

**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 and blue 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 CH₄ 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, CH₄ production combines hydrogenotrophic, acetoclastic, and methylotrophic pathways into a single effective term rather than treating each microbial mechanism explicitly; small contributions from CH₄ production in oxic microsites are neglected. Third, CH₄ 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, CH₄ 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 CH₄ 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 CH₄ 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 were 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):

“Following Calabrese et al. (2021), the effective diffusivity of CH₄ in the soil is fixed at $D = 1.3 \text{ cm}^2 \text{ d}^{-1}$ 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.

Response to Reviewer 2

I have read the manuscript. I thought the paper was well written and followed a logical workflow. This study provides an excellent example of using extensive field data to parameterize a simplified, quasi-mechanistic modeling approach.

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.

The paper could have provided more information on the data source, which fluxes were used? Each wetland had 5 wetland chambers and 3 upland chambers in each wetland. Were only wetland chambers used? Was there any averaging across chambers?

Response: We thank the reviewer for raising this point. For this study, we used flux data exclusively from the central chamber (Chamber 1) in each wetland. We did not include data from upland or peripheral chambers in order to maintain consistency and focus on the primary wetland methane dynamics at the wetland centre. Hence, there was also no averaging across multiple chambers in this analysis. We have clarified this point in the revised manuscript (pg. 11, lines 260–262):

“Given our focus on the wetland centre, we use flux measurements from a single chamber located near the centre of each wetland (Chamber 1 in the dataset). Measurements from other wetland or upland chambers are not included in the analysis.”

I also questioned some assumptions in the notes below. The authors should mention the role of methylotrophic methanogenesis, particularly in PPR wetlands (Dalcin et al. <https://doi.org/10.1111/gcb.13633>, Bechtold et al. <https://doi.org/10.1038/s41467-025-56133-0>).

Response: We thank the reviewer for highlighting the potential importance of methylotrophic methanogenesis, particularly in PPR wetlands. Indeed, whilst traditional wetland methane models focus on hydrogenotrophic and acetoclastic pathways, recent studies demonstrate that methylotrophic methanogenesis can contribute substantially to methane emissions in wetlands with diverse organic inputs. Hence, we have updated the text (pg. 6, lines 133–142) to recognise this alternative pathway:

“Three methanogenic pathways contribute to CH₄ production in wetlands: hydrogenotrophic, whereby bacteria reduce CO₂ with hydrogen to form CH₄; acetoclastic, in which bacteria convert acetic acid from decomposing organic matter into both CH₄ and CO₂; and methylotrophic methanogenesis, in which specialised methanogens convert methylated compounds (e.g. methanol, methylamines, or methyl sulfides) into CH₄, and may be particularly important in PPR wetlands (Conrad, 1999; Bechtold et al., 2025, Dalcin Martins et al., 2017, Thauer et al., 2008). Biomass availability thus plays a crucial role in all three pathways: directly through litter input for acetoclastic methanogenesis, indirectly via CO₂ production for hydrogenotrophic methanogenesis, and by supplying methylated compounds for methylotrophic methanogenesis. Despite differences in substrates and microbial mechanisms, we combine these pathways into a single effective methanogenesis term for model simplicity. To represent this dependence, we use the Normalised Difference Vegetation Index (NDVI) as a proxy for available plant biomass.”

There are also methane production in oxic water columns and anerobic conditions that support methanotrophy. These were not discussed.

Response: We thank the reviewer for raising this important point. Indeed, methane production has been observed in oxic water columns via non-canonical pathways such as demethylation of methylated compounds, and anaerobic methane oxidation can proceed using alternative electron acceptors like nitrate, iron, or sulphate. These processes are particularly relevant in redox transition zones and dynamic wetland environments.

In the present study, we focus on the dominant sedimentary methane production and aerobic oxidation processes most commonly invoked to explain wetland-scale CH₄ emissions. Oxic methane production and anaerobic methane oxidation are not represented explicitly, as incorporating them would require additional mechanistic detail and parameters beyond the scope of this reduced, zero-dimensional model framework. However, we acknowledge their role in the updated manuscript (pg. 6, lines 129–132; pg. 7, 172–175):

“Note that, whilst methanogenesis predominantly occurs in anoxic soil layers, small amounts of CH₄ may also be produced in oxic layers via micro-anoxic microsites within the soil column (Angle et al., 2017). These contributions are not explicitly resolved in our model, which focuses on bulk soil CH₄ production as the dominant source.”

