

## General comments

Authors assessed various aspects of GHG dynamics in four urban ponds of either macrophyte or phytoplankton dominated stable states over a 2.5 year period. The authors have produced an impressive and valuable long-term dataset on greenhouse gas (GHG) dynamics in ponds, which notably includes the often-overlooked ebullitive flux and provides insight into various methane pathways. While the data and results are impactful, the manuscript requires major revisions to be considered for publication. First, the writing needs major improvement for clarity and quality, and second, the authors provide insufficient details about methods and statistics, and should reconsider how their gases are presented. Below are my general comments for each section, followed by more specific line comments. I believe these data can make a good contribution to the body of literature on GHG dynamics in urban ponds, and hope these comments will help improve the manuscript.

Reply: We warmly thank the reviewer for the positive evaluation of our work and for the detailed and useful comments for improvement.

## Abstract

The abstract was long and wordy with the results. It may be helpful to be more concise and summarize some of these major findings. I provide examples in specific comments below.

Reply: Biogeosciences does not impose a word limit to abstracts, and the length of the abstract reflects the extensive data-set and density of results. Following, the reviewer's comments we have reduced the size of the abstract by focusing on the impactful results, and have considered the various comments described below by the reviewer.

## Introduction

The introduction has some good elements to it but overall lacks supporting information for much of the content covered in the study. For example, authors compare macrophyte versus phytoplankton dominated systems but only provide background information on the impact of macrophytes to GHGs. One of the most interesting components of the study to me is methanogenic pathways and methane oxidation, which has not been covered in urban ponds to my knowledge. While authors cover GHG fluxes and drivers, no mention of methane oxidation, methanogenic pathways, and their significance are made in the intro and is only briefly mentioned in the concluding paragraph. I'd like to see some supporting information for phytoplankton dominated ponds, methane oxidation, and methanogenic pathways in the intro.

Reply: We have completed the introduction to take account of the reviewer's comments. We have added general information on the impact of phytoplankton on GHGs that now reads L82: "In phytoplankton-dominated lakes, CO<sub>2</sub> concentrations depend in part on the development stage of the phytoplankton, with the growth and peak phases generally coinciding with lower CO<sub>2</sub> concentrations due to intense photosynthesis (Grasset et al., 2020; Vachon et al., 2020). CH<sub>4</sub> emissions have been reported to increase with the concentration of chlorophyll-*a* (Chl-*a*) in phytoplankton-dominated lakes (DelSontro et al., 2018; Borges et al., 2022)." and L93: "The production of N<sub>2</sub>O predominantly occurs through microbial nitrification and denitrification that depend on DIN and O<sub>2</sub> levels (Codispoti and Christensen, 1985; Mengis et al., 1997). Competition for DIN between primary producers and N<sub>2</sub>O-producing microorganisms can impact N<sub>2</sub>O production. Additionally, the transfer of labile phytoplankton organic matter to sediments fuels benthic denitrification. Combined, these two processes could explain that some lakes can act as sinks of N<sub>2</sub>O under elevated Chl-*a* concentrations (Webb et al., 2019; Borges et al., 2022)."

We have also added a paragraph on the different pathways of methanogenesis in sediments and methanotrophy in the water column at L119: "The two primary metabolic pathways for CH<sub>4</sub> production in sediments by methanogenic archaea are the fermentation of acetate (acetoclastic pathway) and the reduction of carbon dioxide by H<sub>2</sub> (hydrogenotrophic pathway) (Whiticar et al., 1986; Conrad, 1989). CH<sub>4</sub> produced by these two pathways exhibits distinct <sup>13</sup>C/<sup>12</sup>C ratios (δ<sup>13</sup>C-CH<sub>4</sub>) (Whiticar et al., 1986) and can be used to discriminate which pathway is dominant. When CH<sub>4</sub> diffuses from the sediment to the water column, it can be oxidized by methanotrophic bacteria who preferentially consume CH<sub>4</sub> with <sup>12</sup>C over <sup>13</sup>C, resulting in an increase of δ<sup>13</sup>C-CH<sub>4</sub> of the residual CH<sub>4</sub> in the water column (Bastviken et al., 2002). Fractionation models then allow estimating methane oxidation (MOX) from measurements of δ<sup>13</sup>C-CH<sub>4</sub> of dissolved CH<sub>4</sub> in the water column. Bastviken et al. (2008) report that

30 to 99% of the CH<sub>4</sub> produced in sediments of freshwater lakes can be removed by MOX that is as a significant CH<sub>4</sub> sink in these water bodies. MOX is known to be inhibited by light (Dumestre et al., 1998) and increases with the presence suspended particles (Abril et al., 2007) so that MOX might vary between clear and turbid waters (Morana et al., 2020).”

In the closing paragraph the authors do not include any objectives or predictions/hypotheses, but rather focus on some of the methods. I strongly suggest focusing less on methods and including objectives and predictions/hypotheses to help guide readers.

Reply: The reviewer is right, and we have added the objectives and working hypothesis at the conclusion of the introduction. Text now reads L137: “We test the hypothesis that the two alternative states in shallow lakes (a clear-water state dominated by macrophytes, or a turbid-water state dominated by phytoplankton) drive differences in the CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O dissolved concentration and diffusive emissions from the four studied artificial ponds, that have similar depth, surface area, and catchment urban coverage, and that mainly differ by the phytoplankton-macrophyte dominance. We also test the hypothesis that the two alternative states in shallow lakes drive differences in the ebullitive CH<sub>4</sub> emissions, water column MOX, and sedimentary methanogenesis pathway (acetoclastic or hydrogenotrophic) in the four studied ponds. The final objective of the present work is to determine the relative contribution of CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O to the total GHG emissions in CO<sub>2</sub>-eq and to test the hypothesis that the relative contribution of each GHG differs according to the two alternative states in shallow lakes.”

## Methods

The methods section requires some reorganization and lacks a lot of details.

The statistics section is grossly lacking in detail and what methods were used appear concerning, but potentially due to no explanation of the approaches used. Authors need to explicitly state when/why they use one-way ANOVAs (we use one-way ANOVAs to test for the effect of X on Y and the effect of A on B) and linear or exponential regression. Exponential regressions were used but no mention was made in methods. I also don't think that one-way ANOVA is appropriate where it is used. First, if your analyses are including all data over time, and there are repeated observations from the same four sites over time, that is a case of pseudoreplication. I suggest looking into generalized linear mixed effects models (GLMM) to account for time as a repeated measure. Second, as an example looking at Table S6 (a bit hard to interpret), I think what I'm seeing are pairwise comparisons for one-way ANOVAs that looked at the effect of pond and season on a variable listed in a column? (i.e., chl-a ~ pond + season)? If so, I think these are instead two-way ANOVAs, and the type of pairwise comparison needs to be stated. Table S3 suggest PERMANOVA was used but again, this is not clear. Stats for pairwise comparisons are provided but nothing for the model itself. Degrees of freedom would be a helpful term to report along with the model stats, not just pairwise stats.

