Understanding the balance between methane production and oxidation from wetlands: insights from a reduced process-based model

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We thank the Editor and the reviewers for their careful reading of the manuscript and for their constructive and insightful comments. We have addressed all points raised below, with corresponding revisions highlighted in red in the marked version of the manuscript. We believe these changes have strengthened the clarity of the work.

Response to Reviewer 1

In their manuscript, McNicol and co-authors propose a zero-dimensional model of methane production, oxidation, and emission. The authors then investigate its application to a number of wetlands in the Prairie Pothole Region. They reduce the model even further and are able to relate methane emissions to water table position, finding maximum emissions with water table at or above the surface. Overall, this is a very well-written manuscript, presenting a very interesting simple model that allows nicely illustrates the influences on wetland methane emissions. I recommend publication after minor revisions.

Response: We thank the reviewer for their positive feedback on our manuscript. We have carefully considered their revision points and provide detailed responses below to address each comment.

Of course there are a few minor points that could be improved:
1) Section 2: "we focus on soil columns at the centre of each wetlands". Do you know how close to the edge of a wetland one could actually go before the underlying assumptions break down? I suspect it's very close, but I have no solid arguments. If you have any arguments better than gut feeling, it would be very interesting to discuss these here. If not, you are welcome to skip this point.

Response: We thank the reviewer for raising this important question. Our focus on soil columns at the wetland centre reflects the assumption that vertical gradients in water table, substrate availability, and redox conditions dominate CH_4 dynamics in these locations, whilst lateral fluxes and edge effects are comparatively small. Near wetland margins, however, lateral hydrological gradients, variable soil texture, and heterogeneous vegetation can become more pronounced, reducing the validity of a one-dimensional vertical representation. In addition, our simplified linear relationship between water-table depth and WFPS is expected to have limited influence under persistently inundated central conditions, but may be less accurate near the wetland boundary where partial saturation and fine-scale heterogeneity are more common.

We therefore acknowledge that the one-dimensional column assumption is most appropriate for central wetland zones, particularly in areas that remain persistently inundated, but may break down near the wetland boundary depending on local slope, hydrological connectivity, and variability. We have clarified this limitation in the revised manuscript, particularly in the expanded discussion of the associated simplifications (see response to next comment).

2) Section 3.1: You relate WFPS linearly to water table. Strictly speaking, this is not true, as water does not necessarily fill available pore volume from the bottom upwards (nonetheless nearly everyone makes this simplification). You also make some further simplifications, like not considering the three emission pathways explicitly. Please summarize these simplifications and briefly discuss (speculate, if necessary) the effect they have on model results.

Response: We thank the reviewer for this suggestion, and agree that summarising these in the text help model transparency. To address this comment, we have added a short subsection to the manuscript, Sec. 3.2 (pgs. 11–12, lines 289–309), which summarises the assumptions:

"For clarity, we briefly reiterate the key structural assumptions implicit in our model formulation. First, subsurface hydrology is represented through a simplified relationship between water-table depth and WFPS, implicitly assuming monotonic pore saturation with rising water tables and neglecting hysteresis, preferential flow paths, and fine-scale soil heterogeneity. Second, CH_4 production combines hydrogenotrophic, acetoclastic, and methylotrophic pathways into a single effective term rather than treating each microbial mechanism explicitly; small contributions from CH_4 production in oxic microsites are neglected. Third, CH_4 emission is treated as a first-order process, with diffusion represented explicitly and plant-mediated transport and ebullition combined into a single term, k_{EP}. In reality, ebullition is episodic and sensitive to temperature and pressure fluctuations, whilst plant-mediated transport depends strongly on vegetation phenology and water-table depth. This simplification neglects detailed vertical transport dynamics and the pulsed nature of ebullition, but is expected to primarily influence short-term emission variability rather than seasonal totals. Fourth, CH_4 oxidation is assumed to occur predominantly under aerobic conditions in the unsaturated soil zone, whilst anaerobic oxidation in saturated layers, which requires alternative electron acceptors (e.g. nitrate or sulphate), is not explicitly resolved. Fifth, the model resolves dynamics within a vertically homogeneous soil column at the wetland centre, neglecting lateral transport and edge effects; this assumption is likely less appropriate near the wetland boundary. Finally, to reduce bias from extreme CH_4 flux outliers (e.g. large ebullition events or disturbance), we exclude observations exceeding 20 times the interquartile range for each wetland, whilst retaining background ebullitive fluxes.

