

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 2

We would like to thank reviewer 2 for their 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 CH₄ fluxes and how do they compare with previous studies?

2. Do reservoir CH₄ emissions increase along an organic matter deposition gradient?

3. What is the contribution of ghost forest trees to the overall ecosystem CH₄ 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 CH₄ 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 CH₄ 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 CH₄ 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 CH₄ emissions from ghost forest tree stems. Compared to those previous ghost forest tree CH₄ studies, our average CH₄ stem fluxes were within the reported range (Table 5) addressing research question 1.”

Q2 (Lines 538-542): *“CH₄ dynamics showed notable differences among the three investigated sites, suggesting site specific factors driving CH₄ fluxes. Although temperature is a key factor, it is not the only factor influencing CH₄ 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 CH₄ contributions of 15% and 14% during winter and summer, respectively, suggest a persistent, and substantial CH₄ 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 CO₂ data, we have decided to focus the manuscript on CH₄ and have removed CO₂ from the main manuscript. We mention that we did use CO₂ as proxy for the quality of tree stem chamber seal integrity, now including methods details at lines 200-201:

“The CO₂ flux linear regression r^2 values were used as a proxy for assessing air-tight chamber seals, especially in lower CH₄ fluxing trees.”

We also mentioned at line 180 that the CO₂ raw flux data is reported within the data uploaded and as figures in the supplementary data (Fig S4-S6).

3. $\delta^{13}\text{C}$ 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 $\delta^{13}\text{C}$, however this will be included into a subsequent publication therefore it has been removed at all instances. Discussion of $\delta^{13}\text{C}$ 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 CH₄ 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; Sørensen 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; Sørensen 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: $\delta^{13}\text{C}$ in what?

Reference to $\delta^{13}\text{C}$ 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):

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

where V_w is the volume of the water used, K is the solubility coefficient, R is the universal gas constant ($8.205 \times 10^{-5} \text{ m}^3 \cdot \text{atm}^{-1} \cdot \text{K}^{-1} \cdot \text{mol}^{-1}$), T_{air} is the air temperature ($^{\circ}\text{K}$), V_v the volume of the headspace, CH_{4p} 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 $\mu\text{mol/h}$ for CH_4 flux

We use $\mu\text{mol/m}^2/\text{d}$. 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:

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

where F_t is the flux per tree up to 2.5 m ($\mu\text{mol day}^{-1}$), c is the tree circumference (m), F is the in situ measured gas flux rate for that height ($\mu\text{mol m}^{-2} \text{d}^{-1}$) 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:

$$F_{\text{tot}} = xF_t * d * c \quad \text{Eq. 5}$$

where F_{tot} 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 $\mu\text{mol d}^{-1}$ 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 CH₄ 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, 10.5194/amt-10-109-2017, 2017.

Martinez, M., Ardón, M., and Carmichael, M. J.: Identifying Sources and Oxidation of Methane in Standing Dead Trees in Freshwater Forested Wetlands, *Front. Environ. Sci.*, 9, <https://doi.org/10.3389/fenvs.2021.737379>, 2022.

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, 10.1029/2024jg008035, 2024.