Reply: The reviewer is right that the statistics section lacked detail on our methods. We first examined the relationships between environmental variables and GHGs on a pond-by-pond basis, then looked for general relationships across the dataset using Pearson's coefficient and R<sup>2</sup> on log-transformed data. We've updated the statistical table to include exact p-values and degrees of freedom and added supplemental figures to visualize these relationships. After looking at the relationships between variables, we looked at the differences between ponds, and in particular between clear and turbid ponds. To do so, we performed a two-way repeated measures ANOVA with Tukey's HSD post-hoc tests to compare measurements between ponds and seasons, addressing pseudo-replication with the repeated measures. Graphs now display significances, and we've revised the statistical tables for clarity, presenting ANOVA results followed by Tukey HSD tests, and adding degree of freedom in the tables.

We performed LMMs, setting ponds as random effects and environmental variables as fixed effects, following Ray and Holgerson (2023), testing all combinations of different models and keeping only the best model for each GHG. The results are similar to those we obtain with Pearson coefficients. However, we lose some information we find relevant, in particular relationships intrinsic to the ponds, such as pCO<sub>2</sub> explained by Chl-*a* in a turbid pond, or the positive relationship with DIN and SRP in some ponds, which demonstrate the general control of

CO<sub>2</sub> by biological activity. We therefore prefer to keep the explanation of GHG relationships using Pearson coefficients.

We added information on the statistical methods used in the material and methods section. Text now reads L307: “Statistical analysis was conducted with R version 4.4.1. Pearson's linear correlation coefficients and the  $r^2$  coefficient were used to assess relationships between log-transformed variables within each pond and across the dataset, to identify potential pond-specific and overall direct relationships between variables and GHGs. Statistical significance was determined using Fisher's F test and the associated  $p$ -value. This approach was also applied to study the relationships between  $\delta^{13}\text{C-CH}_4$ , FOX and MOX with Chl- $a$  and TSM. To assess the impact of Chl- $a$  concentration, macrophyte cover in summer, water depth, and lake surface area on diffusive and ebullitive CH<sub>4</sub> fluxes, the ratio of ebullitive CH<sub>4</sub> to total CH<sub>4</sub> flux, and CO<sub>2</sub> and N<sub>2</sub>O fluxes, both linear and quadratic relationships were applied to log-transformed averaged data. This approach allowed for the observation of trends between explanatory and dependent variables. For N<sub>2</sub>O fluxes, additional explanatory variables included NO<sub>2</sub><sup>-</sup>, NO<sub>3</sub><sup>-</sup>, NH<sub>4</sub><sup>+</sup>, and DIN concentrations.

A two-way repeated measures analysis of variance (ANOVA) was used to test for differences in categorical variables, with the four seasons and the four ponds serving as independent factors, pond was set as a random effect to account for repeated measurements. A one-way repeated measures ANOVA was used to test for differences in  $\delta^{13}\text{C-CH}_4$  from “perturbed sediments” with the four ponds serving as independent factors. After conducting an ANOVA and establishing significant differences among at least two groups ( $p < 0.05$ ), Tukey's Honestly Significant Difference (HSD) post-hoc test was employed to perform pairwise comparisons across all groups. Statistical outcomes are visually represented on boxplots, where upper- and lower-case letters are used to denote significant differences ( $p < 0.05$ ). Different lower- and upper-case letters indicate significant differences between groups.”

On another note, I can understand using ANOVA to see significant differences between sites for water chemistry variables (chl- $a$ , TSM, %O<sub>2</sub>), but later on (e.g., Figure 8, Figure 10, Figure 12) linear regressions are used to test for effects of environmental variables on GHGs, which is redundant. While I don't think it is wrong to use multiple individual regressions to test for the effect of each environmental variable on a gas, multiple regression models may be more informative, and again, you can account for the pseudoreplication of repeated measures over time. Last suggestion here, the point of the paper is to look at differences between macrophyte versus phytoplankton dominated ponds. Have authors tried grouping by stable state type (macrophyte or phytoplankton), and testing for the effect of that? Two sites per level might not be sufficient enough but curious if this was considered. If you went this route and used a GLMM (for example), perhaps individual site could be set as the random effect.

Reply: We believe that the results are not redundant, and we preferred to keep the figures 8, 10 and 12 as they are. The purpose of Figure 8 is to highlight that it is either the macrophytes, the phytoplankton, or a combination of both that explain the variations in diffusive and ebullitive CH<sub>4</sub> fluxes, and the figure presents this concisely. It also shows that the usual predictors of CH<sub>4</sub> fluxes (surface area and depth) do not explain CH<sub>4</sub> fluxes in the four studied ponds. Figure 10 has been revised to present the differences between the isotopic signatures of CH<sub>4</sub> of perturbed sediments in the form of boxplots. Figure 12 demonstrates that methanotrophy increases along a turbidity gradient with increasing Chl- $a$  and TSM, and presenting the results in this format effectively illustrates our points and supports the text.

Further, I have concerns for how gas concentrations/quantities are presented and equilibrium saturation is calculated. Why present CO<sub>2</sub> as a partial pressure, methane as a concentration, and N<sub>2</sub>O as a percent saturation compared to equilibrium?? I strongly advice all three gases be reported in comparable molar units. In addition to molar concentration, authors should report their deviation from equilibrium, either as a % (like N<sub>2</sub>O. but values will likely be too high for CH<sub>4</sub>), or the factor of super/under saturation, and be consistent for all gases (i.e., the concentration of CO<sub>2</sub> was X  $\mu\text{mol/L}$  and 12-fold supersaturated compared to equilibrium). Deviations from equilibrium are more informative for biological changes to gas concentrations. Alternatively, there is so much information in this manuscript that authors should consider removing results for gas concentrations altogether and focus on air-water fluxes, as some seasonal patterns and environmental drivers appear somewhat similar between concentrations and gases.

**Reply:** These units are used in topical literature. pCO<sub>2</sub> is usually expressed in ppm and readers can easily determine if the values are above or below atmospheric CO<sub>2</sub> of about 400 ppm. In topical literature CH<sub>4</sub> is reported in μmol/L or nmol/L, and the values were systematically above saturation. For N<sub>2</sub>O, given that the values oscillate around saturation, we used percentage of saturation.

