstrate that it works for tropical regions, not just temperate ones as demonstrated previously. In the next sentence, it would be more compelling to call for more work to better quantify spatial and temporal variation for the tropics as it is difficult to explain why you have different results for 2019 vs 2021 and whether these differences capture interannual variation or a trend in shifting mean transit times of respired CO2 in the tropics.

Response: Thank you for the comment. We made the changes as suggested. Now it reads:

"The method of employing end-member mixing analysis to $^{14}CO_2$ measurements seems, thus, promising also for the tropical regions, alongside the temperate regions as demonstrated before by Phillips et al. (2015). Nevertheless, more work is needed to repeat the measurements with seasonal frequency in the Amazonian region and to obtain similar estimates in other tropical regions worldwide. Additional estimates of empirical mean transit time would better quantify spatial and temporal variations of the C mean transit time. Furthermore, they would help to understand whether variations in the mean transit time are due to interannual variability or a trend in shifting mean transit times in tropical terrestrial ecosystems (Sierra et al., 2023)."

L345: This is the first I've seen the difference in seasonality pointed to as an explanation for why October 2019 and December 2021 might provide different mean transit times (or 13C). This seems like something that could be mentioned earlier – then you can include seasonal variation in your statement that the method is useful and data is interesting but more is needed to understand what is happening in the tropics (see comment about L335). Perhaps this could be included in the paragraph that starts on L298.

Response: Thank you for the comment. Information on the environmental factors that could influence the mean transit time and the differences in the climatic conditions of ATTO in 2019 and 2021 was added in the previous (L298). The change is already pointed out in the response to L305, as it is also related to that comment.

L350: remove the decimal places from this paragraph and if you aren't going to describe the difference from 2019 to 2021 provide a range or average. Or do you think the more negative numbers in December of 2021 could be because of differences in airflow vs October 2019? I'm not sure what to make of the description of the other study's data. Can you better connect this study to your findings? If it's that the values are about the same, then provide the values from the Araujo et al 2008 paper.

Response: Thank you. We haven't removed the decimals, because the error is below 1. We added more information from the cited study to clarify the connection.

L355: Avoid 1 sentence paragraphs. This is a nice topical sentence for the statements that follow.

Response: Thank you for the comment. We changed as suggested.

L361: The Carvalhais et al approach also integrates over longer time periods, no? Isn't that based on annual productivity and respiration values? You should start here by stating that their 14 year turnover time is consistent with your range from 2021, though you observed a shorter mean residence time range in 2019. Then describe how they derive their value, that it's integrative over time, doesn't have quite the same ability to identify sources, and so on.

Response: Thank you for the comment. We changed it as suggested. Now it reads:

"The mean transit time for the campaign in 2021 agrees with the turnover time estimated by Carvalhais et al. (2014), however, the same does not hold for the campaign in 2019, when the mean transit time based on end-member mixing analysis is 390 shorter. The approach of Carvalhais et al. (2014) to obtain a turnover time integrates over large temporal and spatial scales by incorporating gross primary production values and C stocks over several years and with a resolution of 0.5°. However, it does not discern between pools of different ages that contribute in varied proportions to the total respiration flux. Therefore, it cannot account for pools with different $\Delta^{14}C$, but can only approximate the radiocarbon signature within a well-mixed total ecosystem respiration. Moreover, some of the potential reasons for the mismatch in 2019 include a seasonal variability of $\Delta^{14}C$-CO_2 in the central Amazon, different contributions of respiration sources from year to year due to climate variations, or even a poor representation of local measurements in a short-term campaign in comparison to the dynamics of the whole Amazon rainforest. More studies in different seasons, targeting individual respiration sources, and covering larger temporal and spatial scales are needed to overcome these different possibilities. The comparison with other estimates of mean

transit time, however, suggests that this metric might not be constant over time, even for old-growth forests in the central Amazon."

L372: Start again by comparing your results to theirs. "In contrast, in studies close to Manaus reported mean residence times similar to our range in 2019…" or something along those lines. Then describe how they derived their numbers and what the differences in methods, site, timing, and so on might mean. What about the Sierra et al results for Colombia? They sort of fall right in the middle of what you observed in 2019 and 2021.