“Moreover, our model assumes that CH₄ oxidation occurs predominantly under aerobic conditions in the unsaturated soil zone. Anaerobic oxidation of CH₄ in saturated layers, which requires alternative electron acceptors (e.g. sulphate or nitrate, see Segarra et al., 2015), is not explicitly resolved and may slightly reduce net CH₄ flux in some wetlands.”

We note that variation in these processes among wetlands is implicitly captured by calibrating methane production parameters (k_p , Q_p) and oxidation parameters (k_o , Q_o , p_2). For example, a shallower decline in oxidation rate (i.e. lower p_2) may reflect the influence of anaerobic methane oxidation.

Title: Minimalist emissions model is very jargony and will lose audience immediately.

Response: We thank the reviewer for this suggestion and have revised the title to:

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

to emphasise process-level insight and improve accessibility to a broad readership.

Find a more universal definition of wetlands. Bansal et al. 2023 methods review paper has a lot of basic wetland info and citations: <https://doi.org/10.1007/s13157-023-01722-2>

There are many other types of wetlands. The PPR wetlands are often less than 0.1 ha. Use a couple PPR-specific citations. newer citation on wetland loss. Fluet-Chouinard et al. <https://www.nature.com/articles/s41586-022-05572-6>

Response: We thank the reviewer for these these suggestions. To address these, we have included some extra PPR-specific references and have revised the opening paragraph of the manuscript (pgs. 1–2, line 15–27):

“Wetlands are soil-water ecosystems in which the persistent or recurring presence of water at or near the land surface strongly influences physical, chemical, and biological processes (Bansal et al., 2023a; Mitsch and Gosselink, 2015). Spanning a wide range of water depths and hydroperiods, wetlands occur in many forms, including marshes, swamps, bogs, fens, floodplain wetlands, tidal wetlands, and small, geographically isolated systems. Individual wetlands can range in size from well under 1 ha, as is typical for wetlands of the Prairie Pothole Region (PPR) of North America (Goldhaber et al., 2014), to expansive complexes covering hundreds or even thousands of square kilometres (Junk, 2024). Serving as a boundary between land and aquatic environments, these diverse and dynamic ecosystems provide a habitat for a variety of plants, animals, and microbes whilst also performing critical ecological functions such as water filtration, retention, and purification (Bridgham et al., 2006; Gleason et al., 2011; Zedler and Kercher, 2005). Given this wide array of functions, wetlands are often touted as a possible nature based solution (e.g. for flood prevention Ferreira et al., 2023) and there has been a recent push for their enhanced conservation given their previous loss due to anthropogenic activities, including widespread drainage, land conversion, and hydrological alteration associated with urbanisation and agricultural intensification (Bradford, 2016; Evenson et al., 2018; Fluet-Chouinard et al., 2023; Verhoeven and Setter, 2010).”

Or uptake GHGs Not clear in sentence that methane or all three gases are the focus.

Response: We thank the reviewer for highlighting this ambiguity. The text has been revised (pg. 2, lines 28–32) to clarify that wetlands may function as both sources and sinks of greenhouse gases, and that our analysis focuses specifically on methane dynamics:

“...sequestering substantial amounts of carbon in their soils and storing roughly one-third of global soil carbon despite covering only 5–8% of the land surface (Mitsch et al., 2013). Through active microbial processes, wetlands can both emit and uptake greenhouse gases (GHGs), including carbon dioxide (CO₂), nitrous oxide (N₂O), and methane (CH₄) (Bridgham et al., 2006; Salimi et al., 2021). In this study, we focus specifically on CH₄ dynamics, examining the balance between CH₄ production, oxidation, and emission from wetlands.”

Fig. 1. Most of the abbreviations are not labeled. More labels/graphics to make this information and a conceptual figure would help.

Response: We thank the reviewer for this suggestion. Accordingly, we have revised Fig. 1 (pg. 5) to include a schematic representation of the wetland showing soil columns under varying hydrological conditions, indicated the modelled environmental drivers for CH₄ production, oxidation, and emissions, and added further details to the figure caption to improve clarity. We have also included conceptual plot (Fig. 1g), highlighting the non-linear dependence of methane emissions on water table depth.

Water in soils doesn't really dilute the DOC. If fresh water was moving through the soils, then maybe.

