Dear editor/referee. Thanks for your in depth reading and critiques of our work. The comments have been valuable, and have led to a clearer, stronger, manuscript.

Here, the reviewers' questions will be shown in black, our answers to these questions in blue, and adjustments to the manuscript text in red. We answer the questions in order of referee comments (RC1, RC2), after dealing with some main points applicable to both reviews.

Main point 1:

Because of a 'math mode error', the following piece of text was missing from line 244 in the original .pdf. We expect that this resolves some of the questions about missing methodological detail.

"m2 footprint). The EC system consisted of a CSAT3 anemometer (Campbell Scientific, Logan, USA) and a LI-COR 7500 open path gas analyser (OPGA, LI-COR Inc, Lincoln, U.S.A.). Two laser spectrometers with high flow rates were placed on a tower balcony at 54 m height with an 8 m inlet to the anemometer (Fig. 4.1). This inlet line was a 1/2 " copper tube which was heated and insulated, and protected from the environment with an aluminium mesh inlet filter. The flow rate exceeded 20 Lmin−1 to provide turbulent conditions which preserved high-frequency fluctuations in the air stream (Moonen et al., 2023). The CO2 isotope analyser used was an Aerodyne TILDAS-CS laser spectrometer (Aerodyne Research Inc., Billerica, USA) and the water isotope analyser was a Picarro L-2130i (Picarro, Santa Clara, USA). Both were placed in temperature controlled enclosures set to 35°C. These stabilised the temperature sensitive instruments, while preventing condensation."

We will refer to this "main point" if we believe a reviewer question is answered through the addition of the text above.

Main point 2:

Regarding the Peclet number, there was a mistake in line 175 (just above eq. 9), where it was suggested that this number could be negative (which it can't).

...where ~~positive numbers~~ numbers larger than one....

RC1

The manuscript by Moonen et al. contributes to recent body of literature developing novel approaches to improve methods to partition net ET and NEE fluxes in its gross components. This is paramount to better understand the role of terrestrial ecosystems for regional water and carbon cycles and to allow better upscaling of these fluxes using remote sensing or modelling tools.

This partitioning is for various reasons not trivial for tropical forest and recently novel neural network-based methods have been developed that try to improve the “traditional” so-called daytime and nighttime methods. I think that besides the limitations, which are clearly recognized by the authors, adding isotopes exchanges to the ET and NEE partitioning “toolbox” disserves to be discussed in the scientific literature allowing for future experiments to improve also this method. Afterall, this isotope flux method It is simply a more direct and physical based method, i.e. we add one isotope flux equation to solve two unknowns.

This paper assesses this method on a very relevant site (ATTO, Brazil) and was made possible using novel eddy covariance measurements of isotopologues of H2O and CO2. The authors address in a very detailed and (often) clear way the followed methodology and potential caveats for an “average day” of a 13-day period in the dry season in the Amazonian tropical rainforest. Despite the limitations, but this is the case for all current existing partitioning methods, this paper disserves to be published to allow further refinement by other interested research groups, this is the only way how the potential of this method can be tested and further developed. Below I give one major comment mainly related to the steady state assumption made and several minor comments that are meant to further increase the clarity of this complex and integrated methodology. Finally, the paper also correctly advocates for detailed isotopic measurements of leaf and soil endmembers, which is paramount further refine and test this methodology.

Major comments

I have some concerns on what the authors describe as a midday steady state (line 68). This is mostly linked to the “isotopic composition of transpiration” (line 125). Can you really consider that mass of water that is entering the leave matches the mass of water evaporating from the leaf? During photosynthesis light splits H2O and is “consumed” to generate glucose. Furthermore, H2O is also used to incorporate H into leaf waxes (alkanes). These processes have a (large) biosynthetic fractionation factor and hence also isotopically enrich leaf water. Maybe there is a misunderstanding in the (too short) description of the steady state here? Furthermore, I agree, in general root water uptake is assumed not to be fractionating, though this is increasingly debated. In addition to that (and see comment on Table A1) root water uptake seems to come from deeper than 10 cm soil depth. This is also something that needs more attention in the text.

On the suggested ‘other uses’ of H2O in plants, we agree that such processes will take place, and will have some associated fractionation effects. However, it is key to consider the orders of magnitude of water evaporation compared to H2O used to generate glucose for example. Comparing our Fig. 2 with Fig. 4, we find that more than 300 times more water is evaporated than CO2 is assimilated by vegetation. Any fractionation effect associated with such alternative pathways will therefore be diluted to such an extent that it cannot affect our results. We agree that in detailed future work, information on the CO2 flux itself could potentially be used here.

On the point of root water uptake depth, indeed this comes – on average – from a depth of more than 40 cm depth. This is why we incorporated ‘runoff water’ from the plateau, which was sampled at a stream some 70 m below the measurement site’s ground level. We agree that we should describe this better (only in table A1 before). We now added the following sentence to section 3.1. In addition, we adjusted Fig. 6 to reflect the true detail better (with separate deep soil water and runoff water taken into account).

