Responses to Reviewer #2

This manuscript presents a process-based model for methane emissions from European lakes, and uses the model to estimate methane emissions from European lakes <1000 km^2 via a gridded approach. The authors compare their model results to a regional dataset of summer point-in-time methane emissions (Rinta et al. 2017) and time series methane data from 4 European lakes (Natchimuthu et al. 2016, Sollberger et al. 2017, Varadharjan et al. 2009, and multiple studies on Villasjön as cited in Tan et al. 2024). The authors note generally good agreement, but also some interesting differences between the model estimates and the time series data. They call for more time series and ancillary data to help refine models. Overall, this paper represents an important advance in our ability to model regional-scale methane emissions from lentic waterbodies.

Response: Thank you very much for your positive feedback. The point-by-point responses to your comments are provided below in blue, with the corresponding revisions to the manuscript in red.

General Comments:

1. I would like to see a bit more discussion of the strengths and weaknesses of the underlying comparison datasets the authors did use. For example, the fact that the FLaMe model estimates higher ebullitive emissions than were reported in Rinta is not surprising to me given that the Rinta et al. 2017 study used floating chambers over a relatively short duration (6hr) and did not employ bubble traps, but rather estimated ebullition based on k_{600} of methane (which could be estimated high if every chamber on a given lake had some amount of ebullition, but not enough to generate "unreasonable" k_{600}). In general, the ability of the studies on these 4 lakes to differentiate the relative role of diffusion and ebullition (and to capture turnover, weather & water level-related hot moments) could be

discussed.

Response: Thank you very much for this valuable comment. Following your suggestion, we have added the description of the datasets of 47 European lakes from Rinta *et al.* (2017), and briefly discussed its strengths and weaknesses in section 2.5.3 (lines 644–659):

"… Next, the simulated diffusive and ebullitive CH_4 emission rates will be evaluated against *in-situ* measurements compiled by Rinta *et al.* (2017) from 17 boreal lakes (in southern Finland and Sweden) and 30 central European lakes (in The Netherlands, Germany and Switzerland). This dataset is adopted because it can not only differentiate the ebullitive and diffusive CH_4 fluxes during late summer (August and September, 2010–2011) but also provides information regarding environmental conditions of the study area (mean annual air temperature, annual precipitation, percentage of forests and managed land in the catchment) and water chemistry of the studied lakes (temperature, conductivity, pH, absorbance, TP and TN in surface water, and average TP and TN in the water column), which are helpful for understanding the lake methane dynamics within these two contrasted regions. However, this dataset of 47 lakes still has some important limitations, in particular as it presents only summer-time observations, and not time-series which would comprise the full seasonal cycle including turnover events and other hot moments. In addition, it contains potential biases induced by the calculation methods used for separating the measured CH_4 fluxes into diffusive and ebullitive pathways. In particular, Rinta *et al.* (2017) used floating chambers over a relatively short duration (6hr), which might not be able to detect sporadic ebullition events, and did not employ bubble traps to estimate the ebullitive flux."

In addition, the weakness related to the calculation methods for separating diffusion and ebullition in Rinta *et al.* (2017) has also been added in section 3.3.1 (lines 859–862):

"The slightly higher ebullitive fluxes simulated by FLaMe-v1.0 may be attributed to not only the uncertain choice of model parameters (e.g., α) but also to the systematically lower measured ebullitive fluxes in Rinta *et al.* (2017), where ebullition was separated from diffusion when the measured fluxes produced unreasonably high k_{600}."

For the four well-surveyed real lakes, the limited observations of diffusive and ebullitive fluxes, especially in Lake Klöntal and Lake Upper Mystic, make it difficult to evaluate the ability of the model to separate these two emission pathways. Moreover, the lack of *in-situ* measured climatic drivers and variations in lake water level and areas limits the model's ability in capturing lake turnovers and weather/water level related hot-moments of emissions. These points have been clarified in lines 602–607:

"Since the lack of concomitant *in-situ* measurements of climatic drivers and variations in lake water levels affect the model's ability to capture the full variability in the time series of observed CH_4 emission, we here focus our evaluation on the magnitude and broad seasonal patterns in observed CH_4 emissions, following what can be achieved for regional and global scale applications. Thus, we evaluated the simulated statistics (mean and SD represented by boxplots) of CH_4 fluxes over the annual cycle against the observational data."

