nature portfolio

Peer Review File

Wetland hydrological dynamics and methane emissions

Corresponding Author: Professor Shubiao Wu

Attachments originally supplied by the reviewers can be found at the end of this file, in order by reviewer number and revision

Version 0:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Professor Wu,

Your manuscript titled "Hydrological Dynamics and Methane Flux in Wetlands: Unraveling the Complexities for Climate Action" has now been seen by 2 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. In particular, we do not allow original analysis to be presented in Review Articles. We would allow a comprehensive compilation of existing data, but it seems as though the analysis presented goes beyond the point of a compilation and the included data are limited. We therefore recommend that you either present a comprehensive compilation of values from the published literature, or that you completely remove this element of the manuscript and focus solely on discussing the relevant literature. Furthermore, your Review should provide a more balanced overview of peatlands and wetlands across a range of ecosystem/vegetation types and climate zones, without overly focusing on a single type (e.g. Sphagnum-dominated peatlands). You should also revise the narrative of your manuscript to succinctly deliver a coherent, compelling and novel overview of the literature and the recent advances in our understanding of hydrological influence on methane dynamics in wetlands. Please note that we expect you to also address the other concerns raised by the reviewers - such as ensuring that the recent relevant literature are included.

Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Clare Davis, PhD
Senior Editor

Communications Earth & Environment

on behalf of

Joshua Dean, PhD
Editorial Board Member
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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The review by Cui and colleagues addresses wetland methane emissions and their relationship to hydrology. Although it is often assumed that there is a simple, and measureable, relationship between water table and methane, numerous studies fail to detect this. This might arise as an artefact of the field design (WT fluctuates over a short range only, minimal replication of flux measurements, etc) or because of real, biogeochemical processes (a lag in CH_4 response, anaerobic microsites above the level of the WT, etc). So the rationale behind the paper is interesting. The authors write about the extent and magnitude of WT fluctuations in wetlands; about relationships (or lack of) between WT and CH_4; about soil conditions and microbes; and about plant-mediated transport and ebullition. They also present some data, including new analysis of existing data, on a sort of case-study/example basis to support their writing.

The paper is well written and interesting, and I enjoyed reading it. The topic is relevant and a good fit for the journal. However, at times I wondered what the papers aims to do – there are passages of review, and then passages focusing on individual sites and certain time periods (section 3 in particular). These "case studies" can be useful to show examples. But I'm less comfortable with the FLUXNET analysis of just ten sites (which comes unannounced with no methods section/primer), which are widely distributed across continents (N America, Europe, Asia) and latitudes (between France and northern Finland). No detail is given as to how the data from these sites is comparable and the authors even state that data was collected over periods between 1-7 years. But this is more like a synthesis paper (much of section 3 e.g. Fig. 1, Fig. 4, most of the supplementary), in which case why not go all in and analyse more FLUXNET sites? After section 3, the paper returns to being more like a conventional review paper (and is all the better for it, I think).

Note that my expertise lies primarily with fluxes and water table, and I cannot comment with any expertise on section 4, 5.2, 6 and 7.

Also, I spotted some issues where sentences are attributed to references that don't really support them.

In several places I link/mention other papers. These aren't "demands" to cite these papers, just included for interest/relevance, and it's up to the authors whether to cite them or not.

Detailed comments follow.

L55. The paper begins as follows:
"Methane (CH_4) in the atmosphere contributes 16 – 25% of the Earth's warming to date. This high-potency greenhouse gas possesses more than 80 times the warming potential of carbon dioxide within the first two decades after its release into the atmosphere, thereby playing a significant role in setting the pace for near-term global warming1."
The problem is that citation 1 is the aquatic synthesis by Rosentreter et al (2021). They may say these things, but they are citing someone else when they do. Citation accuracy matters. It seems like an IPCC report or similar may be a better citation here.

L71. I agree with much of this paragraph, but would suggest emphasising throughout that you're talking about very large scale measurements of emissions. Variation in methane fluxes, and their responses to hydrology and meteorology, can be easily tracked at the ecosystem scale using eddy covariance. And actually, considering you draw on lots of FLUXNET data, it would be good to (concisely) prime the reader about eddy covariance and what it shows (i.e. ecosystem-scale fluxes, continuously measured).

L99. "Thus, the connection between hydrological fluctuations and the CH_4 cycle in wetlands, influenced by both biotic and abiotic environmental factors, remains significantly underestimated."
I don't think "underestimated" is the best word here. Something relating to uncertainty/a knowledge gap would make more sense, e g:
"Thus, a complete understanding of the connection between hydrological fluctuations and the CH_4 cycle in wetlands, influenced by both biotic and abiotic environmental factors, remains lacking."

L116. Ref 26 is given as:
Nrcs, U. (Natural Resources Conservation Service Washington, DC, USA, 2008).
This is not sufficient to enable the reader to figure out what report or document the citation refers to. Please fix.

L116. Ref 27 is given as:
Brinson, M. M. A hydrogeomorphic clasification for wetlands. (1993).
Here there is enough detail to find the reference but more detail is needed as to where the reference is from (it's US Army Corps of Engineers, Wetlands Research Program Technical Report WRP-DE-4).

L123. I work on wetlands, but am not particularly familiar with the terms "slope wetlands" or "mineral flats". Are these country specific terms? I'd suggest expanding slightly to explain, or changing to different terms.

L126. Ref 30 needs a title in the reference list (I assume it's a chapter).

L129. "Typical reports include the frequent water table depth (WTD) fluctuations of wetland caused by the increase of extreme precipitation events in most temperate regions15"
Reference 15 is a mesocosm experiment, so it's hard to see how that can support a sentence about increasing extreme precipitation events and WT fluctuations.

L145. "has led to the long-term degradation of 54 – 57% of wetlands globally"
Perhaps worth citing the recent paper by Fluet-Chouinard et al (2023) that puts the figure much lower at 21%.
https://www.nature.com/articles/s41586-022-05572-6

L156. "Under different climate change scenarios, interannual fluctuations in wetland WTD can exhibit significant variation

due to changes in rainfall and drought patterns, with a 60 cm gap between the wettest and driest conditions"
Does this 60cm come from just one randomly chosen study? There are plenty of examples in the literature with +100cm, from various wetland types, e.g:
https://link.springer.com/article/10.1007/s10021-015-9873-x (see fig. 1)
https://www.sciencedirect.com/science/article/pii/S0167880918304018 (see fig. 1)
https://www.sciencedirect.com/science/article/pii/S0022169402000124 (see fig. 2)
https://onlinelibrary.wiley.com/doi/epdf/10.1002/hyp.8109 (see table 3)

L159. As is sometimes the danger with these review papers, here we are talking about wetlands and then suddenly switch to bogs and fens (i.e. peatlands; a subset of wetlands with organic soils). It might be worth flagging on L161 that you are talking specifically about peatlands here, e.g:
"For instance, when considering peatlands (wetlands with organic soils), bogs mostly rely on precipitation and evapotranspiration to establish their water balance…"

L165. This figure comes unannounced. So far, my understanding was that this was a descriptive review paper, but here we find new figures. I would suggest adding a few sentences in the intro/abstract to highlight that there is data analysis/extraction as part of the paper, and you can refer the reader to the supporting information where the methods are.

Fig 1, Fig 4, Fig S1, Table S1 and methods. The figure comparing WT in fens and bogs is interesting but has problems. Firstly, your sample sizes in Fig.1 are not 5074, 9086, 7053 and 7107, they are 4, 6, 5 and 5 (number of sites). Your measurements of WT and flux are pseudoreplicated in time – that's not a problem in itself, but the data need to be presented with honest sample sizes. You could include this info in the legend so the reader can truly understand what data underlies the figure. Secondly, your somewhat arbitrary selection of ten sites does not make for a truly robust analysis, at least not without more justification – are the ten sites under the same management regimes (conservation/nature-reserve managed?), are they drained or ditched in any way? The sites are found in many countries, not even on the same continent (Europe, Asia, N America), and between 47°N and 68°N – so with a small sample size how can you tell it is fen vs bog, or dominant vegetation, that is driving the results in Fig.1? And the authors even state that WTs were measured between one and seven years – so how is this comparable? It might be that this can be left as, but the authors need to convincingly argue that it is appropriate.