Together, these assumptions favour interpretability and parameter identifiability, capturing dominant seasonal controls on CH_4 flux whilst smoothing short-term variability and episodic pulses. They may, however, bias short-term fluxes under extreme hydrological conditions; for example, linear WFPS assumptions may overestimate production in partially saturated soils, pathway lumping smooths short-term variability, and neglecting oxygen transport may slightly overestimate net emissions. Near wetland edges, lateral flows, variable soils, and heterogeneous vegetation may further alter methane dynamics, meaning central-column assumptions likely break down within a few meters of the boundary."

3) Section 4.1, Figure 5: Very nice fit to the different wetlands. Did you use identical parameter values for all wetlands, or different ones? Maybe the ones in Table A1? Which parameters weree adjusted, and why? A discussion of the parameter choice would be really interesting.

Response: We thank the reviewer for this positive feedback. The reviewer is correct these are the parameters in Table A1, corresponding also to plots in Figs. 3, 4, 6. In particular, we fitted model parameters individually for each wetland to capture site-specific characteristics and environmental conditions. We clarify this in the text (pg. 15, line 369):

"(using the same optimised parameters detailed in Table A1)"

The ranges for parameter values used in the calibration process are provided in Table A2. All parameters, except methane diffusivity, were adjusted during calibration. These parameters influence the balance between methane production, oxidation, and emission pathways, allowing the model to adapt to variations in soil properties, hydrology, and vegetation among wetlands. This approach enables the model to reflect wetland-specific biogeochemical and hydrological dynamics, which is critical given the heterogeneity observed in the Prairie Pothole Region. We have clarified this discussion in Section 4.1 (pg. 9, lines 224–229):

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"Following Calabrese et al. (2021), the effective diffusivity of CH_4 in the soil is fixed at $D = 1.3 \, \mathrm{cm^2 \, d^{-1}}$ in all simulations. All other parameters are adjusted during calibration and fitted individually for each wetland to capture site-specific environmental conditions. This approach allows the model to account for variation in soil properties, hydrology, and vegetation among wetlands, thereby reflecting wetland-specific biogeochemical and hydrological dynamics. The ranges of possible parameter values are detailed in Table A1."

Regarding parameter choice, we now note in the text (pgs. 15–16, lines 388–392)

"We note that no simple monotonic relationship is evident between fitted parameter values (Table A1) and wetland area or classification (e.g. permanent versus temporary). This is not unexpected, as the calibrated parameters represent effective process rates that integrate multiple interacting controls, including vegetation structure, sediment organic matter quality, hydrological regime, and redox conditions. Consequently, parameter variability primarily reflects site-specific biogeochemical and hydrological heterogeneity."

Further little details:
Line 9: "Peak fluxes consistently occur at or just above the soil surface" – reads as if flux at surface is meant, but it is the water table at the surface that is important.
Line 32: "shaped by complex" (without a)
Line 33: "Among these" (not amongst)
Line 41: "where microbes instead release" (not releasing)
Line 180: "aerenchymous plants" (not arenchymous)
Line 283: "captures the trend" (not capturing)
Line 363: "...depends on both the..." Either the both is superfluous, or we are missing the other thing that is relevant in addition to the water table. Or you actually mean the wetland-specific parameters, but that doesn't really become clear from the sentence. I suggest you reformulate it.

Response: We thank the reviewer for pointing out these various errata, each of which have now been corrected in the manuscript.