This study focuses on intra- and interannual trends of GHG emissions from 4 small urban ponds and also addresses their underlying drivers. I believe that the study is a great addition to inland water studies, especially since long-term data on small pond GHG emissions is currently scarce. I commend the authors for curating such a comprehensive dataset. However, the current structure of the paper needs to be revised to make it easier for the reader to follow and also for possible final publications to Biogeosciences. Below are my brief and detailed comments for each subsection for possible improvement of the manuscript.

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

Brief comments

Abstract and introduction

While the introduction was well compiled with the key motivation of the study, the end part did not include clear objectives and hypotheses for the work, which would have guided the reader better throughout the whole manuscript. I suggest that the authors include this and also adjust the intro to highly key gaps that would be addressed later on in the manuscript

Reply: The reviewer is right; we have revised the text providing objectives and working hypotheses at the end of the discussion. 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_2, CH_4, and N_2O 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_4 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_2, CH_4, and N_2O to the total GHG emissions in CO_2-eq and to test the hypothesis that the relative contribution of each GHG differs according to the two alternative states in shallow lakes."

Materials and methods

Although the description of the analysis of the GHG and associated parameters was well done, the statistical analysis part was too short and lacked enough detail. For example, the authors said they used a one-way ANOVA, yet they had a two-factor problem. i.e., seasonal and also pond-type influences. Also, what post hoc tests were used, what correlative analyses were used, and the main aims of this analysis are either lacking or not clearly stated. I suggest that more details should be added addressing the points above.

Reply: The reviewer is right; we have revised the text to provide additional information of the statistical treatment of the data. 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 $\delta^{13}C$-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_4 fluxes, the ratio of ebullitive CH_4 to total CH_4 flux, and CO_2 and N_2O 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_2O fluxes, additional explanatory variables included NO_2^-, NO_3^-, NH_4^+, 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 $\delta^{13}C$-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."

Results and discussion

While combining the results and discussion is an acceptable practice, it sometimes leads to sectors of the manuscript that are not well described. For example, the trends of CO2 and N2O were only mentioned as results and not well substantiated by findings from other studies. In contrast, CH4 trends were well described by the authors, with proper discussions also related to other studies. I suggest either sticking to methane alone or giving also equal focus to the other GHGs.

Reply: The reviewer is right that there is more emphasis on CH_4 than CO_2 and N_2O in the manuscript. This reflects the amount of collected data and variables that was different for each of the three GHGs. For CO_2 and N_2O only dissolved concentrations were collected; for CH_4 additional variables were collected (ebullitive fluxes, $^{13}C/^{12}C$ ratios in water column, and in sedimentary bubbles). As such, we do not see how to provide an equivalent amount of discussion for each of the three GHGs. However, we think that CO_2 and N_2O data bring invaluable added value to the manuscript because it allows us to quantify the relative importance of CH_4 emissions in CO_2 equivalents compared to the other two gases, in the context of the two alternative states, as well as at seasonal and inter-annual scales (former Fig. 14 = new Fig. 13). We feel this is a major outcome of our work, because there are relatively few studies that report simultaneously the diffusive emissions of three GHGs and even fewer with the addition of ebullitive CH_4 fluxes. So, we preferred to keep the data-set of the three GHGs in the paper.

But we followed the reviewer's suggestion, and we have expanded the description of the variations of CO_2 and N_2O diffusive fluxes with new supplemental figures (Figs. S12, S13, S14). The corresponding text now reads L645: "The annual averaged diffusive fluxes of CO_2 (F_{CO2}) and N_2O (F_{N2O}) in the four ponds in the city of Brussels were also plotted against Chl-*a* concentration, total macrophyte cover in summer, water depth, and lake surface area, as well as DIN for N_2O fluxes (Figs. S12, S13, S14). Annual F_{CO2} did not show significant differences between the four studied ponds (Tukey's HSD test: $p>0.05$ for each comparison), and F_{CO2} did not significantly correlate to the other variables (Chl-*a* concentration, total macrophyte cover, water depth, and lake surface area). This might be surprising since other studies have reported lower CO_2 fluxes in more productive lentic systems (*e.g.* Sand-Jensen and Staehr 2007; Borges et al., 2022). We hypothesize that given that the four systems were either phytoplankton-dominated or macrophyte-dominated, in both cases, the ponds had an important submerged productivity resulting in a relatively invariant F_{CO2} as function of either Chl-*a* or macrophyte cover. Annual mean F_{CO2} was also uncorrelated to water depth and lake area (Fig. S12). This might have resulted from the relative similarity of depth and surface area of the four studied ponds, as it is well established that CO_2 emissions strongly increase with decreasing size of ponds (Holgerson and Raymond, 2016). Annual F_{N2O} was not significantly different between clear-water and turbid-water ponds. F_{N2O} was significantly lower in the slightly deeper Pêcheries pond than the two slightly shallower Leybeek and Silex ponds (Fig. S13) (Tukey's HSD test $p=0.0012$ for Pêcheries versus Leybeek, and $p=0.0052$ for Pêcheries versus Silex), and F_{N2O} showed a significant negative relationship with water depth (Fig. S13). We hypothesize that this might reflect a larger dilution of N_2O diffusing from sediments in the deeper systems. F_{N2O} did not correlate to DIN, NH_4^+, NO_2^-, and NO_3^- (Fig. S14). We hypothesize that this reflects the rather narrow range of annual DIN average values in the four studied ponds (~24 to ~29 µmol L^{-1}), as DIN, NH_4^+, NO_2^-, and NO_3^- were not statistically different between ponds (Tukey's HSD test $p>0.05$ for every comparison)."

The manuscript also has 13 figures. While this is fine, readers may end up missing the most crucial part of the results. The unwritten rule of thumb is 6 to a maximum of 8 graphics, which include both tables and figures. I suggest the authors reevaluate the key figures guided by the objectives of the study and then reduce the current number and keep the rest in the suplimentary.

Reply: The reviewer is right that the manuscript has a large number of figures. *Biogeosciences* does not impose a size limit (text and figures) to submissions; the Associate Editors of *Biogeosciences* are required to carefully evaluate the manuscript before they are published in the *Biogeosciences Discussion* forum, which is an indication that for our present submission the Associate Editor decided that the length of text and number of figures were acceptable for an article in *Biogeosciences*. The larger than usual number of figures of our submission reflects the size of the data-set. However, we feel that the length of the manuscript and the number of figures of our submission justifies the scientific merit of keeping the data of the three GHGs in a single manuscript, rather than slicing the data-set into several papers and going down the path of "salami science". As mentioned above, there are relatively few studies that report simultaneously the diffusive emissions of the three GHGs and even fewer with the addition of ebullitive CH_4 fluxes. We feel this justifies the scientific merit of keeping this large data-set together in a single manuscript. As suggested by the reviewer, we have carefully reconsidered the usefulness and value of each of the figures, and we have moved former Figure 13 to the supplementals, reducing by one the number of figures in the main text. The other figures were kept because we feel they meet the objectives and hypotheses of the paper that have been added at the end of the Introduction.

Most of the results also lacked tests of significance and I suggest that this should be included in the revised drafts. If differences are not significant, its always acceptable to refer to them as trends

Reply: Most of differences presented in the previous version of the submitted manuscript had been tested, but the related statistical tests were in the supplementary tables and were only referenced in the legends of the figures. We have revised the manuscript to mentioned statistical significance in L357, L366, L368, L370, L371, L386, L388, L389, L413, L437, L464, L493, L553, L554, L576, L577, L578, L596, L598, L599, L611, L623, L625, L647, L657, L661, L672, L676, L694, L734, L735, L736, L737, L738, L739, L745, L767, L771, L772, L790, L793, L796, L797.

Conclusion

It needs to be focused on the objectives of the study and also to have a general outlook on the potential of urban ponds of inland water GHG dynamics.

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. We do not think that it would be appropriate to provide here, based on only four systems, a general outlook on the potential of urban ponds of inland water GHG emissions. This was to some extent addressed in a previous publication from our group based on a larger data-set in 22 ponds in the city of Brussels (but with more sparse temporal coverage) by a comparison of the urban ponds GHGs emissions with other emissions of GHGs from the city of Brussels (Bauduin et al. 2024; https://doi.org/10.1016/j.watres.2024.121257).Additionally, there are several synthesis papers that extensively address this issue, for example:

- Holgerson and Raymond (2016, https://doi.org/10.1038/ngeo2654)
- Peacock et al. (2021; https://doi.org/10.1111/gcb.15762)
- Deemer and Holgerson (2021, https://doi.org/10.1029/2019JG005600)
- Ray et al. (2023, https://doi.org/10.1002/lno.12362)

We prefer not to repeat the content of these papers, and we feel that the Conclusions reflect the main findings based on our data-set and addresses the objectives stated at the end of the Introduction section.

