We would like to thank the Associate Editor and the two reviewers for their thoughtful comments. We have addressed the reviewer comments below. Please note that our answers are in blue and that the line numbers correspond to the changed manuscript. The quotations in italics are the modified parts of the manuscript.

Reviewer 1

We like to thank reviewer one for they time and the very good and thoughtful comments. The suggested changes did improve the manuscript and made it better.

This paper addresses a relatively un-explored greenhouse gas flux pathways from reservoirs—specifically the role of dead standing trees, which the authors term "ghost forests", as a conduit (and/or source) of methane. The authors study a subtropical reservoir whose dam wall height was raised in 2011, resulting in the death of trees around the reservoir perimeter (estimated to affect 20% of the reservoir surface area). The authors measured methane diffusion, ebullition, and dead tree flux in 3 regions of the reservoir (close to the inflow, mid-reservoir, and far from the inflow) during 2 seasons. They then estimate that ghost forests contribute about 15% of the total methane emissions. The study is a nice follow-up to a more qualitative discussion started by Abril et al. 2013 (Journal of South American Earth Sciences, https://www.sciencedirect.com/science/article/abs/pii/S0895981112001642), which could be discussed in the introduction. The authors borrow methods from studies of live and dead tree fluxes in other ecosystem types, but to my knowledge this is the first study specifically addressing flooded dead forests caused by dam impoundment.

Thank you for pointing it out. We have added this into the introduction section and the discussion as follows (Line 80-83):

"The idea was originally proposed by Abril et al. (2013) suggesting wood decomposition from reservoir ghost forest trees will eventually release carbon, representing 26-45% of the total reservoir emissions, over 100-year time scale. However, the relative importance of CH_4 as the final respiration product of this organic matter decomposition remains unclear, as does the role of ghost trees in mediating the flux of CH_4 from the sediments to the atmosphere."

and discussion lines 676-679:

'...Previous estimation of CH_4 emissions of wood decay in a reservoir represented 26-45% integrated over 100-year time scale (Abril et al. 2013). Here we measured the CH_4 flux emitted by the trees, including CH_4 originating from the sediments. Based on our findings, emissions based on total carbon biomass are underestimating overall emissions, and it is crucial to measure the actual CH_4 flux from the trees."

While the authors don't mention it, the removal of trees and other vegetation prior to impoundment has been implemented prior to the closure of some dams, for a variety of purposes including to harvest usable timber before flooding or to reduce debris and hazards in the reservoir itself. This emphasizes the management relevance of the question.

We have added further text on this topic in the discussion in section 4.5 as follows (lines 696-701):

"Pre-impoundment vegetation clearing is a recognized management strategy and can been implemented prior to dam construction. This harvesting of timber would prevent emissions of CH_4 from ghost forest trees. Dams are typically constructed in rugged, hilly terrain to maximize storage capacity, making large-scale timber removal logistically difficult and cost-prohibitive. Trees were cleared during the initial construction of the Hinze Dam, but once the reservoir was filled and the dam level increased, it was difficult to reach the trees at the then edges of the reservoir."

Overall, this is a well thought out study with findings that will be of interest to the readership of Biogeosciences. I do not find any fatal flaws or have any major concerns, but list some suggestions and questions that I hope can improve the paper. From a writing clarity perspective, the paper could benefit from some editing, particularly in the discussion. I suggest opening the discussion with the most novel findings from this study (e.g. pertaining to ghost forest emissions). The discussion regarding the potential control of temperature on emission seems tangential to the questions you were asking in this study (and awkward to place at the very beginning of the discussion section).

Thank you for this suggestion. We moved 4.3 "Drivers of ghost forest emission" to the start of the discussion, which gives a good overview about the drivers and magnitude of emissions which sets the stage for the rest of the discussion. We feel the upscaling of results is best suited to the end of the discussion.

While the title of the paper and upscaling exercise focus on methane, the authors also measured carbon dioxide fluxes. Is there a reason why the same upscaling exercise was not conducted for carbon dioxide (e.g. in Table 4)?

Based on your query and the comments of reviewer 2, we have removed CO_2 data from the manuscript and focus on CH_4 cycling. CO_2 fluxes essentially represent a "recycling" term – i.e. the CO_2 that was fixed from the atmosphere, not organic matter, is simply returned to the atmosphere, while CH_4 fluxes represent a net increase in radiative forcing over years to century timeframes due to the GWP of CH_4 exceeding 1.