2. The sensitivity analysis is very helpful for understanding the importance of various model parameters (which are informed by literature values). The importance of f_{mm} (fraction of mineralization that results in methane production) is interesting & the authors could discuss some literature finding very different sediment methane production potential in different lentic sediment

types (Bodmer et al. 2025 L&O). On a related note, did this upscaling include reservoirs? The authors mention using the Messager et al. 2016 dataset, but don't describe whether they filtered out reservoirs. Finally, I was surprised at the relatively limited effect of methane oxidation rate and Q_{10}. At least mentioning that the model requires oxygen for methane oxidation (but that anaerobic methane oxidation can also be important) would help better represent some uncertainty here I think.

Response: Thank you very much for this valuable comment. First, following your suggestion, we have added the findings related to the importance of f_{mm} (lines 941–943):

"This is also supported by the findings of high potential methane production rates in various freshwater systems (including the lakes, reservoirs and rivers) (Bodmer *et al.*, 2025)."

Second, we clarify here and in the revised manuscript that the lakes simulated in this study are all natural lakes. The description of lakes used in the European domain application was thus clarified as follows (lines 612–614):

"To implement the model at the scale of Europe (25°W–60°E, 36°–71°N), we extracted the natural lakes (type I) within this domain from the HydroLAKES database (Messager *et al.*, 2016; n=108407, total area = 1.33×10^5 km^2 for lakes with $0.1 \leq A_0 \leq 1000$ km^2 within the European domain)."

Third, the maximum oxidation rate k_{max} barely affects the total CH_4 emissions, because this parameter mostly controls the location and thickness of the oxidation zone, while the volume-integrated rates remain essentially unaltered, a characteristic feature of many secondary redox reactions (e.g., Thullner and Regnier, 2019; Grossart et al., 2011). As for the temperature dependence of oxidation ($Q_{10,ox}$), the sensitivity is even weaker because changing the $Q_{10,ox}$ value has a lower impact on the oxidation rates than changing k_{max}. Moreover, we here clarify that changes in parameters controlling methane oxidation rate modulate European lake CH_4 emissions by -4%–9%, revealing that it indeed plays a role in regulating regional lake CH_4 emissions. However, its effect is more limited than the ones induced by other model parameters (such as f_{mm} and $P_{chl,max}$). This suggests that CH_4 emissions are mainly sensitive to carbon related processes of primary production and mineralization than to methane oxidation rates (and $Q_{10,ox}$), which here requires the presence of oxygen. Note that this result is only specific to FLaMe-v1.0, and the sensitivity of CH_4 emissions to oxidation parameters might be higher (e.g., in ALBM model, Tan *et al.*, 2024). Furthermore, we agree with the reviewer that anaerobic methane oxidation pathways (omitted in our current model version) should be mentioned. In the revised manuscript, these points are highlighted as follows (lines 970–980):

"The parameter k_{max} barely impacts the total CH_4 emissions, as this parameter mostly influences the thickness of the water layers where the profiles of oxygen and methane overlap and the oxidation occurs, while the volume-integrated rates remain essentially unaltered (Thullner and Regnier, 2019; Grossart *et al.*, 2011). As for the temperature dependence of oxidation ($Q_{10,ox}$), the sensitivity is even weaker because changing the $Q_{10,ox}$ value has a lower impact on the oxidation rates than changing k_{max}. Compared to other parameters (such as f_{mm} and $P_{chl,max}$), the relatively low effects of k_{max} and $Q_{10,ox}$ do not mean that the methane oxidation is not important, but highlight the dominant role of organic carbon production and decomposition on lake CH_4 emissions, which were seldom simulated in previous models. Note that in our current model version, CH_4 oxidation only occurs through the aerobic pathway and thus neglects the potential additional controls induced by anaerobic pathways (Mostovaya *et al.*, 2022; Su *et al.*, 2020)."

3. Finally, the authors present a range of estimated European lake methane emissions in the

introduction, but they do not revisit the range of estimates in their discussion. How does the emission they estimate with a process-based model compare? Can the authors separate the effect of lakes >1000 km^2 on the existing estimates to do a direct comparison?

