We appreciate the associate editor's feedback, and the additional comments from one of the three original reviewers. Following are specific responses to the AE's and referee's comments in blue; excerpts from the original and revised manuscript are *italicized blue*.

Associate Editor Decision: Publish subject to minor revisions (review by editor) (09 Jul 2021) by Ji-Hyung Park
Comments to the Author:
Dear Authors,

Thank you for your comprehensive revisions to incorporate the comments and suggestions offered by three reviewers.

One of the three reviewers has kindly provided a second referee report, as copied below. I agree with the reviewer on the improved structure and clarity of the manuscript.

Thank you for this feedback. The constructive comments received during the revision process has led to an improved manuscript.

However, the reviewer also noted that you could further improve the interpretation of some important results while reducing the complexity shown in the long title and the large number of tables and figures. In addition, I would like to draw your attention to editorial details, including
- Some mismatch between the clean and track-changes versions, such as "and XX% of total annual emissions" (L 33, track-changes version) We addressed this mismatch for this submission:

Line 33 in tracked-changes v1: *The main difference between years was a period of elevated emissions lasting less than two weeks in the spring of 2018, which contributed 17% of the annual emissions in the shallow region of the reservoir, and XX% of total annual emissions.*

Line 32 in tracked-changes v2: *The main difference between years was a period of elevated emissions lasting less than two weeks in the spring of 2018, which contributed 17% of the annual emissions in the shallow region of the reservoir.*

- Reference format: capitalized titles (e.g., DelSontoro et al.)

Changed capitalized titles to sentence case.

- Inconsistent table format (Tables 1-4) and some corrections required for Table 1 (indicate the number of sites under "Spatial coverage") Table 3 (use the same unit for the three N species), and Table 4 (indicate significance levels and remove "20" if it is a typo).

Made the table fonts consistent; changed the units in Table 3.

Your manuscript can be published after a careful revision to address the reviewer comments and editorial errors. I would like to ask you to submit your revised manuscript together your responses as instructed in the author guidelines.

The author's response in case of "minor" or "major" revisions must be submitted as one separate *.pdf file (indicating page and line numbers), structured in a clear and easy-to-follow sequence: (1) comments from referees/public, (2) author's response, and (3) author's changes in manuscript. Regarding author's changes, a marked-up manuscript version (track changes in Word, latexdiff in

LaTeX) converted into *.pdf and combined with the author's response should be provided.

Sincerely,

Ji-Hyung Park
Associate Editor, Biogeosciences

General comments The revised version of the manuscript has a much better quality compared to the previous one. The structure and clarity have particularly improved. Authors have clearly put a lot of effort in responding to the comments, and their responses are overall satisfactory. However, I would like to point out few additional comments below that should be resolved before publication:

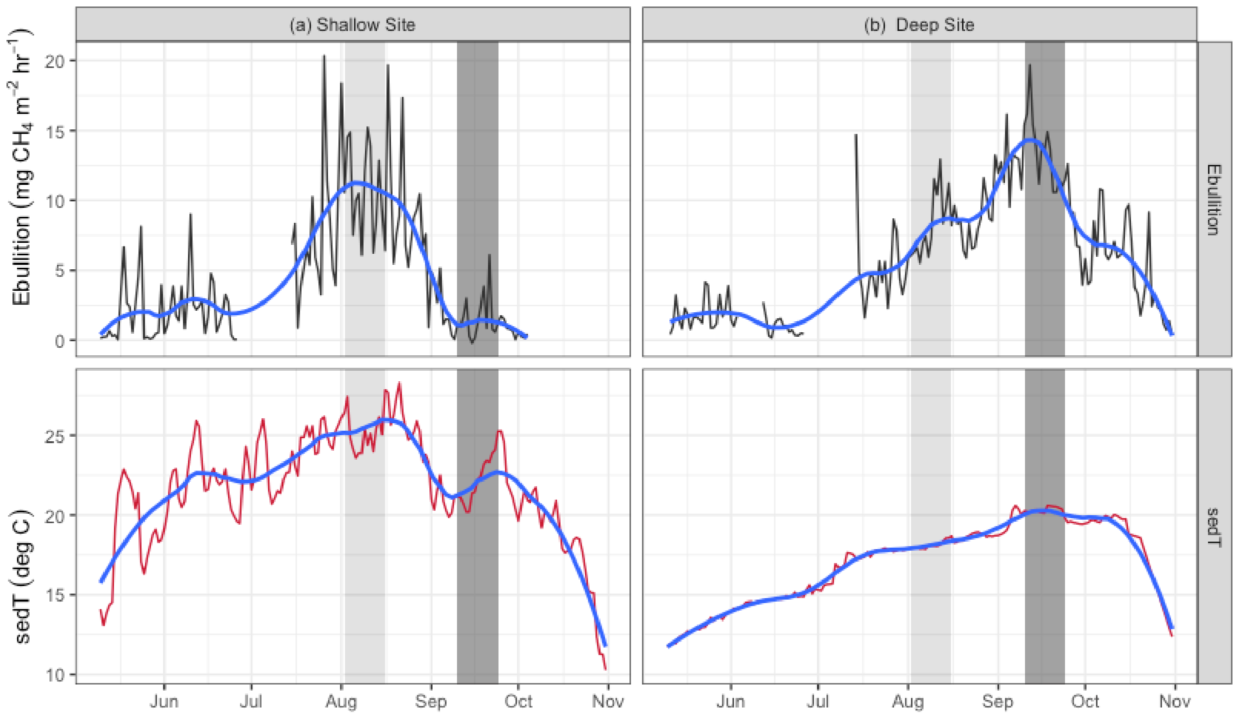
The title could be drastically reduced. Here is a suggestion: 'Temporal trends in methane emissions from a small eutrophic reservoir: the key role of spring burst'.