The upscaling of dead tree fluxes is tricky for a number of reasons that the authors discuss. One decision the authors made was to only consider the first 2.5 meters of exposed trunk above the reservoir surface as the emissive surface area. Do the authors have a reference to cite for this decision? Can they present a back of the envelope estimate considering a larger emission height to help the reader understand the impact of this decision?

We choose 2.5 meters as this was the highest location that we measured to, thus provide a conservative estimate. Most tree stem CH_4 studies generally only upscale to lower stem heights, due to declining CH_4 fluxes with stem height. However, based on your suggestions we showed the effects of upscaling to 10m during the comparison at a per surface m^2 level. This changes the max CH_4 flux from ghost forest tree stem from 428 to 1711µmol m^{-2} surface d^{-1}. We also added text about the uncertainty at lines 683-659 as follows:

"*Comparing the three CH_4 pathways at a per water surface area, ghost forest trees (upscaled to 2.5m) contribute up to 428 µmol m^{-2} surface d^{-1}. More than the diffusive flux (306 µmol m^{-2} surface d^{-1}), but ebullition (6286 µmol m^{-2} surface d^{-1}) is still the main contributor on a per* *water surface area, same as in our total reservoir upscaling. Comparing the fluxes on a per surface area has its limits, due to the three dimensions of trees, but it can help show the differences and directly compare between the fluxes. While comparing three-dimensional tree structures to two-dimensional water surfaces has geometric limitations, it allows us to directly compare flux magnitudes across the reservoir's primary CH_4 pathways.*"

A recent study also found that degassing flux may be a globally significant component of reservoir methane emissions (Harrison et al. 2021), but this pathway is not discussed here. Do the authors have a sense for whether the degassing from this reservoir may be significant?

The dam is a drinking water reservoir so water release is limited. We mentioned at line 662-666 that it is likely not a significant emission pathway in our system. Due to the dry year in 2023, no spilling was registered at Hinze dam, according to the data from the site management (SEQ-water).

"*Degassing downstream of the dam wall has been found to emit significant amount of CH_4 (Harrison et al., 2021). This is particularly important in hydropower dams. However, Hinze Dam is a drinking water reservoir and water outflow of surface water is restricted (none during 2023) resulting in minimal degassing. Although we could not quantify that pathway, we argue that it is not a significant contributor in our system.*"

Line by Line:

Line 10: change "being a major driver" to "driving"

Amended.

Line 40: Could add lake littoral zones here as well. See: Desrosiers, K., T. DelSontro, and P. A. del Giorgio. 2022. Disproportionate Contribution of Vegetated Habitats to the CH4 and CO2 Budgets of a Boreal Lake. Ecosystems **25:** 1522-1541, doi:10.1007/s10021-021-00730-9.

Added

Line 85: I'm unfamiliar with the term "supply volume", is this the reservoir capacity?

Amended to 'reservoir capacity'.

Line 184: The subtraction of diffusive flux seems unusual here. In my group, we don't typically do this since the ebullition trap consists of a funnel with a very small surface area at the air-water interface. Please explain. Also, you may want to use a different term than "v" in equation 2 since it is referring to the volume of bubbles captured in the trap (rather than the headspace volume in the floating chamber).

Apologies this was not clear. Our chambers float above the water surface and therefore capture the ebullitive and diffusive flux at the same time, similar to prior studies using the same approach (Hoffmann et al., 2017; Sø et al., 2024). We sample the headspace of these chambers every 24 hours (to limit gas diffusion back into the water) which gives us total flux (i.e. ebullitive + diffusive flux) and by subtracting the average diffusive flux (measured using a portable greenhouse gas analyser connect to a floating chamber), we estimate the ebullitive flux. "v" in our case is referring to the headspace of the chamber.

We have now made this method clear in the manuscript and have added citations of others using this approach as follows (Lines 207-209):

"*As our chambers measured total flux (i.e. diffusive + ebullitive), the average diffusive flux was subtracted from each ebullition chamber to estimate the ebullitive flux, similar to prior studies using the same approach (Hoffmann et al., 2017; Sø et al., 2024)*"

Line 204 and throughout: The use of the term "aquatic flux" to refer to diffusive flux is confusing to me since I think of both ebullition and diffusion as types of aquatic fluxes. I encourage the authors to stick to the term "diffusive flux" throughout the paper.

Corrected throughout.

Line 298: What do you mean by "between, within, and across"? It seems like you measured variability within a site as well as across sites. Not sure how the "between" and "across" are different from each other?