But we agree that the mix of different units might be confusing, although used frequently in the publications from our group:

- Borges AV et al. (2019) Biogeosciences, 16, 3801-3834, <https://doi.org/10.5194/bg-16-3801-2019>
- Borges AV et al. (2022) Sci Adv 8, eabi8716, 1-17, <https://doi.org/10.1126/sciadv.abi8716>
- Borges AV et al. (2015) Nat Geosci 8, 637-642, <https://doi.org/10.1038/NGEO2486>
- Borges AV et al. (2023) J Great Lakes Res 49, 229-245, <https://doi.org/10.1016/j.jglr.2022.11.010>
- Chiriboga G & AV Borges (2023) Communications Earth & Environment, 4, 76 <https://doi.org/10.1038/s43247-023-00745-1>
- Bauduin et al. (2024) Water Research, 253, 121257. <https://doi.org/10.1016/j.watres.2024.121257>
- Chiriboga, G et al. (2024) Aquatic Sciences, 86(2), 24. <https://doi.org/10.1007/s00027-023-01039-6>

We recently published a companion paper on GHGs dynamics in Brussels' urban ponds based on an independent data-set using units of CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O used here (Bauduin et al. 2024; <https://doi.org/10.1016/j.watres.2024.121257>); if readers want to compare the results and conclusions from both papers, it is preferable that the units are consistent.

Please note that the full data-set is publically available, so the readers can re-use the data in their preferred units.

#### Results/discussion

This sounds more like a results section than a combined results and discussion section. I recommend keeping results and discussion separate. I provide specific comments up to some of the results, as I imagine results reporting will change when statistics are improved, and discussion will change when these sections are split apart.

**Reply:** We opted to keep a unified “Results and Discussion” section after thoroughly evaluating how to best present our extensive and varied dataset. We found that integrating the results and discussion into one section was more effective than separating them. This approach allows us to present the data in a logical sequence, beginning with basic variables (meteorological data and dissolved concentrations), advancing to CH<sub>4</sub> dynamics (ebullition and MOX), and concluding with a comprehensive analysis of total emissions in CO<sub>2</sub> equivalents. We believe this format provides a coherent and accessible narrative for readers. The results section has not been changed, but the related figures and text have been revised to address the reviewer's specific comments regarding the redundancy of certain sentences and the results presented in some figures.

#### Conclusion

Major results should be broadly summarized here but the significance of the work should also be included. If authors include predictions, they can be circled back on here as well.

**Reply:** We have now listed the objectives and hypotheses of the paper at the end of the “Introduction” section, and we feel that the Conclusions section addresses these objectives.

#### Specific comments

##### ABSTRACT

Line 8-9: Suggest rewording as “...but it is unclear if these two states affect the emission of greenhouse gases carbon dioxide (CO<sub>2</sub>), methane (CH<sub>4</sub>), and nitrous oxide (N<sub>2</sub>O) to the atmosphere.”

**Reply:** Text was modified and now reads L7: “Shallow ponds can occur either in a clear-water state dominated by macrophytes or a turbid-water state dominated by phytoplankton, but it is unclear if and how these two states affect the emission to the atmosphere of greenhouse gases (GHGs) such as carbon dioxide (CO<sub>2</sub>), methane (CH<sub>4</sub>) and nitrous oxide (N<sub>2</sub>O).”

Line 9-12: Suggest rewording to something like the following and including “fluxes”: “We measured the saturation and air-water flux of CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O gases, and ancillary variables 46 times over 2.5 years in four urban ponds in Brussels, Belgium: two clear-water macrophyte dominated ponds and two turbid-water phytoplankton dominated ponds.”

Reply: Text was modified and now reads L10: “We measured on 46 occasions over 2.5 years (between June 2021 and December 2023) the dissolved concentration of CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O from which the diffusive air-water fluxes were computed, in four urban ponds in the city of Brussels (Belgium): two clear-water macrophyte-dominated ponds (Silex and Tenreuken), and two turbid-water phytoplankton-dominated ponds (Leybeek and Pêcheres).”

Line 12-15: Here and throughout, I suggest authors use first person instead of passive voice. I reword the next two sentences to include the objective up front and change to passive voice. I also include the method for ebullitive fluxes: “To quantify CH<sub>4</sub> ebullitive fluxes we conducted 8 bubble trap deployments totaling 48 cumulated measurements. To characterize methanogenic pathways (acetoclastic or hydrogenotrophic) and quantify water column methane oxidation (MOX) we measured the <sup>13</sup>C/<sup>12</sup>C isotope ratio of CH<sub>4</sub> ( $\delta^{13}\text{C-CH}_4$ ) from bubble traps and sediment bubbles.”

Reply: Text was modified and now reads L16: “CH<sub>4</sub> ebullitive fluxes were measured with bubble traps in the four ponds during deployments in spring, summer, and fall, totalling 48 days of measurements. To characterize methanogenic pathways (acetoclastic or hydrogenotrophic) and quantify water column methane oxidation (MOX) we measured the <sup>13</sup>C/<sup>12</sup>C ratio of CH<sub>4</sub> ( $\delta^{13}\text{C-CH}_4$ ) from gas trapped in the bubble traps, from bubbles deliberately released by the perturbation of the sediments, and in dissolved CH<sub>4</sub> in the water column.”

Line 15-18: These results could be removed from the abstract. Temperature and precipitation are already touched on later when discussing fluxes.

Reply: We have removed these results from the abstract

Line 18: Remove “The sampled”.

Reply: We have removed the two words, text now reads L26: “The turbid-water and clear-water ponds did not differ significantly in terms of diffusive emissions of CO<sub>2</sub> and N<sub>2</sub>O.”

Line 22-23: The sentence beginning with “The temperature sensitivity..” could be removed or combined with previous.

Reply: We reduced the length of the sentence, text now reads L32: “The temperature sensitivity of ebullitive CH<sub>4</sub> fluxes decreased with increasing water depth.”

Line 23-28: These sentences could be combined to reduce wordiness.

Reply: We have reduced the length of this part, text now read L33: “In summer, the  $\delta^{13}\text{C-CH}_4$  values of sediment bubbles indicated that the hydrogenotrophic methanogenesis pathway seemed to dominate in clear-water ponds and acetoclastic methanogenesis pathway seemed to dominate in turbid-water ponds. The  $\delta^{13}\text{C-CH}_4$  values of bubbles traps suggested a seasonal shift from the acetoclastic methanogenesis pathway in spring-summer to the hydrogenotrophic methanogenesis pathway in fall.”

Line 35: I suggest adding a concluding sentence to highlight the implications and usefulness of the results.

Reply: We thank the reviewer for the suggestion, but we think that the present version of the abstract achieves the purpose of an abstract that is to list main results/findings of a paper.

## INTRODUCTION

Line 39-41: This sentence leads me think you are going to further discuss lentic versus lotic GHGs. I suggest replacing with a sentence highlighting estimated GHG emissions from lakes combined to guide the reader into the next sentence about small pond contributions.

