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)	CH₄		Ecosystem description	Stem concentration
			Average $\mu mol\ m^{-2}\ d^{-1}$	Highest $\mu mol\ m^{-2}\ d^{-1}$		Highest Average CH₄ 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

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