“The isotopic composition of xylem water (δ_xyl) was approximated as the average of deep soil samples (40–100 cm depth, −3.83 ‰ for $\delta^{18}O$) and a water sample collected from a stream at approximately 70 m lower elevation (-4.86 ‰ for $\delta^{18}O$) which drains the plateau that hosts the ATTO site.”

Minor comments

I believe also “non-isotope” experts would be highly interested in this methodology. Hence a consistent and unfirm terminology is important, i.e. what is a “delta flux” (line 7, 48, 90, etc.) or (H2O and CO2) isotopic fluxes (line 88, 68, etc.), explain the relation between delta and isotope ratio notation (e.g., Eq; 6, 7 vs. Eq. 4, as both are used in the various equations.

This has now been made more consistent within the text (based on this and other comments). Importantly, we changed isotope flux to ‘isotopologue flux’ to specify that we are indeed dealing with molecules. In addition, we use “delta flux” to specify the methodological method to express isotopologue fluxes.

Line 81: I don’t understand this sentence: … “describe the composition of a negative flux”?

In theory, the Keeling plot method is designed to determine the source signature of a gas source added to the atmosphere. We added this sentence to clarify that in our current approach, we also use the inverse application, namely: to find the source signature of a gas ‘sink’, i.e. CO_2 leaving the atmosphere.

Line 120, Eq. 6, 10: explain all parameters in the text. Note here you use isotope ratio’s (R) and not a delta notation (make clear to a non-expert public). Please check this also consistently for all other equations in the text.

In Eq.1 we detail how delta values relate to isotope ratios.

We added: “… See Eq.1 to see how the isotope ratios (R in Eq.6) relate to delta values.”

Line 210: is it correct that “d18c” is calculated from Eq. 6?

Not quite. Eq.6 describes d18O (and dD) in (gaseous) water that evaporates from the soil (and thus the isotopic composition of E).

Line 243: what is the meaning of 10^5 here?

Incorrect use of math mode. Adjusted to 10^5 m^2… (footprint area) Also see Main point 1 about the rest of the sentence missing.

Line 255: It is unclear how the outlier filtering was done.

The outlier filtering was done and described in Moonen et al. (2023). To not elongate an already long manuscript, we have decided to reference this paper.

Line 259: You are sure these uncertainties are correct?

Yes, they are indeed large, specifically due to values close to zero in the denominator of the equation used to determine these values (Eq. 4 when solving for dD).

Figure 1 and related text: please add a bit more technical details on the equipment and which isotope standards that have been used.

See main point 1 (loss of methodological paragraph due to 'math mode error')

Line 269: Here you refer to the "steady state" (see major comment), we need a bit more explanation on this I believe.

See answer to your main comment. Given the widespread use and application of the term 'steady state' in the water isotope context, we do not believe additional detail is required.

Line 280: Explain why this background isotopic composition was unstable.

… as it had a temperature dependent instability.

Line 301: "This finding was confirmed by,…" this "co-spectra" notation is not clear.

Notation changed from cospectra to co-spectra, and we added an appendix Fig. A2, which shows two co-spectral examples, which we refer to throughout the text now.

Fig. 2: improve legend and caption for a better understanding of the figure. For a) also add it is a "30 min water flux".

top) Composite day 30 min average water flux derived using the closed path isotope analyser and an Open Path Gas Analyser (OPGA, shown in red). middle) and bottom) Composite day 30 min average D and 18O δ fluxes derived using the water isotope analyser combined with the EC method. …

Fig. 3: idem for legend in this figure.

We believe the reviewer means Fig. 4, given the suggested similarity with Fig.2

top) Composite day 30 min average CO2 flux derived using the closed path isotope analyser and an Open Path Gas Analyser (OPGA, shown in red). middle) and bottom) Composite day 30 min average 13C and 18O δ fluxes derived using the CO2 isotope analyser combined with the EC method. …

Line 311-312: “stable conditions lead to horizontally heterogeneous conditions”; make clear please.

Moreover, these stable thermodynamic conditions suppress vertical mixing, which leads to horizontally heterogenous conditions …

Line 322: correct terminology please: write (maybe) CO2-isotopologue flux,…

Agreed. Now changed to ‘isotopologue flux’ throughout the text. Note that in our knowledge framework a delta flux is a specific (methodological) way in which isotopologue fluxes can be expressed.

Line 343-345: this section is unclear to me. Can you try to rephrase?

Following Eq 4, the isotopic composition of the net ecosystem exchange flux ~~composition~~ must be interpreted in line with the sign of the flux ~~direction~~. Thus, during daytime it represents the isotopic composition of the (negative) uptake flux. For δ13C, ~~this means that~~ we find ~~source~~ uptake flux isotopic compositions which are depleted by 20‰ compared to the atmosphere during both day and nighttime (see Fig. 5).

Line 348: where we see this -15 per mill lower values?

-15‰ lower than the one of ambient atmospheric CO2 during the night according to the bottom panel of Fig. 5, …

Fig. 5: explain the y-axis titles well in the caption.