Reply: We feel that it is relevant to contextualize the GHG emissions from lentic systems in broader context of inland water emissions, as such the comparison between lentic and lotic systems is relevant.



Line 42: change “could be” to “are” or “can be”

Reply: Text was modified and now reads L57: “The contribution of CO<sub>2</sub> and CH<sub>4</sub> emissions from small lentic water bodies (small lakes and ponds) can be disproportionately high compared to large systems (Holgerson and Raymond, 2016) as small lakes and ponds are the most abundant of all water body types in number (Verpoorter et al., 2014, Cael et al., 2017), and flux intensities (per m<sup>2</sup>) are usually higher in smaller water bodies.”

Line 42: shallow lakes are not always ponds (see Richardson et al. (2023) on defining ponds, DOI: 10.1038/s41598-022-14569-0). Maybe cite Downing (2010; DOI: 10.23818/limn.29.02) to make the point in this sentence for small ponds?

Reply: We agree that shallow lakes are not always ponds, and that are differences in functioning that are indeed discussed in detailed by Richardson et al. (2023) Yet, there is so little information and publications on GHGs from ponds that frequently information from publications in lakes are needed to discuss certain aspects of GHG dynamics in ponds. Text was modified and now reads L57: “The contribution of CO<sub>2</sub> and CH<sub>4</sub> emissions from small lentic water bodies (small lakes and ponds) can be disproportionately high compared to large systems (Holgerson and Raymond, 2016) as small lakes and ponds are the most abundant of all water body types in number (Verpoorter et al., 2014, Cael et al., 2017), and flux intensities (per m<sup>2</sup>) are usually higher in smaller water bodies.”

Line 44-49: Not sure if I would say artificial ponds are “seldom” investigated these days, maybe that the body of literature is growing. Other artificial and/or stormwater pond papers looking at GHGs and carbon inputs: Goeckner et al. 2022 (DOI: 10.1038/s43247-022-00384-y), Ray and Holgerson 2023 (DOI: 10.1029/2023GL104235), and Kalev et al. 2020 for DOC and POC inputs (DOI: 10.1016/j.scitotenv.2020.141773).

Reply: We thank the reviewer for these references. Text was modified and now reads L66: “Among artificial systems, urban ponds are the subject of a growing body of literature (Singh et al., 2000; Natchimuthu et al., 2014; van Bergen et al., 2019; Audet et al., 2020; Peacock et al., 2021; Goeckner et al., 2022; Ray and Holgerson, 2023; Bauduin et al., 2024).”

Line 50: I’m not sure that I agree that urban ponds are mostly in green spaces. If you are referring to a particular region, I would specify that, but this point contradicts what you say in the next sentence that they are surrounded by impervious surfaces.

Reply: The reviewer is right, we have re-worded the sentence and text now read L71:” Urban ponds are generally small, shallow, and usually their catchment consists in majority of impervious surfaces with a smaller contribution from soils (Davidson et al., 2015; Peacock et al., 2021).”

Line 53-54: This sentence is redundant with the sentence on lines 46-48 on C & N inputs. I suggest moving this up to replace that sentence and added a concluding sentence here that highlights a knowledge gap covered in your study.

Reply: We have removed the sentences and added information earlier in the text. Text now read L63: “These higher emissions seem to result from higher external inputs of anthropogenic carbon and nitrogen in artificial systems such as rainfall runoff that brings organic matter and dissolved inorganic nitrogen (DIN), but might also reflect other differences compared to natural systems such as in hydrology (Clifford and Heffernan, 2018).”

Line 55-56: This sentence is a little confusing to me. When you say submerged aquatic primary production, are you referring to the contribution of submerged aquatic vegetation to primary production? If so, I don’t think phytoplankton is typically referred to as submerged vegetation. I would simply say primary production or reorganize the beginning of this paragraph to begin with the alternative stable states.

Reply: Text was modified and now reads L75: “In shallow ponds and lakes, including urban ponds, aquatic primary production is either dominated by submerged macrophytes or by phytoplankton, corresponding to two alternate states (Scheffer et al., 1993).”

Line 57: Indicate which stable state is associated to clear or turbid water.

Reply: Text was modified and now reads L76: “These two alternative states correspond to clear waters (macrophyte-dominated) or turbid waters (phytoplankton-dominated), during the productive period of the year (spring and summer in mid-latitudes).”

Line 58: Macrophytes also impact CO<sub>2</sub> cycling (e.g., in the Theus et al. 2023 you cite in the next sentence). Further, no background is provided for the effect of stable states on CO<sub>2</sub> at all in the introduction. This should be included as it is for CH<sub>4</sub> and N<sub>2</sub>O.

Line 62: Ojala et al. 2011 may also be a relevant paper to check out but they focus on clear versus brown-water lakes (DOI: 10.4319/lo.2011.56.01.0061).

Reply: We have included the suggested references and add information about CO<sub>2</sub> cycling in the two stable states. Text now reads L78: “Submerged macrophytes and phytoplankton regulate CO<sub>2</sub> dynamic directly through photosynthesis that can be more or less balanced by community respiration in the water column. However, it is not clear whether the presence of macrophytes increases or decreases the CO<sub>2</sub> emissions from ponds and lakes. Some studies have shown a decrease of CO<sub>2</sub> emissions with increasing macrophyte density (Kosten et al., 2010; Ojala et al., 2011; Davidson et al., 2015), but other studies showed the opposite pattern (Theus et al., 2023). In phytoplankton-dominated lakes, CO<sub>2</sub> concentrations depend in part on the development stage of the phytoplankton, with the growth and peak phases generally coinciding with lower CO<sub>2</sub> concentrations due to intense photosynthesis (Grasset et al., 2020; Vachon et al., 2020).”

Line 67-70: I would combine these sentences to highlight where positive N<sub>2</sub>O-macrophyte relationships have been reported and save the details for the discussion.

Reply: Text was modified and now reads L101: ” N<sub>2</sub>O emissions has been showed to follow diurnal cycles of O<sub>2</sub> concentrations in areas dominated by submerged macrophytes in Lake Wuliangshuai (China) (Ni et al., 2022) and the seasonal cycle of aboveground biomass of emerged macrophytes (*Phragmites*) in Baiyangdian Lake (China) (Yang et al., 2012).”

Line 71: Authors haven’t described why denitrification and N<sub>2</sub>O are associated. I would either include their association (i.e., that N<sub>2</sub>O can be produced is an intermediate product of denitrification or nitrification), or remove and save this for the discussion.