Detailed comments

Abstract and introduction

Line 15,16: Consider mentioning the direction of the relationship.

Reply: We have removed this sentence because this information was already given elsewhere in the abstract

Line 20: Consider adding a statement relating light availability in the clear ponds to enhance macrophyte growth. The current statement may be unclear at first read.

Reply: Text was modified and now reads L28: "Clear-water (macrophyte-dominated) ponds exhibited higher values of annual ebullitive CH_4 fluxes compared to turbid-water (phytoplankton-dominated) ponds, most probably in relation to the delivery to sediments of organic matter from macrophytes."

Line 21: Trim down the statement about pond methane fluxes, for instance, 'Pond methane fluxes to the atmosphere… '.

Reply: Text was modified and now reads L30: "At seasonal scale, CH_4 emissions exhibited a temperature dependence in all four ponds, with ebullitive CH_4 fluxes having a stronger dependence to temperature than diffusive CH_4 fluxes."

Line 37: Consider rephrasing: Greenhouse gas emissions from inland waters to the atmosphere….

Reply: Text was modified and now reads L50: "Greenhouse gas (GHG) emissions from inland water (rivers, lakes, and reservoirs) to the atmosphere such as carbon dioxide (CO_2), methane (CH_4) and nitrous oxide (N_2O) are quantitatively important for global budgets (Lauerwald et al., 2023)."

Line 39: GHG emissions from lakes….

Reply: Text was modified and now reads L52: "GHG emissions from lakes are lower than from rivers for CO_2 (Raymond et al., 2013) and N_2O (Lauerwald et al., 2019; Maavara et al., 2019)."

Line 44: Remove the …. You can replace it with such as, which indicates that these are just examples and there could be more.

Reply: Text was modified and now reads L60: "The emissions of GHGs from artificial water bodies such as agricultural reservoirs, urban ponds, and storm-water retention basins could be higher than those from natural systems (Martinez-Cruz et al., 2017; Grinham et al., 2018; Herrero Ortega et al., 2019; Gorsky et al., 2019; Ollivier et al., 2019; Peacock et al., 2019, 2021; Webb et al., 2019; Bauduin et al., 2024)."

Line 46: Noun required after the word this…for example, this finding, this conclusion….check here and everywhere in the manuscript.

Reply: Text was modified and now reads here and elsewhere:

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)."

L429: "The slopes of these correlations were not significantly different between ponds and were not correlated with surface area, depth, or dominance of type of primary producers (phytoplankton or macrophyte) (Table S6)."

L535: "The resulting calculated ebullitive CH_4 fluxes allowed to compare and integrate seasonally both components of CH_4 emissions to the atmosphere, and to calculate the relative contribution of ebullition to total (diffusive+ebullitive) CH_4 emissions."

L555: "This finding is consistent with other studies showing that ebullitive CH_4 fluxes can account for more than half of total CH_4 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)."

L602: "The higher ebullitive CH_4 emissions from the clear-water ponds would suggest that the delivery of organic matter to sediments from macrophytes sustained a larger methane production than from phytoplankton."

L604: "This finding is consistent with the notion that vegetated littoral zones of lakes are hot spots of CH_4 production and emission (*e.g.* Hyvönen et al., 1998; Huttunen et al., 2003; Juutinen et al., 2003; Desrosiers et al., 2022)."

L615: "An increase in methane production with phytoplankton biomass in turbid-water ponds has also been reported by other studies in lakes (*e.g.* Yan et al., 2019; Bartosiewicz et al., 2021)."

L627: "These patterns are consistent with the idea of an increase of ebullition relative to diffusive CH_4 emissions in vegetated sediments compared to unvegetated sediments (e.g. Desrosiers et al., 2022; Ray et al., 2023; Theus et al., 2023)."

L639: "This hypothesis is consistent with the two clear-water ponds in Brussels having higher ebullitive fluxes than in the ponds compiled by Deemer and Holgerson (2021) at equivalent Chl-*a* values (Fig. S11)."

L695: "This pattern of $\delta^{13}C\text{-}CH_4$ of perturbed sediments could suggest a higher contribution of the hydrogenotrophic methanogenesis pathway compared to the acetoclastic pathway in the clear-water ponds where organic matter for methanogenesis was assumed to be mainly related to macrophytes rather than phytoplankton."

L865: "Years 2022 and 2023 were abnormally dry and wet, respectively, and consequently, the GHG emissions were higher in 2023 mainly due to an increase in the relative contribution of CO_2 emissions, probably in response to a strong El Niño event."

Line 52-54. The sentence on runoff comes from nowhere. Did you mean the rainfall runoff gets into the ponds? Consider revising it to make it clearer.

Reply: Text was modified and now reads 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)."

Materials and methods

Line 99. Did you mean the institute laboratory? The use of a home may imply a laboratory located in a personal house/apartment.

Reply: Text was modified and now reads L175: "A 2 liter polyethylene water container was filled with surface water for conditioning the samples for other variables at the laboratory in Université Libre de Bruxelles. "

Line 109. Consider revising from "consistent in" to "consisted of"

Reply: Text was modified and now reads L181: The bubble traps consisted of inverted polypropylene funnels (diameter 23.5 cm) mounted with 60 ml polypropylene syringes, with three way stop valves allowing to collect the gas without contamination from ambient air."

Line 111. The statement is a bit confusing. Consider revising it to make it clearer. How were the gases measured with 60ml syringes?

Reply: Text was modified and now reads L184: "The volume of gas collected in the funnels was sampled with graduated polypropylene 60 ml syringes every 24 hours. 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_4 concentration and $\delta^{13}C-CH_4$ in the laboratory."

Line 113. Consider revising the statement. Did you mean that the measurements at Silex were of a longer frequency?

Reply: Text was modified and now reads L188: "The time-series of measurement were longer at the Silex pond than the other three ponds, because the Silex pond is closed to the public during the week, while the other three ponds are open to the public all the time."

Line 210-211. How were seasonality and pond type considered in your ANOVA analysis? The current statement is too short and lacks details. Was the ANOVA a repeated measures ANOVA as you sampled on the same pond multiple times?

Reply: The reviewer is right; we have expanded the description of the statistical tests, and text now reads L316: "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 $\delta^{13}C-CH_4$ 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."

Results and discussion

Line 223. Wetter and colder

Reply: Text was modified and now reads L338: "Year 2021 had warmer and drier months in June and September, colder and wetter months in July and August, and was overall wetter and colder than 2022 (Fig. 2)."

Line 228-229. Consider adding the reference period at first mention and not at the end of the statement

Reply: Text was modified and now reads L345: "Figure 2: Temperature anomaly (difference between the average annual temperature and the normal annual temperature for the reference period 1991-2020 (11 °C), in °C) plotted against precipitation anomaly (ratio between annual precipitation and normal annual precipitation for the reference period 1991-2020 (837 mm), in %) from 2003 to 2023."

Line 233. Missing article; "with the silex pond"

Reply: Text was modified and now reads L353: "The four sampled ponds are situated in the periphery of the city of Brussels, with the Silex pond bordered by the Sonian Forest (Fig. 1)."

Line 244. In Figure 3, I suggest adding letters to the boxplots to indicate significant differences from the ANOVA test. This will help the reader quickly follow the graphs and avoid looking at an extra table in the supplementary.

Reply: The figures were modified accordingly.

Line 252. Are you reporting significant differences or trends? Check here and everywhere where you report comparisons of means. Also, indicate the level of significance as the information is currently missing in the results.

Reply: We have revised the manuscript to mentioned statistical significance in L357, L366, L368, L370, L371, L386, L388, L389, L413, L437, L464, L493, L553, L554, L576, L577, L578, L596, L598, L599, L611, L623, L625, L647, L657, L661, L672, L676, L694, L734, L735, L736, L737, L738, L739, L745, L767, L771, L772, L790, L793, L796, L797.

Line 253. I would move the explanations to the discussion, i.e., owing to primary production…

Reply: We preferred to keep the format of a joint "Results and Discussion" section. We have contemplated extensively how to present and discuss our data-set that is quite large and varied. We concluded that a joint "Results and Discussion" section was a better option than separated "Results" and "Discussion" sections. We feel that the text was articulated in a logical way that was relatively straightforward to follow by readers, going from relatively simple variables (meteorological and dissolved concentrations) to processes related to CH_4 dynamics (ebullition and MOX), and ending an integrative issue with the overall emissions in CO_2 equivalents.