L169, 172. Sphagnum should be capitalised.

L169. "Compared to aerenchymatous plant, sphagnum can form a dense cover on peatlands, with its unique cellular structure enabling it to absorb and retain water, acting like a sponge55. This results in high WTD with relatively small fluctuation ranges in sphagnum-dominated peatlands (Fig. 1b&S1b)"
As for my comment about the small sample size. If you want to keep this in, I would also suggest adding more references to the literature to support an effect of Sphagnum on WT fluctuations.

I find some of the figures/their labelling non-intuitive. Consider that Baird & Low (2022) which you cite says:
"there seems to be some confusion over how this depth should be expressed. Many authors use positive values to indicate the depth of the water table below the surface, while others use negative values. The confusion arises because the direction of the vertical axis is not properly considered. A depth is a positive value below a surface (the vertical axis is positive downwards – depth increases downwards)…A negative WTD is either meaningless or implies a water level (no longer a water table) above the ground surface."
You consistently use WTD on your y axes, and show this declining into negative numbers as droughts occur (e.g. fig. 3b); i.e. in contradiction to what Baird & Low suggest is sensible. I'd suggest swapping "WTD" for "WT" on your axes, and perhaps also reversing the +/- signs so that a negative WT represents surface flooding. Somewhere you should explicitly state what your convention is (i.e. whether negative or positive WT values show surface flooding).

Fig.3c. The lines showing significance here are really confusing because they overlap. And it seems statistically dubious to compare every year individually to the 2018 drought year, rather than running something like an ANOVA with post-hoc which will tell you which years differ (not just compared to 2018). You state that 2018 was extreme drought for this site but present no annual WT data to back this up – the reader has to take your claim at face value. It would be better to delete panel a and insert a multi-year WT graph instead. Also, panel c is so small it's impossible to see detail – it might be better as a stand-alone figure.

L201. " In wetlands, WTD has long-standing served as a driver of CH_4 flux variations."
Served is not quite right here. Perhaps, "In wetlands, WTD has long been considered a primary driver of CH_4 flux variations"?

L214. As per my comment about Baird & Low, is it WTD is it's "permanently inundated"? For surface flooded, "water level" or "water depth" is probably more accurate.

Supplementary, L57. What is "linear fitting"? Presumably you mean linear regression or Pearson correlations, but be explicit. Is it appropriate to test for linear relationships? For comparison, Extended Data Fig. 4 in Evans et al (2021), which you cite, shows non-linear relationships between CH_4 and WT for their data, as well as that from Levy et al, Couwenberg et al and Turetsky et al.

Section 4. I very much enjoyed this part. After section 3 which couldn't decide if it was a review or a synthesis, this section is very much a proper review, and it does a great job of explaining why WT might not be a robust proxy for CH4 flux in all sites/at all times. I think this section will particularly be appreciated by those who spend a lot of time measuring fluxes in the field.

L421. Should be “Humic substances are…”. But the very existence of humic substances are debated (https://www.nature.com/articles/nature16069) so better to swap for “DOM” instead (also L427, L429, L431)

L433. “Humus”. Perhaps swap for: “Similarly to natural DOM, pyrogenic carbon…”

L550. “Additionally, plant-mediated transport generally dominates CH4 emissions from wetlands with emergent vegetation through aerenchyma”
This is quite a claim for “wetlands” as a global category. It might be true of some types of wetlands but I’m not sure can confidently said for all wetlands. The recent review by Bastviken et al shows the myriad ways plants can enhance or supress wetland methane emission. And Kosten et al’s review states which shows vegetation can be important, but they highlight large uncertainty around total contributions.
https://www.sciencedirect.com/science/article/pii/S0304377022001085
https://eartharxiv.org/repository/view/2904/

L589. This is the first mention of SAR. Why not provide some refs here showing its use for keen readers to take a look at?

Reviewer #2 (Remarks to the Author):

Comments on Cui et al: Hydrological dynamics and methane flux in wetlands….

General Comments

This paper is a detailed and worthy review of the literature on the interaction between hydrology and methane flux in wetlands.

The paper deserves to be published, but after moderate revision. The authors should be encourage to revise the paper, first by shortening it, then by addressing the points of concern listed below, and by updating generally.

The paper begins with an introduction to the methane problem: the importance of biogenic methane emissions in driving climate change. That’s good, but the text needs much revision and updating. The paper then goes on to consider the impacts of hydrological fluctuations on methane fluxes. This discussion includes soil impacts. The paper then goes on to discuss methanogenesis and methanotrophy, and finally transport.
I have both general and specific comments.
More generally, the review is highly qualitative and at times rambling and profligate with words – a lot thrown in, but in places the focus wanders. It’s not waffle, but again and again as I read the paper, being a bear of little brain, I asked myself ‘do I really need to know this’

Points of concern
a) the focus is almost entirely on boreal wetlands – or at least sphagnum wetlands: in short, the bias of FLUXNET sites. Not much is said about African and South American wetlands. In particular there’s no discussion about the significance of C4 plants in tropical wetlands. Plants like C . papyrus dominate many African wetlands and have parallels in S. America: because of their tall stems they must also impact methane egress.
b) little is said about Temperature impacts – global warming is primarily about heat; that leads to meteorological change and thus precipitation change. But don’t forget that Temperature has direct impacts – the Arrhenius (exponential) relationship of productivity – including methane) to Temperature rises, provided nutrients are accessible.
c) the paper mentions plant stem transport as a pathway for egress to air (e.g. see Fig 6) but only comparatively briefly. (see comment about papyrus above.)
d) isotopic fractionation is passed over – but that’s the best tool atmospheric chemistry has for differentiating between methane from fossil fuel leaks and methane from wetlands.
e) the introduction section is somewhat out of date and reads rather like an excerpt from a PhD thesis.

Specific comments
Abstract – very discursive. Should be made more concise and simultaneously more informative.
Line 55 needs s specific reference. This is a complicated number and it depends how you assess worming.
Lines 55-70- needs updating. Reads like a PhD introduction. See ref list below.
Line 84 – might be a good idea to add a paragraph about the difference between Tropical C4 and Boreal C3 wetlands. After all, this is a ‘paper’ and paper comes from ‘papyrus’!
Line 93 – Anaerobic oxidation of methane is mentioned in various places (e.g.289, 336, 459) but each time it’s only briefly passed over and comments tend to repeat earlier text….Maybe AOM deserves more attention in the paper, perhaps a paragraph of explanation.

L127 – Climate change impacts – essentially the 'meterological tropics' are expanding. Rainfall increases in a warmer world so wetlands grow, and Temperature rise by itself also drives methane emission…
L137 – glacier melting – well, actually ice caps as much as mountain glaciers. Also a significant part of the rise is thermal expansion.
L145 – 54-57% number - I'm not so sure, and it seems impossibly precise. Right now there is large expansion of wetlands going on in central Africa. The Tropical patterns are shifting. Also there is a lot more fertiliser and sewage flowing into tropical wetlands than there was 50 years ago. See for example Kayima et al. (listed below, and other papers)
L167 – need to mention just how different Tropical wetlands are from boreal. The world isn't just sphagnum.
Line 188-199 – maybe this should be followed by a paragraph on temperature directly impacting plant productivity and hence methane emissions. The manuscript has a very 'FLUXNET" view of the world – has little to say about Africa and S America.
Line 247 – also need to discuss transport in plant stems and trees (see Bastviken and Gauci papers in ref list.
L288-293 – the paper needs a coherent paragraph on AOM. L459 repeats what is said here.
L331-2 – good.
L531 – typo?? 'my"
L588 – the temperature impact is a knowledge gap – for example see Chadburn paper in fref list (12% increase per degree C warming).

Overall comments
The paper should be published after moderate revision.
I would suggest the authors should first go though the paper and shorten the existing text by about 30%. This will make it much tighter and better structured.
Then the authors should address the points of concern raised in review.

Listed below are some possible recent References to think about, and to pick and choose from….
(I'm not suggesting you cite more than a small number in the revised paper, but you do need to update on the global methane budget).

