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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 #2

Received and published: 19 August 2019

This paper describes CO2 and CH4 concentration measurements made during the summer season on 101 farm reservoirs in an agricultural region of Saskatchewan, Canada. The authors then use a series of floating chamber measurements to infer diffusive fluxes of these two greenhouse gases at the pond surface via estimations of gas transfer. The authors also collect data on a number of abiotic and biotic landscape/waterbody characteristics that may help predict farm pond GHG concentrations. They then use general additive modeling to describe controls on waterbody concentration. While not currently emphasized, this paper follows up on a previous article that described novel N2O uptake dynamics in these same ponds. The authors emphasize a few findings: 1) more than half of farm ponds are net CO2 sinks, 2) some (19%)

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farm ponds are net CO2-eq sinks when looking at diffusive emissions, 3) CO2 concentrations are governed most by hydrology/landscape position, 4) CH4 emissions are governed most by autochthonous production.

The current framing of this paper is difficult for me to digest given the complete lack of any CH4 ebullition measurements from these systems (and given that fluxes were estimated based on highly uncertain estimates of gas transfer). While the authors acknowledge that their estimates of CO2-eq emissions are likely low due to the lack of ebullition measurements, this is done at the very end of their paper. I think this point should be made sooner as it is an important detail that influences the interpretation of their findings. The relative contribution of ebullition to total methane flux can vary widely from system to system and the controls on the proportion of methane flux that is ebullitive are not well understood (Deemer et al. 2016 BioScience). It would be helpful to know if the authors observed any evidence of ebullition events during their floating chamber surveys? How much ebullition would have to be observed to push the net CO2-eq sink systems towards net-source? Also, what is the uncertainty in sink vs. source estimations due to uncertainty in system gas transfer velocity? To this same end, it is difficult to see the 19% of systems that are net CO2-eq sinks by looking at the authors' figures. Is this because the net CO2-eq sink is very small? For example, Figure 4 does not seem to show that over 50% of the systems in your study were net CO2 sinks. I suggest adding a zero line to your figures and possibly creating an additional figure that shows fluxes site-by-site for the farm ponds in your study. The visual aids currently offered for showing the distribution of your own dataset are sort of overshadowed by a comparison with the broader literature.

Also, while I am not very familiar with GAMs, I found this analysis a bit opaque and difficult to interpret as currently described. For example, were both N and P variables put into the model and NOx/DIN came out as more important? Also, how were the variables plotted in figures 2 and 3 selected? From what I can gather, you have plotted more than just the variables in the best model. For the sake of discussion, it would be nice to see a consistent set of variables and their relationship to both CH_4 and CO_2.

To me, the more novel part of this data set is the high fraction of ponds that are net CO_2 sinks. This is also a finding that is most strongly backed by the data that was collected since the conclusion doesn't rely as much on gas transfer estimates and since CO_2 ebullition is typically an extremely small fraction of total CO_2 emission. The extent of the CO_2 sink in these small agricultural ponds could be compared to the lesser extent reported in the global data set of artificial reservoir GHG dynamics (Deemer et al. 2016). It is also interesting that the CO_2 sink seems to scale more with landscape and hydrological factors than with ecosystem productivity. While multiple other studies have already emphasized the potential importance of nutrient management/eutrophication on lake, pond, and reservoir methane emissions (see Beaulieu et al. 2019 for a very recent global scale discussion), the findings you present in this paper suggest that landscape placement of farm reservoirs may help buffer GHG emissions independent of trophic status (via carbonate buffering and groundwater DIC chemistry dynamics). See paper by Pacheco et al 2013 in Inland Waters (which asks if eutrophication can reverse the aquatic C budget). To this end, it would also be nice to see plots comparing emission by land use for both CH_4 and CO_2 (right now the plot is only shown for CH_4).

The comparison between human-made and natural waterbodies is also interesting and novel. I think it would be good to more thoroughly introduce this question/concept (that the systems might fundamentally differ from each other) earlier in the paper and then come back to it in the discussion. A good reference for comparing human-made and natural waterbodies is Hayes et al. 2017 L&O Letters as well as Doubek & Carey 2017 Inland Waters.

Line by Line Edits

Line 18: add “surface” before “concentrations”

Lines 20-21: this is a little misleading since pH was actually a better predictor

Lines 23-24: state the timescale over which you are calculating CO2-equivalents

Line 26: bringing up depth doesn't seem appropriate here since depth didn't come out as a significant predictor variable in your models

Line 30-31: Holgerson and Raymond 2016 didn't look at ebullition

Line 45-46: Also check out Couto and Olden 2018. . . there aren't really global papers that distinguish surface area of small farm reservoirs/ponds from small hydropower.

Lines 46-47: I suggest listing out numbers of reservoirs by country since the current phrasing is difficult to interpret. Either that or use a word like "collectively" to indicate that 8 million is the sum across multiple countries.

Line 51: What does It mean to create reservoirs at a rate of up to 60% of standing stock? I'm a bit confused by this wording.

Lines 56-57: It is a bit awkward to suggest that eutrophication results in potent CO2 release since autochthonous production actually works to fix CO2 (see Pacheco et al. 2013).

Lines 76-77: I suggest clarifying: you are identifying drivers of surface water concentration, not total flux. Although these are related, they are not the same thing.

Lines 86-87: How did you select your sites? Randomly?

Lines 197-202: What were N:P ratios like in these systems?

Results section: I suggest including a summary of the fluxes you estimate (and associated gas transfer rates from the floating chamber surveys). Can you estimate how variability in k might affect variability in your flux estimates? Are there cases where you have both a floating chamber and a concentration based estimate of flux? How much did these differ from each other?

Line 227: change "by" to "with"

Line 246: Not a complete sentence.

Lines 261-262: This doesn't seem like a very satisfying explanation to me. Is it also possible that differing hydrology leads to the more stratified systems also being the ones that are higher in CO_2?

Line 269: add "of" between "effect" and "increased"

Line 270: Nitrification doesn't produce CO_2; it is an autotrophic process.

Line 272: This is a pretty vague topic sentence. It would be helpful to be a little more specific.

Line 303: get rid of "by"

Lines 306-307: Deemer et al. 2016 and Beaulieu et al. 2019 are also good references here.

Lines 312-315: Higher CH_4 from higher C:N sediments suggests more (not less) important role for allochthonous C right?

Line 318-319: I would expect thermal stratification to influence bottom water CH_4 concentration more than surface water CH_4, but you only have surface water concentrations in your model.

Line 331: Get rid of second "effect"

Line 334-335: Avoid using the word "clearly". Also, it would be helpful to show the relationship between CH_4 and salinity in your Figure 3 to support this discussion.

Lines 365-366: State the actual factor that you used here too. Was it 34?

Lines 392-393: It seems like it would be nice to mention this parallel study earlier in your paper and give it a bit more discussion.

Lines 378-383: This all seems very speculative. As do lines 400-403.

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