Line 256. Consider using low instead of minimal

Reply: Text was modified and now reads L394: "Low values of pCO_2 were generally observed in spring and summer probably due to uptake of CO_2 by primary production from either phytoplankton or submerged macrophytes."

Line 257. Same comment as 253

Reply: We preferred to keep the format of a joint "Results and Discussion" section, refer to justification given above.

Line 258. Replace Maximal to High

Reply: Text was modified and now reads L396: "High values of pCO_2 were observed in fall in the four ponds and probably reflect the release of CO_2 from degradation of organic matter due to the senescence of phytoplankton or macrophytes (Fig. 3)."

Line 259-263. Correlation results are important for GHG process information as also discussed in this paragraph. I suggest moving them to the main text and, if possible, using scatterplots for the main relationships and indicating the correlation coefficients in the graphs. Also, always include the direction of the relationship, i.e., how was pco2 related to precipitation? Was it a negative or positive correlation?

Reply: We have added several new supplemental figures (Figs. S3, S4, S5, S6, and S7) showing the correlations for each pond and for the whole data set. Text was modified and now reads L403: "In all four ponds, pCO_2 strongly correlated positively to precipitation (Table S3; Figs S3, S4, S5, S6) suggesting a control of external inputs of carbon either as organic carbon sustaining internal degradation of organic matter or as soil CO_2 (*e.g.* Marotta et al., 2011)."

Line 264-266. I now see that the results and discussion are combined. While this is fine, the way it's currently written includes a lot of speculative statements that have not been substantiated by the findings of other studies. I suggest taking a closer look at all statements made and trying to support them with other studies. Putting a citation at the end without stating where the authors found similar results is also not encouraged. You can use (e.g., ……) in the citation to make clear that these authors found similar findings.

Reply: We agree and we have carefully revised the text to mention when relevant references with similar results and we have use "e.g." when relevant. Text was amended L114, L399, L405, L500, L507, L509, L517, L530, L551, L556, L581, L582, L605, L607, L618, L628, L643, L650, L834.

Line 269. See comment on line 253

Reply: We preferred to keep the format of a joint "Results and Discussion" section, refer to justification given above.

Line 277. See the comment on the use of "this" above

Reply: Please refer to the reply above, the text was modified in several places (including here) following the above comment.

Line 278. Add a comma between ponds and the

Reply: The comma was added.

Line 279. Were these differences based on the other factors also tested, i.e., the effect of the size of the pond? This analysis would validate the statement. At the moment, it's a bit speculative

Reply: We tested whether the slopes of the relationships differed between ponds (based on an analysis of covariance (ANCOVA)), and whether these slopes showed a relationship to surface area, depth, chl-*a* concentration and macrophyte cover (based on Pearson's linear correlation coefficients, with Fisher's F test and the associated p-value), and added the statistical results in a new supplementary Table S6. Text was modified and now reads L429: "The slopes of these correlations were not significantly different between ponds and were not correlated with surface area, depth, or dominance of type of primary producers (phytoplankton or macrophyte) (Table S6). These results suggest that the effect of precipitation on pCO_2 and the impact of temperature on dissolved CH_4 concentration outweigh other factors in explaining seasonal variations."

Line 282. Citation of figure or table needed here.

Reply: Text was modified and now reads L436: "The $\%N_2O$ values did not show significant seasonal variations in any of the four sampled ponds (ANOVA F(3,174)=1,127, p=0.4091) (Fig. 3). In individual ponds, $\%N_2O$ correlated negatively to temperature in the Tenreuken pond and Chl-*a* in the Silex pond, and positively to SRP in the Silex pond and TSM concentration in the Tenreuken pond (Table S3; Fig S3, S4)."

Line 284. Were surprising…

Reply: Text was modified and now reads L440: "The correlations with Chl-*a* and TSM were surprising since they were observed in the two clear-water ponds and might indirectly reflect seasonal variations (with minimal values of these two quantities in summer)."

Line 291-295. This paragraph is a bit confusing. I know what the authors mean, but I suggest it be rephrased in order to explain better the lack of correlation between N2O and DIN and its link to nitrogen deposition. How much is the nitrogen deposition in the region and how does it decrease from the edges of the city to the inner parts? Without this data, the current statement is somehow speculative

Reply: Following the reviewer's comment we have added a new supplemental figure showing the relation between $\%N_2O$ and DIN/atmospheric NO_2/distance from the city center (Fig. S8). We have also modified text that now reads L450: "A lower atmospheric nitrogen deposition in the periphery than in the city center is consistent with the correlation between $\%N_2O$ and atmospheric nitrogen dioxide (NO_2), and the correlation between $\%N_2O$ and the distance from the city center (Fig. S8)."

Line 297 -298. How do these bubble fluxes compare with other values from similar studies? Are they on the higher end or lower end? I suggest adding a few comparison studies in all fluxes reported to give an idea of where your study stands in terms of the magnitude of the fluxes.

Reply : We compared our data with those of Wik et al (2013), Delsontro et al (2016), and Ray and Holgerson (2023), and we have modified text that now reads L465: "The bubble flux values in the four sampled ponds in the city of Brussels were within the range of values reported in lentic systems of equivalent size by Wik et al. (2013) (0 to 2772 mL m^{-2} d^{-1}), Delsontro et al. (2016) (11 to 748 mL m^{-2} d^{-1}) and Ray and Holgerson (2023) (0 to 2079 mL m^{-2} d^{-1}). The mean CH_4 content of the bubbles in the four sampled ponds in the city of Brussels was 31±21%, and comparable to the values obtained by Wik et al. (2013) (35±25%), Delsontro et al. (2016) (58±25%) and Ray and Holgerson (2023) (25±13%) in lentic systems of equivalent size."

Line 304. I suggest adding the equation of the fit on the graph.

Reply: We preferred not to overcrowd the figure (and for consistency we should have added equations in all the figures). But following the reviewer's comment, we have added the equation in the legend of the figure so that it is easy to access by the readers. Text now reads L477: "Figure 4: Bubble flux (ml m^{-2} d^{-1}) as a function of water temperature (°C) and the relative CH_4 content in bubbles ($\%CH_4$, in %) in four urban ponds (Leybeek, Pêcheries, Tenreuken, and Silex) in the city of Brussels (Belgium). Bubbles fluxes were measured with three bubble traps in spring, summer, and fall of 2022 and 2023, totalling 8 days in the Leybeek, Pêcheries, and Tenreuken ponds and 24 days in the Silex pond. Given the shallowness of the sampled systems (<1.5 m, Fig. 1) we assume that sediments experience the same temperature as surface waters. Solid lines represent exponential regression fit of bubble flux as function of temperature ($Y = 28 \cdot e^{0.14 \cdot X}$, n=139), and as function of relative CH_4 content in the bubbles ($Y = 164 \cdot e^{0.0.3 \cdot X}$, n=123) (Table S11)."

310-312. This is what I mean by referencing of other studies to support your findings/

Reply: We agree with the reviewer and we have referenced other studies elsewhere in the manuscript (L390, L407, L435, L465, L470, L507, L530, L551).

Line 337. I suggest always using, e.g.,…. Or "similar to what was found…" for every citation quoted in the discussion, particularly those that involve specific findings. This form of citing guides the reader better

Reply: Text was amended L114, L399, L405, L500, L507, L509, L517, L530, L551, L556, L581, L582, L605, L607, L618, L628, L643, L650, L834.

Line 338. I suggest adding the equations of the relationships to Figure 6, which may be useful for future comparisons with other studies and also allow them to be potentially used to estimate ebullition methane fluxes where temperature data is available, as this study has done.

Reply: We preferred not to overcrowd the figure (and for consistency we should have added equations in all of the figures). But following the reviewer's comment, we have added the equation in the legend of the figure so that it is easy accessed by the readers. Text now reads L519: "Figure 6: Measured ebullitive CH_4 fluxes (mmol m^{-2} d^{-1}) as function of water temperature (°C) in four urban ponds (Leybeek, Pêcheries, Tenreuken, and Silex) in the city of Brussels (Belgium), in spring, summer, and fall of 2022 and 2023, totalling 8 days in the Leybeek, Pêcheries, and Tenreuken ponds and 24 days in the Silex pond, with three bubble traps. Solid lines represent exponential fit for the Leybeek ($Y = 0.01 \cdot e^{0.32 \cdot X}$, n=22), Pêcheries ($Y = 0.16 \cdot e^{0.15 \cdot X}$, n=22), Tenreuken ($Y = 0.10 \cdot e^{0.23 \cdot X}$, n=19), Silex ($Y = 0.54 \cdot e^{0.18 \cdot X}$, n=72) ponds (Table S7) dashed lines represent exponential fit established in similar systems: four small ponds in Québec ($Y = 0.06 \cdot e^{0.25 \cdot X}$) (DelSontro et al., 2016) and a small urban pond in the Netherlands ($Y = 0.51 \cdot e^{0.17 \cdot X}$) (Aben et al., 2017). Each exponential curve allows to determine a Q_{10} of CH_4 ebullition, plotted against water depth, solid line represents exponential regression fit ($Y = 92 \cdot e^{-0.02 \cdot X}, n = 6$) (Table S11)."