Thank you for this suggestion. We adopted this title with one small change: using an indefinite article, as we only saw one spring burst: "Temporal trends in methane emissions from a small eutrophic reservoir: the key role of **a** spring burst".

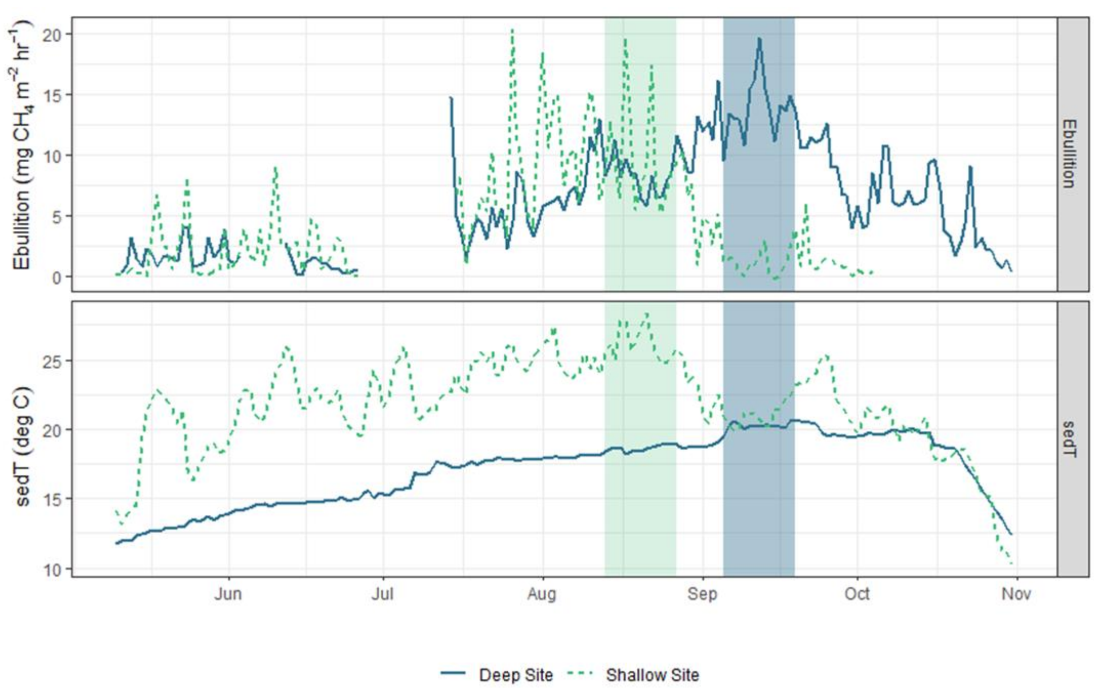
While the number of figures and tables was reduced from the previous version, it is still quite high for a standard article. Thus, some of the tables and figures can easily be moved to the supplementary section (ex: Table 1 and Figure 6). Also, in Figure 8, combining the panels for shallow and deep sites would make the message more obvious and reduce the number of figure panels.