Albuhaisi, Y.A., van der Velde, Y. and Houweling, S., 2023. The Importance of Spatial Resolution in the Modeling of Methane Emissions from Natural Wetlands. Remote Sensing, 15(11), p.2840.
Arnold, W., Taylor, M., Bradford, M., Raymond, P. and Peccia, J., 2023. Microbial activity contributes to spatial heterogeneity of wetland methane fluxes. Microbiology Spectrum, 11(5), pp.e02714-23.
Bastviken D, Treat CC, Pangala SR, Gauci V, Enrich-Prast A, Karlson M, Gålfalk M, Romano MB, Sawakuchi HO. The importance of plants for methane emission at the ecosystem scale. Aquatic Botany. 2023 Jan 1;184:103596.
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Chadburn SE, Aalto T, Aurela M, Baldocchi D, Biasi C, Boike J, Burke EJ, Comyn-Platt E, Dolman AJ, Duran-Rojas C, Fan Y. Modeled microbial dynamics explain the apparent temperature sensitivity of wetland methane emissions. Global Biogeochemical Cycles. 2020 Nov;34(11):e2020GB006678
Chang, K.Y., Riley, W.J., Collier, N., McNicol, G., Fluet-Chouinard, E., Knox, S.H., Delwiche, K.B., Jackson, R.B., Poulter, B., Saunois, M. and Chandra, N. (2023) Observational constraints reduce model spread but not uncertainty in global wetland methane emission estimates. Global Change Biology.
Curry, C. L. (2007). Modeling the soil consumption of atmospheric methane at the global scale. Global Biogeochemical Cycles, 21, GB4012. https://doi.org/10.1029/2006GB002818
Dangal, S.R., Tian, H., Xu, R., Chang, J., Canadell, J.G., Ciais, P., Pan, S., Yang, J. and Zhang, B. (2019) Global nitrous oxide emissions from pasturelands and rangelands: magnitude, spatiotemporal patterns, and attribution. Global Biogeochemical Cycles, 33(2), pp.200-222.
Dean, J.F., Middelburg, J.J., Röckmann, T., Aerts, R., Blauw, L.G., Egger, M., Jetten, M.S.M., Jong, A.E.E.d., Meisel, O.H., Rasigraf, O., Slomp, C.P., Zandt, M.H.i.t., and Dolman, A.J., (2018) Methane feedbacks to the global climate system in a warmer world. Rev. Geophys, 56, 207-250.
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Feng, L., Palmer, P.I., Zhu, S., Parker, R.J. and Liu, Y. (2022) Tropical methane emissions explain large fraction of recent changes in global atmospheric methane growth rate. Nature communications, 13, pp.1-8.

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France, J.L., Lunt, M.F., Andrade, M., Moreno, I., Ganesan, A.L., Lachlan-Cope, T., Fisher, R.E., Lowry, D., Parker, R.J., Nisbet, E.G. and Jones, A.E. (2022). Very large fluxes of methane measured above Bolivian seasonal wetlands. Proceedings of the National Academy of Sciences, 119(32), p.e2206345119.
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Author Rebuttal letter:

REVIEWER COMMENTS:
Reviewer #1 (Remarks to the Author):
The review by Cui and colleagues addresses wetland methane emissions and their relationship to hydrology. Although it is often assumed that there is a simple, and measurable, relationship between water table and methane, numerous studies fail to detect this. This might arise as an artefact of the field design (WT fluctuates over a short range only, minimal replication of flux measurements, etc) or because of real, biogeochemical processes (a lag in CH4 response, anaerobic microsites above the level of the WT, etc). So the rationale behind the paper is interesting. The authors write about the extent and magnitude of WT fluctuations in wetlands; about relationships (or lack of) between WT and CH4; about soil conditions and microbes; and about plant-mediated transport and ebullition. They also present some data, including new analysis of existing data, on a sort of case-study/example basis to support their writing. The paper is well written and interesting, and I enjoyed reading it. The topic is relevant and a good fit for the journal. However, at times I wondered what the papers aims to do – there are passages of review, and then passages focusing on individual sites and certain time periods (section 3 in particular). These "case studies" can be useful to show examples. But I'm less comfortable with the FLUXNET analysis of just ten sites (which comes unannounced with no methods section/primer), which are widely distributed across continents (N America, Europe, Asia) and latitudes (between France and northern Finland). No detail is given as to how the data from these sites is comparable and the authors even state that data was collected over periods between 1-7 years. But this is more like a synthesis paper (much of section 3 e.g. Fig. 1, Fig. 4, most of the supplementary), in which case why not go all in and analyse more FLUXNET sites?Econventional review paper (and is all the better for it, I think).
Note that my expertise lies primarily with fluxes and water table, and I cannot comment with any expertise on section 4, 5.2, 6 and 7.
Also, I spotted some issues where sentences are attributed to references that don't really support them.
In several places I link/mention other papers. These aren't "demands" to cite these papers, just included for interest/relevance, and it's up to the authors whether to cite them or not.
Response: Thank you for your comments. We agree with your concerns regarding the site selection from FLUXNET. After extensive discussions with our co-authors, we have implemented the following key changes:
1) Included more data from other types of wetland sites, totaling 32 sites. This includes 6 bog sites, 7 fen sites, 3 swamp sites, 7 marsh sites, 3 salt marsh sites, 5 rice sites, and 1 mangrove site (site information in Table S1). Given this paper's primary focus on examining the influence of hydrological fluctuations on methane emissions at large scales, the inclusion of long-term monitoring sites from FLUXNET serves as illustrative examples showcasing these fluctuations.
2) Removed statistical analyses from the figures. The water table fluctuations from different wetland types are illustrated in updated Fig. 1, without any statistical analysis, aiming to illustrate the common phenomenon of water table fluctuations in wetland ecosystems. Additionally, considering the current limitations in understanding the impact of hydrological fluctuations on methane emission under diverse climatic conditions, the new Fig. 1 does not illustrate the differences between site locations and climatic conditions.
The reference list has been updated, such as Lines 53, 70, 72, 125, 162, 172-173, 615, 622, 659.

Detailed comments follow.

L55. The paper begins as follows:
"Methane (CH4) in the atmosphere contributes 16 – 25% of the Earth's warming to date. This high-potency greenhouse gas possesses more than 80 times the warming potential of carbon dioxide within the first two decades after its release into the atmosphere, thereby playing a significant role in setting the pace for near-term global warming1."

The problem is that citation 1 is the aquatic synthesis by Rosentreter et al (2021). They may say these things, but they are citing someone else when they do. Citation accuracy matters. It seems like an IPCC report or similar may be a better citation here.
Response: Thank you for the suggestions. We have updated the references (Line 53) as suggested, now incorporating the IPCC 2014 report and the research conducted by Etminan and colleagues.
1) Masson-Delmotte, V. et al. Climate change 2021: the physical science basis. Contribution of working group I to the sixth assessment report of the intergovernmental panel on climate change 2 (2021).
2) Etminan, M., Myhre, G., Highwood, E. J. & Shine, K. P. Radiative forcing of carbon dioxide, methane, and nitrous oxide: A significant revision of the methane radiative forcing. Geophysical Research Letters 43, 12-614 (2016).

L71. I agree with much of this paragraph, but would suggest emphasizing throughout that you're talking about very large-scale measurements of emissions. Variation in methane fluxes, and their responses to hydrology and meteorology, can be easily tracked at the ecosystem scale using eddy covariance. And actually, considering you draw on lots of FLUXNET data, it would be good to (concisely) prime the reader about eddy covariance and what it shows (i.e. ecosystem-scale fluxes, continuously measured).
Response: Thank you for the suggestions. We have incorporated "large-scale measurements" into this paragraph and succinctly briefed the reader on the contribution of eddy covariance to our understanding of methane fluxes in response to hydrological fluctuations. The revision can be found in Lines 87-91.