Line 356. Add letters from posthoc tests to indicate seasonal differences to this figure, similar to my comment on Figure 3

Reply: The figures were modified accordingly.

Line 373. Than for diffusive fluxes…

Reply: Text was modified and now reads L562: "Other studies have also reported higher Q_{10} for CH_4 ebullition than for CH_4 diffusion in lentic systems (DelSontro et al., 2016; Xun et al., 2024)."

Line 389. How do you explain the polynomial U fit in the first panel?

Reply: The cause of the U shape is discussed further down in the text.

Line 393. Was there a statistical test to show that the differences were significant? Judging by the error bars, which sometimes overlap, it may be that the differences were not significant, but I do agree that the trends are there. In cases where the relationships are not significant, I suggest sticking to … showed trends of being higher in…even though the difference was not significant.

Reply: The error bars reflect the seasonal variability. In order to analyze the differences among the 4 ponds as function of size and phyto/macrophyte dominance it is required to average the data and remove the seasonal variability. For transparency we have nevertheless shown the error bars.

Line 401. I now see the explanation for the polynomial fit, which also makes sense. However, this may not be so clear at first read. Hence, it may help to reference the result first and then link it to the explanation. Also, has the relationship with phytoplankton been found in other turbid pond studies. The current reference talks about lakes.

Reply: We feel that the text is structured conventionally were the results are presented first and the explanations given after. We have added comparison with other turbid ponds and text reads L615: "An increase in methane production with phytoplankton biomass in turbid-water ponds has also been reported by other studies in lakes (*e.g.* Yan et al., 2019; Bartosiewicz et al., 2021)."

Line 411. Where is the regression done in the results?

Reply: The figure was modified and now includes the results of the regression.

Line 421-423. Lakes and ponds are used as synonyms here, even thou they may have different characteristics. Check here and everywhere to ensure that references made on lakes are assumed to be related also to ponds.

Reply: We have modified text that now reads L642: "Consequently, global scaling of CH_4 fluxes in lentic systems using Chl-*a* as a predictor as used in lakes (*e.g.* DelSontro et al., 2018) might under-estimate ebullitive CH_4 emissions due to a misrepresentation of macrophyte-dominated clear-water ponds."

Line 472. "as" not "than"

Reply: We have replaced "than" by "as" (L731).

Line 485. Modify the graph to include posthoc tests showing significance across ponds and seasons

Reply: The figures were modified accordingly.

Conclusion

Line 557-562. Reads more like the results and discussion part. I suggest rewriting the conclusion part to focus more on what were the objectives, what were the main conclusions from each objective, and finally future perspectives on what can be done better.

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 focusses on these objectives.

Figure S3: Partial pressure of CO_2 (pCO_2, ppm), dissolved CH_4 concentration (CH_4, nmol L^{-1}) and N_2O saturation level (%N_2O, %), versus water temperature (°C), oxygen saturation level (%O_2, %), concentration of soluble reactive phosphorus (SRP, µmol L^{-1}), concentration of dissolved inorganic nitrogen (DIN= NH_4^+ + NO_2^- + NO_3^- , µmol L^{-1}), concentration of chlorophyll-*a* (Chl-*a*, µg L^{-1}), and total suspended matter (TSM, mg L^{-1}) in the clear-water Tenreuken pond in Brussels, sampled from June 2021 to December 2023. Coefficient of determination, r^2, and associated *p*-value are indicated in boxes and solid lines indicate significative linear regression lines of the log-transformed data (*p*-value < 0.05).

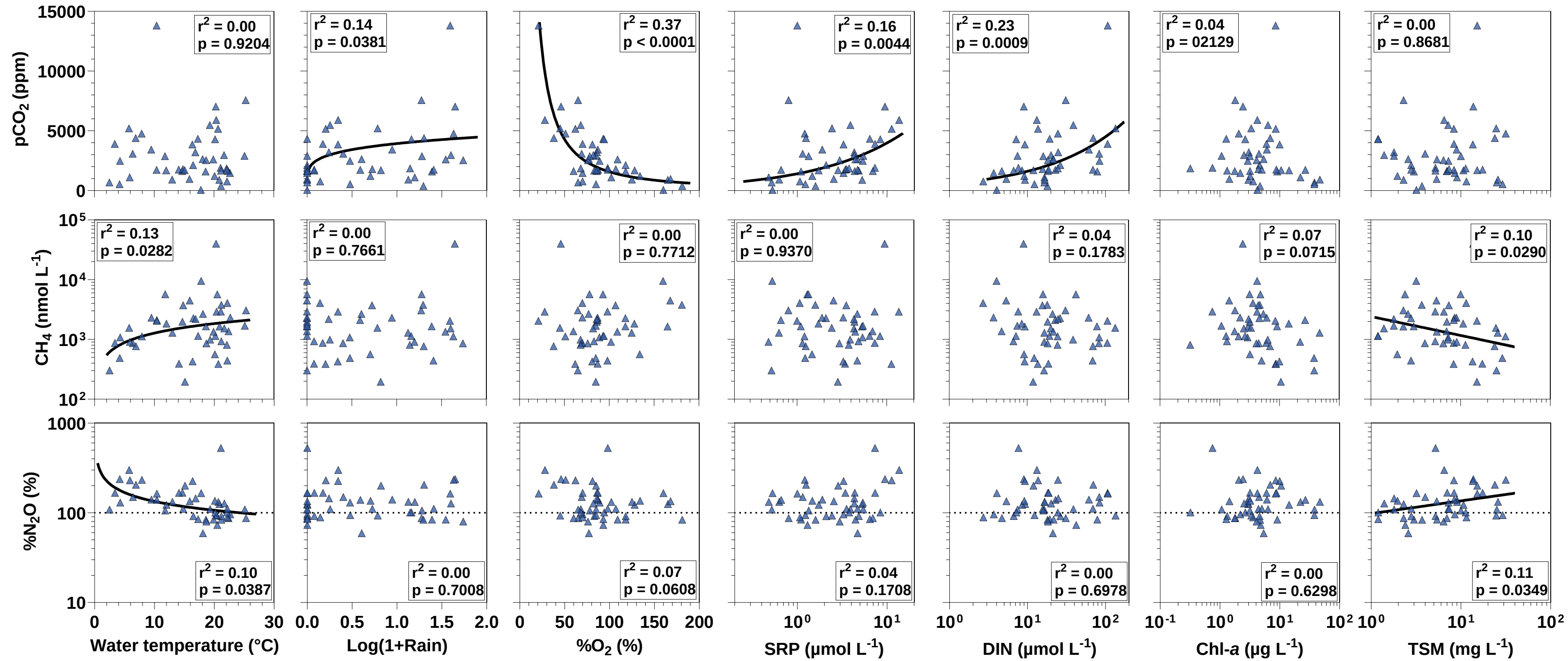


Figure S4: Partial pressure of CO_2 (pCO_2, ppm), dissolved CH_4 concentration (CH_4, nmol L^{-1}) and N_2O saturation level (%N_2O, %), versus water temperature (°C), oxygen saturation level (%O_2, %), concentration of soluble reactive phosphorus (SRP, µmol L^{-1}), concentration of dissolved inorganic nitrogen (DIN= NH_4^+ + NO_2^- + NO_3^- , µmol L^{-1}), concentration of chlorophyll-*a* (Chl-*a*, µg L^{-1}), and total suspended matter (TSM, mg L^{-1}) in the clear-water Silex pond in Brussels, sampled from June 2021 to December 2023. Coefficient of determination, r^2, and associated *p*-value are indicated in boxes and solid lines indicate significative linear regression lines of the log-transformed data (*p*-value < 0.05).