Response: Thank you! We have changed the order as suggested and also added the comparison with the Porce model of Colombia (Sierra et al. 2021). The addition reads as:

"A 7-pool model developed for a tropical forest in Colombia (Porce model) has a mean transit time of 10 to 12 years (Sierra et al., 2021b), which falls in between the mean transit time we estimated for October 2019 and December 2021. Therefore, it suggests that a multi-compartmental model estimates an average of the differences or trends of the ecosystem's mean transit time. The Porce model accounts for the C composition and C age structure of different compartments. A similar model for the central Amazon could be parameterised to account for the potential respiration sources that could drive the radiocarbon isotopic signature of ecosystem respiration by being large enough and with high radiocarbon contents such as dead wood (Chambers et al., 2004). This way, the empirical estimate of mean transit time can help to constrain a multi-compartmental model more representative of the central Amazon forest. "

L388: I think it's better not to say "not necessarily mean a limitation of the method" but to go ahead and emphasize that your method provides resolution that the others do not in being able to look at spatial and temporal variability.

Response: Thank you! We deleted the phrase as suggested and added the emphasis of the resolution of the method. It now reads:

"The differences from one year to the other or even between seasons imply a potential natural variability of the weights of fluxes from different C pools with large differences in their turnover times. This variability could influence the C balance calculation in Amazon forests more than previously thought. In this sense, a practical method to calculate an ecosystem time metric such as transit time might improve our understanding of the C balance in Amazon forests and their role as C sources and sinks of atmospheric CO_2. This method also has the resolution to tackle temporal and spatial variabilities of the mean transit time of ecosystem respiration.»

L395: Specify for ecosystem respiration – this has been done for soils previously. Also take care, because there may be some mean transit times for ecosystem respiration from non-forested sites using NEE chambers (I'm thinking specifically of CIPHER and other experiments of Ted Schuur's group in Alaska on permafrost thaw). You have very cool and very novel results, but don't overstate their uniqueness by being to general in your statements.

Response: Thanks, we have now specified the ecosystem respiration. It was already stated that it was the first time for a tropical forest, so we kept it. Now it reads:

"We obtained, for the first time in a tropical forest, an empirical estimate of a mean transit time of carbon of ecosystem respiration based on end-member mixing analysis of radiocarbon measurements of ambient and atmospheric CO_2."

L400-403: I don't follow this statement at all. Better to not introduce normal vs non-normal distributions of transit times in the Conclusion section! Cut or reframe – or move to the discussion where it won't confuse your takeaway message.

Response: Thank you for the comment. We deleted the mention of the shape of distributions.

L410-415: This is good, but the paragraph before this is not very interesting. Better to move this last paragraph closer to the beginning of the Conclusions and follow with you demonstrate a method that can help address these open questions about variability and change in mean residence time of C in tropical forests.

Response: Thank you! We modified the order of the paragraphs as suggested.

Referee #2 - anonymous

The work by Chanca et al. provides an empirical determination of the C turnover in the Amazon rainforest, the most important ecosystem for the global carbon balance. The determination of this parameter is therefore of great importance, also because there is hardly any data of this kind. In this study, however, the authors were only able to provide a rough quantification, which underlines the need for further investigations.

The estimation of the mean transit time in this study is subject to large variations. This is due in part to methodological uncertainties and in part to biological variations such as seasonal changes. The authors are aware of the sources of these variations and the discussion of these points is well done.

Overall, the authors have submitted a solid manuscript, which I am happy to support. I have only one somewhat larger criticism related to Table 1 and a number of smaller comments, which I list below:

Response: Thank you for the insightful comments and suggestions. They were very important to improve our manuscript. We addressed all your queries and the specific changes are detailed below as responses as well as highlighted with the changes suggested by the other referee in the pdf attached.