… derived using the flux method (δ…,F), following Eq 4. The deviations from the atmospheric background are plotted. …

Line 379: Make clear in the captions that all data are for 14:00 hour and a 13-day composite diurnal flux - > independent reading of Tables and Figures.

Now present in Figs 2,3,4,5,6,7,8.

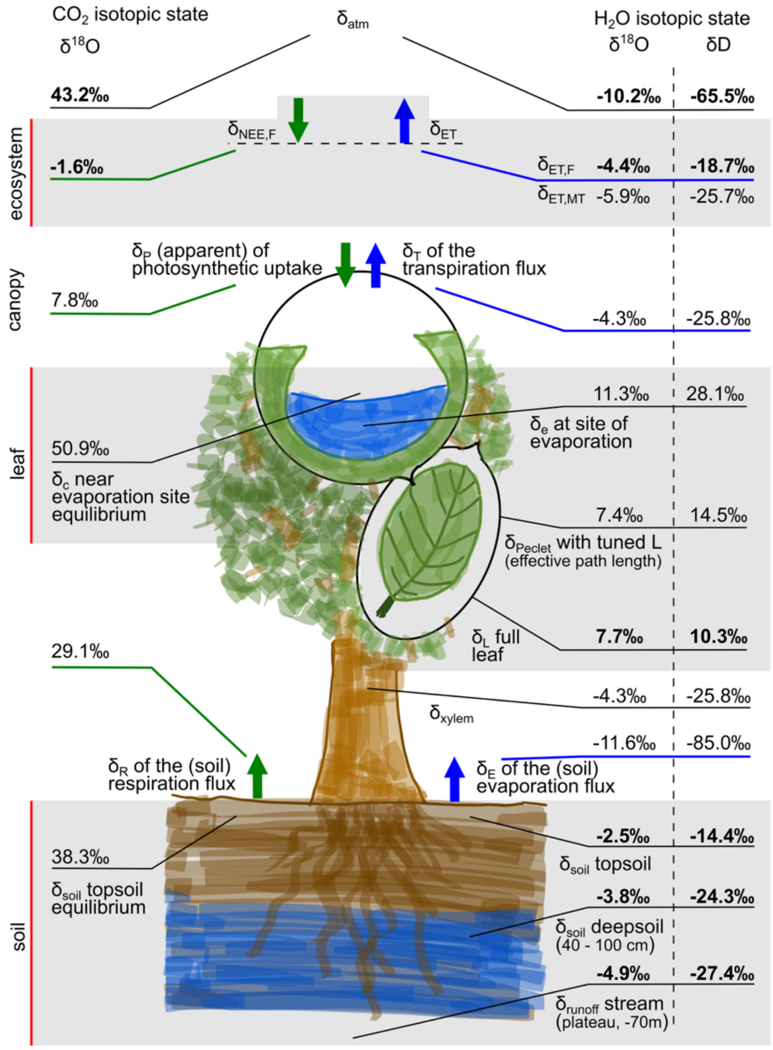
Table 1: please not that Tair and Tleaf can be different. So not sure what the 33°C represents.

Certainly agreed. At the same time, leaf temperatures are highly heterogeneous, given (upper) leaf shading, making it unrealistic to ‘measure’ a realistic value for T_{leaf}. While not perfect, we expect the air temperature to represent the temperature of the shaded leaves (which will be most leaves), reasonably well. A better method for future studies might be using a measured longwave outgoing flux and using the Stefan Boltzmann law to infer an ensemble temperature of the tree crowns.

Line 394: “isotopic composition of the topsoil”. Please note root water uptake is not from the topsoil alone and, depending on tree species, distributed over different soil layers. Please make this and its complications clear here. This is also a bit misleading at the bottom of figure 6, as root water uptake is water with (for dD) a value between -25.8 and -14.4 per mil. Furthermore, see also my comment for table A1 and in the major comment.

Agreed. And while we took this into account, we should have explained it better (in table A1 before). We now added the following sentence to section 3.1. In addition, we adjusted Fig. 6 to reflect the true detail better (with separate deep soil water and runoff water taken into account).

“The isotopic composition of xylem water (δ_xyl) was approximated as the average of deep soil samples (40–100 cm depth, −3.83 ‰ for $\delta^{18}O$) and a water sample collected from a stream at approximately 70 m lower elevation (-4.86 ‰ for $\delta^{18}O$) which drains the plateau that hosts the ATTO site.”



Line 399-400. Here is the concept of mass conservation again. See my comments above where I argue that is not really the case. Can this be adjusted?

See response to major comment.

Line 420: explain clearly to which “reservoir” you refer.

As mentioned above, part of this isotopically enriched CO2 diffuses back through the stomata ~~into the atmosphere~~ and isotopically enriches the atmospheric reservoir ~~that reservoir~~.

Fig. 6: I can't recall from the text how/where the "canopy" values were derived? Maybe this can be further clarified?

In section 3.1, the ancillary variables are detailed, where we mention the collection of leaf and soil samples. We have now included a back reference from Fig. 6 to Sec. 3.1.

