

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*'.

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.

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.
- 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.
- 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).
- 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.

- 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 chl_a), they do not align like the sentence suggests. Please reformulate to clarify the interpretation here.
- 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.

Specific comments

- Line 19: 'arenot' add space
- Line 62: remove 'by phytoplankton' as this is not the only hypothesized pathway for CH₄ production in surface waters.
- Line 64-65: the term 'surface mixed layer CH₄' is confusing, reformulate the sentence.
- Line 74: not sure what you mean by 'stochastic systems'
- Line 237: 'They surveys' should be 'The surveys'?
- Line 349: 'FCH₄' the CH₄ should be subscript
- Line 413-416 and 426-428: these sentences belong in the discussion section rather than the results.
- Line 443 and line 451: 'Deemer et al.' the reference lacks the date
- Line 466: representative of what? Please rephrase this sentence.
- Line 479 and 482: replace '*' by 'x'
- Line 512-514: these two sentences are very unclear
- Line 547: 'wefocus' add a space