L99. "Thus, the connection between hydrological fluctuations and the CH4 cycle in wetlands, influenced by both biotic and abiotic environmental factors, remains significantly underestimated." I don't think "underestimated" is the best word here. Something relating to uncertainty/a knowledge gap would make more sense, e g: "Thus, a complete understanding of the connection between hydrological fluctuations and the CH4 cycle in wetlands, influenced by both biotic and abiotic environmental factors, remains lacking."
Response: We agree. We have incorporated your suggested revisions in Lines 109-111.

L116. Ref 26 is given as: Nrcs, U. (Natural Resources Conservation Service Washington, DC, USA, 2008). This is not sufficient to enable the reader to figure out what report or document the citation refers to. Please fix.
Response: Thank you for the suggestion. This reference is a Technical Note. We have now updated the information of this reference as following: Brinson, M. Hydrogeomorphic wetland classification system: an overview and modification to better meet the needs of the Natural Resources Conservation Service. USDA Technical Note NO. 190-8-76. Natural Resources Conservation Service Washington, DC, USA (2008).

L116. Ref 27 is given as: Brinson, M. M. A hydrogeomorphic classification for wetlands. (1993). Here there is enough detail to find the reference but more detail is needed as to where the reference is from (it's US Army Corps of Engineers, Wetlands Research Program Technical Report WRP-DE-4).
Response: Thank you for the suggestion. We have updated the information of this reference as following: Brinson, M. M. A hydrogeomorphic classification for wetlands. Wetlands Research Program Technical Report WRP-DE-4. U.S. Army Corps of Engineers, Washington, DC (1993).

L123. I work on wetlands, but am not particularly familiar with the terms "slope wetlands" or "mineral flats". Are these country specific terms? I'd suggest expanding slightly to explain, or changing to different terms.
Response: Thank you for the suggestion. This part was originally grounded in the hydrogeomorphic-based wetland classification from Brinson, M. Hydrogeomorphic wetland classification system: an overview and modification to better meet the needs of the Natural Resources Conservation Service. USDA Technical Note NO. 190-8-76. Natural Resources Conservation Service Washington, DC, USA (2008). However, taking into account the suggestions from the Editor and Reviewer 2 to streamline and concise this paper, we have opted to remove this part.
L126. Ref 30 needs a title in the reference list (I assume it's a chapter).

Response: Thank you for the suggestion. In response to the feedback provided earlier, we have decided to remove also this reference from the paper.

L129. “Typical reports include the frequent water table depth (WTD) fluctuations of wetland caused by the increase of extreme precipitation events in most temperate regions15” Reference 15 is a mesocosm experiment, so it’s hard to see how that can support a sentence about increasing extreme precipitation events and WT fluctuations.
Response: Apologies, this citation is not appropriate for our context. The correct reference is Pachauri, Rajendra K., et al. Climate change 2014: synthesis report. Contribution of Working Groups I, II and III to the fifth assessment report of the Intergovernmental Panel on Climate Change. Ipcc, 2014.

L145. “has led to the long-term degradation of 54 – 57% of wetlands globally” Perhaps worth citing the recent paper by Fluet-Chouinard et al (2023) that puts the figure much lower at 21%. https://www.nature.com/articles/s41586-022-05572-6
Response: Thank you for the suggestion. We have revised the reference accordingly, changing 54 – 57% to 21% in Line 161.

L156. “Under different climate change scenarios, interannual fluctuations in wetland WTD can exhibit significant variation due to changes in rainfall and drought patterns, with a 60 cm gap between the wettest and driest conditions”. Does this 60cm come from just one randomly chosen study? There are plenty of examples in the literature with +100cm, from various wetland types, e.g:
https://link.springer.com/article/10.1007/s10021-015-9873-x (see fig. 1)
https://www.sciencedirect.com/science/article/pii/S0167880918304018 (see fig. 1)
https://www.sciencedirect.com/science/article/pii/S0022169402000124 (see fig. 2)
https://onlinelibrary.wiley.com/doi/epdf/10.1002/hyp.8109 (see table 3)
Response: Thank you for the suggestions. We have excluded this specific example and instead characterized the amplitude of seasonal water table fluctuations in wetlands as “vary widely, ranging from several tens of centimeters to over a meter”. The corresponding citation has also been updated. The revision can be found in Lines 171-173.

L159. As is sometimes the danger with these review papers, here we are talking about wetlands and then suddenly switch to bogs and fens (i.e. peatlands; a subset of wetlands with organic soils). It might be worth flagging on L161 that you are talking specifically about peatlands here, e.g:
“For instance, when considering peatlands (wetlands with organic soils), bogs mostly rely on precipitation and evapotranspiration to establish their water balance…”
Response: Thank you for the suggestion. Considering the conciseness of this review requested by the Reviewer 2 and Editor, we have decided to remove this specific comparison.

L165. This figure comes unannounced. So far, my understanding was that this was a descriptive review paper, but here we find new figures. I would suggest adding a few sentences in the intro/abstract to highlight that there is data analysis/extraction as part of the paper, and you can refer the reader to the supporting information where the methods are.
Response: Thank you for the suggestions. Combined with the Editor’s suggestions, we chose not to perform statistical analyses on the data. The updated Fig. 1 retains only the raw water table data across various wetland types, with the intention of offering readers an intuitive understanding of water table fluctuation phenomenon.

Fig 1, Fig 4, Fig S1, Table S1 and methods. The figure comparing WT in fens and bogs is interesting but has problems. Firstly, your sample sizes in Fig.1 are not 5074, 9086, 7053 and 7107, they are 4, 6, 5 and 5 (number of sites). Your measurements of WT and flux are pseudoreplicated in time – that’s not a problem in itself, but the data need to be presented with honest sample sizes. You could include this info in the legend so the reader can truly understand what data underlies the figure. Secondly, your somewhat arbitrary selection of ten sites does not make for a truly robust analysis, at least not without more justification – are the ten sites under the same

management regimes (conservation/nature-reserve managed?), are they drained or ditched in any way? The sites are found in many countries, not even on the same continent (Europe, Asia, N America), and between 47°N and 68°N – so with a small sample size how can you tell it is fen vs bog, or dominant vegetation, that is driving the results in Fig.1? And the authors even state that WTs were measured between one and seven years – so how is this comparable? It might be that this can be left as, but the authors need to convincingly argue that it is appropriate.
Response: Thank you for the suggestions. Combined with your, Reviewer 2's, and the Editor's suggestions, we have updated Fig. 1 to include more wetland types that provide water table data (32 sites, including 6 bog sites, 7 fen sites, 3 swamp sites, 7 marsh sites, 3 salt marsh sites, 5 rice sites, and 1 mangrove site). In the new Fig. 1, we only illustrate the diversity in water table fluctuations as a common phenomenon among different wetland types, without statistical analyses. Besides, we have removed the original Fig.4 and Fig. S1 and updated Table S1.

L169, 172. Sphagnum should be capitalised.
Response: Thank you for the suggestion. We have now revised it in Line 198.

L169. "Compared to aerenchymatous plant, sphagnum can form a dense cover on peatlands, with its unique cellular structure enabling it to absorb and retain water, acting like a sponge55. This results in high WTD with relatively small fluctuation ranges in sphagnum-dominated peatlands (Fig. 1b&S1b)" As for my comment about the small sample size. If you want to keep this in, I would also suggest adding more references to the literature to support an effect of Sphagnum on WT fluctuations.
Response: Thank you for the suggestions. Following the Editor's suggestion, we should not conduct deep statistical analysis on water table data for different wetland types. We therefore removed Fig. 1b&S1b. Besides, in Lines 191-202 of this paragraph, we have added more descriptions of vegetation-water table interactions.

I find some of the figures/their labelling non-intuitive. Consider that Baird & Low (2022) which you cite says:"there seems to be some confusion over how this depth should be expressed. Many authors use positive values to indicate the depth of the water table below the surface, while others use negative values. The confusion arises because the direction of the vertical axis is not properly considered. A depth is a positive value below a surface (the vertical axis is positive downwards – depth increases downwards) … A negative WTD is either meaningless or implies a water level (no longer a water table) above the ground surface." You consistently use WTD on your y axes, and show this declining into negative numbers as droughts occur (e.g. fig. 3b); i.e. in contradiction to what Baird & Low suggest is sensible. I'd suggest swapping "WTD" for "WT" on your axes, and perhaps also reversing the +/- signs so that a negative WT represents surface flooding. Somewhere you should explicitly state what your convention is (i.e. whether negative or positive WT values show surface flooding).
Response: Thank you for the suggestions. We agree with you that a positive water table represents distance below the surface, while a negative water table represents surface flooding. This convention is now explicitly stated in the captions of updated Fig. 1 and Fig. 2. Additionally, we have replaced "WTD" with "water table" on the vertical axes of both figures and reversed the +/- signs.