We have kept "between" and deleted "across".

Figure 2: It is hard to see the ebullition trap in panel E very well, do you have a better photo?

We added a clearer picture of our floating ebullition trap.

Figure 6: I suggest considering a correction for tree flux that would allow you to compare the flux per m-2 of inundated surface area that the tree occupies. This was done in Amaral et al 2025 to facilitate easier comparison to diffusive and aquatic fluxes (Inland Waters: https://www.tandfonline.com/doi/full/10.1080/20442041.2024.2432804). In other parts of the paper when the authors report a m-2 flux for dead trees I think they are reporting per surface area of tree trunk, but I'm a bit unclear on that.

You are correct that all areal (i.e., m^{-2}) fluxes are the flux rate measured at the surface (eg on water or tree stem), while flux $tree^{-1}$ is the upscaled values representing the total emissions per tree (from 0-2.5 m).

We conducted the upscaling exercise that you suggested and added these into the results and discussion. We also add the stem height into this calculation to show the difference between upscaling to different heights as follows (Lines 690-694):

"When upscaled to 10m stem height (or about half way to the canopy), the contribution of ghost forest tree fluxes increased to 1711 µmol m^{-2} surface d^{-1}. This demonstrates the sensitivity of total flux estimates to stem height and suggests that upscaling to 2.5 m provides a conservative estimate for these systems. Although logistically challenging, future measurements extending beyond 2.5 m would help better constrain the full contribution of ghost forest stem emissions to the ecosystem CH_4 budget."

Line 337: Check this 8614 number, it looks like it is reported as 8613 in Table 3

Thank you, corrected to 8614.

Figure 8: I suggest keeping the color scale the same across the two panels so that the reader can more easily compare the magnitude of methane concentrations across the two seasons.

Great suggestion, is the same colour scale for both campaigns now.

Line 404: change "resulting" to "resulted"

Amended.

Line 424: Avoid switching the topic to methane mid paragraph. The topic sentence of the paragraph suggests that the paragraph should be focusing on carbon dioxide.

As mentioned above, we removed the discussion around CO_2 throughout.

Line 451: Did you present your SOD results in the results section?

Yes. They are presented in Table 2 and Results section 3.3.

Table 5: Can you add the age of the forest to this table? And perhaps a description of the ecosystem?

We added the year of the death of the trees and a short ecosystem description into the table 5.

Publication	Height measurements *n* (cm)	Tree death (year)	CH_4		Ecosystem description	Stem concentration
			Average µmol m^{-2} d^{-1}	Highest µmol m^{-2} d^{-1}		Highest Average CH_4 ppm
Carmichael and Smith (2016)		~2007			Temperate coastal ghost forest	104 ±19
Covey et al. (2016)		Unknown.			Dead wood in temperate forest	286.4 ±148
Carmichael et al. (2018)	2 (10, 60)	~2007	599 ± 150	1044	Temperate coastal ghost forest	78
Jeffrey et al. (2019)	4 (10, 40, 80, 170)	~2015	249 ± 41.0	4035	Tropical dead mangroves	64,056
Martinez et al. (2021)	1 (60)	~2007	449 ± 135	4644	Temperate coastal ghost forest	
Martinez et al. (2022)		~2007			Temperate coastal ghost forest	904 ±415
Carmichael et al. (2024)	1 (30, 60, 120)	~2007	314 ± 224	763	Temperate coastal ghost forest	23.7 ± 7.5
Sakabe et al. (2025)	3 (30, 80, 130)	~2013 ~2020		2.505	Temperate wetland forest	
Our study	3 (20, 100, 180)	~2011	465 ± 76	8614	Subtropical reservoir ghost forest	51,086 ± 10,127

Line 512: “One tree” not “On tree”

Corrected

Line 547: add “and it” before “therefore”

Amended.

Line 591: “We” not “I”

Corrected.

References

Abril, G., Parize, M., Pérez, M. A. P., and Filizola, N.: Wood decomposition in Amazonian hydropower reservoirs: An additional source of greenhouse gases, J. South A. E. Sci., 44, 104-107, https://doi.org/10.1016/j.jsames.2012.11.007, 2013.

Carmichael, M. J. and Smith, W. K.: Standing Dead Trees: a Conduit for the Atmospheric Flux of Greenhouse Gases from Wetlands?, Wetlands, 36, 1183-1188, https://doi.org/10.1007/s13157-016-0845-5, 2016.