Reply: The reviewer is right and we have added information about processes leading to N<sub>2</sub>O production. Text now reads L93: “The production of N<sub>2</sub>O predominantly occurs through microbial nitrification and denitrification that depend on DIN and O<sub>2</sub> levels (Codispoti and Christensen, 1985; Mengis et al., 1997). Competition for DIN between primary producers and N<sub>2</sub>O-producing microorganisms can impact N<sub>2</sub>O production. Additionally, the transfer of labile phytoplankton organic matter to sediments fuels benthic denitrification. Combined, these two processes could explain that some lakes can act as sinks of N<sub>2</sub>O under elevated Chl-*a* concentrations (Webb et al., 2019; Borges et al., 2022).”

Line 72: I suggest concluding with a sentence on the significance of quantifying GHG fluxes in macrophyte vs. phytoplankton dominated systems. Further, no background information is provided on GHG dynamics from phytoplankton dominated ponds, of which there is plenty of literature on.

Reply: We have added concluding sentences to the paragraph that now reads L106: “There have been a very limited number of studies investigating systematically how emissions differ between ponds dominated by phytoplankton and those dominated by macrophytes (Harpenslager et al., 2022; Baliña et al., 2023), and none investigating simultaneously CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O emissions including both diffusive and ebullitive components.”

Line 73 / paragraph 3: This paragraph is good. I suggest adding some support from Ray and Holgerson (2023) on the contribution of ebullition to CH<sub>4</sub> fluxes in artificial ponds. Also, you don’t mention anything about methanogenesis or methane oxidation until the closing paragraph, whereas this is a profound and really interesting part of your work! The significance of understanding methanogenic pathways, and methane oxidation, should be included in this paragraph.

Reply: We thank the reviewer for pointing this very interesting paper that we have added in the text that now reads L111: “At annual scale, ebullitive CH<sub>4</sub> flux usually represents more than half of total (diffusive+ebullitive)

CH<sub>4</sub> emissions from shallow lakes (Wik et al., 2013; Deemer and Holgerson, 2021), although the relative contribution of ebullitive and diffusive CH<sub>4</sub> emissions is highly variable seasonally (e.g. Wik et al., 2023; Ray and Holgerson, 2023).”

We also added a paragraph about methanogenesis and methane oxidation after this paragraph with several references. New paragraph L119: “The two primary metabolic pathways for CH<sub>4</sub> production in sediments by methanogenic archaea are the fermentation of acetate (acetoclastic pathway) and the reduction of carbon dioxide by H<sub>2</sub> (hydrogenotrophic pathway) (Whiticar et al., 1986; Conrad, 1989). CH<sub>4</sub> produced by these two pathways exhibits distinct <sup>13</sup>C/<sup>12</sup>C ratios (δ<sup>13</sup>C-CH<sub>4</sub>) (Whiticar et al., 1986) and can be used to discriminate which pathway is dominant. When CH<sub>4</sub> diffuses from the sediment to the water column, it can be oxidized by methanotrophic bacteria who preferentially consume CH<sub>4</sub> with <sup>12</sup>C over <sup>13</sup>C, resulting in an increase of δ<sup>13</sup>C-CH<sub>4</sub> of the residual CH<sub>4</sub> in the water column (Bastviken et al., 2002). Fractionation models then allow estimating methane oxidation (MOX) from measurements of δ<sup>13</sup>C-CH<sub>4</sub> of dissolved CH<sub>4</sub> in the water column. Bastviken et al. (2008) report that 30 to 99% of the CH<sub>4</sub> produced in sediments of freshwater lakes can be removed by MOX that is as a significant CH<sub>4</sub> sink in these water bodies. MOX is known to be inhibited by light (Dumestre et al., 1998) and increases with the presence suspended particles (Abril et al., 2007) so that MOX might vary between clear and turbid waters (Morana et al., 2020). “

Line 81 / paragraph 4: This whole paragraph should be re-written to outline the objectives of this study. To me they were (1) to understand annual variability in saturation/fluxes, (2) characterize and quantify CH<sub>4</sub> cycling pathways (methanogenesis/methanotrophy), and (3) identify drivers of these fluxes/pathways including pond type and environmental variables. Then you can briefly say you collect 2.5 years worth of data (so impressive!) on GHG dynamics and environmental conditions in four urban ponds of differing stable states. I also suggest adding predictions based on supporting information provided earlier in the intro. Then conclude with why the study contributes to the body of lit on urban pond GHG dynamics.

Reply: The reviewer is right. We have reduced the size and briefly talk about the 2.5 years of data. Text now reads L130 : “Here, we report a dataset of CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O dissolved concentrations in four shallow and small urban ponds (Leybeek, Pêcherries, Silex, and Tenreuken) in the city of Brussels (Belgium) (Fig. 1), with data collected 46 times at regular intervals (between June 2021 and December 2023) on each pond. The air-water diffusive fluxes of CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O were calculated from dissolved concentrations and the gas transfer velocity, while the ebullitive CH<sub>4</sub> fluxes were measured with inverted funnels during 8 deployments (totalling 48 days) in the four ponds. The δ<sup>13</sup>C-CH<sub>4</sub> in the sedimentary bubbles and in the water provides additional information on CH<sub>4</sub> dynamics such as the methanogenesis pathway (acetoclastic or hydrogenotrophic) and MOX. We test the hypothesis that the two alternative states in shallow lakes (a clear-water state dominated by macrophytes, or a turbid-water state dominated by phytoplankton) drive differences in the CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O dissolved concentration and diffusive emissions from the four studied artificial ponds, that have similar depth, surface area, and catchment urban coverage, and that mainly differ by the phytoplankton-macrophyte dominance. We also test the hypothesis that the two alternative states in shallow lakes drive differences in the ebullitive CH<sub>4</sub> emissions, water column MOX, and sedimentary methanogenesis pathway (acetoclastic or hydrogenotrophic) in the four studied ponds. The final objective of the present work is to determine the relative contribution of CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O to the total GHG emissions in CO<sub>2</sub>-eq and to test the hypothesis that the relative contribution of each GHG differs according to the two alternative states in shallow lakes.”

## METHODS

Line 98: Did you visit each pond on the say day? What time of day (approximate window) did you sample? If you remove the methodological specifics from the conclusion of the introduction, add the details here about the 46 sampling days and period of time you sampled sites from (June 2021 – Dec. 2023). Also, how would you describe the climate/precipitation of Brussels? These can be added before the sentence here.

Reply: We have removed the methodological specifics from conclusion of the introduction to the material and methods section. Text was modified and now reads L155: “Sampling was carried out from a pontoon in the four ponds on the same day between 9am and 11am, 46 times on each pond between June 2021 and December 2023 at a frequency ranging from one (winter) to three (summer) times per month at a single fixed station in each of the four ponds.”



