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Interactive comment on "Regulation of carbon dioxide and methane in small agricultural reservoirs: Optimizing potential for greenhouse gas uptake" *by* Jackie R. Webb et al.

Anonymous Referee #1

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The paper by Webb et al presents CH4 and CO2 data from 101 farm ponds. Alongside these GHG measurements are an impressive array of variables of water chemistry, hydrological characteristics, and landscape attributes. The authors investigate these variables as drivers of the GHG emissions. The paper is well written and I enjoyed reading it. It is within the scope of BG, and presents novel data insomuch as the fact that more pond GHG data is needed (and this point was explicitly raised in the recent IPCC refinement). If small, artificial waterbodies can be designed to minimise CH4 emissions, and to act as CO2 sinks, then this could lead to them acting as natural climate solutions.

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Methods and analysis are well explained with sufficient detail, and the results support the conclusions. Presentation is good, language is fluent, abstract is suitable. The work is mostly well referenced (I suggest two older references of farm pond emissions that the authors may have missed). I particularly enjoyed reading the succinct and to-the-point results section, which was enough to get the authors' points over without endlessly writing numbers out, as so many results sections do.

The one thing I find lacking from the paper is a visual presentation of the underlying CO2 and CH4 data, and in my comments I suggest a way to address this. I think it is important that readers are offered an easy way to understand the variation in the GHG data across all 101 waterbodies.

I suggest the paper is acceptable following minor revisions. Below are my detailed comments.

L29. "Small waterbodies have recently been recognised as substantial contributors to global carbon emissions from inland waters." This is true, and missing from somewhere in the introduction (and discussion) is a mention that the recent 2019 IPCC Refinement explicitly addresses the issue of CH4 emissions from artificial ponds. The Refinement can be found at the link below, and the relevant chapter is in vol. 4 (AFOLU), chapter 7 (Wetlands). The emission factor given for artificial ponds is 183 kg CH4/ha/yr, but there is currently not enough data to disaggregate pond emissions by climate zone. How does your data compare to this emission factor? https://www.ipcc-nggip.iges.or.jp/public/2019rf/index.html

L36. It's worth noting the recent paper by van Bergen et al who measured CH4 (including ebullition) and CO2 emissions, and C burial of an urban pond. Ideally we need studies that quantify GHG emissions and C burial, so the net balance can be calculated. van Bergen, T.J., Barros, N., Mendonça, R., Aben, R.C., Althuizen, I.H., Huszar, V., Lamers, L.P., Lürling, M., Roland, F. and Kosten, S., 2019. Seasonal and diel variation in greenhouse gas emissions from an urban pond and its major drivers. Limnology

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and Oceanography.

L60. “Currently, only three studies have comprehensively assessed C fluxes from small agricultural reservoirs.” What does “comprehensively” mean in this case? These three studies are slightly different – Ollivier et al did not measure ebullition whilst the other two studies did. Ollivier et al and Paneer Selvam et al were ‘snapshot’ studies whilst Grinham included some temporally repeated measurements (but didn’t measure CO2). So are they all comprehensive really? I accept this is a minor point of language but it does matter. Additionally, there are two other papers that have measured farm ponds. Stadmark et al made repeated measurements of CH4 and CO2 emissions from agricultural ponds created to retain N: Stadmark, J. and Leonardson, L., 2005. Emissions of greenhouse gases from ponds constructed for nitrogen removal. Ecological Engineering, 25(5), pp.542-551. There is also data in an old and rather blandly titled paper from two farm ponds. Baker-Blocker, A., Donahue, T.M. and Mancy, K.H., 1977. Methane flux from wetlands areas. Tellus, 29(3), pp.245-250.

L62. “Large fractions of CH4 being released.” Fractions seems like an odd and unsuitable word. Change for “volumes”, “amounts”, “quantities”, etc?

L80. The study region occupies a large area, but seeing as temperatures are given it would also be good to give a value (or range) for annual precipitation. Reading on, I see the results says “precipitation $\sim$60% less than the long-term climate average of 390 mm in Regina.” Please give the value in the methods.

L86. It says 101 ponds were sampled, but in table 1 some variables have N = 102. Where does 102 come from?

L113, L118. Floating chambers are not “incubations”. This word should be altered to something like “deployments” or similar.

L121. It says DO was measured in mg/l but in table 1 it is given as %. The methods text should be amended to % instead.

L149. Inflow is mentioned here. Do these systems have inflows? Is water pumped in for storage, or do they simply collect rainwater?

L183. “To avoid multicollinearity, correlation coefficients between pairs from Pearson linear correlation tests was used to guide covariate choice before model fitting.” This is vague. Did you use a Pearson correlation coefficient of a certain value to decide when multicollinerity was present?