Line 423, 433 (and elsewhere): I think you need to better clarify in the text (e.g. M&M) what you mean with kinetic, physical, apparent and equilibrium fractionation. This is now used a bit randomly/confusing. Please clarify this throughout the text.

…it seems as though 16O-CO2 is apparently assimilated faster than 18O-CO2. It is important to realize that this is not ~~a physical fractionation effect~~ what happens physically, but that an apparent fractionation effect takes place.

So, the underlying physical process is the bi-directional exchange combined with isotope equilibration in the stomata, named - in literature – an apparent fractionation effect.

In line 433 the term 'kinetic fractionation' is used correctly, as it relates to a diffusive process.

Line 439-443: unclear please revise.

We realize that grasping the complex phenomenon being described is hard. Below a revised attempt to clarify this concept (also included in the revised manuscript).

In line with this, the isotopic composition of this net negative (uptake) flux (δNEE,F) is depleted compared to the atmospheric background. In our case, δNEE,F = -1.6‰. This is lower than both of its individual component fluxes, the canopy and the soil flux end-members δP and δR. Whereas this may seem to violate isotope mass balance, it is the mathematical result of attributing the depletion associated with both large and opposing gross fluxes to one, smaller, net flux. In fact, we show below that the derived value of δNEE,F does allow for the flux partitioning equations to be solved…

Line 447-448 this equality (for dE) needs to be better explained.

δE was assumed to equal δ^{vap}_{soil}, as described in Sec. 2.1, and δT was represented by δxyl

Line 458: can you better explain that equation?

Fout ∝ ci/ca and Fin ∝ 1−ci/ca given K theory (describing how gradients relate to fluxes).

To interpret δP, it helps to consider ci/ca as describing a balance of opposing diffusive fluxes (Fout / Fin ∝ (ci/ca) / (1−ci/ca)).

Thus, while an = sign is mathematically correct, ‘equality’ is not the point, but ‘proportionality’ is. Hopefully the adjusted formulation clarifies that.

Line 462: “Uptake fractionation”? See my earlier comments on fractionation

…the effect is leveraged 4 fold when interpreting it as ~~an uptake~~ a fractionation effect resulting from photosynthetic uptake alone.

Line 498: you might consider discussing the possibility to derive integrated ci/ca ratios via 13C analyses in cellulose of leaves.

This is an interesting proposition! Unfortunately, this is not easily applicable for our rather short timescale analysis, given that we need to know the ci/ca on the min/hour scale.

Line 507: the ci/ca of 0.7 seems to me on the high end? I would expect values of 0.5-0.6. Can you back this up with literature data?

Farquhar suggested the value of 0.7 for plants with the C3 photosynthetic pathway (Farquhar et al., 1989). Based on our leaf chamber measurements, we find a value of 0.79, and up to 0.9 at night (González-Armas et al., 2025). A reason for these ‘high’ ci/ca ratios is the high relative humidity and limited maximal temperature (32 deg C) in tropical rainforests. This means that the vegetation does not suffer (as) much water loss with more open stomatal apertures.

Line 522, 542-544: if the isotope steady state is so crucial, I think you need to guide/explain the (new) reader a bit better on this here and in the Materials and Methods.

This assumption emerges from the principle of mass conservation, and implies that during continuous transpiration, a balance is established between the isotopic composition of the water taken up by a plant, and the water vapour transpiring into the atmosphere. This ~~results in~~ directly relates to the strong isotopic enrichment of the water at the evaporation sites in the leaves, which leads to and maintains the enrichment of the transpiration (vapour) flux.

Line 560 and beyond: I agree you need better description of the diurnal dynamics of the isotope reservoirs. But I think to use this isotope-based partitioning method on a longer time scales than just days we need also seasonal (e.g. dry-wet season) information on the isotope reservoirs. Second, it would be good, if feasible, to compare your isotope-based partitioning with other (debated) flux partitioning methods such as the (modified day-time method) or more recent neural network-based approaches.

We agree that it is relevant to present this broader perspective:

A logical next step would be to apply isotope-based partitioning on longer (seasonal) time scales - which will require explicit characterization of the seasonal evolution of the *relevant isotope reservoirs (soil and leaf water, atmospheric background) – as discussed in for example (Wehr & Saleska, 2015). In addition, cross-validation of our isotope-based partitioning with other widely used, but debated, NEE partitioning frameworks such as modified daytime and neural-network based methods would be a valuable exercise (Tramontana et al., 2020).*

Figure A1: check the title of the y-axis. Is this notation correct?

Thanks for noticing. The y axis title was adjusted to the following (with "chi" and added units). The caption details that "chi" refers to mole fractions. Below the revised version of the figure included in the revised manuscript.

