16, July, 2026

I thank the Reviewer for taking the time to look at our revised manuscript. I have incorporated their minor revisions in the document as described below.

Reviewer

Thank you to the authors for addressing most of my concerns and clarifying numerous details. I still think this is important work, but have some final concerns that need addressing.

L120. Thank you for the added detail on redox depth and the calibration procedure. However, the original question has not been addressed. ZoBell's solution verifies electrode performance but is separate from referencing the values to the standard hydrogen electrode (SHE). Please state explicitly whether the reported redox potentials are given relative to SHE or to the Ag/AgCl reference electrode. If the latter, the values should be converted (adding the temperature-corrected Ag/AgCl offset) or the reference basis should at least be stated clearly so readers can interpret the Eh values in the paper correctly.

Thank you very much for the guidance on this matter, and sorry I misunderstood the first revision. We have now converted our redox potential values (which were relative to the Ag/AgCl reference) to values relative to SHE:

L120: "At each plot, surface (0-2 cm deep) reduction-oxidation potential (3-5 measurements) and soil temperature were measured (HQ 11d, ORP-meter, and HQ2100 ORP-meter, with a MTC101 probe with an Ag/AgCl reference HACH, Pacific). Redox values are reported relative to a Standard Hydrogen Electrode (SHE; Eh) by adding 207 mV (25°C) as recommended by the manufacturer."

Tables and Figures have been modified accordingly to the new Eh values.

L133. Thank you for adding the missing details around flux measurements and the new Table S1. Linearity was assessed only in a single continuously-sampled chamber per plot, with the remaining two-point (0 and 60 min) chambers assuming the same behaviour. A problem is that several of the chambers that can be checked over time are themselves often poorly linear (eg R2 of 0.10, 0.29, 0.52, 0.60, 0.63 in Table S1). If some of the checkable chambers frequently fail, I cannot see how to have strong confidence in the remaining two-point chambers (4 out of 5 measurements per site), where linearity over the full hour is simply assumed. Although the chambers are large in size, 60 min can be a long closure for an open soil system to rest on only one start and one end point. At minimum, this approach must be stated explicitly as a major caveat that linearity could be verified in only one chamber per plot, that at low concentrations some of those were often non-linear, and that most reported fluxes rest on an unverified two-point assumption.

Emissions of CH_4 from freshwater tropical wetlands are commonly nonlinear, as they are emitted through both diffusion and ebullition, which can cause large spikes (e.g., Jeffrey et al. 2019). To address this problem, we aim to sample as many plots as possible within each treatment to reduce spatial variability. Because we are certain that these high values are representative of the conditions of these freshwater wetlands, we did not remove them. However, we agree that there is uncertainty

in the high and low ends of the emissions range; thus, we used median values for our calculations, as the median excludes the highest and lowest measurements. For transparency, we have included all the data as Supplementary Material, highlighting the values that were removed from the final calculations. We have also clarified our criteria acknowledging the limitations:

L150: “The chamber with continuous measurements was checked for linearity using a regression of time for each gas within each plot. Coefficients for each regression are shown in Table S1; chambers with poor fit (R2 < 0.50) were identified at sites with very low N2O fluxes or very low and high CH4 emissions. Because CH4 fluxes in tropical freshwater wetlands are characterised by nonlinearity and spikes due to soil ebullition (Jeffrey et al. 2019), poorly linearly fitted plots were retained in the calculations, acknowledging uncertainty at the upper and lower ends of CH4 emissions. Nevertheless, to remain conservative, we used median values and interquartile ranges, which remove the effect on our final values of very low and very high fluxes.”

Jeffrey et al. 2019. Wetland methane emissions dominated by plant-mediated fluxes: Contrasting emissions pathways and seasons within a shallow freshwater subtropical wetland. Limnology and Oceanography. 64: 1895-1912.

The explanation that poor fits occurred at high-methane sites is also not consistent with Table S1: the only sub-threshold CH4 fit is 'UB' reference with a slope 0.0002 (near-zero), while the high CH4 chambers all fit well.

Please also correct ‘R^2 < 50’ to ‘< 0.5’

We have clarified that the poor fitting was found in very high and very low methane sites:

L151: “Coefficients for each regression are shown in Table S1; chambers with poor fit (R^2 < 0.50) were identified at sites with very low N_2O fluxes, and very low or very high CH_4 emissions.”

Please also report the GC detection limits so it can be established whether these low R2 fluxes are distinguishable from zero. The stated 0.2, 0.5 and 1.5% are accuracies, not precision; please confirm whether any samples fell outside the calibration range.

We have included the precision for the gas analyses.

L143: “Back in the laboratory, the CO2, CH4, and N2O concentrations within each sample were measured in a gas chromatograph (Shimadzu GC-2010 Plus, DESITI, Queensland Government). The analyses had a detection limit of 0.01 ppm and accuracies of 0.2%, 0.5%, and 1.5% for CO_2, CH_4, and N_2O, respectively. ”

My question about ambient samples is not yet addressed, and it is the key QA point for the uptake and near-zero fluxes. Without ambient air sample references your starting concentrations cannot be confirmed as near-atmospheric, nor "uptake" endpoints as sub-ambient rather than drift/contamination.

We have taken ambient air samples in previous studies (e.g., Kavehei et al., 2021), but we found they closely matched our time-zero measurements, so we have removed them from our standard procedures.

Kavehei, E. N Iram, MR Rashti, GA Jenkins, C Lemckert, MF Adame. 2021. Greenhouse gas emissions from stormwater bioretention basins. Ecological Engineering. 159: 106120.

Also methods outstanding: confirmation of the airtight soil seal; and gas volume withdrawn and over-pressurisation (30 mL into evacuated 12 mL Exetainers implies >2× over-pressurisation? which is fine, but state it for clarity).

We used 30 mL syringes, but added only 20 mL of air per evacuated container. We have clarified as follows:

L137: “After deployment, 20 mL of gas samples were collected from the headspace of the chambers using a plastic 30 mL syringe with a 26G needle and transferred to 12mL vacuumed glass containers”

Finally, the method text still reads "after 15 minutes of deployment," contradicting the 0–60 min scheme, so please correct.

What we meant was that we started at time zero, 15 min after deployment. To avoid confusion, we have removed this statement:

L139: “The air samples were collected at 0 and 60 min at all chambers and at 0, 20, 40, and 60 min in one chamber per plot.”