Fig.3c. The lines showing significance here are really confusing because they overlap. And it seems statistically dubious to compare every year individually to the 2018 drought year, rather than running something like an ANOVA with post-hoc which will tell you which years differ (not just compared to 2018). You state that 2018 was extreme drought for this site but present no annual WT data to back this up – the reader has to take your claim at face value. It would be better to delete panel a and insert a multi-year WT graph instead. Also, panel c is so small it's impossible to see detail – it might be better as a stand-alone figure.
Response: Thank you for the suggestions. Following the Editor's suggestion, we should not conduct statistical analyses on the data and have therefore removed the original Fig. 3. Instead, for this section, we have referenced a recent study by our collaborators, Kalhori et al. (2024), to support the long-term effects of extreme drought events on methane emissions (Kalhori, A. et al. Temporally dynamic carbon dioxide and methane emission factors for rewetted peatlands. Communications Earth & Environment 5, 62 (2024).).

L201. “In wetlands, WTD has long-standing served as a driver of CH4 flux variations.” Served is not quite right here. Perhaps, “In wetlands, WTD has long been considered a primary driver of CH4 flux variations”?
Response: We agree. We have revised it following your suggestion in Line 231.

L214. As per my comment about Baird & Low, is it WTD is it’s “permanently inundated”? For surface flooded, “water level” or “water depth” is probably more accurate.
Response: We agree. We have revised the “WTD” to “water level” in Line 244.

Supplementary, L57. What is “linear fitting”? Presumably you mean linear regression or Pearson correlations, but be explicit. Is it appropriate to test for linear relationships? For comparison, Extended Data Fig. 4 in Evans et al (2021), which you cite, shows non-linear relationships between CH4 and WT for their data, as well as that from Levy et al, Couwenberg et al and Turetsky et al.
Response: Thank you for the suggestions. We agree with you that testing for a linear relationship may not be suitable for properly describing the relationship between CH4 and water table at the SE-Deg and FI-Sii wetland sites. Additionally, considering the Editor's suggestion to limit statistical analysis, we have updated Fig. 2, retaining only the raw data.

Section 4. I very much enjoyed this part. After section 3 which couldn’t decide if it was a review or a synthesis, this section is very much a proper review, and it does a great job of explaining why WT might not be a robust proxy for CH4 flux in all sites/at all times. I think this section will particularly be appreciated by those who spend a lot of time measuring fluxes in the field.
Response: Thank you for the recognition.

L421. Should be “Humic substances are…”. But the very existence of humic substances are debated (https://www.nature.com/articles/nature16069) so better to swap for “DOM” instead (also L427, L429, L431)
Response: We agree. We have swapped “humic substances” for “DOM” in Lines 446, 452, 454, and 456.

L433. “Humus”. Perhaps swap for: “Similarly to natural DOM, pyrogenic carbon…”
Response: We agree. We have swapped “humus” for “natural DOM” in Line 457.

L550. “Additionally, plant-mediated transport generally dominates CH4 emissions from wetlands with emergent vegetation through aerenchyma”. This is quite a claim for “wetlands” as a global category. It might be true of some types of wetlands but I’m not sure can confidently said for all wetlands. The recent review by Bastviken et al shows the myriad ways plants can enhance or supress wetland methane emission. And Kosten et al’s review states which shows vegetation can be important, but they highlight large uncertainty around total contributions.
https://www.sciencedirect.com/science/article/pii/S0304377022001085
https://eartharxiv.org/repository/view/2904/
Response: Thank you for the suggestions. We have clarified this in Lines 620-622 as follows: “plant-mediated transport dominates CH4 transport from some emergent plant-dominated wetlands compared to ebullition and diffusion”. This can be referred to the literature survey by Ueyama et al. (2023) (see Table 1 in this reference, Ueyama, M. et al. Modeled production, oxidation and transport processes of wetland methane emissions in temperate, boreal, and Arctic regions. Global Change Biology (2023).). Certainly, we agree with you that plants can influence CH4 cycling in wetlands in various ways, such as providing substrate, transporting oxygen, and providing habitat for CH4 oxidizing microorganisms. However, we do not think this is a point we need to discuss in Section 8.

L589. This is the first mention of SAR. Why not provide some refs here showing its use for keen readers to take a look at?
Response: Thank you for the suggestion. We have added two references in Line 659. These two papers detail the principles, progress, trends, and challenges of InSAR in hydrological monitoring of wetlands.

1) Mohammadimanesh, F., Salehi, B., Mahdianpari, M., Brisco, B. & Motagh, M. Wetland water level monitoring using interferometric synthetic aperture radar (InSAR): A review. Canadian journal of remote sensing 44, 247-262 (2018).
2) Lee, H., Yuan, T., Yu, H. & Jung, H. C. Interferometric SAR for wetland hydrology: An overview of methods, challenges, and trends. IEEE Geoscience and Remote Sensing Magazine 8, 120-135 (2020).
Reviewer #2 (Remarks to the Author):
Comments on Cui et al: Hydrological dynamics and methane flux in wetlands….
General Comments
This paper is a detailed and worthy review of the literature on the interaction between hydrology and methane flux in wetlands.
The paper deserves to be published, but after moderate revision. The authors should be encourage to revise the paper, first by shortening it, then by addressing the points of concern listed below, and by updating generally.
The paper begins with an introduction to the methane problem: the importance of biogenic methane emissions in driving climate change. That's good, but the text needs much revision and updating. The paper then goes on to consider the impacts of hydrological fluctuations on methane fluxes. This discussion includes soil impacts. The paper then goes on to discuss methanogenesis and methanotrophy, and finally transport.
I have both general and specific comments.
More generally, the review is highly qualitative and at times rambling and profligate with words – a lot thrown in, but in places the focus wanders. It's not waffle, but again and again as I read the paper, being a bear of little brain, I asked myself 'do I really need to know this'Points of concern
a) the focus is almost entirely on boreal wetlands – or at least sphagnum wetlands: in short, the bias of FLUXNET sites. Not much is said about African and South American wetlands. In particular there's no discussion about the significance of C4 plants in tropical wetlands. Plants like C . papyrus dominate many African wetlands and have parallels in S. America: because of their tall stems they must also impact methane egress.
Response: Thank you for the suggestions. We have now included more wetland types that provide water table data, totaling 32 sites, including 6 bog sites, 7 fen sites, 3 swamp sites, 7 marsh sites, 3 salt marsh sites, 5 rice sites, and 1 mangrove site. We have also added more discussions about (sub)tropical wetlands, such as Lines 144-147, 176-178, 194-198, and 624-628.
We acknowledge the significance of papyrus wetlands within wetland ecosystems. However, there exists limited research, to the best of our knowledge, addressing the impact of hydrological fluctuations on CH_4 production, oxidation, and transport in papyrus wetlands. Notably, one of the few studies conducted by Were et al. (2021) found that CH_4 fluxes in papyrus-dominated tropical wetlands did not exhibit significant differences between the dry and wet seasons (Were, David, et al. "Carbon dioxide and methane fluxes from a tropical freshwater wetland under natural and rice paddy conditions: implications for climate change mitigation." Wetlands 41.5 (2021): 52.). Besides, it was challenging to find published literature on the dominant plant-mediated transport pathway for CH_4 in Cyperus papyrus, as highlighted in the study by Helfter et al. (2022) (Helfter, C. et al. Phenology is the dominant control of methane emissions in a tropical non-forested wetland. Nature communications 13, 133 (2022).). Given the importance of papyrus, we consider this as a knowledge gap in Lines 676-678.