Response: Thank you very much for this valuable comment. Following your suggestions, we came up with two strategies to estimate the CH_4 emissions from European lakes ≥1000 km^2 as follows:

(1) We used the FLaMe-v1.0 directly to simulate lakes ≥1000 km^2 within the European domain and found that the CH_4 emissions from European lakes ≥1000 km^2 accounts for only 6% of those from smaller lakes, i.e., 0.06 Tg CH_4 yr^{-1}. Thus, the European lakes have a total CH_4 emissions of 1.03 Tg CH_4 yr^{-1}.

(2) Despite with very limited samples, Johnson *et al.* (2022) found that the total CH_4 emission (sum of diffusion and ebullition) rates per unit area from lakes ≥5000 km^2 are in the range of 0–25% of emissions from smaller lakes. Following Johnson *et al.* (2022), we assumed that the CH_4 emission rates per unit area from lakes ≥1000 km^2 falls in the higher end of 0–25% and adopt a value of 20% to estimate the CH_4 emission rates for lakes ≥1000 km^2.

From our simulations for lakes smaller than <1000 km^2, we obtained that the mean CH_4 emission rate per unit lake area amounts to 7.39 g CH_4 m^{-2} yr^{-1}. Thus, the mean CH_4 emission rate per unit lake area for lakes larger than 1000 km^2 is estimated as 7.39•20% = 1.48 g CH_4 m^{-2} yr^{-1}. By multiplying this mean CH_4 emission rate (1.48 g CH_4 m^{-2} yr^{-1}) with the area of 0.88•10^5 km^2 for lakes ≥1000 km^2, we obtain CH_4 emissions from large lakes as 0.13 Tg CH_4 yr^{-1}, broadly similar to what is obtained with the first method. Thus, the European lake emissions reach in this case a total CH_4 of 1.10 Tg CH_4 yr^{-1}.

Combining these two strategies, we provide a back of the envelope estimate for the total CH_4 emissions from European lakes as 1.03–1.10 Tg CH_4 yr^{-1}, which can be directly compared to previous estimates. We added the comparison between our estimates with previous ones in section 3.3.2 (lines 893–897):

"Note that, by including the estimated emissions from European lakes larger than 1000 km^2 with two different strategies (Supplementary Text S5), we provide a back of the envelope estimate for the mean total annual emission as 1.03–1.10 Tg CH_4 yr^{-1}, which falls within the lower end of a previously reported range (0.9–2.5 Tg CH_4 yr^{-1}) (Petrescu *et al.* 2023; Lauerwald *et al.*, 2023)."

Detailed Comments

1. Title: I think the acronym FLaMe is fine, but I did want to make sure the authors were aware that there is already a method for "high speed limnology" that has been used to study methane dynamics in rivers called FLAMe, see original description: https://pubs.acs.org/doi/epdf/10.1021/es504773x?ref=article_openPDF, and example of application to methane: Large Spatial and Temporal Variability of Carbon Dioxide and Methane in a Eutrophic Lake - Loken - 2019 - Journal of Geophysical Research: Biogeosciences - Wiley Online Library

Response: Thank you very much for drawing our attention to this point. We consider the acronym FLaMe (**F**luxes of **La**ke **Me**thane) is still the best for our model, but propose to systematically use FLaMe-v1.0 as model name to avoid any confusion. In addition, we commit to keep numbering subsequent model upgrades using their version number (e.g., v2.1, v3.2) in the future. We have thus systematically replaced FLaMe occurrences by FLaMe-v1.0 in the revised manuscript.

2. Line 24: I don't love the term "CH_4 cycling" since I think of elements cycling (e.g. ch4 is part of

carbon cycling)

Response: Agreed. We have removed the word "cycling" here and elsewhere in the text when associated with CH_4.

3. Line 25: maybe add that these are theoretical lakes (not lakes with data to verify against)

Response: "Theoretical" has been added, and this sentence has been revised as follows:

"We first test the performance of FLaMe-v1.0 by analyzing physical and biogeochemical processes in two theoretical lakes with characteristics that can be considered representative for many lakes (an oligotrophic, deep lake driven by cold climate *versus* a eutrophic, shallow lake driven by warm climate)."