Carmichael, M. J., Helton, A. M., White, J. C., and Smith, W. K.: Standing Dead Trees are a Conduit for the Atmospheric Flux of CH4 and CO2 from Wetlands, Wetlands, 38, 133-143, https://doi.org/10.1007/s13157-017-0963-8, 2018.

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Covey, K. R., Bueno de Mesquita, C. P., Oberle, B., Maynard, D. S., Bettigole, C., Crowther, T. W., Duguid, M. C., Steven, B., Zanne, A. E., Lapin, M., Ashton, M. S., Oliver, C. D., Lee, X., and Bradford, M. A.: Greenhouse trace gases in deadwood, Biogeochemistry, 130, 215-226, https://doi.org/10.1007/s10533-016-0253-1, 2016.

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Sø, J. S., Sand-Jensen, K., and Kragh, T.: Self-Made Equipment for Automatic Methane Diffusion and Ebullition Measurements from Aquatic Environments, J. Geophys. Res. G: Biogeosci., 129, https://doi.org/10.1029/2024jg008035, 2024.

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Sakabe, A., Liu, Z., and Kosugi, Y.: Methane Emissions from the Stems of Living and Dead Chamaecyparis obtusa Sieb. Et Zucc, Wetlands, 45, 54, https://doi.org/10.1007/s13157-025-01941-9, 2025.

Reviewer 2

We would like to thank reviewer 2 for they helpful comments and questions, as well as the time they put into reviewing our submission. The changes proposed have helped to improve the manuscript and we are thankful for those suggestions.

Dittmann et al ask whether ghost forests are source of methane from reservoir. This is an important question considering the role of natural wetlands in the global methane budget and its implication in the rise of atmospheric methane

The quality of the manuscript can be improved. There is a difference between a student thesis, even an excellent one, and a scientific article, and guidance is needed to transform the former into the latter.

1. The introduction does not present any clear research questions or hypotheses, which would be useful for structuring the discussion and writing the conclusion, as well as highlighting the originality of this research. The section "4.4 Comparison with other studies" would benefit from better integration by using these elements to discuss the results of the present study.

   We added three key research questions and have integrated these into the discussion and conclusion (Lines 90-93).

   1. What are the drivers and magnitude of ghost forest tree stem CH_4 fluxes and how do they compare with previous studies?

   2. Do reservoir CH_4 emissions increase along an organic matter deposition gradient?

   3. What is the contribution of ghost forest trees to the overall ecosystem CH_4 flux?

   Also due to comments of reviewer one we restructured the discussion section and moved 4.3 to 4.1, to start the discussion with the measured flux overview. Additionally, we added the research question answers into the sections (Lines 450 onwards).

   Q1: *"Answering research question 1, all trees emitted CH_4 during both campaigns and from all three measured stem heights. …"*

   We tried to better incorporate section 4.4 into the manuscript, using Q1 as well as adding to the beginning of the section (Lines 617-622):

   "This is the first study measuring CH_4 fluxes from ghost forest trees inside a reservoir. Ghost forests can originate from diverse forest types and due different causes. Due this heterogeneity, ghost forest tree CH_4 fluxes are still relatively unexplored. Therefore, further measurements across different environments are needed to compare with our reservoir observations and better constrain the potential range of CH_4 emissions from ghost forest tree stems. Compared to those previous ghost forest tree CH_4 studies, our average CH_4 stem fluxes were within the reported range (Table 5) addressing research question 1."

Q2 (Lines 538-542): "*CH_4 dynamics showed notable differences among the three investigated sites, suggesting site specific factors driving CH_4 fluxes. Although temperature is a key factor, it is not the only factor influencing CH_4 concentration and fluxes. Site orientation and adjacent bathymetry affect both the amount of sun and wind, in turn influencing site physicochemical conditions. The South site consistently showed the highest ebullition and tree stem fluxes between all three sites, confirming research question two.*"

Q3 (Lines 668-669): "*Addressing research question 3, ghost forest tree stem CH_4 contributions of 15% and 14% during winter and summer, respectively, suggest a persistent, and substantial CH_4 source.*"

2. CO2 is a "ghost" in the manuscript. It appears neither in the title nor in the state-of-the-art review, only once in the abstract but without any results, and in the first and last paragraphs of the introduction but without justification. It is found in several places in the "Methods" section, except for the subsection titles, and almost everywhere in the "Results" section, but only in certain parts of the discussion, which focuses primarily on CH4. Just because an analyzer can measure the concentrations of two gases does not mean that both gases must necessarily be mentioned. If the CO2 flux is significant, it should be introduced and discussed more thoroughly. Otherwise, it can simply be omitted.