b) little is said about Temperature impacts – global warming is primarily about heat; that leads to meteorological change and thus precipitation change. But don't forget that Temperature has direct impacts – the Arrhenius (exponential) relationship of productivity – including methane) to Temperature rises, provided nutrients are accessible.
Response: Thanks for your valuable input. This review's purpose is to discuss the impacts of water table fluctuations on CH_4 cycling. We do understand that these effects may differ under various climatic conditions and temperatures. However, comprehensively analyzing the interactive effects of these two factors remains a significant challenge, given the limited existing information. Therefore, we would much like to consider the temperature-affected hydrological effect on CH_4 emissions as a crucial knowledge gap that needs future research endeavors. These additions are provided in Lines 104-107 and 687-691.

c) the paper mentions plant stem transport as a pathway for egress to air (e.g. see Fig 6) but only comparatively briefly. (see comment about papyrus above.)
Response: Thank you for the suggestion. We have updated the content about plant-

mediated transport in Lines 611-631 of Section 8.

d) isotopic fractionation is passed over – but that's the best tool atmospheric chemistry has for differentiating between methane from fossil fuel leaks and methane from wetlands.
Response: Thank you for the suggestions. We have supplemented relevant content on isotope fractionation in Lines 67-70. As follows: "Importantly, over the past decade, there has been a continuous increase in atmospheric CH4 concentration, coupled with significant variations in growth rates. Isotope fractionation-based analysis suggests that biogenic CH4 emissions may strongly contribute to this trend."

e) the introduction section is somewhat out of date and reads rather like an excerpt from a PhD thesis.
Response: Thank you for the suggestion. While we may not fully grasp the intent of your comment, we have done our best to revise the Introduction, such as Lines 67-73, 85, 87-91, 96, 104-107, and 109-111.

Specific comments
Abstract – very discursive. Should be made more concise and simultaneously more informative.
Response: Thank you for the suggestion. We have revised the Abstract (Lines 38-48) to concisely emphasize the key aspects of this review, aiming to improve its clarity and informativeness.

Line 55 needs specific reference. This is a complicated number and it depends how you assess warming.
Response: Thank you for the suggestion. We have added two references in Line 53.
1) Masson-Delmotte, V. et al. Climate change 2021: the physical science basis. Contribution of working group I to the sixth assessment report of the intergovernmental panel on climate change 2 (2021).
2) Etminan, M., Myhre, G., Highwood, E. J. & Shine, K. P. Radiative forcing of carbon dioxide, methane, and nitrous oxide: A significant revision of the methane radiative forcing. Geophysical Research Letters 43, 12-614 (2016).

Lines 55-70- needs updating. Reads like a PhD introduction. See ref list below.
Response: Thank you for the suggestion. We have revised this paragraph (Lines 52-73). As follows: "Methane (CH4) in the atmosphere contributes ~ 16 – 25% of the Earth's warming to date. Importantly, over the past decade, there has been a continuous increase in atmospheric CH4 concentration, coupled with significant variations in growth rate. Isotope fractionation-based analysis suggests that biogenic CH4 emissions strongly contribute to this trend. Wetlands, recognized as the largest natural source of CH4, are considered as the main contributor to the recent CH4 growth rate anomaly. However, their emissions exhibit high uncertainty and vary across spatial and temporal scales."

Line 84 – might be a good idea to add a paragraph about the difference between Tropical C4 and Boreal C3 wetlands. After all, this is a 'paper' and paper comes from 'papyrus'!
Response: Thank you for the suggestions. We think that adding a comparison between tropical and boreal wetlands in the Introduction may disrupt the logical flow. To incorporate more content on tropical wetlands, we have taken the following steps:
(i) Expanded wetland sites from 10 to 32, including three swamp sites in tropical regions (site information in Table S1).
(ii) Added more discussions on (sub)tropical wetlands in Sections 2-8, such as Lines 144-147, 176-178, 194-198, and 624-628.

Line 93 – Anaerobic oxidation of methane is mentioned in various places (e.g.289, 336, 459) but each time it's only briefly passed over and comments tend to repeat earlier text….Maybe AOM deserves more attention in the paper, perhaps a paragraph of explanation.
Response: Thank you for the suggestions. In fact, the AOM-related content is primarily presented in Lines 483-497 of Section 6 and Lines 558-588 of Section 7.2, where we

respectively discuss how hydrological fluctuations impact AOM through abiotic pathways and biological pathways. Besides, in the original Line 93, we aimed to succinctly convey the latest discoveries in microbial CH4 processes over the past decade, such as CH4 production in microaerobic zones and CH4 oxidation in anaerobic zones, challenging the traditional boundaries of the CH4 cycle. In the original Line 289, there is a misunderstanding here. It is not discussing anaerobic CH4 oxidation but rather stating that the CH4 production process theoretically occurs after anaerobic organic carbon oxidation.

L127 – Climate change impacts – essentially the 'meteorological tropics' are expanding. Rainfall increases in a warmer world so wetlands grow, and Temperature rise by itself also drives methane emission…
Response: Thank you for the suggestions. We have emphasized the intrinsic effect of rising temperature on rainfall in Lines 142-144 and supplemented examples about the impacts of rainfall on the expansion of tropical wetlands in Lines 144-147.

L137 – glacier melting – well, actually ice caps as much as mountain glaciers. Also a significant part of the rise is thermal expansion.
Response: Thank you for the suggestions. We have revised the content in Lines 148 and 153.

L145 – 54-57% number - I'm not so sure, and it seems impossibly precise. Right now there is large expansion of wetlands going on in central Africa. The Tropical patterns are shifting. Also there is a lot more fertiliser and sewage flowing into tropical wetlands than there was 50 years ago. See for example Kayima et al. (listed below, and other papers)
Response: Thank you for the suggestions. Combined with Reviewer 1's suggestion, we have updated the reference to the reference below. In this new reference, the global wetland rate loss is 21%. We have revised it in Line 161.
Fluet-Chouinard, E. et al. Extensive global wetland loss over the past three centuries. Nature 614, 281-286 (2023).

L167 – need to mention just how different Tropical wetlands are from boreal. The world isn't just sphagnum.
Response: Thank you for the suggestion. We have added more examples of vegetation-water table interactions in Lines 191-202. In addition, as mentioned earlier, we have expanded wetland sites from 10 to 32, including 3 swamp sites in tropical regions.

Line 188-199 – maybe this should be followed by a paragraph on temperature directly impacting plant productivity and hence methane emissions. The manuscript has a very 'FLUXNET" view of the world – has little to say about Africa and S America.
Response: Thank you for the suggestions. We agree with you that temperature can have a significant impact on wetland CH4 emissions by directly influence the kinetics of CH4 biogeochemical reactions and indirectly affect plant traits (Lines 104-107). However, this review primarily seeks to elucidate how hydrological fluctuations affect wetland CH4 emissions through biotic and abiotic pathways. Moreover, the analysis of the interactive effects involving water table fluctuations, temperature, and CH4 emissions remains a challenging endeavor, with the existing information still considerably limited.
For covering tropical wetlands, we have incorporated more discussion on tropical regions in Sections 2-8, such as Lines 144-147, 176-178, 194-198, and 624-628.

Line 247 – also need to discuss transport in plant stems and trees (see Bastviken and Gauci papers in ref list.
Response: Thank you for the suggestion. In Section 3, we mainly discussed the phenomenon of wetland CH4 uncertainty under the influence of hydrological fluctuations. In Lines 611-631 of Section 8, we have updated the content about plant (including plant stems and trees)-mediated transport.

L288-293 – the paper needs a coherent paragraph on AOM. L459 repeats what is said here.

Response: Thank you for the suggestion. There is a misunderstanding in the original Line 289-293. It is not discussing anaerobic CH4 oxidation but rather stating that the CH4 production process theoretically occurs after anaerobic organic carbon oxidation. In fact, in Lines 483-497 of Section 6, we presented the effects of hydrological fluctuations on anaerobic CH4 oxidation via abiotic routes; in Lines 558-588 of Section 7.2, we discussed the effects of hydrological fluctuations on anaerobic CH4 oxidation via biotic routes.

L331-2 – good.
Response: Thanks!
L531 – typo?? 'my"
Response: Thank you for the suggestion. We have changed "my" to "may" in Line 577.