L197. Something I desperately miss from the paper is a figure allowing the reader to visualise the raw CH4 and CO2 data and its distribution. I strongly advise the addition of a figure to show this. It could take numerous forms, such as a scatter plot of CH4 vs CO2 for all 101 ponds, or a box plot of GHGs (grouped by pond size, or pasture vs cropland), or even a bar plot showing individual concs for 101 ponds (large and unwieldy perhaps, but visually useful). Reading on I see figure.3 has a very small land-use graph, but I think a more obvious, up-front figure would be better.

Fig 2 and fig 3. In part this relates to my point above. Wouldn’t these figures be improved by adding the underlying data points on to these figures as a scatter? That way the reader can see the model, and the raw data. It would help the reader visually determine the robustness of the models.

L210. “CO2concentrations displayed a positive response with. . .NOx” Whilst the upper 95% credible interval continues to increase, the black line presumably suggests that CO2 decreases at the highest NOx levels. Is there a mechanism that can explain this?

Figure 3 has a land use graph, but figure two doesn’t. Even if there is no difference in CO2 between land use a figure would still be interesting to see, and there is room for an extra panel at the bottom right anyway.

For the land use panel in figure 3, the categories are pasture, livestock and cropland. However, line 87 in the methods only mentions pasture (n = 80) and cropland (n = 21). Where do these livestock ponds come from?

L224. “Our comprehensive spatial analysis revealed wide variations among CO2 and CH4 concentrations between farm reservoirs” As per my previous comment, there’s currently no easy way to assess this until the raw data is more visible in a figure.

L227. “CH4 was most correlated by internal abiotic and biotic mechanisms” Should this not be “most correlated with”?

L282. “Additionally, smaller waterbodies with shorter WRT can support higher rates of internal CO2 production due higher rates of allochthonous DOC mineralisation” Needs amending to read “due to”

L285. “This mechanism is also suggested by the observation that higher reservoir CO2concentrations are predicted in high CEC soils Alkaline high CEC soils retain more calcium ions within clay particles which releases carbonates and bicarbonates into soil porewater” It seems like something has gone awry in the writing here, and this should be two sentences or some words need removing.

L331. “The effect potential effect of sulfate” The first “effect” needs deleting

L336. “In contrast to the external drivers found for CO2, local land use had a significant effect on CH4 concentrations in farm reservoirs (Fig.3I), with significantly higher CH4 levels in cropland waterbodies than those in pasture. This finding contrasts with those from Australian farm reservoirs where diffusive CH4 fluxes were 250% higher in reservoirs with livestock compared to crops,” I find this section of the discussion interesting. As the authors write, the intensive agricultural practices associated with cropland could be expected to result in elevated CH4 concentrations. Conversely, pasture/livestock emissions would depend on the system (intensive or extensive), livestock, etc. Intensive grassland systems could easily result in high emissions, whilst low-level grazing might result in emissions being less than those from cropland. So cropland > grassland and grassland < cropland are both explicable it seems to me.

Figure 4 and fig. 5. The study by Grinham et al of Australian ponds is referenced in

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the text but doesn't seem to be included in these figures. Is there any reason their data was left out?

L365. "Here, CH4 fluxes were converted to CO2-efluxes using the sustained-flux global warming potential over 100 years" I am not familiar with this metric, and suggest a few lines are included in the methods as to what it is and how it is calculated.

Section 4.4. What (if any) vegetation colonises these pools? Is there no role for encouraging certain plant species that might promote C uptake? For instance, Moore & Hunt say: "The carbon sequestration assessment of constructed stormwater wetlands and ponds suggests that emergent vegetation is a significant source to the soil carbon pool (compared to allochthonous sources) and a critical component of carbon sequestration in these systems." Moore, T.L. and Hunt, W.F., 2012. Ecosystem service provision by stormwater wetlands and ponds–A means for evaluation?. Water research, 46(20), pp.6811-6823.

L392. "The flux of N2O was constrained in our earlier study (Webb et al., 2019), which found a small CO2-e sink (-89 to -3 mg CO2m-2d-1) for the majority of these farm reservoirs despite high N concentrations." Something of a diversion here, but doesn't this depend on how the data are interpreted though? In your earlier study the median N2O flux was negative, but the mean was positive (with 33% of ponds emitting N2O), whilst in this study (figs 4 and 5) you present mean CH4 and CO2. There's probably a debate to be had concerning what average is most appropriate to use, but note the IPCC Refinement used a mean value calculated from log-transformed values.

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