We also added information about climate in Belgium in Results and Discussions, before discussing seasonal variations of GHG. Text now reads L330: “Belgium has a west coast marine climate with mild weather year-round, and evenly distributed abundant rainfall totalling on average 837 mm annually for the reference period 1991-2020. The average annual air temperature was 11°C, with summer average of 17.9 °C and winter average of 4.1 °C for the reference period 1991-2020.”

Line 98-99. I would separate GHGs and “other variables” here and focus on the “other variables” first. Then move on to GHG sample collection. In any case, how far below the surface did you collect water from?

Reply: We have modified the text to start with the sampling of GHG and finish with the sampling for “other variables” (see next comments), and we add the distance from the surface for the sampling. Text now reads L157: “Water was sampled 5cm below the surface with 60ml polypropylene syringes for analysis of dissolved concentrations of CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O.”

Line 100-103: pCO<sub>2</sub> analytical approach needs to be moved to the same section as CH<sub>4</sub> and N<sub>2</sub>O unless it was a portable analyzer (unclear). When you say headspace approach, are you referring to the headspace equilibrium approach? If you used the same approach following the cited Borges et al. (2019) then I think so? Unclear. If so, more information is needed here for the headspace approach. How much water volume versus headspace volume did you equilibrate? How long did you equilibrate? Did you use N<sub>2</sub> gas as the headspace or ambient air??

Reply: We have added information on GHG measurements and the headspace technique for pCO<sub>2</sub>. The three gases were collected with syringes in the field, but pCO<sub>2</sub> was measured directly in the field with a portable Li-Cor Li-840 infrared gas analyser. Samples for CH<sub>4</sub> and N<sub>2</sub>O were collected in the field and measurements were done after in laboratory. Text now reads L158: “Samples for CH<sub>4</sub> and N<sub>2</sub>O were transferred from the syringes with a silicone tube into 60 ml borosilicate serum bottles (Weathon), preserved with 200 µl of a saturated solution of HgCl<sub>2</sub>, sealed with a butyl stopper and crimped with aluminium cap, without a headspace, samples were stored at ambient temperature protected from direct light prior to analysis in laboratory. The partial pressure of CO<sub>2</sub> (pCO<sub>2</sub>) was measured directly in the field, within 5 minutes of sample collection, with a Li-Cor Li-840 infrared gas analyser (IRGA) based on the headspace technique with 4 polypropylene syringes (Borges et al., 2019). A volume of 30 ml of sample water was equilibrated with 30 ml of atmospheric air within the syringe by shaking vigorously for 5 minutes. The headspace of each syringe was then sequentially injected into the IRGA and a fifth syringe was used to measure atmospheric CO<sub>2</sub>. The final pCO<sub>2</sub> value was computed taking into account the partitioning of CO<sub>2</sub> between water and the headspace, as well as equilibrium with HCO<sub>3</sub><sup>-</sup> (Dickson et al., 2007) using water temperature measured in-situ and after equilibration, and total alkalinity (data not shown). Samples for total alkalinity were conditioned, stored and analysed as described by Borges et al. (2019).”

Line 102: After each cruise? Do you mean when you sampled a pond from the pontoon? I suggest saying “before and after each sampling event”. Also, is the Li-Cor Li-840 a portable gas analyzer? I didn’t think so but now I’m wondering if it is. If so, measurements of CO<sub>2</sub> in the field needs to be explicitly stated and the approach described better.”

Reply: Text was modified and now reads L163: “The partial pressure of CO<sub>2</sub> (pCO<sub>2</sub>) was measured directly in the field, within 5 minutes of sample collection, with a Li-Cor Li-840 infrared gas analyser (IRGA) based on the headspace technique with 4 polypropylene syringes (Borges et al., 2019).” and L170: “The IRGA was calibrated in the laboratory with ultrapure N<sub>2</sub> and a suite of gas standards (Air Liquide Belgium) with CO<sub>2</sub> mixing ratios of 388, 813, 3788 and 8300 ppm.”

Line 104: Change “in” to “into” and “poisoned” to “preserved”.

Reply: Text was modified and now reads L158: “Samples for CH<sub>4</sub> and N<sub>2</sub>O were transferred from the syringes with a silicone tube into 60 ml borosilicate serum bottles (Weathon), preserved with 200 µl of a saturated solution of HgCl<sub>2</sub>, sealed with a butyl stopper and crimped with aluminium cap, without a headspace, samples were stored at ambient temperature protected from direct light prior to analysis in laboratory.”

Line 117: How did you store the gas prior to analysis? Same with other types of samples collected, storage prior to analysis should be included.

Reply: We added this information and text now reads L158: “Samples for CH<sub>4</sub> and N<sub>2</sub>O were transferred from the syringes with a silicone tube into 60 ml borosilicate serum bottles (Weathon), preserved with 200 µl of a saturated solution of HgCl<sub>2</sub>, sealed with a butyl stopper and crimped with aluminium cap, without a headspace, samples were stored at ambient temperature protected from direct light prior to analysis in laboratory.”, L185: “The value of the collected volume of gas was logged, and the gas was transferred immediately after collection to pre-evacuated 12 ml vials (Exetainers, Labco, UK) that were stored at ambient temperature protected from direct light prior to the analysis of CH<sub>4</sub> concentration and δ<sup>13</sup>C-CH<sub>4</sub> in the laboratory.” and L191: “The gas collected in the funnels was stored in pre-evacuated 12 ml vials (Exetainers, Labco, UK) that were stored at ambient temperature protected from direct light prior to the analysis of δ<sup>13</sup>C-CH<sub>4</sub> in the laboratory.”

Line 125: Ok so for CH<sub>4</sub> and N<sub>2</sub>O you collected water, then used the headspace equilibration approach in the lab? If you also used this approach for CO<sub>2</sub> but it was done in the field for CO<sub>2</sub>, this still should be described earlier.

Reply: Text was modified to clarify that pCO<sub>2</sub> was measured directly in the field and that samples for CH<sub>4</sub> and N<sub>2</sub>O were stored and analyzed in laboratory.

Line 130-131: I strongly advice authors to report each gas as both a concentration and some form of their deviation from equilibrium. Authors say reporting pCO<sub>2</sub>, nmol/L of CH<sub>4</sub>, and %N<sub>2</sub>O is “with convention in existing topical literature”, but no references are provided, and I disagree with this approach. Other impactful pond GHG papers focusing on concentrations alone maintain the same units (e.g., Holgerson 2015, DOI: 10.1007/s10533-015-0099-y), and this allows for easier comparison. If N<sub>2</sub>O is presented as a deviation from equilibrium, I think the same should be included for CO<sub>2</sub> and CH<sub>4</sub>, as deviation from equilibrium is insightful for biological changes to these gases. This helps readers understand to what degree the gases are “systematically and distinctly above saturation”.