L588 – the temperature impact is a knowledge gap – for example see Chadburn paper in fref list (12% increase per degree C warming).
Response: Thank you for the suggestion. We have added this knowledge gap in Lines 687-691. As follows: "(vi) The impact of hydrological fluctuations on methane cycling may vary under different climatic conditions with different temperatures. However, the current information remains challenging in deciphering the interactive effects of these two factors. Thus, further research is essential to explore the temperature-affected hydrological effect on methane emissions."

Overall comments
The paper should be published after moderate revision. I would suggest the authors should first go through the paper and shorten the existing text by about 30%. This will make it much tighter and better structured.
Response: Thank you for the suggestion. We have streamlined the paper, such as removing the original Lines 120-126, 128-133, 150-159, 161-166, 180-182, 197-199, 244-249, 254-269,325-327, 352-354, 365-369, 577-581.

Then the authors should address the points of concern raised in review. Listed below are some possible recent References to think about, and to pick and choose from….(I'm not suggesting you cite more than a small number in the revised paper, but you do need to update on the global methane budget).
Response: Thank you for the suggestion. These references provided significant contributions to enhancing the quality of this review. We have added some of these references in our work. For instance:

1) Bastviken, D. et al. The importance of plants for methane emission at the ecosystem scale. Aquatic Botany 184, 103596 (2023).
2) Basu, S. et al. Estimating emissions of methane consistent with atmospheric measurements of methane and δ 13 C of methane. Atmospheric Chemistry and Physics 22, 15351-15377 (2022).
3) Cai, W. et al. Pantropical climate interactions. Science 363, eaav4236 (2019).
4) Chadburn, S. E. et al. Modeled microbial dynamics explain the apparent temperature sensitivity of wetland methane emissions. Global Biogeochemical Cycles 34, e2020GB006678 (2020).
5) Dean, J. F. et al. Methane feedbacks to the global climate system in a warmer world. Reviews of Geophysics 56, 207-250 (2018).
6) Gauci, V. et al. Non-flooded riparian Amazon trees are a regionally significant methane source. Philosophical Transactions of the Royal Society A 380, 20200446 (2022).
7) Lunt, M. F. et al. Rain-fed pulses of methane from East Africa during 2018–2019 contributed to atmospheric growth rate. Environmental Research Letters 16, 024021 (2021).
8) Nisbet, E. G. (2023). Climate feedback on methane from wetlands. Nature Climate Change, 13(5), 421-422.
9) Oh, Y. et al. Improved global wetland carbon isotopic signatures support post-2006 microbial methane emission increase. Communications Earth & Environment 3, 159 (2022).
10) Peng, S. et al. Wetland emission and atmospheric sink changes explain methane growth in 2020. Nature 612, 477-482 (2022).
11) Saunois, M. et al. The global methane budget 2000–2017. Earth system science data 12, 1561-1623 (2020).
12) Schaefer, H. et al. A 21st-century shift from fossil-fuel to biogenic methane emissions indicated by 13CH4. Science 352, 80-84 (2016).
13) Ueyama, M. et al. Modeled production, oxidation and transport processes of wetland

methane emissions in temperate, boreal, and Arctic regions. Global Change Biology (2023).

Albuhaisi, Y.A., van der Velde, Y. and Houweling, S., 2023. The Importance of Spatial Resolution in the Modeling of Methane Emissions from Natural Wetlands. Remote Sensing, 15(11), p.2840.
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Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Professor Wu,

Your revised manuscript titled "Hydrological Dynamics and Methane Flux in Wetlands: Unraveling the Complexities for Climate Action" has now been seen by the two original reviewers, who are satisfied with your revisions and a new third reviewer. Reviewer #3 was brought in at this round because it became apparent that the microbiology aspect of the article had not been adequately assessed. We apologise that this was not caught at the previous round, when the internal editor was covering the handling of your manuscript on behalf of the Editorial Board Member. We include Reviewer #3's comments at the end of this message. They find your work of interest and are supportive, however they raise a number of queries and clarifications that should be addressed. We remain interested in the possibility of publishing", perhaps something more like "We hope that you will find the comments helpful for improving the manuscript and continue to be interested in publishing your review article if you can address these points.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Joshua Dean, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0001-9058-7076

Clare Davis, PhD
Senior Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have spent significant effort in revising their manuscript to address my comments, and those of R2. I particularly appreciate the deletion of lots of text to slim down the draft. I have two minor comments on this revision, as follows below, then believe the paper will be acceptable for publication, and well read and widely cited.

1) In the revised figure one, the legend says “Note: positive values of the water table represent distance below the ground surface and negative values represent surface flooding.”
This might not be an issue, but I do wonder; swamps, rice, and mangroves all skew way up above zero into positives. According to your legend this suggests that these ecosystems all suffer quite extreme water table drawdown. But I would assume these are the ecosystems most prone to surface flooding. Are your axes correct here, or is it just a case that negative numbers should be below the zero intercept?

2)Original comment: L165. This figure comes unannounced. So far, my understanding was that this was a descriptive review paper, but here we find new figures. I would suggest adding a few sentences in the intro/abstract to highlight that there is data analysis/extraction as part of the paper, and you can refer the reader to the supporting information where the methods are.

Response: Thank you for the suggestions. Combined with the Editor’s suggestions, we chose not to perform statistical analyses on the data. The updated Fig. 1 retains only the raw water table data across various wetland types, with the intention of offering readers an intuitive understanding of water table fluctuation phenomenon.

New comment: This is fine, but the FluxNet data still comes totally unannounced. Ideally, mention in your abstract that you synthesise data, because currently you just say that you “review”. I would also suggest adding a few words around L181 (in the track changes document) to again mention that you synthesise/collate FluxNet data.

Reviewer #2 (Remarks to the Author):

The manuscript has been carefully amended to take into account the comments made by the referees. Overall, from a rapid read-through, the work now looks substantially improved. Section 8 now has a much clearer exposition of plant-mediated transport of methane. The paper is a valuable contribution, reviewing a complex topic, and should now be published.

I only have very minor comments now.
1. It would help the flow of the first few paragraphs if the references were renumbered in sequence. It's just the first 10 or so that have become muddled, after a couple of deletions. Just restore a couple of the many new papers to fill the gaps and then number refs sequentially through to line 74. After that it doesn't matter. Ref 6 (Dean et al) is important and should be restored.
2. Ref 12 (Schaefer) is good but not now up to date: they thought the 13C shift was primarily driven by agriculture, whereas more recent papers like Basu et al, Lan et al, and Nisbet et al primarily focus on wetlands. Maybe keep ref 12 but replace ref 8 (nisbet general paper) with Nisbet 2023 which makes the wetland point much more strongly. Nisbet, E.G., et al. (2023)Atmospheric methane: Comparison between methane's record in 2006–2022 and during glacial terminations. Global Biogeochemical Cycles 37: e2023GB007875.
3. The paper still has a strongly 'northern' flavour, but now does think harder about the tropics...I'd still encourage the authors to deepen the discussion of tropical wetlands somewhat, though I agree the literature is much thinner than for wetlands in Europe and North America.

Reviewer #3 (Remarks to the Author):

Dear authors,

The submitted manuscript addresses a very complex topic and provides a nice overview of the complex links between hydrological dynamics and methane emissions from wetlands. It is an actual topic that has been extensively studied over the last decade. Therefore, a critical review of the current knowledge is timely and it makes a valuable contribution to the field. The paper is well written and I really enjoyed reading the work.

Although the topic is relevant, there is some work to be done in order to clarify and better visualize how hydrological fluctuations specifically impact the microbiology, the wetland methane cycle. Let me emphasize that my expertise lies in the microbiology of the methane cycle, and I am not an expert on wetland hydrology. My review therefore focuses on the microbiological aspects of the manuscript. I hope that my feedback is useful for you, and that it can improve the manuscript.

A general point of feedback for the manuscript is that, overall, a bit more detail needs to be added to aid the reader into understanding the story without having to look up the references. More specifically, if “particular genes”, “more sensitive” or “longer period” is mentioned, try to be more specific and mention the names of the genes, a percentage of increased sensitivity, the exact period in time. This improves the impact and understandability of the work. I have mentioned several instances under “general comments” in the uploaded document with all my comments.