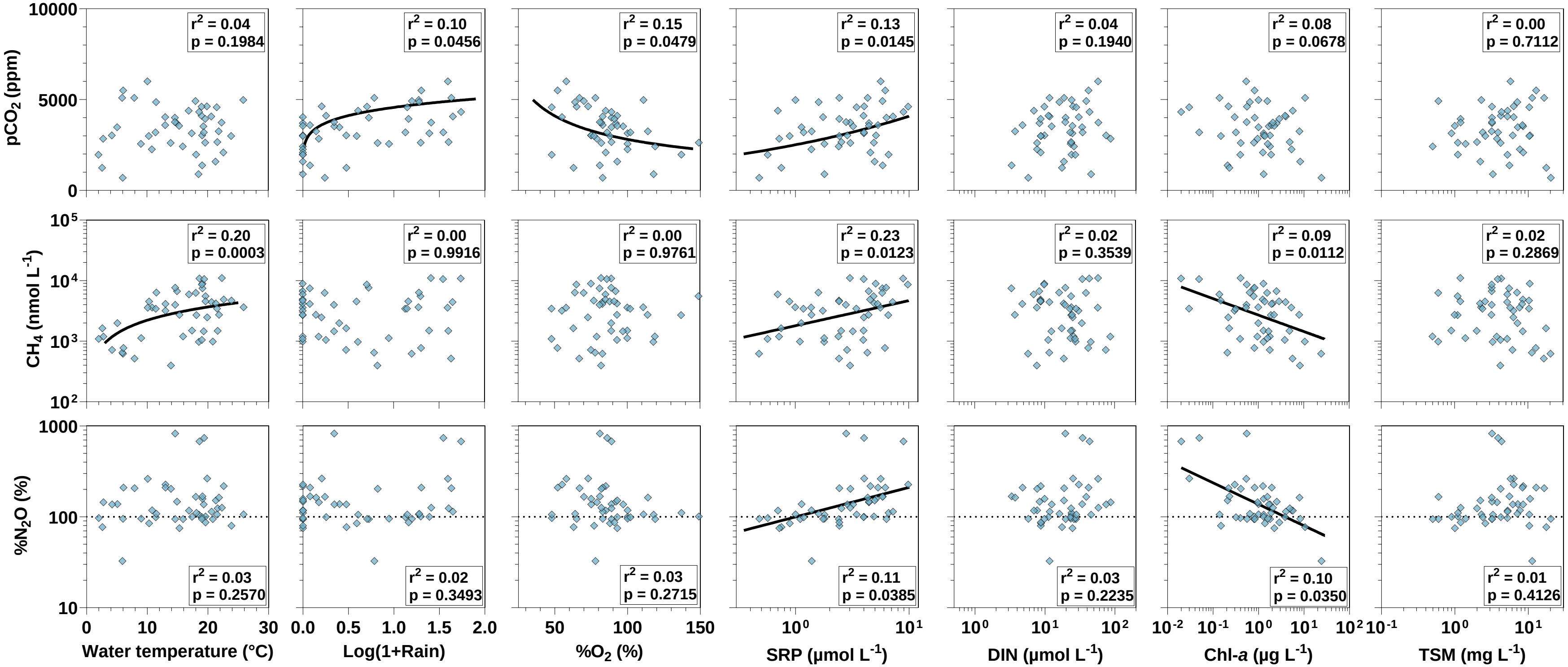


Figure S5: Partial pressure of CO_2 (pCO_2, ppm), dissolved CH_4 concentration (CH_4, nmol L^{-1}) and N_2O saturation level (%N_2O, %), versus water temperature (°C), oxygen saturation level (%O_2, %), concentration of soluble reactive phosphorus (SRP, µmol L^{-1}), concentration of dissolved inorganic nitrogen (DIN= $NH_4^+ + NO_2^- + NO_3^-$, µmol L^{-1}), concentration of chlorophyll-*a* (Chl-*a*, µg L^{-1}), and total suspended matter (TSM, mg L^{-1}) in the turbid-water Leybeek pond in Brussels, sampled from June 2021 to December 2023. Coefficient of determination, r^2, and associated *p*-value are indicated in boxes and solid lines indicate significative linear regression lines of the log-transformed data (*p*-value < 0.05).

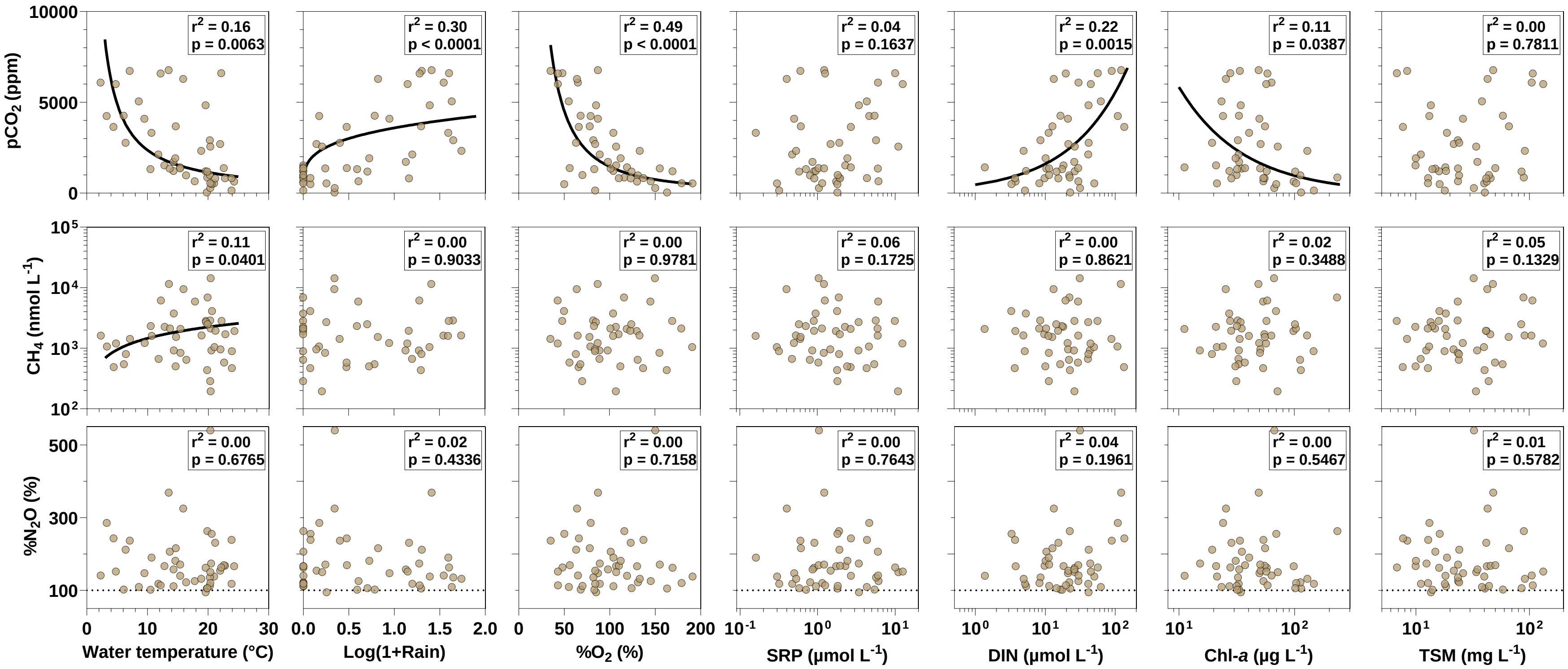


Figure S6: Partial pressure of CO_2 (pCO_2, ppm), dissolved CH_4 concentration (CH_4, nmol L^{-1}) and N_2O saturation level (%N_2O, %), versus water temperature (°C), oxygen saturation level (%O_2, %), concentration of soluble reactive phosphorus (SRP, µmol L^{-1}), concentration of dissolved inorganic nitrogen (DIN= $NH_4^+ + NO_2^- + NO_3^-$, µmol L^{-1}), concentration of chlorophyll-*a* (Chl-*a*, µg L^{-1}), and total suspended matter (TSM, mg L^{-1}) in the turbid-water Pêcheries pond in Brussels, sampled from June 2021 to December 2023. Coefficient of determination, r^2, and associated *p*-value are indicated in boxes and solid lines indicate significative linear regression lines of the log-transformed data (*p*-value < 0.05).