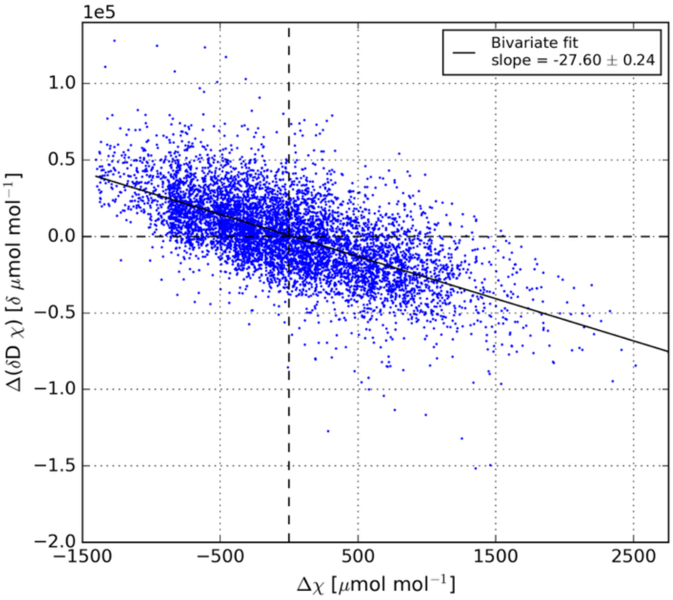


Table A1/A2: mention in the caption this data is at 14h00 and 13-day composite average. Make sure your parameters have all the same notation as in the text (e.g. for the fractionation factors this seems different. If I look at the dD and d18O data for topsoil and xyleme water, it is clear (assuming no fractionation) that trees take up water from below 10 cm depth. I have added an earlier comment on root water uptake depth. You might need to (re)consider this aspect in the text.

The time and 'composite' elements were added to tables A1 and A2. The answer and adjustments relating to the root water uptake depth and Fig. 6 are presented above.

RC2

Moonen et al. report on a very innovative measurement campaign using stable isotopes in CO_2 and H2O in eddy covariance flux measurements with the aim to partition net fluxes into its components. At the moment, there are very few of such using stable-isotope enabled eddy covariance measurements. Carrying out these measurements in a tropical environment is even more novel. In my view, this study is a tremendous measurement effort with a very interesting ecosystem flux analysis.
In my view, the manuscript is in general well written. There are, however, a number of smaller aspects that could help to improve clarity. At some places, details are missing to really understand what and how it done and also what were the limits of their analysis. Some parts that I was expecting in the Methods section only come later in the results, which makes the reading unnecessarily complicated.

Specific comments

Introduction

Line 54: Here the authors describe, what they do, but do not provide clear objectives or hypotheses. In my view, it is helpful for the reader to have clear objectives and hypotheses were possible.

We agree that this would benefit the reader.

...has shown to be a valuable link between the carbon and water cycles (Gillon and Yakir, 2001).

In this work, we ~~for the first time investigate~~ aim to use the 18O isotopic signature in both H2O and CO2 to describe the isotopic state throughout the ecosystem in detail, which should allow for the net fluxes of H2O and CO2 to be partitioned into seperate soil and vegetation fluxes. To this end, we make use of comprehensive measurements of...

Theory

Line 102: The section on measuring eddy covariance fluxes for isotopes could be strengthen by providing an overview of previous work on isotope EC.

We have now included some relevent works, as we did for ‘gradient based appoaches’.

... At present, high flow rate laser spectrometers are available which measure the isotopic composition at sub-second frequencies, which allow for direct isotope flux measurements using the eddy correlation technique (Griffis, 2013; Sturm et al., 2012; Wahl et al., 2021) . The difficultly...

Line 105: I would stay with the term “eddy covariance” to be consistent with the rest of the manuscript.

Implemented

Line 130: Here assumptions about steady state are made. In my view, it would be help to have a stronger overview of literature here on this assumption and its limitations. I am aware that this is discussed later, but maybe one sentence with 1-2 references would help here.

…This is an expansion of the idea that when taking the leaf as a reservoir, the mass of the water entering the leaf matches the mass of the water evaporating from the leaf. Analogous to mass, no isotopologue species should be able to accumulate in leaves on longer timescales (Farquhar and Cernusak, 2005; Barbour et al., 2017). …

Line 153/Eq 8: how are rs, rb, and ra calculated? Are they coming from Table 1 (but check that there the R are in capital)? If yes, please mention already here. What about ra?

Indeed the used values for the resistances are specified in Table 1. We now updated this table to include the aerodynamic resistance ra and we eliminated the capital "R's" which slipped in there. A reference to this table was also added to the text.