4. Line 26: I think by a "trophic" lake, you mean "eutrophic"

Response: Thank you, and the word "trophic" has been corrected as "eutrophic".

5. Line 76: You might also mention that our appreciation for emergent vegetation (aerenchyma transport) has also lagged… I see you mention this in the discussion

Response: Agreed. The transport through vegetation aerenchyma has been added in this sentence:

"… measurements related to lake turnover events (release of previously accumulated CH_4 due to stratification and ice cover) and transport through vegetation aerenchyma remain highly challenging …"

6. Line 109: remove "with" and just say "to tackle these challenges"

Response: Agreed. The word "with" has been removed.

7. Line 120: define ESM

Response: The definition of ESM has been added in the sentence:

"…(iii) a potential coupling to Earth System Models (ESMs) in subsequent stages of its development."

8. Line 127: add "the" between real and world

Response: The word "the" has been added, and the phrase is revised as "in the real world".

9. Line 147: how do you know the arithmetic mean lake depth?

Response: Since the HydroLAKES database provides the depths for all lakes compiled, we calculated the arithmetic mean of lake depths of all lakes pertaining to the respective size class in each of the grid cell of our model domain. In the revised manuscript, this point has been clarified as follows (lines 146–151):

"We then run a FLaMe-v1.0 simulation for one representative lake per size class within each grid cell, using the arithmetic mean of lake area, depth and trophic status of all lakes pertaining to the respective size class across the respective grid cell. Note that the areas and depths of all lakes are available from HydroLAKES database (Messager *et al.*, 2016) while trophic status is derived from outputs of the GlobalNEWS model (Mayorga *et al.*, 2010; Lauerwald *et al.*, 2019)."

10. Line 181: "are" to "is"

Response: We guess that this is a misunderstanding, as "variations in water depth" should be followed by "are" and not "is".

"However, this morphology is unsuitable for the simulation of biogeochemical processes, especially when variations in water depth within each lake are important."

11. Figure 2: describe the colors in your legend. I think purple is the water column and orange is the "sediment column"?

Response: Following your comment, the colors have been added in the figure caption:

"Purple color indicates the water layers, and orange color indicates the sediment columns."

12. Line 218: You might consider a "but see" statement about the Grasset et al. 2018 paper, they did find degradation of fresh allochthonous OC to stimulate methane production, but they found that autochthonous organic carbon generally decomposed faster https://aslopubs.onlinelibrary.wiley.com/doi/full/10.1002/lno.10786

Response: Thank you very much for your suggestion. This sentence has been revised accordingly (lines 236–242):

"The allochthonous C inputs delivered from surrounding catchments are more refractory and generally have a lower decomposition rate (Grasset *et al.*, 2018; Guillemette *et al.*, 2017; DelSontro *et al.*, 2018), although CH_4 production from allochthonous OC has in some instances been reported to be higher than from autochthonous compounds in laboratory incubations (Grasset *at al.*, 2018). Thus, we consider the allochthonous OC as less important substrates for CH_4 production, and consider the autochthonous primary production as the only labile OC source in this first model version; the allochthonous OC contribution will be added in the future upgrade of the model."

13. Line 480: It won't change the Schmidt numbers much at all, but there is a more updated paper by Wanninkhof with small adjustments to the coefficients: Wanninkhof 2014 L&O Methods

Response: Thanks for bringing our attention to this point. We acknowledge that newer formulations to constrain the Schmidt number have been published, but these adjustments are expected to induce minor changes in the gas exchange rates and, thus, CH_4 emissions. Yet, we have added an explicit reference to these new formulations in the revised text (lines 522–525):

"Note that more recent formulations of k_{ge} have been published in the last decade (Wanninkhof *et al.*, 2014; McIntire et al., 2020) but we here choose to use Eq. (33) to be consistent with previous lake modelling studies (Tan et al., 2015; Stepanenko *et al.* 2016; Tan *et al.*, 2018)."

14. Line 505: fix typo: should be "anaerobic"

Response: Corrected.