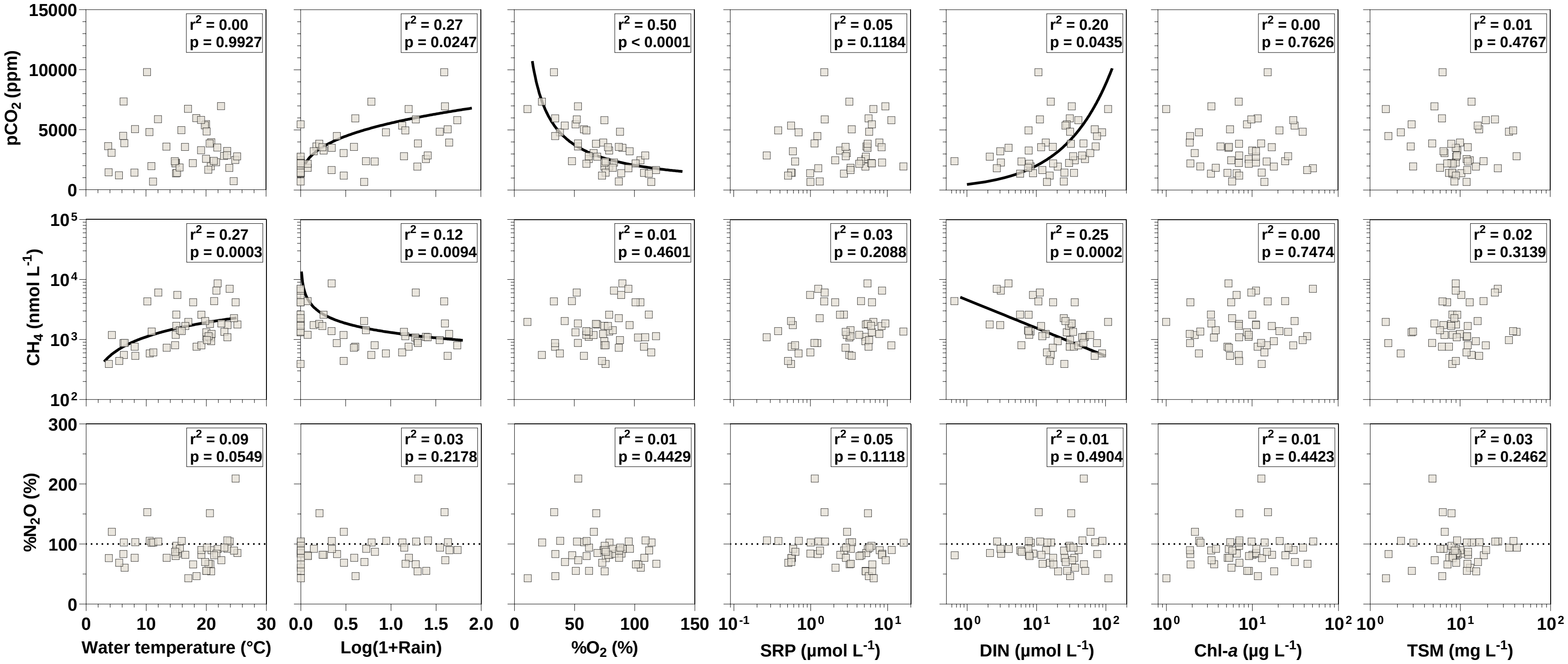


Figure S7: Partial pressure of CO_2 (pCO_2, ppm), dissolved CH_4 concentration (CH_4, nmol L^{-1}) and N_2O saturation level (%N_2O, %), versus water temperature (°C), oxygen saturation level (%O_2, %), concentration of soluble reactive phosphorus (SRP, µmol L^{-1}), concentration of dissolved inorganic nitrogen (DIN= NH_4^+ + NO_2^- + NO_3^- , µmol L^{-1}), concentration of chlorophyll-*a* (Chl-*a*, µg L^{-1}), and total suspended matter (TSM, mg L^{-1}) in four ponds in Brussels sampled from June 2021 to December 2023 (Leybeek, Pêcheries, Tenreuken, and Silex). Coefficient of determination, r^2, and associated *p*-value are indicated in boxes and solid lines indicate significative linear regressions lines of the log-transformed data (*p*-value < 0.05).

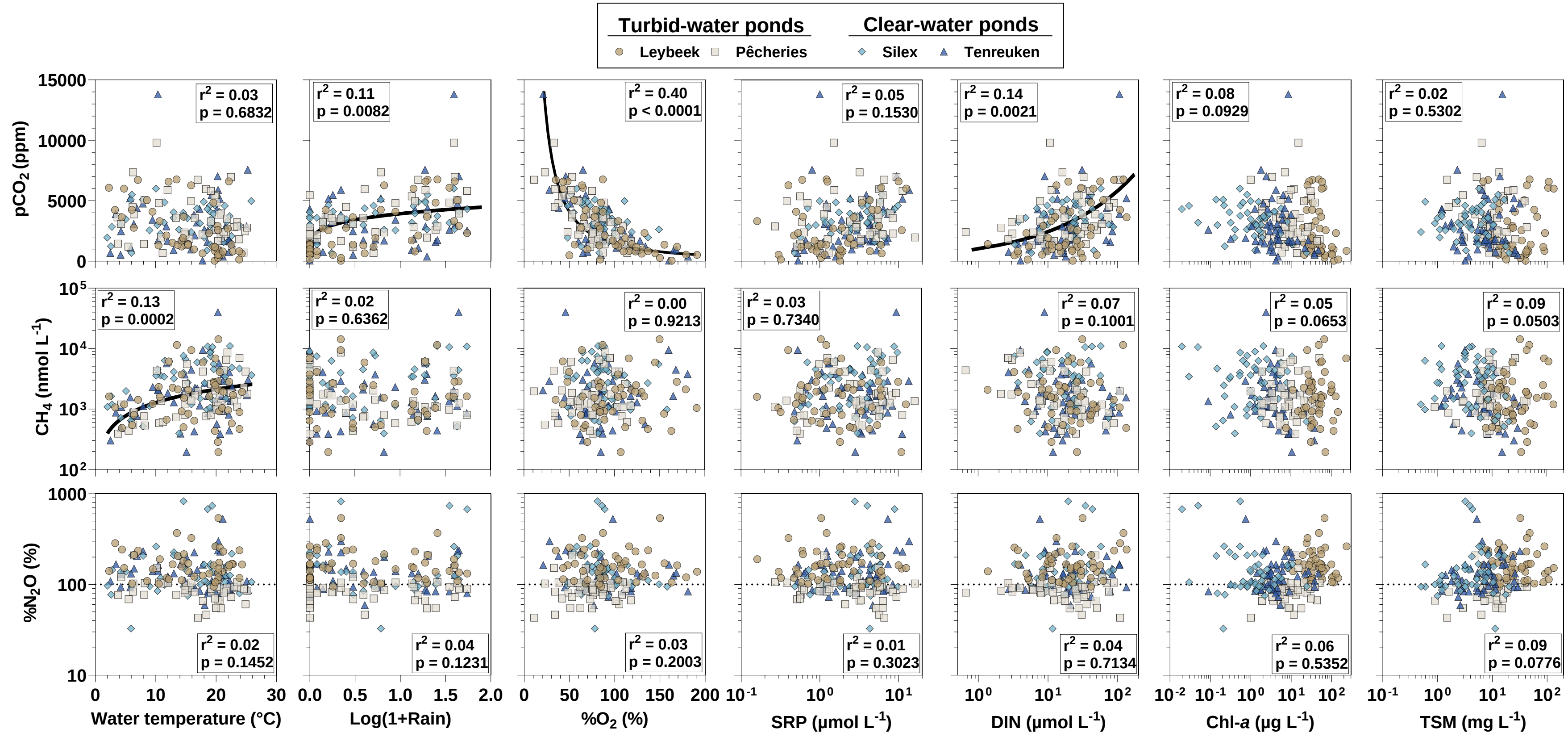


Figure S8: Mean diffusive N_2O saturation level (%N_2O, %) versus dissolved inorganic nitrogen (DIN= NH_4^+ + NO_2^- + NO_3^-, in µmol L^{-1}), distance of the pond from city center (km), and atmospheric NO_2 (µg m^{-3}) in four ponds in Brussels sampled from June 2021 to December 2023 (Leybeek, Pêcheries, Tenreuken, and Silex), and in other ponds in the city of Brussels sampled in 2021 and 2022 from Bauduin et al. (2024). The atmospheric NO_2 concentration was extracted from the Curieuzenair initiative which analyzed 2483 air samples in September 2021 covering the whole of the city of Brussels with a homogeneous distribution (https://curieuzenair.brussels/en/the-results/). Coefficient of determination, r^2, and associated *p*-value for data from Bauduin et al. (2024) are indicated in boxes and solid lines indicate significative linear regression lines of the log-transformed data (*p*-value < 0.05).