Reply: We acknowledge that using different units can be confusing. However, the units we have used for CO<sub>2</sub> and CH<sub>4</sub> are standard in topical literature and used in numerous publications from our group. For N<sub>2</sub>O, presenting values as percentages around 100% makes it straightforward to determine whether the pond is acting as a source or sink. We recently published a companion paper on GHGs dynamics in Brussels’ urban ponds based on an independent data-set using units of CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O used here (Bauduin et al. 2024; <https://doi.org/10.1016/j.watres.2024.121257>); if readers want to compare the results and conclusions from both papers, it is preferable that the units are consistent; Please note that the full data-set is publically available, so the readers can re-use the data in their preferred units.

Line 133: How did you calculate the equilibrium solubility of N<sub>2</sub>O in water?? Did you calculate N<sub>2</sub>O solubility using the water temperature at the time you collected samples (based on Henrys law)?

Reply: The reviewer is right and indeed we used the temperature at the time we collected our samples. We have included this information in the text that now reads L222: “The N<sub>2</sub>O concentrations fluctuated around atmospheric equilibrium, so data are presented as percent of saturation level (%N<sub>2</sub>O, where atmospheric equilibrium corresponds to 100%).”

Line 142: I suggest moving this section above the GHG analytical section to improve the flow of methods. (GHG analysis -> GHG calculation)

Reply: We have moved the pre-mentioned section on measurement of other variables above at L201.

Line 154: Is this DIN the sum of NH<sub>4</sub>-N, NO<sub>3</sub>-N, and NO<sub>2</sub>-N? or the sum of the full concentration of each?

Reply: DIN is the sum of concentrations of NH<sub>4</sub><sup>+</sup>, NO<sub>3</sub><sup>-</sup> and NO<sub>2</sub><sup>-</sup> expressed in µmol/L (and not as mg/L). This was clarified in text L212: “Concentration of dissolved inorganic nitrogen (DIN) was calculated as the sum NH<sub>4</sub><sup>+</sup>, NO<sub>2</sub><sup>-</sup> and NO<sub>3</sub><sup>-</sup> concentrations in µmol L<sup>-1</sup>.”

Line 158: Finally I see that CO<sub>2</sub> was measured in the field with the Li-Cor Li-840. This needs to be made clear much early on...

Reply: We have included the information about the Li-Cor Li-840 earlier in the text to clearly specify that we measured pCO<sub>2</sub> directly in the field.

Line 160: Change “on” to “in”.

Reply: Text was modified and now reads L255: “The atmospheric pCO<sub>2</sub> was measured in the field with the Li-Cor Li-840.”

Line 160: Where did you get this value of 1.9 ppm for CH<sub>4</sub>?

Reply: We have taken this value from the NOAA Global Monitoring Laboratory measurements, whose data is available online. Text was modified and now reads L255: “For CH<sub>4</sub>, the global average present day atmospheric mixing ratio of 1.9 ppm was used (Lan et al., 2024).”

Lan, X., K.W. Thoning, and E.J. Dlugokencky: Trends in globally-averaged CH<sub>4</sub>, N<sub>2</sub>O, and SF<sub>6</sub> determined from NOAA Global Monitoring Laboratory measurements [data set]. Version 2024-08, <https://doi.org/10.15138/P8XG-AA10>, 2024.

Line 160-163: I’m a little confused. Because authors use the phrasing “equilibrium with atmosphere for N<sub>2</sub>O” it sounds like equilibrium solubility in water. Because authors also say air mixing ratios here, I think that this is not what authors intend and instead they mean the atmospheric concentration of N<sub>2</sub>O.

Reply: The reviewer is right and we have changed the text to better clarify the sentence. Text was modified and now reads L257: “Atmospheric N<sub>2</sub>O concentration was calculated from the average air mixing ratios of N<sub>2</sub>O provided by the GMD of the NOAA ESRL (Dutton et al., 2017).”

Line 211 / statistics: This statistics section requires much more detail and I’m a little concerned about the statistics overall but this may be because things are hard to follow. I cover my concerns in the general comment above for this section.

Reply: The reviewer is right; we have expanded the description of the statistical tests used (see the response to the general comment above).

## RESULTS & DISCUSSION

Line 237-238: this sentence is redundant with the previous where you already report these values. I would remove and site Figure 3 with the previous sentence.

Reply: We have revised this paragraph to be more concise and explain differences between clear-water and turbid-water ponds. Text now reads L353: “The four studied ponds had significantly different Chl-*a* concentration values during summer, with the Leybeek pond having higher Chl-*a* ( $78.8 \pm 49.5 \mu\text{g L}^{-1}$ ), followed by the Pêcherries pond ( $19.1 \pm 13.7 \mu\text{g L}^{-1}$ ), the Tenreuken pond ( $3.3 \pm 2.4 \mu\text{g L}^{-1}$ ), and the Silex pond ( $1.0 \pm 1.2 \mu\text{g L}^{-1}$ ) (Tukey's HSD test  $p \leq 0.0001$  for each pair of comparisons, Figs. 1, 3). The Leybeek and Pêcherries ponds with higher summer Chl-*a* concentration had turbid-water (summer TSM =  $48.7 \pm 36.2$  and  $13.7 \pm 10.7 \text{ mg L}^{-1}$ , respectively), and undetectable submerged macrophyte cover in summer (Fig. 1, Table S1). The Tenreuken and Silex ponds with lower summer Chl-*a* concentrations had clear-water (summer TSM =  $4.9 \pm 3.2$  and  $4.0 \pm 3.2 \text{ mg L}^{-1}$ , respectively), and a high total macrophyte cover during summer (68 and 100%, respectively, Fig. 1, Table S1). Values of Chl-*a* were higher in summer than in winter in the turbid-water Leybeek and Pêcherries ponds (Tukey's HSD test  $p = 0.0107$  for the Leybeek pond,  $p = 0.0211$  for the Pêcherries pond) related to summer algal blooms. Values of Chl-*a* were higher in winter than in summer in the clear-water Tenreuken and Silex ponds (Tukey's HSD test =  $0.0296$  for the Tenreuken pond,  $p = 0.0056$  for the Silex pond), probably related to competition for inorganic nutrients from macrophytes, with the Silex pond showing lower summer Chl-*a* (Tukey's HSD test  $p < 0.0001$ ), lower summer TSM concentrations (Tukey's HSD test  $p < 0.0001$ ) and higher summer total macrophyte cover compared to the Tenreuken pond (Fig. 1).”

Line 252: Were these in the surface of the water? Would be helpful if the depth of measurements are specific in methods.