   Thank you for bringing this to our attention. As reviewer #1 also raised concerns around the inconsistent use of CO_2 data, we have decided to focus the manuscript on CH_4 and have removed CO_2 from the main manuscript. We mention that we did use CO_2 as proxy for the quality of tree stem chamber seal integrity, now including methods details at lines 200-201:

   "*The CO_2 flux linear regression r^2 values were used as a proxy for assessing air-tight chamber seals, especially in lower CH_4 fluxing trees.*"

   We also mentioned at line 180 that the CO_2 raw flux data is reported within the data uploaded and as figures in the supplementary data (Fig S4-S6).

3. δ13C was measured but the results are missing, while they seem to be discussed. Or is the discussion based on published data (Martinez et al., 2022)? Clarification is needed.

   We measured $\delta^{13}C$, however this will be included into a subsequent publication therefore it has been removed at all instances. Discussion of $\delta^{13}C$ results from Martinez et al., (2022) are still included as they are used to draw conclusions that tree stem methane may originate from in the soil.

4. It is unclear how diffusive and ebullitive fluxes were separated. Ebullition can occur in the floating chambers that were used for measuring diffusive fluxes. In this case, the ebullitive fluxes are underestimated and the diffusive fluxes are overestimated. This is a critical point as ebullitive flux is the dominant CH4 source.

   We agree this was not clear and was also raised by Reviewer #1. The diffusive flux and total flux (i.e. ebullition + diffusive) estimates were measured separately.

For the diffusive fluxes we monitored the CH_4 concentration in a floating chamber connected to a portable greenhouse gas analyser, allowing us to observe real time data of each flux. Our ebullition chambers float above the water surface as transects and therefore capture the ebullitive and diffusive flux at the same time, similar to prior studies using the same approach (Hoffmann et al., 2017; Sø et al., 2024). We sample the headspace of these floating chambers every 24 hours (to limit gas diffusion back into the water). By subtracting the average diffusive flux from the 24h total flux, we estimate the net ebullitive flux.

We have now made this method clear in the manuscript and have added the relevant citations and text as follows (Lines 207-209):

"*As our chambers measured total flux (i.e. diffusive + ebullitive), the average diffusive flux was subtracted from each ebullition chamber to estimate the ebullitive flux, similar to prior studies using the same approach (Hoffmann et al., 2017; Sø et al., 2024)*"

5. Missing information in the Methods sections are listed as minor comments below. Overall, provide references for each method and each formula.

   These have been added.

6. It seems to be implied that the word "forest" is being used as a synonym for "tree," which is imprecise. Why not systematically replace "ghost forests" with "ghost forest trees" and "ghost forest stems " with "ghost forest tree stems "throughout the text, including the title?

   Thank you for this suggestion, we have made those changes to make it clearer.

Additional minor comments:

lines 86-87: This sentence is unnecessary, somewhat irrelevant.

Removed.

lines 102-103: do not discuss the results in the method section.

I removed the results part from the sentence, but keep details about the river inflow in the reservoir to make it clear this process is happening.

line 119: what means "phosphorus samples". Is it soil samples for phosphorus determination.

Yes. Now amended as '*Soil samples for phosphorus determination...*'

line 126: What is the function of "Hach, LDO101" in addition to its commercial reference?

I added that it's the dissolved oxygen sensor as:

'...concentrations of dissolved oxygen at 0 and 48 hours using a luminescent/optical dissolved oxygen sensor (Hach, LDO101).'

line 137: δ13C in what?

Reference to $\delta^{13}C$ have been removed as per earlier comments

line 140: provide details regarding the calculation of dissolved concentration from headspace concentration

This is now Eq. 1. We moved that equation into 2.4 to make it clearer (Lines 153-159):

"...and then corrected for the dilution with ambient air. according to Eq. 1 (De La Paz et al., 2021):

$$CH_{4\,\text{w sample}} = \frac{CH_{4\,p}}{V_w}\left(KV_w + \frac{1}{RT}V_V\right)$$

where Vw is the volume of the water used, K is the solubility coefficient, R is the universal gas constant (8.205 × 10−5 $m^3.atm^{-1}.K^{-1}.mol^{-1}$), Tair is the air temperature (°K), Vv the volume of the headspace, $CH_{4\,p}$ is the CH_4 concentration (ppmV) measured with the SSIM CRDS."