…does not cause kinetic fractionation. The used values for the resistances are specified in Table 1 …

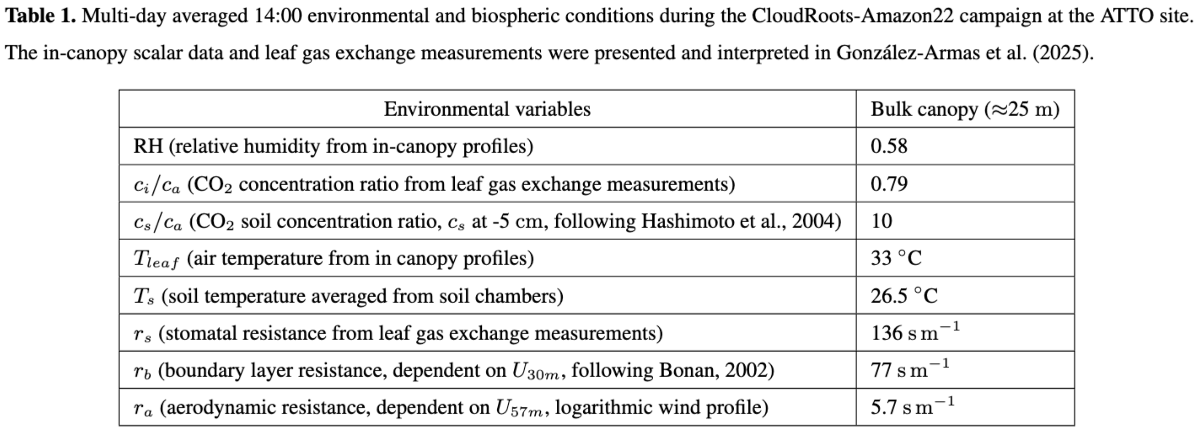


Table 1. Multi-day averaged 14:00 environmental and biospheric conditions during the CloudRoots-Amazon22 campaign at the ATTO site. The in-canopy scalar data and leaf gas exchange measurements were presented and interpreted in González-Armas et al. (2025).

Environmental variables	Bulk canopy (≈25 m)
RH (relative humidity from in-canopy profiles)	0.58
c_i/c_a (CO_2 concentration ratio from leaf gas exchange measurements)	0.79
c_s/c_a (CO_2 soil concentration ratio, c_s at -5 cm, following Hashimoto et al., 2004)	10
T_{leaf} (air temperature from in canopy profiles)	33 °C
T_s (soil temperature averaged from soil chambers)	26.5 °C
r_s (stomatal resistance from leaf gas exchange measurements)	$136\ \mathrm{s\,m^{-1}}$
r_b (boundary layer resistance, dependent on U_{30m}, following Bonan, 2002)	$77\ \mathrm{s\,m^{-1}}$
r_a (aerodynamic resistance, dependent on U_{57m}, logarithmic wind profile)	$5.7\ \mathrm{s\,m^{-1}}$

Lines 180: Peclet effect: why assuming an effective path length on 8 mm? In the text it refers to Barbour and Farquhar 2004, which however shows a wider range. Barbour and Farquhar 2004 refer to other studies with an L=8 mm, but this is for winter wheat, not a tree species. Why were not own measurements done? Alternatively, Lai et al. 2008 Oecologia estimate a wide range of values for L from 0.01 to 0.57 m for seven tree species in the Amazon. This could help with a sensitivity analysis and/or error propagation.

This is interesting additional information. We incorporated the range appropriate for the dry season Amazon (Lai et al. 2008), and made new calculations with the average (0.054 m). We found that the values for d(lam) in Fig. 6 result in changes from 8.1‰ and 16.8‰ to 0.2‰ and -10.0‰ (for d18O and dD respectively). While this is consistent with our source water being fed to the plant, it leads to a mismatch with the isotopic composition of measured leaf water (7.7‰ ; 10.3‰).

We can however suggest why this change occurred. The new value for L is 7 times larger than the previously used value of 8mm. This pushes the Peclet number from ~0.5 to ~3, thus becoming ‘advection dominant’. As a concequence, the leaf lamina isotopic composition transitions from being ‘similar to the water in the evaporative site’, to becoming ‘similar to the water transported to the leaf through the xylem’.

Given these new insights, we have decided to come up with a representative effective path length for the 13 day composite 14:00 LT case, using the comprehensive measurements we have made in CloudRoots Amazon22. This effective path length incorporates both the Peclet effects from capilary and leaf vein transport processes, and it is tuned to the leaf average water isotopic composition measured in the leaf isotopic water samples. The value is 10mm, which results in a Peclet number of 0.61 for d18O, and a Peclet number of 0.6 for dD, indicating slight diffusive dominance.

Note that it is not possible to use one universal Peclet value or effective path length to reflect the wide range of actual conditions, and that we therefore believe it is impossible to find a “universal” Peclet number / path lenth to translate leaf water enrichment to evaporation site enrichment reliably.

The following changes were made to the manuscript.

Methods.
Barbour and Farquhar (2004) determined that L = 8 mm provides a reasonable length scale, with l estimated at 0.1 mm. For the Amazonian dry season specifically, Lai et al. (2008) reported a much higher averaged value of L = 54 mm.

Given that more complete formulations lack validation on key coefficients, Eq. 10 is generally used (Barbour et al., 2017). With the comprehensive data we collected, we were able to estimate a realistic value for L, which we describe in Sec. 4.3.