Reply: We added the information on the depth of measurement in methods. Text now reads L157: “Water was sampled 5cm below the surface with 60ml polypropylene syringes for analysis of dissolved concentrations of CO<sub>2</sub>, CH<sub>4</sub>, and N<sub>2</sub>O.” and L174: “Water temperature, specific conductivity, and oxygen saturation level (%O<sub>2</sub>) were measured in-situ with VWR MU 6100H probe 5cm below the surface.”

Line 256: The range is from 40 to 13804 ppm but in the methods authors stated that pCO<sub>2</sub> was “systematically and distinctly above saturation level” which was cited at 400 ppm. I’m curious now if authors meant atmospheric CO<sub>2</sub>, of CO<sub>2</sub> dissolved in water, which would differ based on temperature.

Reply: The reviewer is right. However, under-saturation of pCO<sub>2</sub> in water relatively to pCO<sub>2</sub> measured in-situ in ambient air were only observed on 5 occasions out of 187 measurements. We have added a sentence at the beginning of the paragraph to add this information. Text now reads L391: “Undersaturation of CO<sub>2</sub> with respect to atmospheric equilibrium was only observed on five occasions out of the 187 measurements, three times in the turbid-water Leybeek pond in summer (40 ppm on 13 August 2021, 220 ppm on 27 June 2022 and 149 ppm on 13 June 2023), and twice in the clear-water Tenreuken pond in spring and summer (383 ppm on 13 August 2021 and 55 ppm on 2 May 2022).”

Line 257: warmer waters also hold less gas. Including percent or factor of saturation compared to equilibrium may be helpful to understand lower CO<sub>2</sub> concentrations in the summer.

Reply: We agree. Yet, the temperature effect is overshadowed by the enormous range of variations of pCO<sub>2</sub> (40 to 13804 ppm).

Line 261: What kind of model was used for the results in Table 3? Are these results from a PERMANOVA? If so, nothing about a PERMANOVA was included in the stats section. Again, pseudoreplication from repeated sampling over time. There are also linear regressions of these gases over some of these variables in other analyses, which is redundant.

Reply: We have updated the statistics paragraph in the material and methods to better stated tests that was used in the article. In table S3 are reported Pearson coefficient between one GHG and one environmental variable in one pond to investigate relationships among each pond. ANOVA were used to compare one variable between seasons and ponds. The pseudoreplication problem was assessed in the comparisons using a repeated measures two-way ANOVA.

We believe that the relationships presented in the different figures are useful and support the text and the results we present.

Line 270: What do you mean by “sometimes correlated”. As in during certain seasons or only in some ponds?

Reply: We have modified the text that now reads: L410: “In individual ponds, dissolved CH<sub>4</sub> concentration was negatively correlated to precipitation and DIN in the Pêcherries pond (Table S3; Fig S6), and positively correlated to SRP in the Silex pond (Table S3; Fig S4).”

Line 283: Not having explanations for correlations included in models is a good example of why a prior hypotheses are useful. There is already a lot of information in this manuscript so maybe some of these analyses for drivers of gas concentrations are not needed in the first place? Just a thought.

Reply: We feel that these explanations are useful.

Line 286: This makes me wonder if this DIN is the N fraction of inorganic nitrogen forms or their full concentration (i.e., NH<sub>4</sub>-N versus NH<sub>4</sub>) and what the difference would be for results here between either option, but I think the sum of N fraction in the inorganic forms is more appropriate.

Reply: DIN is given as the sum of concentrations of NH<sub>4</sub><sup>+</sup>, NO<sub>3</sub><sup>-</sup> and NO<sub>2</sub><sup>-</sup> expressed in μmol/L (and not as mg/L). This was clarified in text L213.

Line 295: Would be helpful to summarize at the end of this section what the differences in GHGs for macrophyte versus phytoplankton dominated ponds.

Reply: We added a summarizing paragraph to conclude on differences in relationships between GHG and environmental variables observed between clear-water and turbid-water ponds. Text now reads L455: “The relationships between GHG dissolved concentrations and other variables were similar in clear-water macrophyte-dominated ponds and turbid-water phytoplankton-dominated ponds. pCO<sub>2</sub> was positively correlated with precipitation, and dissolved CH<sub>4</sub> concentration was positively correlated with temperature, while no significant correlation was found between %N<sub>2</sub>O and other variables in the four ponds taken individually. The negative correlation between pCO<sub>2</sub> and %O<sub>2</sub> reflected the photosynthesis-respiration balance independently from the community driving aquatic primary production (macrophytes in clear-water ponds and phytoplankton in turbid-water ponds).”

Line 348-349: The observation of higher CH<sub>4</sub> in summer and spring was already noted for CH<sub>4</sub> concentration. Maybe a good example of why reporting concentrations and fluxes is not needed?

Reply: We prefer to keep the information because it shows that CH<sub>4</sub> fluxes depend on temperature (like CH<sub>4</sub> concentration) rather than on wind. It is conceivable that the variability of fluxes depends mainly on the variations of the gas transfer velocity rather than variations on concentrations.

Line 367: See also Ray and Hoglerson (2023), DOI provided above.

Reply: We added this reference in the sentence. Text now reads L545: “This finding is consistent with other studies showing that ebullitive CH<sub>4</sub> fluxes can account for more than half of total CH<sub>4</sub> emissions in small and shallow lentic systems (*e.g.* Wik et al., 2013; Deemer and Holgerson, 2021; Ray and Holgerson, 2023; Rabaey and Cotner, 2024).”

Line 380: This is an example of where a multiple regression model could be used.

Reply: The reviewer is right. This section aims to compare CH<sub>4</sub> fluxes between clear and turbid ponds and identify controlling factors. However, the aim of this figure is to emphasize that the variations in diffusive and ebullitive CH<sub>4</sub> fluxes can be attributed to either macrophytes, phytoplankton, or a combination of both. The figure presents this information concisely and clearly conveys the message of the text.

Line 404: No mention was made in statistics of using nonlinear regressions

Reply: The reviewer is right. We have added why used linear and quadratic regressions in the statistics section of the material and methods. Text now reads L311: “To assess the impact of Chl-*a* concentration, macrophyte cover in summer, water depth, and lake surface area on diffusive and ebullitive CH<sub>4</sub> fluxes, the ratio of ebullitive CH<sub>4</sub> to total CH<sub>4</sub> flux, and CO<sub>2</sub> and N<sub>2</sub>O fluxes, both linear and quadratic relationships were applied to log-transformed averaged data. This approach allowed for the observation of trends between explanatory and dependent variables. For N<sub>2</sub>O fluxes, additional explanatory variables included NO<sub>2</sub><sup>-</sup>, NO<sub>3</sub><sup>-</sup>, NH<sub>4</sub><sup>+</sup>, and DIN concentrations.”