line 142: The floating bubble traps should be described (dimension, geometry …)

Added in lines 163-164:

"The chamber diameter was 41cm and 12.5 cm height. with a total volume of 12.9 L and surface area of 0.132 m2."

line 176: the unit nmol/s is better than µmol/h for CH4 flux

We use $\mu mol/m^2/d$. We argue our fluxes are high enough to use µmol rather than nmol/s and all three flux pathways are in the same unit. That is the unit we use in our working group.

line 189: a line break is needed

Done

line 198: a line break is needed

Done

lines 192-212: rewrite with a better structure to avoid duplication

Good point, thank you. We deleted duplication and restructured it a bit as follows (Lines 191-235):

"The tree was divided into three radial bands and calculated as follows (Eq. 4) similar to Jeffrey et al, 2023:

$F_t = \int_0^{2.5} (c * h * F)$ *Eq. 4*

where F_t is the flux per tree up to 2.5 m (µmol day^{-1}), c is the tree circumference (m), F is the in situ measured gas flux rate for that height (µmol m^{-2} d^{-1}) and h is the height of each band (m). The height per band was the same as the height of the chamber plus half the distance to the next chamber, or the distance to either end of 2.5 m.

The density of ghost forest trees (trees ha^{-1}) was estimated from duplicate 100 m^2 plots in the Northern site and four 100 m^2 plots in the South and Mid sites. Tree heights were measured during May 2025 using a laser range finder (Nikon, Forestry Pro). The overall ecosystem CH_4 flux from trees for each site was calculated using Eq. 5:

$F_{tol} = xF_t * d * c$ *Eq. 5*

where F_{tol} is the total tree flux per ha (mol d^{-1}), xF_t is the average tree flux per site, d is the tree density (trees ha^{-1}) and c is the conversion factor from µmol d^{-1} to mol d^{-1}. To upscale the diffusive and ebullition flux inside the ghost forest area, the sum of the tree basal surface areas was subtracted from the total aquatic surface area. The remaining aquatic surface area was multiplied by the average areal diffusive and ebullition CH_4 flux rates per site."

line 226: The Shapiro-Wilk normality test is used to reject the null hypothesis of normality when $P<0.05$. The way this is written can be confusing. Furthermore, it is important to note that, like any other statistical test, it is designed to reject the null hypothesis, not to accept it. In other words, when the p value is below the threshold (e.g., 0.05, a commonly used value), the null hypothesis (here, normality) can be rejected. Conversely, when it is above this threshold, it is statistically incorrect to conclude that the null hypothesis is confirmed. This could just as easily mean that the test lacks power to reject it, and a few more repetitions would suffice. It is better to visually examine the data distribution rather than using a normality test. Also note that heteroscedasticity is probably more problematic than non-normality.

We agree this was poorly written, so have now clarified it. We did use Q-Q plots to look at it as well, we added details now as follows:

"The Shapiro-Wilk normality test and Q-Q visualisation were used to test for normality."

line 335: Do you mean "All trees at all stem heights"?

Corrected.

lines 432-433: Add references on the influence of orientation and bathymetry and explain how orientation and bathymetry account for the accumulation of organic carbon. Delete the sentence unless the link between this sentence and the following can be clarified.

Deleted

line 466: affects?

Yes, changed it.

line 479: BOD in full at first use

Corrected. This should have been SOD (soil oxygen demand) - which was defined at first instance.

lines 486-487: I agree that your “summer results do not necessarily point to a wood source”, but I would expect a less wordy explanation that it “rather show the complexity of tree stem fluxes in such a dynamic system”.

We combined it with the previous sentences to make it less wordy (Lines 483-485:

“We acknowledge that further factors, like wood water content or changing internal CH_4 production and oxidation likely also played a role. These factors complicate our system and lead us to the conclusion, that our summer results do not necessarily point to a wood source.”

line 566: 15% is maybe not so crucial, but still substantial (line 551)

Amended

All figure and table captions: add the number of replicates when it is missing and explain what the plus/minus is referring to.

Added.

References

Hoffmann, M., Schulz-Hanke, M., Garcia Alba, J., Jurisch, N., Hagemann, U., Sachs, T., Sommer, M., and Augustin, J.: A simple calculation algorithm to separate high-resolution CH4 flux measurements into ebullition- and diffusion-derived components, Atmospheric Measurement Techniques, 10, 109-118, https://doi.org/10.5194/amt-10-109-2017, 2017.

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