We moved Table 1 to the supplement. We combined the panels in Figure 8:

Original:



Revised:



We retained Figure 6 because it is important in illustrating the intra-lake variability, a key consideration in interpretation and upscaling of reservoir F_{CH4}.

The interpretation of the results is problematic in some cases, either due to sentence formulation or to lack of evidence from the results of the study:

• Line 447-448: the suggestion to assume zero wintertime methane flux from all temperate reservoirs based only on the results from this one studied temperate system is very unreasonable.

We removed the prescriptive statement. Changed from:

> *Most studies that report F_{CH4} from inland waters monitor during the warm season, with less than six months of measurements (cf. Deemer et al., 2016; DelSontro et al., 2018 Bastviken et al., 2011), and the mean F_{CH4} value is then extrapolated to annual total emissions. It may however be better to assume zero F_{CH4} during wintertime months for temperate reservoirs, given the very low (on the same order as the warm-season uncertainty) wintertime F_{CH4} measured in this study.*

To:

> *Most studies that report F_{CH4} from inland waters monitor during the warm season, with less than six months of measurements (cf. Deemer et al., 2016; DelSontro et al., 2018 Bastviken et al., 2011), and the mean F_{CH4} value is then extrapolated to annual total emissions. However, we measured very low (on the same order as the warm-season uncertainty) wintertime F_{CH4} in this study.*

• Line 464: 'in contrast to the earlier assessment of age and latitude as the main drivers'. While productivity was recently identified as a methane driver, it does not mean at all that age and latitude are less important drivers.

We intended this statement to convey that the Barros et al. age and latitude models predict much lower emission rates than we observed, thus something else (productivity) must be important. We changed this statement from:

> *This result strengthens the finding that midlatitude, eutrophic reservoirs in the Midwest US can support high CH4 emission rates (cf. Beaulieu at al., 2014, 2016), and also supports the emerging body of knowledge around the importance of reservoir productivity as a key indicator for FCH4 (cf. Deemer et al., 2016; West et al., 2012; DelSontro et al., 2018b) in contrast to the earlier assessment of age and latitude as the main drivers (Barros et al., 2012).*

To:

> *This result strengthens the finding that midlatitude, eutrophic reservoirs in the Midwest US can support high CH_4 emission rates (cf. Beaulieu at al., 2014, 2016) than would be predicted by age and latitude alone (Barros et al., 2012). The high annual F_{CH4} also supports the emerging body of knowledge around the importance of reservoir productivity as a key indicator for F_{CH4} (cf. Deemer et al., 2016; West et al., 2012; DelSontro et al., 2018b).*

• Line 510-511: the sentence is presented as a definite conclusion, however, alloOC and autoOC were not measured in this study, so the interpretation of this result should be presented as a potential explanation rather than a fact. Also it is not clear to which results exactly the authors refer to when saying that FCH4 was more stable (not visible in Fig 2).

We agree that this should be presented as a potential explanation, and that the statement about F_{CH4} being "more stable" in 2017 is unclear. We changed this section from:

> *The difference in hydrologic regimes and subsequent availability of autoOC versus allochthonous OC (alloOC, i.e. particulate or dissolved C derived from terrestrial plant tissue) also sheds light on interannual differences beyond the spring burst. The lab study by Grasset et al. (2018) found that while additions of autoOC led to pulses of F_{CH4}, alloOC took longer to decompose and additions led to more gradual but sustained F_{CH4}. Thus, the wet spring of 2017 loaded the reservoir with slow-burning alloOC, and F_{CH4} was more stable, tracking with sedT to peak emissions in early fall (Fig. 2).*

To:

> *The difference in hydrologic regimes and subsequent availability of autoOC versus allochthonous OC (alloOC, i.e. particulate or dissolved C derived from terrestrial plant tissue) may also shed light on interannual differences beyond the spring burst. The lab study by Grasset et al. (2018) found that while additions of autoOC led to pulses of F_{CH4}, alloOC took longer to decompose and additions led to more gradual but sustained F_{CH4}. Thus, the wet spring of 2017 may have loaded the reservoir with slow-burning alloOC, which could partially explain the smaller magnitude of F_{CH4} pulses in 2017 compared to 2018 (Fig. 2).*

• Line 514-517: 'However…(Eqn 7)' the phrasing of these two sentences suggest that there is no existing literature on the climatic drivers of methane flux, except the relation with productivity. This makes little sense and I suggest removing the sentences completely. This was not our intent, and we have re-worked this whole paragraph to improve the messaging, see response to next comment.