Results (4.3):
...more enriched than δDxylem. ~~δlam, the isotopic composition of water in the leaf lamina, illustrates that this enrichment occurs only locally within the leaf as a consequence of the Péclet effect (see Eq. 9)~~. Liquid water samples of entire leaves (δL), which comprise both the leaf veins and the leaf lamina, bridge the composition of the enriched evaporation sites, and the comparatively depleted xylem source water (Cernusak et al., 2016). Given that we know both of these end members, and the leaf water isotopic composition, we are able to estimate a value for the Peclet number using Eq. 10. Since we also know the transpiration rate from the partitioning result, we can subsequently solve for L in Eq. 9, which is the effective path length over which water transport in leaves takes place. This results in a value of L = 10 mm, should be seen as a representative value for the entire footprint of the EC system, and accounts for transport through both the larger leaf veins and the smaller leaf capillaries. This value for L was

associated with Peclet numbers of 0.61 for d18O, and 0.6 for dD. *Table A1 specifies the important…*

Discussion:
The limit here is that the magnitude of the Péclet effect is highly dependent on hard to determine variables like the effective path length ~~the tortuosity of the water path~~ (L, See Eq. 9). Estimates for this variable vary considerably between experiments and species (Lai et al., 2008). The value L = 10 mm that we determined for the 14:00 steady state of the 13 day composite, is in line with previously reported values by for example Barbour and Farquhar (2004), who found L = 8 mm.

Eq 9: How is transpiration (T) estimated here? Is this obtained from the ET partitioning or somehow assumed.

Given that there is no 'circular dependence', we used T from the water isotopic partitioning.

Line 206/eq. 11: please check the use of symbols. Here T stands for temperature, where as in eq. 9 it stands for transpiration. Maybe good to use a subscript somewhere.
Eq. 12 and 13: How is ci calculated? Is this also coming from Table 1. If yes, please mention already here. Why is mesophyll conductance not considered here (please check Langendörfer et al. 2002).

Equation 11 Temperature symbol changed to T_K, to indicate the Temperature in Kelvin.

Table reference added to description of Eq. 12.

We agree that an even more comprehensive study should also look into / take into account the mesophyll conductance. For our scale, focussed on the ecosystem not on the leaf level, we did not concider this necessary to incorporate.

Methods
Line 244: Which anemomenter was used? What is the OPGA? The Li-7500? Which CO_2 isotope analyzer and which H2O isotope analyzer was used? Please provide details.

See main point 1 (math mode error related loss of methodological detail).

Line 252: Regarding the calibration procedure it is referred to Moonen et al. 2025. This is fine, but nevertheless, it would be nice to have some information here on the instrument performance, e.g. on precision for 18O and D

Also, see main point 1.

Results

Line: 290: here the term “moisture flux” is used. I assume H2O flux is meant here as written earlier. Please use only one term for one thing.

Changed to H2O flux.

Line 295: the terms “delta flux” is used whereas in the caption of Fig. 2 the term “isotope flux” is used. Pleas use also delta flux in the figure caption as delta flux was well defined in eq. 3 (with proper unit) whereas isotope flux is more a general term and not really defined. Please also check Fig. 4.

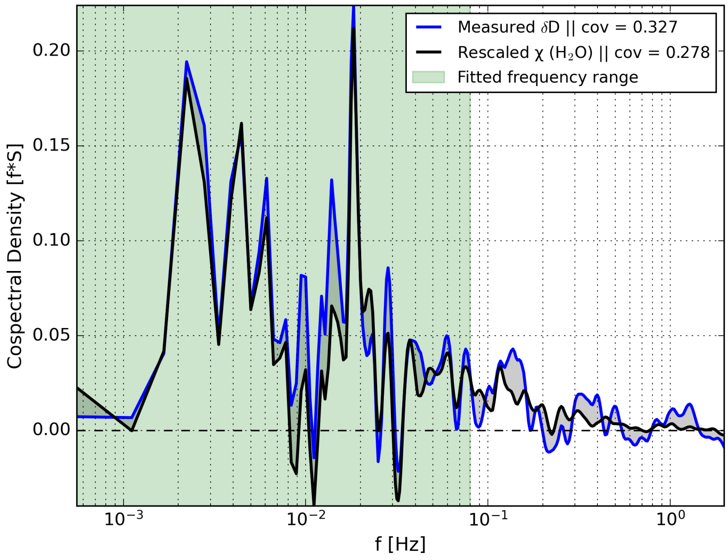
Agreed, we now unified the terminology throughout the text, where isotopologue fluxes refer to the phenomenon, and delta fluxes refer to the methodological approach we used to describe them. This means that the captions of Fig. 2 and 4 now mention d fluxes instead of isotope fluxes.