Response: We thank the reviewer for highlighting this issue. We agree that water in saturated soils does not inherently dilute dissolved organic carbon unless there is advective exchange with fresh water. We have therefore revised the manuscript (pg. 6, lines 119–122) to clarify that waterlogged conditions influence CH₄ production primarily through changes in redox conditions, substrate accessibility, and transport processes, rather than simple dilution effects:

“However, waterlogged conditions can reduce CH₄ production if the available carbon substrate becomes distributed over an increasingly large water column above the soil surface, effectively reducing substrate availability for methanogenesis depending on the degree of mixing.”

There are three pathways. Methylo-trophic methanogenesis may be very important in PPR wetlands

Response: In response to the reviewer’s earlier comment, we have revised the manuscript to include a discussion of methylo-trophic methanogenesis.

> 30 degree days are more likely in the future. Impact?

Response: We appreciate the reviewer’s comment on temperature dependence. Our model uses a simplified function (Eq. 5) in which methane production increases to 30°C and is held constant thereafter. This approach provides parsimony and captures typical saturation in production rates. In reality, microbial responses are complex, with thermal optima varying between pathways and communities; sustained temperatures above 30°C may either continue to increase production or suppress it if microbial activity declines beyond the optimum. The net effect is likely to vary across wetlands and warrants further investigation. We have added a brief discussion of this point in the manuscript (pg. 7, lines 156–159):

“Under future climate scenarios with more frequent extreme heat, CH₄ production may continue to increase above 30°C, in which case our current formulation would be conservative. Alternatively, enzymatic and microbial activity may decline above their thermal optimum due to heat stress, potentially suppressing production. The net effect will likely vary among wetlands with different thermal regimes and microbial communities, and warrants further modelling investigation.”

Plant transport is very dependent on water table. If $z_w = z_b$, then plant transport will not be any different than diffusion in terms of methanotrophy. When water table is higher, $z_w > z_s$, then plant transport allows methane to skip past methanotrophy (Bansal et al. 2020)

Response: We thank the reviewer for highlighting this point. We have clarified in the revised manuscript (pg. 8, lines 203–205) that plant-mediated methane transport is strongly dependent on water table depth:

“The effectiveness of this bypass increases with higher water tables, particularly when $z_w > z_s$, as plant-mediated transport can transport CH₄ directly from saturated soil to the atmosphere, thereby avoiding oxidation in near-surface oxic zones (Bansal et al., 2020).”

use a PPR citation

Response: We thank the reviewer for this suggestion. As the specific location for the PPR citation was not indicated, we have added appropriate PPR references to our discussion of precipitation and temperature variability in mid-latitude wetlands (pg. 9, line 232), as well as to our section on the role of ebullition in wetland emissions (pg. 8, line 208).

Variables could use more details, NDVI (from Landsat satellite imagery), soil temperature (or water temperature when $z_w > 5$ cm), water-filled pore space (top

5 cm soils). Do these detail affect model or interpretation?

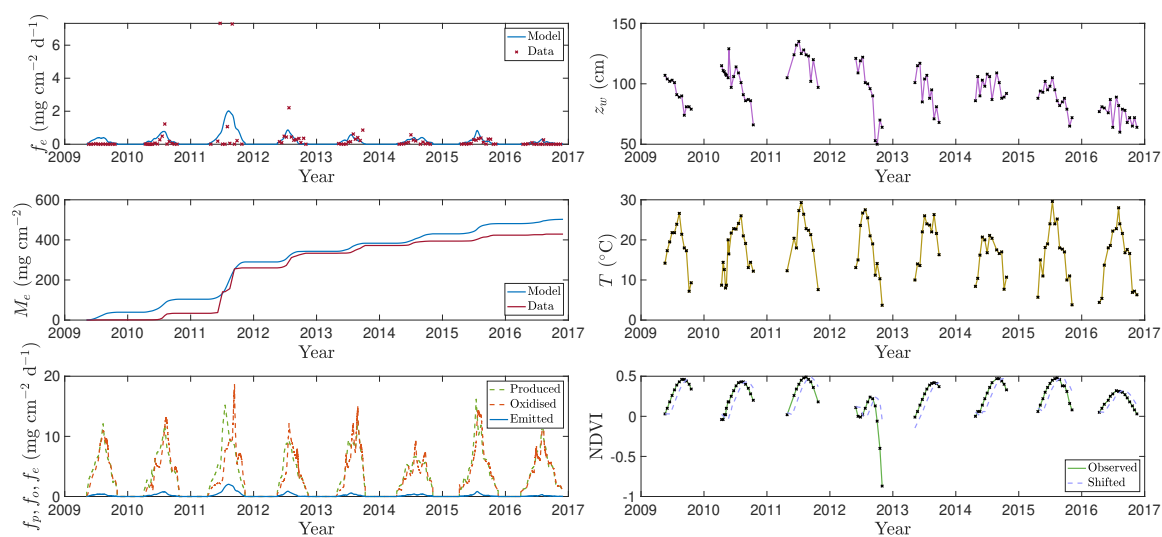
Response: We appreciate the reviewer’s request for clarification. Full measurement details are provided by Bansal et al. (2023b), but we have added a brief summary in the revised manuscript (pg. 11, lines 278–287) to describe the key variables used to force the model:

“Full details on the measurements used to force the model are provided by Bansal et al. (2023b), but we summarise briefly here. Methane fluxes were measured in the wetland zone of undrained Prairie Pothole wetlands using static chambers deployed on sediment or floats along a single transect per wetland, with five wetland-zone sampling locations spaced from the edge to the centre. Measurements were collected approximately every two weeks during the growing season (May–November), between 10:00 h and 14:00 h, and included water depth, soil temperature (or water temperature when the water table exceeded 10 cm), air temperature, and soil volumetric water content (with WFPS calculated by dividing by soil porosity). NDVI corresponding to each sampling event was obtained from Landsat imagery to represent vegetation availability. These variables provide the spatially and temporally resolved forcing used in the chamber-scale model. As measurements are limited to the top 5–10 cm of soil (or water surface), daytime sampling, and relatively coarse NDVI (30 m), short-term heterogeneity may be smoothed or under-represented.”

As measurements are limited to the top 5–10 cm of soil (or water surface), daytime sampling, and relatively coarse NDVI (30 m), short-term spatial or diurnal heterogeneity may be smoothed. These limitations could modestly affect model interpretation at fine temporal or spatial scales, but the overall seasonal and interannual patterns captured by the model are likely robust.

It is possible that removing those high emissions from the dataset is causing the poor performance in P8, which is driven by high rates of ebullition.

Response: We thank the reviewer for this suggestion and tested fitting the model without removing extreme outliers to assess whether this improves performance for wetland P8 (see figure below).



Model performance did not noticeably improve. The model still fails to capture the emissions peak in 2012 (present in both datasets) and cannot reproduce the extreme peaks in 2011

(removed in the outlier-filtered dataset). The model does, however, approximate similarly high cumulative annual emissions by smoothing these effects over the year. This limitation likely stems from our simplified assumption of constant ebullition throughout the year, which neglects hydrostatic, atmospheric pressure, and temperature effects, each of which can strongly influence ebullition rates (2011 was particularly hot, for example). As discussed in the manuscript, this represents an avenue for future model refinement.

Fig 5. Perhaps put area in hectares

Response: We thank the reviewer for this suggestion. To improve clarity, we have updated Fig. 5 (pg. 16) to report wetland area in hectares.

methylo-trophic pathways too.

Response: We thank the reviewer for pointing this, and have updated the text (pg. 23, line 559) to acknowledge this pathway too:

“...hydrogenotrophic, acetoclastic, and methylo-trophic methanogenesis pathways...”

Fig 6s. How did T9 have such high emissions (Me) but very low emissions in panel c?

Response: We thank the reviewer for raising this question. Wetland T9 exhibits very high methane production, reflected by a large methanogenesis scaling constant ($k_p = 7.55 \times 10^1$ mg CH₄ cm⁻² d⁻¹), alongside a high oxidation rate constant ($k_o = 2.00 \times 10^2$ d⁻¹) with weak decay with water depth ($p_2 = 1.87 \times 10^{-2}$). This creates a dynamic balance in which intense methanotrophic oxidation substantially offsets the high methane production.

Production in T9 is strongly temperature-dependent and peaks during summer, particularly under inundated conditions (reflecting the weak decay of production with increasing water depth), whereas oxidation exhibits minimal temperature dependence and can persist outside the summer months. Although production rates are extreme, strong oxidation suppresses net emissions; nonetheless, the elevated production, combined with predicted ebullition and plant-mediated transport, still yields relatively high observed CH₄ fluxes. The model therefore captures T9’s methane cycling as characterised by vigorous production coupled with efficient oxidation. This demonstrates the necessity of jointly considering both processes and their seasonal dynamics when interpreting emission patterns across wetlands.