15. Line 512: See comments in general summary above. The Rinta dataset is not the most robust

Response: We agree with the reviewer that the dataset from Rinta *et al.* (2017) is not the most robust, but it is nevertheless highly suitable for demonstrating the ability of our model to capture CH_4 emission rates across a large number of European lakes spanning gradients of climate conditions and trophic levels. As mentioned already in our response to General Comment #1, we have provided a more detailed description of the strengths and weaknesses of the dataset from Rinta *et al.* (2017).

16. Line 553: contrast to "contrasting"

Response: Corrected.

17. Line 562: citation for lake methane data?

Response: The lake methane data comes from Tan *et al.* (2024), and the citation has been added.

18. Line 578: Did you just use natural lakes (type 1) or did you include type 2 and type 3 (reservoirs)?

Response: We used only natural lakes (type I), and this point has now been clarified in the revised manuscript (lines 612–614):

"To implement the model at the scale of Europe (25°W–60°E, 36°–71°N), we extracted the natural lakes (type I) within this domain from the HydroLAKES database (Messager *et al.*, 2016; n=108407, total area = 1.33×10^5 km^2 for lakes with $0.1 \leq A_0 \leq 1000$ km^2 within the European domain)."

19. Line 617: "to" should be "with"

Response: Corrected.

20. Table 3: It is difficult to tell how the range of methane oxidation Q_{10}s compare to the Q_{10} of methanogenesis since the methanogenesis is tied directly to the Q_{10} of overall mineralization. Did your scenarios include cases where the Q_{10} of methane oxidation is higher than for methanogenesis (as was seen in rivers in Shelley et al. 2015 Freshwater Biology)?

Response: Although all the parameters indicating temperature dependence are provided in Table 1, we agree that it is difficult to compare the relative magnitudes of Q_{10} for methanogenesis and oxidation. In Table 1, we have three parameters related to temperature dependence, $Q_{10,prod}$ for primary production, θ for mineralization, and $Q_{10,ox}$ for methane oxidation. The collective effects of $Q_{10,prod}$, θ, and the fraction of mineralization channeled into methanogenesis (f_{mm}) determine the overall temperature dependence of Q_{10} for methanogenesis, which is difficult to be quantified/evaluated.

21. Figure 5: Is this just showing modeled estimates without data?

Response: We would like to clarify that Fig. 5 is used to show the model's capability in capturing the contrasting physical and biogeochemical behaviors with two ***theoretical***, representative cases. These two lakes are set ***theoretically***, and thus there are no observations in this figure for comparison.

We acknowledge that the comparison between simulations and observations is necessary for model evaluation and validation, and thus conducted the analysis in section 3.2, i.e., Evaluation of simulated temporal lake CH_4 emissions against observations from four well-surveyed lakes.

22. Line 675: I suggest reporting in mg m^{-2} d^{-1}. Also, this is a very low rate… There are only 4 lake/reservoirs in Rosentreter et al. 2021 dataset that are below 0.24 mg CH4 m^{-2} d^{-1} (and they are all reservoirs as it happens): Solina (Gruca-Rokosz et al. 2010), St. Aniol (Gómez-Gener et al. 2015), CB2 and CB3 (Teodoru et al. 2015).

Response: We agree and corrected the unit here to "mg m^{-2} d^{-1}".

As for the very low CH_4 emission rate reported here, we understand the concerns raised by the reviewer, and indeed could adjust the model configuration to reach higher CH_4 emission values. But we would like to remind the reviewer that this result is for the extreme theoretical case of a deep oligotrophic lake driven by cold climate (hence with very low CH_4 emissions). The purpose of adopting these two extreme theoretical cases (i.e., deep oligotrophic lake driven by cold climate *versus* shallow eutrophic lake driven by warm climate) is to demonstrate the model's capability of capturing the whole range of CH_4 emission rates found in the real world. Both our real case applications and the theoretical cases produced in the supplementary information actually fall within the extreme range reported here.

23. Line 690: again, I think you mean "eutrophic" not "trophic" here

Response: Thanks a lot. The word "trophic" has been replaced by "eutrophic".