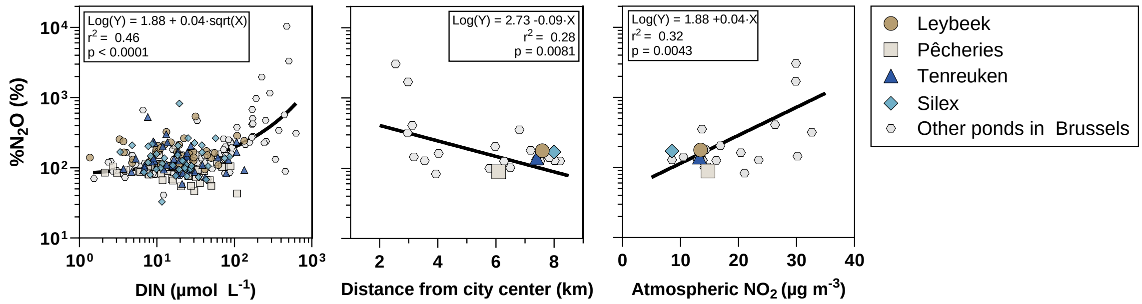


Figure S12: Mean annual diffusive CO_2 flux (F_{CO2} in mmol m^{-2} d^{-1}) versus chlorophyll-*a* (Chl-*a*, µg L^{-1}), total macrophyte cover in summer (%), water depth (cm), and lake surface area (ha) in four ponds (Leybeek, Pêcheries, Tenreuken, and Silex) in the city of Brussels (Belgium) from June 2021 to December 2023. Error bars indicate the standard deviation.

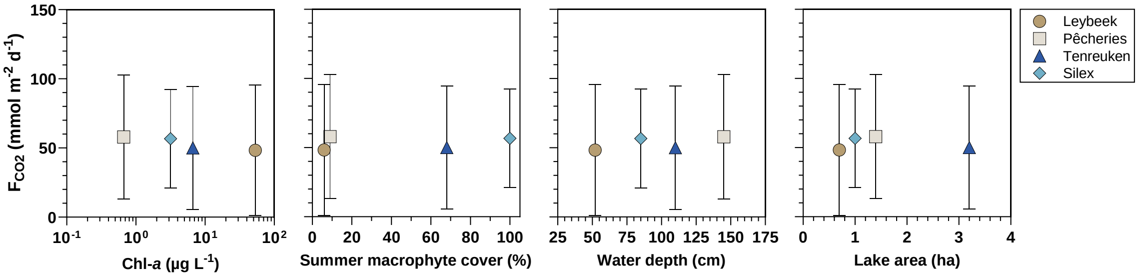


Figure S13: Mean annual N_2O flux (F_{N2O}, µmol m^{-2} d^{-1}) versus chlorophyll-*a* (Chl-*a*, µg L^{-1}), total macrophyte cover in summer (%), water depth (cm), and lake surface area (ha) in four ponds (Leybeek, Pêcheries, Tenreuken, and Silex) in the city of Brussels (Belgium) from June 2021 to December 2023. Error bars indicate the standard deviation. Solid lines indicate trends in relationship between variables.

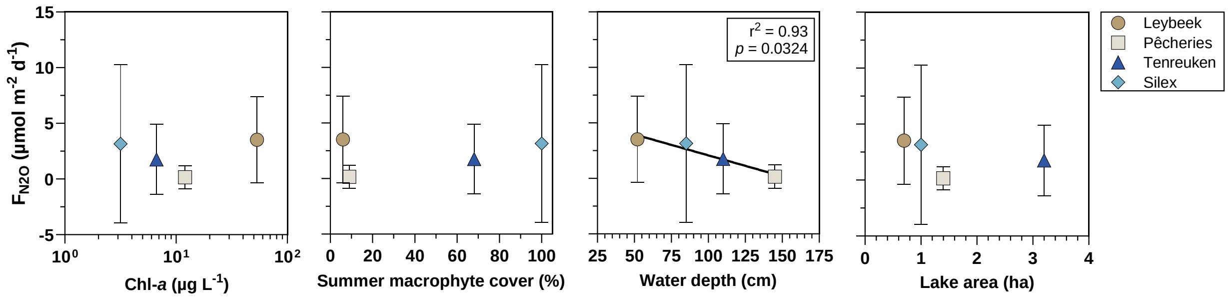
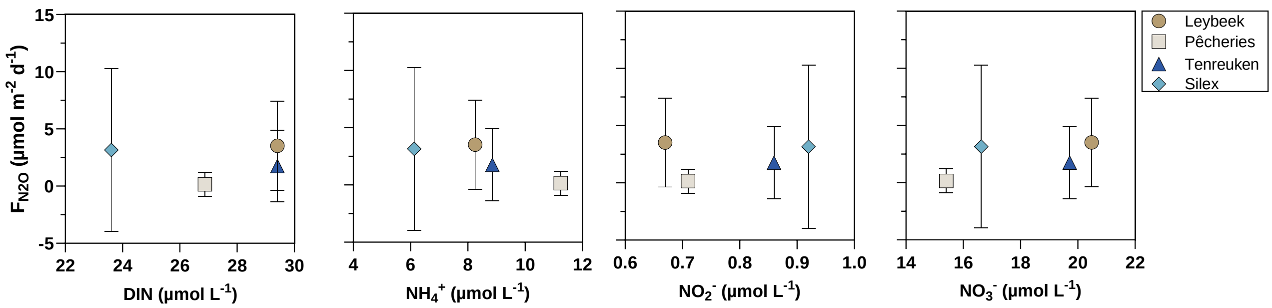


Figure S14: Mean annual diffusive N_2O flux (F_{N2O}, µmol m^{-2} d^{-1}) versus dissolved inorganic nitrogen (DIN= NH_4^+ + NO_2^- + NO_3^- , µmol L^{-1}), ammonium (NH_4^+, µmol L^{-1}), nitrite (NO_2^-, µmol L^{-1}) and nitrate (NO_3^-, µmol L^{-1}) in four ponds (Leybeek, Pêcheries, Tenreuken, and Silex) in the city of Brussels (Belgium) from June 2021 to December 2023.



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_2 concentrations depend in part on the development stage of the phytoplankton, with the growth and peak phases generally coinciding with lower CO_2 concentrations due to intense photosynthesis (Grasset et al., 2020; Vachon et al., 2020). CH_4 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_2O predominantly occurs through microbial nitrification and denitrification that depend on DIN and O_2 levels (Codispoti and Christensen, 1985; Mengis et al., 1997). Competition for DIN between primary producers and N_2O-producing microorganisms can impact N_2O 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_2O 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_4 production in sediments by methanogenic archaea are the fermentation of acetate (acetoclastic pathway) and the reduction of carbon dioxide by H_2 (hydrogenotrophic pathway) (Whiticar et al., 1986; Conrad, 1989). CH_4 produced by these two pathways exhibits distinct $^{13}C/^{12}C$ ratios ($\delta^{13}C$-CH_4) (Whiticar et al., 1986) and can be used to discriminate which pathway is dominant. When CH_4 diffuses from the sediment to the water column, it can be oxidized by methanotrophic bacteria who preferentially consume CH_4 with ^{12}C over ^{13}C, resulting in an increase of $\delta^{13}C$-CH_4 of the residual CH_4 in the water column (Bastviken et al., 2002). Fractionation models then allow estimating methane oxidation (MOX) from measurements of $\delta^{13}C$-CH_4 of dissolved CH_4 in the water column. Bastviken et al. (2008) report that

30 to 99% of the CH_4 produced in sediments of freshwater lakes can be removed by MOX that is as a significant CH_4 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_2, CH_4, and N_2O 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_4 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_2, CH_4, and N_2O to the total GHG emissions in CO_2-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^2 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_2 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_2 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 $\delta^{13}C$-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_4 fluxes, the ratio of ebullitive CH_4 to total CH_4 flux, and CO_2 and N_2O 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_2O fluxes, additional explanatory variables included NO_2^-, NO_3^-, NH_4^+, 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 $\delta^{13}C$-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, %O2), 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_4 fluxes, and the figure presents this concisely. It also shows that the usual predictors of CH_4 fluxes (surface area and depth) do not explain CH_4 fluxes in the four studied ponds. Figure 10 has been revised to present the differences between the isotopic signatures of CH_4 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 CO2 as a partial pressure, methane as a concentration, and N2O 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 N2O. but values will likely be too high for CH4), or the factor of super/under saturation, and be consistent for all gases (i.e., the concentration of CO2 was X umol/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. pCO2 is usually expressed in ppm and readers can easily determine if the values are above or below atmospheric CO2 of about 400 ppm. In topical literature CH4 is reported in µmol/L or nmol/L, and the values were systematically above saturation. For N_2O, 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_2, CH_4 and N_2O 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_4 dynamics (ebullition and MOX), and concluding with a comprehensive analysis of total emissions in CO_2 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 (CO2), methane (CH4), and nitrous oxide (N2O) 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_2), methane (CH_4) and nitrous oxide (N_2O)."