• Line 517-520: the results from the comparison with the predictive model contrast with the observed ones (higher observed fluxes in 2018 despite the lower mean chla), they do not align like the sentence suggests. Please reformulate to clarify the interpretation here. We meant to convey that the predicted magnitudes compare well to the measured F_{CH4}, but using mean annual chla flip-flops which year had larger emissions. We have revised the whole paragraph to address this comment as well as comments on lines 512-517. The original read:

> *The implications of the spring burst phenomenon on upscaling to total FCH4 are twofold. In terms of characterizing current total reservoir FCH4, the spatial and temporal variability of the spring burst mitigate its influence. This is illustrated by comparing the lake-wide survey results to the hybrid upscaling results, which agree well in both 2017 and 2018 (Fig. 5). However, in predicting future reservoir FCH4 under changing climatic regimes, it is important to characterize underlying processes that relate 515 to the climatic drivers of precipitation and temperature. Using reservoir productivity to predict FCH4 is a good place to start: the size-productivity model (Del Sontro et al., 2018) uses annual mean chla levels to predict FCH4 (Eqn 7). Acton Lake's mean chla was higher in 2017 than 2018 (Fig. 7), and the model predicts 11.1 and 10.3 mg CH4 m-2 hr-*

> *1, respectively for 2017 and 2018. These values agree well with our estimates using the hybrid upscaling approach (Table 2) but flip the finding of which year had larger CH4 emissions, which was driven by sub-annual productivity dynamics. A burgeoning body of knowledge points to the importance of phytoplankton ecology on lake and reservoir CH4 production, in terms of both the amount (Hartman et al., 2020; McClure et al., 2020; Zhang et al., 2021) and type (Bartosiewicz et al., 2021). Furthermore, warmer springs have increased the frequency and intensity of cyanobacterial blooms in midwestern US reservoirs over the past two decades (Smucker et al., 2021), and continued warming will likely intensify this phenomenon. Thus, the underlying factors that led to the 2018 spring burst at Acton Lake may be more common in the future and have a greater effect on the reservoir CH4 budget.*

Revised version:

> *The impact, or lack thereof, of the spring burst on reservoir-wide cumulative F_{CH4} has implications for the value of higher-resolution measurements. This is analogous to the question of whether the increased complexity of process-based models improves prediction over empirical models (cf. Adams et al., 2013). While the EC monitoring results almost doubled from 2017 to 2018, the hybrid upscaled estimate had only an 11% difference (Table 2, Fig. 5). Furthermore, the cumulative F_{CH4} determined via the lake-wide surveys was closer to the hybrid upscaled estimate than the EC results in 2018 (Fig. 5). Using the recent size-productivity model (Del Sontro et al., 2018) to predict F_{CH4} at Acton Lake based on mean annual chla levels (Eq. 7, Fig 7) yields estimates of 11.1 and 10.3 mg CH_4 m^{-2} hr^{-1} for 2017 and 2018, respectively. These values are in the same range as the warm season mean fluxes determined via the hybrid approach for Acton Lake (Table 2). However, the model results contrast with measured results in terms of which year had higher F_{CH4}. Furthermore, the model results would over-estimate cumulative annual F_{CH4} for Acton Lake as they do not take low wintertime emissions into account.*
>
> *Sub-annual climatic patterns and productivity dynamics may become more important in understanding and predicting reservoir F_{CH4}. Recent research demonstrates how warmer springs have increased the frequency and intensity of cyanobacterial blooms in midwestern US reservoirs over the past two decades (Smucker et al., 2021), and continued warming will likely intensify this phenomenon. There is also a burgeoning body of knowledge that points to the importance of phytoplankton ecology on lake and reservoir CH_4 production, in terms of both the amount (Hartman et al., 2020; McClure et al., 2020; Zhang et al., 2021) and type (Bartosiewicz et al., 2021). Furthermore, Thus, the underlying factors that led to the 2018 spring burst at Acton Lake may be more common in the future and have a greater effect on the reservoir CH_4 budget.*

- Lines 566-568: I don’t understand how the authors come to the conclusion that system productivity is a more important predictor of methane compared to latitude since they studied only one system thus one latitude. Besides, latitude was previously used in the literature as a proxy for temperature for predicting methane. In their study, the authors find sediment temperature to be the best predictor of methane flux, not productivity. Thus, their interpretation of the most important driver is contradictory.