24. Line 695: would be nice to quantify how much lower the emissions are when the lake is modeled as deeper instead of just saying that emissions were "lower"

Response: We agree, and the sentence has been revised as follows:

"By increasing lake depth from 15 m to 35 m (Fig. S14), the CH_4 production rates remain almost the same, i.e., 20 mg CH_4 m^{-2} d^{-1} for the yearly mean and 60 mg CH_4 m^{-2} d^{-1} for the peak, while the CH_4 emissions are overall lower (35 to 22 mg CH_4 m^{-2} d^{-1} for the peak without considering the storage flux) for the deeper lake."

25. Figure 6: The y axes are labelled mg CH_4 m^{-2} d^{-1}, but the text is discussing very high emissions in panel A in units of g CH4 m^{-2} d^{-1}. Check for accuracy? I agree that 18.76 g CH4 m-2 d-1 (line 712) would be a very very high lake-wide flux.

Response: Thank you very much for spotting this typo. The unit in Figure 6 is correct, and we changed the units in the text into "mg CH_4 m^{-2} d^{-1}".

26. Line 728-730: Figure 3 depicts hydrostatic pressure…is the model input incorporating barometric pressure (such that it is not sensitive to weather events)? Also unclear if this model is setup to estimate ebullition events associated with water level fluctuation

Response: Yes, the model input includes the atmosphere pressure. Yet, the model does not simulate ebullition events associated with water level fluctuations, as the lake area and depth are set as constant. To clarify this point, this sentence has been revised as follows:

"Since *in-situ* water level measurements are lacking and the lake area and depth are set as constant in the model, the simulated temporal variations cannot capture these observed erratic patterns well."

Moreover, we admit that the set-up of constant lake area and depth is a model limitation, and addressed this point in the manuscript as follows (lines 1033–1038):

"In future model developments, these limitations could be addressed by (i) integrating or routing the lake water, carbon and nutrient fluxes along the global river network, which would allow to simultaneously solve the issue of time-invariant lake water levels in current global lake models (Golub *et al.*, 2022), including ours;…"

27. Line 749: "will require assembling"—Also, you could cite some examples of nice time series here (either that are outside the European domain or are reservoirs instead of lakes). Rodriguez-Velasco et al. 2024 Limnology and Oceanography Letters is an example (but for a reservoir) https://aslopubs.onlinelibrary.wiley.com/doi/full/10.1002/lol2.10409. It also took a bit of hunting for me to figure out where the data for the 4 verification lakes came from (and its spatial temporal resolution). Right now the authors cite Tan et al. 2024 in their data availability statement, but I think these studies (and a description of the types of data) should be integrated in the text.

Response: Thank you very much for this comment. We have added some examples of nice time-series here, and this sentence has been revised as follows (lines 803–805):

"Resolving these issues will require to assemble a much larger dataset of observed long time-series of CH_4 fluxes and associated physical and biogeochemical variables, such as those reported by Velasco *et al.* (2024) and Natchimuthu *et al.* (2016)."

To the best of our knowledge, the lake datasets from the Inter-Sectoral Impact Model Intercomparison Project (ISIMIP) is the largest dataset we can obtain so far; thus, this dataset, together with Tan *et al.* (2024), is now cited in the text (section 2.5.2).

"To evaluate the ability of FLaMe-v1.0 to reproduce the observed temporal patterns of CH_4 fluxes, we selected four lakes from the Inter-Sectoral Impact Model Intercomparison Project (ISIMIP) lake dataset for which monthly resolved temporal CH_4 fluxes were available (Tan *et al.*, 2024). These lakes cover different lake depths, areas, climate conditions and trophic statuses, as summarized in Table 2."

The spatial and temporal resolutions for the CH_4 emission rates of the four lakes used for model evaluation have been added in Table 2.

28. Figure 8- consider plotting y axis on a log scale so it is easier to see the comparison for the boreal systems

Response: Thank you very much for the suggestion. We also have a log scale plot provided as supplementary Fig. S19.

29. Line 859: What data source are you using to estimate the depth distribution of European lakes? Or is this based on the assumed shape? Clarify.

Response: The depths of all lakes are also from HydroLAKES database (Messager *et al.*, 2016). The citation is added in this sentence as follows (lines 921–924):

"This slight time-lag is further amplified by the time required for the benthic CH_4 to reach the water-air interface, although this effect is secondary due to the dominance of shallow lakes (with mean depth <7.8 m for 90% of lakes; Messager *et al.*, 2016) within the European domain."

