Comment on egusphere-2022-329
Anonymous Referee #1

Referee comment on "Mechanistic modelling of the variability of methane emissions from an artificial reservoir" by Victor Lomov et al., EGU sphere, https://doi.org/10.5194/egusphere-2022-329-RC1, 2022

The manuscript applies the LAKE model, version 2.3, to the Mozhaysk reservoir to simulate temperature, oxygen and methane dynamics over multiple years, discusses the fit with observed field data, and provides next steps for lake model development. The averaging over the horizontal axis of the 3D equations in a set of 1D equations and the resulting good fit of biogeochemical dynamics and fluxes is very exciting, and without a doubt of high interest to modelers and limnologists. Although I think the study is methodologically sound and all results support the conclusions, the overall writing feels rushed with style and grammar errors appearing more frequently in the latter part of the paper. I hope the authors can revise the manuscript accordingly to publish an otherwise very exciting and interesting manuscript.

Major points:

- L580: It is still unclear to me if Vmax and q10 were also used in calibrating the model to Mozhaysk? It is not mentioned in the methods but only in L 593. Was the model calibrated with these variables after a sensitivity analysis? Please provide more details.

Minor points:
L12: I doubt that LAKE2.3 is the “[...] first mechanistic model [...] for the prediction of methane emissions from artificial reservoirs” as e.g., models like GLM-AED2 or ALBM mechanically simulate carbon dynamics and methane fluxes.

L34: Could you please provide an explanation of the terms hydrogenotrophic and acetoclastic here?

L44: Wouldn’t any organic carbon input provide CH4 production by decomposition, is there a specific reason to focus on littoral macrophytes here?

L78: “[...] to the remaining (unexplored) reservoirs, [...]”

L138: remove “which worked”

L158: I recommend replacing space scanners with satellites

L203: Equation number one is missing (manuscript starts with (1))

L207: F_f is stated in the equation explicitly as an eddy diffusivity approach, I’d focus here on explaining K_t and k_m

L222: The relationship between salinity and mineralization is unclear to me

L226 and 227: I’d remove “atoms”

L235: I’m confused here as maybe I’m misunderstanding something, but wouldn’t this cause Eq. 2 to have two identical terms for advection?

L239: I’d write imbalance instead inequality

L242: Isn’t the continuity equation number 4?

L268: exudation

L281: What do you mean by liquid moisture?

L291: ‘Data were corrected for the modelling period 2015-2019’

L307: Is k BD added to k_t and k_m then for depths equal thermocline?

L318: enhancing sounds here like artificial engineering solutions to provide more methane, would increasing be better suited

L331: So, there were seven total measurements in 3 years?

L347: “was assumed to be 0.4”

L369: “do not allow”

L381: “Neglecting the total emission estimations, [...]”

L6: I am wondering why that additional diffusion k BD still resulted in that strong difference of thermocline depth temperatures (roughly 10 m) here

L397: transport

L399: “(equation 2)”

L402: Why was the seiche parameterisation switched off when the model has shortcomings in reproducing vertical mixing and thermocline oscillations?

L403: “reproduced the averaged”

L403: I think you mean spatially averaged here, not to confuse the reader with temporally averaged

L406: “are visualized in Fig. 7”

L419: “In the model, oxygen concentrations are in close [...]”

L419: Why should atmospheric exchange cause an underestimation? This is very unclear to me

L425 “are too high for limiting the methanotrophic [...]”

L427: Is there a specific reference of a k_1/2 of 0.33?

L431: “which is important”

L435: “data are available”

L8: I would argue the model does a not sufficient job in replication the oxygen drawdown at 14 m in summer 2017 and 2018, as the modeled DO consumption happens too slowly

L436: “were not calculated”

L438: Why is it not feasible to do a statistical approach with 7 measurements as the regressions in Fig. 5a had also only 7 measurements?

L440: “[...], modeled concentrations were lower by 2.08 mg L-1”

L9: Why is so much of summer 2017 missing?

L450: “The process-based modeled underestimated observed surface oxygen concentrations, but there is a strong agreement between modeled bottom water
temperature to observed data.

- L456: “This can be partially related to the spatial [...]“
- L464: “[...] methane is accumulating less than what was observed [...]“
- L468: So, the important linkage to organic matter decomposition is not included yet, could the model then have a bias for simulating diffusive and ebullitive flux?
- L479: “were attributed to methane degassing from turbines“
- L483: Can you show a plot highlighting reservoir level change to methane fluxes?
- L503: “annual emission of methane, [...]“
- L507: Which figure are you referring here to for the period in 2018 and 2019?
- L522: “except for 2018. Most possible this is [...]“
- L523: “the rate exceeds the“
- L527: “for reservoirs in the temperate zone“
- L532: Isn’t it “during the autumn convection“?
- L558: “get oxidized“
- L560: Any degree over 10 deg C would directly multiply the methane flux, or was there a multiplier for a multitude of 10 deg C, e.g., 0, 10, 20, n+10?
- L561: “are shown in Fig. 14“
- L563: “measurements were carried“
- L585: criteria
- L587: Wouldn’t a seiche parameterization be a perfect candidate for this?
- L597: “it contributes 95 % of the total flux“
- L598: “, respectively“
- L662: “better estimates of annual emission“