Line 9-12: Suggest rewording to something like the following and including "fluxes": "We measured the saturation and air-water flux of CO2, CH4, and N2O 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_2, CH_4, and N_2O 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 Pêcheries)."

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 CH4 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 13C/12C isotope ratio of CH4 (δ13 13 C-CH4) from bubble traps and sediment bubbles."

Reply: Text was modified and now reads L16: "CH_4 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 $^{13}C/^{12}C$ ratio of CH_4 ($\delta^{13}C$-CH_4) from gas trapped in the bubble traps, from bubbles deliberately released by the perturbation of the sediments, and in dissolved CH_4 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_2 and N_2O."

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_4 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 $\delta^{13}C$-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 $\delta^{13}C$-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_2 and CH_4 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^2) 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_2 and CH_4 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^2) 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 CO2 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 CO2 at all in the introduction. This should be included as it is for CH4 and N2O.

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_2 cycling in the two stable states. Text now reads L78: "Submerged macrophytes and phytoplankton regulate CO_2 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_2 emissions from ponds and lakes. Some studies have shown a decrease of CO_2 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_2 concentrations depend in part on the development stage of the phytoplankton, with the growth and peak phases generally coinciding with lower CO_2 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 N2O-macrophyte relationships have been reported and save the details for the discussion.

Reply: Text was modified and now reads L101: " N_2O emissions has been showed to follow diurnal cycles of O_2 concentrations in areas dominated by submerged macrophytes in Lake Wuliangsuhai (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 N2O are associated. I would either include their association (i.e., that N2O 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 N2O production. Text now reads L93: "The production of N_2O predominantly occurs through microbial nitrification and denitrification that depend on DIN and O_2 levels (Codispoti and Christensen, 1985; Mengis et al., 1997). Competition for DIN between primary producers and N_2O-producing microorganisms can impact N_2O 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_2O 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_2, CH_4, and N_2O 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 CH4 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_4 flux usually represents more than half of total (diffusive+ebullitive)

CH_4 emissions from shallow lakes (Wik et al., 2013; Deemer and Holgerson, 2021), although the relative contribution of ebullitive and diffusive CH_4 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_4 production in sediments by methanogenic archaea are the fermentation of acetate (acetoclastic pathway) and the reduction of carbon dioxide by H_2 (hydrogenotrophic pathway) (Whiticar et al., 1986; Conrad, 1989). CH_4 produced by these two pathways exhibits distinct $^{13}C/^{12}C$ ratios ($\delta^{13}C$-CH_4) (Whiticar et al., 1986) and can be used to discriminate which pathway is dominant. When CH_4 diffuses from the sediment to the water column, it can be oxidized by methanotrophic bacteria who preferentially consume CH_4 with ^{12}C over ^{13}C, resulting in an increase of $\delta^{13}C$-CH_4 of the residual CH_4 in the water column (Bastviken et al., 2002). Fractionation models then allow estimating methane oxidation (MOX) from measurements of $\delta^{13}C$-CH_4 of dissolved CH_4 in the water column. Bastviken et al. (2008) report that 30 to 99% of the CH_4 produced in sediments of freshwater lakes can be removed by MOX that is as a significant CH_4 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 CH4 cycling pathways (methanogenesis/methonotrophy), 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_2, CH_4, and N_2O dissolved concentrations in four shallow and small urban ponds (Leybeek, Pêcheries, 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_2, CH_4, and N_2O were calculated from dissolved concentrations and the gas transfer velocity, while the ebullitive CH_4 fluxes were measured with inverted funnels during 8 deployments (totalling 48 days) in the four ponds. The $\delta^{13}C$-CH_4 in the sedimentary bubbles and in the water provides additional information on CH_4 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_2, CH_4, and N_2O 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_4 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_2, CH_4, and N_2O to the total GHG emissions in CO_2-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_2, CH_4, and N_2O."

Line 100-103: pCO2 analytical approach needs to be moved to the same section as CH4 and N2O 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 N2 gas as the headspace or ambient air??

Reply: We have added information on GHG measurements and the headspace technique for pCO_2. The three gases were collected with syringes in the field, but pCO_2 was measured directly in the field with a portable Li-Cor Li-840 infrared gas analyser. Samples for CH_4 and N_2O were collected in the field and measurements were done after in laboratory. Text now reads L158: "Samples for CH_4 and N_2O 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 $HgCl_2$, 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_2 (pCO_2) 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_2. The final pCO_2 value was computed taking into account the partitioning of CO_2 between water and the headspace, as well as equilibrium with HCO_3^- (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 CO2 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_2 (pCO_2) 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_2 and a suite of gas standards (Air Liquide Belgium) with CO_2 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_4 and N_2O 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 $HgCl_2$, 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_4 and N_2O 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 $HgCl_2$, 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_4 concentration and $\delta^{13}C\text{-}CH_4$ 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 $\delta^{13}C\text{-}CH_4$ in the laboratory.”

Line 125: Ok so for CH4 and N2O you collected water, then used the headspace equilibration approach in the lab? If you also used this approach for CO2 but it was done in the field for CO2, this still should be described earlier.

Reply: Text was modified to clarify that pCO_2 was measured directly in the field and that samples for CH_4 and N_2O 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 pCO2, nmol/L of CH4, and %N2O 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 N2O is presented as a deviation from equilibrium, I think the same should be included for CO2 and CH4, 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 CO2 and CH4 are standard in topical literature and used in numerous publications from our group. For N_2O, 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_2, CH_4 and N_2O 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 N2O in water?? Did you calculate N2O 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_2O concentrations fluctuated around atmospheric equilibrium, so data are presented as percent of saturation level (%N_2O, 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 NH4-N, NO3-N, and NO2-N? or the sum of the full concentration of each?

Reply: DIN is the sum of concentrations of NH_4^+, NO_3^- and NO_2^- 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_4^+, NO_2^- and NO_3^- concentrations in µmol L^{-1}.”

Line 158: Finally I see that CO2 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_2 directly in the field.

Line 160: Change "on" to "in".

Reply: Text was modified and now reads L255: "The atmospheric pCO_2 was measured in the field with the Li-Cor Li-840."

Line 160: Where did you get this value of 1.9 ppm for CH4?

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_4, 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 CH4, N2O, and SF6 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 N2O" 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 N2O.

Reply: The reviewer is right and we have changed the text to beter clarify the sentence. Text was modified and now reads L257: "Atmospheric N_2O concentration was calculated from the average air mixing ratios of N_2O 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±49.5 μg L^{-1}), followed by the Pêcheries pond (19.1±13.7 μg L^{-1}), the Tenreuken pond (3.3±2.4 μg L^{-1}), and the Silex pond (1.0±1.2 μg L^{-1}) (Tukey's HSD test p ≤0.0001 for each pair of comparisons, Figs. 1, 3). The Leybeek and Pêcheries ponds with higher summer Chl-*a* concentration had turbid-water (summer TSM = 48.7±36.2 and 13.7±10.7 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±3.2 and 4.0±3.2 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 Pêcheries ponds (Tukey's HSD test p=0.0107 for the Leybeek pond, p=0.0211 for the Pêcheries 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_2, CH_4, and N_2O." and L174: "Water temperature, specific conductivity, and oxygen saturation level (%O_2) 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 pCO2 was "systematically and distinctly above saturation level" which was cited at 400 ppm. I'm curious now if authors meant atmospheric CO2, of CO2 dissolved in water, which would differ based on temperature.

Reply: The reviewer is right. However, under-saturation of pCO_2 in water relatively to pCO_2 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_2 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 CO2 concentrations in the summer.

Reply: We agree. Yet, the temperature effect is overshadowed by the enormous range of variations of pCO_2 (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_4 concentration was negatively correlated to precipitation and DIN in the Pêcheries 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., NH4-N versus NH4) 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 NH4+, NO3- and NO2- 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_2 was positively correlated with precipitation, and dissolved CH_4 concentration was positively correlated with temperature, while no significant correlation was found between $\%N_2O$ and other variables in the four ponds taken individually. The negative correlation between pCO_2 and $\%O_2$ 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 CH4 in summer and spring was already noted for CH4 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_4 fluxes depend on temperature (like CH4 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_4 fluxes can account for more than half of total CH_4 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_4 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 CH4 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_4 fluxes, the ratio of ebullitive CH_4 to total CH_4 flux, and CO_2 and N_2O 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_2O fluxes, additional explanatory variables included NO_2^-, NO_3^-, NH_4^+, and DIN concentrations."