30. Line 867: Not sure what you mean by "dashed colors" maybe "colors with transparency"?

Response: Thank you very much for pointing this out. In the figure caption, the description has been modified as follows (lines 931–933):

"…(b) Seasonality of total CH_4 production (wide bars with full lines) and emission (narrow bars with dashed lines) fluxes and their split between ebullitive and diffusive pathways (period 2010-2016)."

31. Line 913-914: grammar: change to: "suggesting that it can capture the relationship between… well"

Response: Thanks. This sentence has been corrected according to your suggestion.

32. Line 916-917: maybe remind readers again here that this estimate excludes the largest lakes? Possibly present a back of the envelope range for the relative possible contribution from the largest lakes?

Response: Agreed. We have clarified the range of lake sizes in this sentence as follows (lines 988–990):

"Therefore, during the period of 2010-2016, the European lakes (with surface areas between 0.1–1000 km^2) have an annual mean emission of 0.97±0.23 Tg CH_4 yr^{-1}."

Moreover, we have also added an estimation of CH_4 emissions for the large lakes (>1000 km^2; Supplementary Text S5), and added a statement in the manuscript as follows (lines 893–897):

"Note that, by including the estimated emissions from European lakes larger than 1000 km^2 with two different strategies (Supplementary Text S5), we provide a back of the envelope estimate for the mean total annual emission as 1.03–1.10 Tg CH_4 yr^{-1}, which falls within the lower end of a previously reported range (0.9–2.5 Tg CH_4 yr^{-1}) (Petrescu *et al.* 2023; Lauerwald *et al.*, 2023)."

33. Table 4: provide some explanation of the numbers presented in the table… I think these are fractional percentages of the estimate generated by the baseline model run?

Response: Thanks for this comment. Following your suggestion, additional explanation is now provided in the table caption, and we have added the percentage changes of the estimates compared to the baseline model run. The revised table (table 3 in the revised manuscript) is modified as follows:

Table R1. Sensitivity of European lake CH_4 emissions (Tg CH_4 yr^{-1}) to key model parameters. Mean and SD are the mean and standard deviation of a particular parameter. Mean±SD indicates that the parameter values are adjusted by ± one SD; Mean±0.5SD indicates that the parameter values are adjusted by ± 0.5 SD.

Parameter setting		Mean±SD				Mean±0.5SD			
		–SD		+SD		–0.5SD		+0.5SD	
		Absolute/percent		Absolute/percent		Absolute/percent		Absolute/percent	
Primary production	P_{chl_max}	0.344	-65%	1.743	+80%	0.642	-34%	1.376	+42%
	$K_{s,P}$	1.432	+48%	0.754	-22%	1.170	+21%	0.852	-12%
Mineralization and burial rates	k_{20}	0.578	-40%	1.164	+20%	0.758	-22%	1.141	+18%
	k_{bur}	1.317	+36%	0.761	-22%	1.107	+14%	0.856	-12%
	θ	1.028	+6%	0.928	-4%	0.989	+2%	0.968	0%
	f_{mm}	0.302	-69%	1.888	+95%	0.605	-38%	1.437	48%
Methane oxidation	k_{max}	1.057	+9%	0.930	-4%	1.009	+4%	0.953	-2%
	$Q_{10,ox}$	0.992	+2%	0.983	+1%	0.978	+1%	0.973	0%
Base value of the shape parameter	α_{min}	1.222	+26%	0.840	-13%	1.077	+11%	0.891	-8%

34. Line 944: change from "large scale" to "regional scale" and possibly clarify lakes <1000 km^2

Response: Following your comment, this sentence has been revised as follows (lines 1018–1020):

"Our results thus suggest that the FLaMe-v1.0 modelling framework performs well in providing reliable spatio-temporal patterns of lake CH_4 emissions at the regional scale (with lake areas <1000 km^2)."

35. Line 966: you mean aerenchyma flux?

Response: Yes. We have revised the sentence as follows (lines 1040–1043):

"For instance, the present version of FLaMe (i.e., v1.0) neglects the plant-mediated emission pathway (through aerenchyma in rooted plants) in the littoral zone due to the lack of observational data for model calibration."

References

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