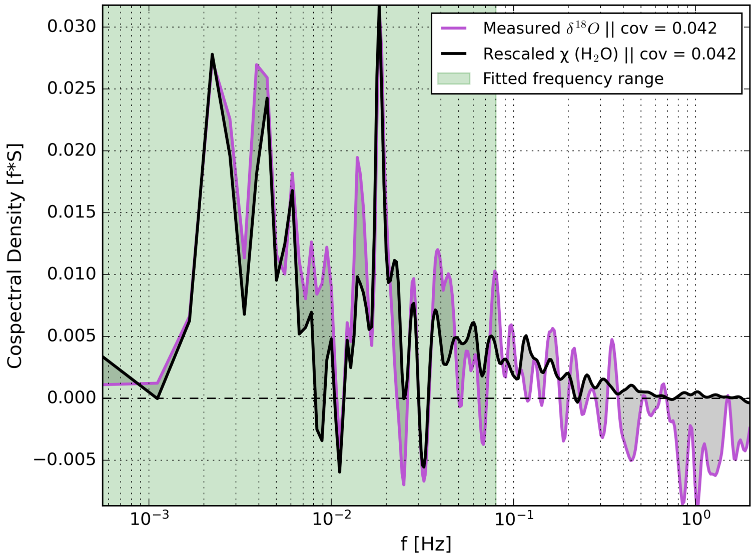
Line 304: where can we see the cospectra? How did you quantify that the highest frequency had negative contributions? Please add this to an appendix or show here in the discussion.

In the cited study, Moonen et al. 2023, we show multiple detailed examples of cospectral analysis. Yet we agree that these co-spectra can be interesting to the reader, because they illustrate the spectral scaling method, and provide an example of the odd negative d18O flux contributions at eddy frequencies of ~1s. Therefore we have added examples to the Appendix as suggested.

... but the highest frequency eddies had negative contributions (see appendix Fig. A2).

time-lagged cross correlation on the CO2 or H2O mole fractions measured by both (Moonen et al., 2023). Additionally, the spectral correction method described there is applied to correct for the spectral errors in the fluxes (see for example Fig.4). An example of the spectral correction algorithm in co-spectral space is presented in appendix Fig. A2. The





Line 315/Fig. 3 and line 339/Fig. 5: Why is deltaET-delta_atmo or deltaNEE-delta_atmo shown and not just deltaET or deltaNEE? In my view, it would make it easier to interpret Fig. 3 and Fig. 5.

We ideally would have shown deltaET and deltaNEE irrispective of delta_atmo indeed. However, for the CO2 analyser, we only know the difference in detail, as delta_atmo was not reliable due to instrument instability. We note here that the *difference* can be inferred directly from delta flux measurements. For concistency, the same y-axis style was chosen for deltaET in Fig. 3.

Line 339/Fig. 5: The diel cycle of delta18O of NEE looks very similar to the work to Wehr et al. 2013, Sturm et al. 2012 and Griffis et al. 2008. Please check these references and refer to if meaningful.

...The strongly depleted δ18O composition of the daytime carbon flux is thus a consequence of the enrichment of the atmosphere due to back-diffusion. Previous studies have found similar diurnal cycles for d18O to the ones we present in the bottom panel of Fig. 5 (for example Sturm et al. 2012, their fig. 9b (dry conditions)). The apparent...

Line 378: the term ABL is not defined before.

Now defined: *...atmospheric boundary layer (ABL)...*

Line 455: here the authors calculate a respiration value of 44% of NEE. It would be interesting to compare this to the classical eddy covariance flux partitioning approaches (Reichstein et al. 2005 and Lasslop et al. 2010.

We agree that it is relevant to present this in the discussion, and have added the following addition paragraph to the "broader perspective" section:

A logical next step would be to apply isotope-based partitioning on longer (seasonal) time scales - which will require explicit characterization of the seasonal evolution of the relevant isotope reservoirs (soil and leaf water, atmospheric background) – as discussed in for example (Wehr & Saleska, 2015). In addition, cross-validation of our isotope-based partitioning with other widely used, but debated, NEE partitioning frameworks such as modified daytime and neural-network based methods would be valuable (Tramontana et al., 2020).

Line 464, Fig. 6: please add to the caption that the values shown here are for average conditions at 14:00.

Incorporated

Line 469: I don’t understand why it was not possible to determine uncertainties for the partitioning results.

Given the number of dependent variables - most clearly indicated in Fig. 6 - exacerbated by the fact that the type of error per determined end member will be different (se vs std), formal error propagation is highly challenging, and in our view, not befitting the scope of the current study. Note that we do identify key driving variables, like the ci/ca ratio for example, which determines dP, and with that the partitioning of the NEE flux.

Line 513 to 520: Here a fix value for ci/ca is used. It is good to see some discussion here on the sensitivity of the partitioning to this value. Nevertheless, I am wondering why the authors did not adapt an approach similar to Bowling et al. 2001 or Wehr et al. 2015 where they estimated canopy conductance based on the water flux to calculate ci/ca. I am aware this introduces some circularity, but might still provide a meaningful constraint.

This is an interesting point. Indeed this circularity would be a concern. During this specific campaign we did perform very detailed stomatal aperture and ci/ca measurements at the actual leaf scale by the ecophysiology team (González-Armas et al., 2025). This is why we prefer to use these explicit values. So, the ‘fixed value’ is actually the result of intensive sampling of many leaves from multiple hights, collected with a strategy to incorporate proportinal numbers of shaded and sunlit leaves.