Regarding Table 1: The table is basically clear, but the text describing the data does not match the data in the table. The Keeling plot data are not described at all. The Mitter-Trans data are different in the table and in the text. Have I missed something here? In my opinion, the text for this table needs to be fundamentally revised. Also, just for clarification: Please also state the data origin (i.e. references) for the lower 3 values in the table legend.

Response: Thank you! We revised all values and reporting of data. Before we were reporting 95% confidence intervals in the text and mean values plus-minus standard error in the table, therefore the mismatch. We fixed it now and the whole text has been made consistent.

L52: Do you mean "leaf sugars"?

Response: Thank you for the comment. We don't mean leaf sugars in the sentence pointed out, but indeed in the previous one, mentioning freshly fixed plant metabolites. The age of C in leaf sugars *will be young as well as of the CO2 respired by canopy leaves. To the sentence pointed out, we changed from 'leaves' to 'canopy leaves' to distinguish them from dead leaves in leaf litter.*

L54: I think it is not about the age of the organic matter itself, but about the age of the respired C (from organic matter), right?

Response: Thanks, we changed it.

L76-79: As you rightly point out, the Keeling plot approach is based on two sources. But the "CO2 released from the ecosystem respiration" is not a stable source, but composed of many different individual sources. This is not ideal, and I would like to encourage the authors to clarify this point further (either in the M&M or in the discussion).

Response: Thank you for the comment. We expanded this point in the Discussion. In the comparisons to other approaches, we also discuss the relation between models that incorporate more pools (consequently more sources) and their estimates of C transit time.

L140: Please provide some details on the nature of this quality control. This may be helpful to other researchers.

Response: Thank you, it is a good point. We now mention for example that concentrations of some trace gases are particular of the dry air used to fill the flasks for transportation, thus, the air inside the flask was not completely replaced by ambient air during sampling. Such samples are not considered in the analysis.

L233/4: We are talking about a difference of 1.2 per mil, right. In my opinion, this indicates different respiration sources or different isotopic composition of these sources. Please clarify.

Response: Thank you for the comment. This sentence was deleted from the manuscript, following also suggestions of another referee. The discussion on this difference was expanded in the Discussion, when we mentioned the environmental conditions that could alter the respiration fluxes or isotopic composition of the sources.

L238/9: Higher daytime del13C: Any idea what is going on?

Response: Thank you for the comment. Given the lower number of samples in 2021 than in 2019, it is hard to say for sure the reason for the enriched signature in 2021 during daytime, as it could be simply an effect of the sampling times used in 2021 not being enough to capture the daytime variability of delta13C. However, during the day, pCO2 is affected by two processes, photosynthesis and ecosystem respiration, whereas at night it is affected only by one process, ecosystem respiration. Because 14C is corrected for mass-dependent fractionation, this may not be a problem (photosynthesis will affect 1/CO2 but not Delta14C). Photosynthesis also enriches 13C in 'background' air, depending on which hemisphere you are in. Thus it likely also affects your 'background' but in the case of 14C that is unlikely to make much difference. At ATTO, internal reports and personal communication indicate that 13C-CO2 enriches during the daytime at 79 m - values approach -8.3 or so when CO2 concentrations reach their minimum (below the atmosphere background).

Expanding on this point would go beyond the scope of what is aimed by this work and could confuse readers. We decided to modify the sentence to comprise this difference in the number of samples leading to this mean value. It reads:

"Daytime mean delta13C was more enriched in the heavier isotope (-9.3 permil), based on 13 daytime samples in 2019 and 4 daytime samples in 2021."

L300-3: The references from 2002 and 2003 are too old to support the interpretation of the del13C data. What was the precipitation in 2019 and 2021? Was 2019 drier than 2021?

Response: Thanks, we mention the actual environmental conditions observed in the ATTO site during the sampling campaigns. The plots were already in the supporting information and in the main text we discuss the wetter conditions in December 2021 that could influence the isotopic signatures of the sources.

Figures: The figures have slightly different designs, i.e. points, information on equations, r2,… Please homogenize.

Response: Thank you for the comment! We homogenized all the figures in the manuscript.