Similar to the comment on line 468, we intended this statement to convey that the Barros et al. age and latitude models predict much lower emission rates than we observed, thus something else (productivity) must be important. We changed this statement from:

> *These levels of emissions place Acton Lake in the upper quartile of emission rates reported from reservoirs (Deemer et al., 2016), further supporting the concept that system productivity is a more important factor than latitude in predicting CH4 emission rates (Del Sontro et al., 2018).*

To:

> *These levels of emissions place Acton Lake in the upper quartile of emission rates reported from reservoirs (Deemer et al., 2016), further supporting the concept that highly productive mid-latitude reservoirs can have higher magnitude CH4 emission rates than would be predicted by age and latitude alone (Del Sontro et al., 2018).*

Specific comments

- Line 19: 'arenot' add space done
- Line 62: remove 'by phytoplankton' as this is not the only hypothesized pathway for CH4 production in surface waters. Done
- Line 64-65: the term 'surface mixed layer CH4' is confusing, reformulate the sentence. Changed to "flux from surface waters".
- Line 74: not sure what you mean by 'stochastic systems' Changed to "irregular systems"
- Line 237: 'They surveys' should be 'The surveys'? Yes, changed to 'the surveys'.
- Line 349: 'FCH4' the CH4 should be subscript Changed
- Line 413-416 and 426-428: these sentences belong in the discussion section rather than the results.
    - Line 413 – 416: *The results from the six spatial surveys indicate an inconsistent spatial pattern in FCH4 that differs from previous findings on CH4 emissions from temperate, eutrophic reservoirs which has shown that the river – reservoir transition zone near the tributary inlets tends to be a hot spot for emissions compared to the lacustrine zone (Beaulieu et al., 2014; Beaulieu et al., 2016; DelSontro et al., 2011; Tuser et al., 2017).*
        - We moved this statement to section 4.2 of the discussion: Implications for upscaling
    - Line 426 – 428: *Emission behaviour at sites U-09 and U-06 was substantially different than at other sites: these two sites had consistently low FCH4 and tend to have higher rates of CH4 diffusion than ebullition. Much of this behaviour is likely explained by the proximity of these sites to Acton Lake's swimming beach, which has a sandy substrate that likely inhibits methanogenesis at these sites.*
        - We retained this statement in the results section. This explanation of the emissions at these sites does not directly relate to any part of the study findings explored in the discussion. We think it fits better in the results as it is intended to explain why those two sites weren't investigated further.
- Line 443 and line 451: 'Deemer et al.' the reference lacks the date Added
- Line 466: representative of what? Please rephrase this sentence. Changed to: "what is representative of reality?"

- Line 479 and 482: replace '*' by 'x' done
- Line 512-514: these two sentences are very unclear
    - This relates to the reviewer comments on lines 514-517 and 517-522, above. We have reworked this paragraph to clarify our point about process-based models. The sentences in 512-514 are: *The implications of the spring burst phenomenon on upscaling to total FCH4 are twofold. In terms of characterizing current total reservoir FCH4, the spatial and temporal variability of the spring burst mitigate its influence. This is illustrated by comparing the lake-wide survey results to the hybrid upscaling results, which agree well in both 2017 and 2018 (Fig. 5).*

      Which we changed to: *The spring burst's impact, or lack thereof, on reservoir-wide cumulative FCH4 has implications for the value of higher-resolution measurements. This is analogous to the question of whether the increased complexity of process-based models improves prediction over empirical models (cf. Adams et al., 2013). While the EC monitoring results almost doubled from 2017 to 2018, the hybrid upscaled estimate had only an 11% difference (Table 2, Fig. 5). Furthermore, the cumulative FCH4 determined via the lake-wide surveys was closer to the hybrid upscaled estimate than the EC results in 2018 (Fig. 5).*
- Line 547: 'wefocus' add a space done

References:

Adams, H., Williams, A., Xu, C., Rauscher, S, Jiang, X., and McDowell, N., 2013. Empirical and process-based approaches to climate-induced forest mortality models, Frontiers in Plant Science, 4. 10.3389/fpls.2013.00438