Furthermore, some microbial key processes and pathways are missing from the review. These points need to be addressed, and I have indicated these instances in the “general comments” section.
The figures provide a good overview of the current knowledge in the field, but they could be used more to their full potential to support the review. I have added the detailed comments regarding the figures at the end of the “General comments” section.

As I want to clearly mention before you read the suggestions, I mention several papers that could be relevant for your work. These are only recommendations and I do in no way want to give the impression that these papers should be cited. The choice to incorporate this work is entirely up to the authors.

You can find my general and detailed comments, as well as the comments listed here in the attachment file "

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Author Rebuttal letter: The author's response to these comments can be found at the end of this file.

Version 2:

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Dear Professor Wu,

Your manuscript titled "Hydrological Dynamics and Methane Flux in Wetlands: Unraveling the Complexities for Climate Action" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

Martina Grecequet, PhD
Associate Editor,
Communications Earth & Environment
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REVIEWERS' COMMENTS:

Reviewer #3 (Remarks to the Author):

The manuscript has received major updates and the authors did a good job taking into account all the feedback from the reviewers. The work improved on both structure and contents, I enjoyed reading it. This paper is a valuable contribution to the field, addressing many complex topics and challenges, and I recommend publication of the manuscript.

The only minor comment that is straightforward to adjust is the use of the words oxic/anoxic and aerobic/anaerobic. Please be consistent in using oxic/anoxic for the environment and aerobic/anaerobic for (microbial) metabolism/processes.

Author Rebuttal letter:

REVIEWER COMMENTS:
Reviewer #3 (Remarks to the Author):
The manuscript has received major updates and the authors did a good job taking into account all the feedback from the reviewers. The work improved on both structure and contents, I enjoyed reading it. This paper is a valuable contribution to the field, addressing many complex topics and challenges, and I recommend publication of the manuscript.
The only minor comment that is straightforward to adjust is the use of the words

oxic/anoxic and aerobic/anaerobic. Please be consistent in using oxic/anoxic for the environment and aerobic/anaerobic for (microbial) metabolism/processes.
Response: Thank you for the suggestions. We have made the necessary adjustments to ensure consistent use of the terms oxic/anoxic for the environment and aerobic/anaerobic for microbial metabolism and processes throughout the manuscript. The revision can be found in Lines 79, 284, 311-312, 321, 323-324, 390, 446, 449, 490, 574, and 650.

Comments on Cui et al: *Hydrological dynamics and methane flux in wetlands….*

General Comments

This paper is a detailed and worthy review of the literature on the interaction between hydrology and methane flux in wetlands.

The paper deserves to be published, but after moderate revision. The authors should be encourage to revise the paper, first by shortening it, then by addressing the points of concern listed below, and by updating generally.

The paper begins with an introduction to the methane problem: the importance of biogenic methane emissions in driving climate change. That's good, but the text needs much revision and updating. The paper then goes on to consider the impacts of hydrological fluctuations on methane fluxes. This discussion includes soil impacts. The paper then goes on to discuss methanogenesis and methanotrophy, and finally transport.
I have both general and specific comments.
More generally, the review is highly qualitative and at times rambling and profligate with words – a lot thrown in, but in places the focus wanders. It's not waffle, but again and again as I read the paper, being a bear of little brain, I asked myself 'do I really need to know this'

Points of concern
a) the focus is almost entirely on boreal wetlands – or at least *sphagnum* wetlands: in short, the bias of FLUXNET sites. Not much is said about African and South American wetlands. In particular there's no discussion about the significance of C4 plants in tropical wetlands. Plants like *C . papyrus* dominate many African wetlands and have parallels in S. America: because of their tall stems they must also impact methane egress.
b) little is said about Temperature impacts – global warming is primarily about heat; that leads to meteorological change and thus precipitation change. But don't forget that Temperature has direct impacts – the Arrhenius (exponential) relationship of productivity – including methane) to Temperature rises, provided nutrients are accessible.
c) the paper mentions plant stem transport as a pathway for egress to air (e.g. see Fig 6) but only comparatively briefly. (see comment about papyrus above.)
d) isotopic fractionation is passed over – but that's the best tool atmospheric chemistry has for differentiating between methane from fossil fuel leaks and methane from wetlands.
e) the introduction section is somewhat out of date and reads rather like an excerpt from a PhD thesis.

Specific comments
Abstract – very discursive. Should be made more concise and simultaneously more informative.
Line 55 needs s specific reference. This is a complicated number and it depends how you assess worming.
Lines 55-70- needs updating. Reads like a PhD introduction. See ref list below.
Line 84 – might be a good idea to add a paragraph about the difference between Tropical C4 and Boreal C3 wetlands. After all, this is a 'paper' and paper comes from 'papyrus'!

Line 93 – Anaerobic oxidation of methane is mentioned in various places (e.g.289, 336, 459) but each time it's only briefly passed over and comments tend to repeat earlier text….Maybe AOM deserves more attention in the paper, perhaps a paragraph of explanation.
L127 – Climate change impacts – essentially the 'meterological tropics' are expanding. Rainfall increases in a warmer world so wetlands grow, and Temperature rise by itself also drives methane emission…
L137 – glacier melting – well, actually ice caps as much as mountain glaciers. Also a significant part of the rise is thermal expansion.
L145 – 54-57% number - I'm not so sure, and it seems impossibly precise. Right now there is large expansion of wetlands going on in central Africa. The Tropical patterns are shifting. Also there is a lot more fertiliser and sewage flowing into tropical wetlands than there was 50 years ago. See for example Kayima et al. (listed below, and other papers)
L167 – need to mention just how different Tropical wetlands are from boreal. The world isn't just sphagnum.
Line 188-199 – maybe this should be followed by a paragraph on temperature directly impacting plant productivity and hence methane emissions. The manuscript has a very 'FLUXNET" view of the world – has little to say about Africa and S America.
Line 247 – also need to discuss transport in plant stems and trees (see Bastviken and Gauci papers in ref list.
L288-293 – the paper needs a coherent paragraph on AOM. L459 repeats what is said here.
L331-2 – good.
L531 – typo?? 'my"
L588 – the temperature impact is a knowledge gap – for example see Chadburn paper in fref list (12% increase per degree C warming).

Overall comments

The paper should be published after moderate revision.
I would suggest the authors should first go though the paper and shorten the existing text by about 30%. This will make it much tighter and better structured.
Then the authors should address the points of concern raised in review.

Listed below are some possible recent References to think about, and to pick and choose from….

(I'm not suggesting you cite more than a small number in the revised paper, but you do need to update on the global methane budget).

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Comments on Cui et al: *Hydrological dynamics and methane flux in wetlands….*

General Comments

This paper is a detailed and worthy review of the literature on the interaction between hydrology and methane flux in wetlands.

The paper deserves to be published, but after moderate revision. The authors should be encourage to revise the paper, first by shortening it, then by addressing the points of concern listed below, and by updating generally.

The paper begins with an introduction to the methane problem: the importance of biogenic methane emissions in driving climate change. That's good, but the text needs much revision and updating. The paper then goes on to consider the impacts of hydrological fluctuations on methane fluxes. This discussion includes soil impacts. The paper then goes on to discuss methanogenesis and methanotrophy, and finally transport.
I have both general and specific comments.
More generally, the review is highly qualitative and at times rambling and profligate with words – a lot thrown in, but in places the focus wanders. It's not waffle, but again and again as I read the paper, being a bear of little brain, I asked myself 'do I really need to know this'

Points of concern
a) the focus is almost entirely on boreal wetlands – or at least *sphagnum* wetlands: in short, the bias of FLUXNET sites. Not much is said about African and South American wetlands. In particular there's no discussion about the significance of C4 plants in tropical wetlands. Plants like *C . papyrus* dominate many African wetlands and have parallels in S. America: because of their tall stems they must also impact methane egress.
b) little is said about Temperature impacts – global warming is primarily about heat; that leads to meteorological change and thus precipitation change. But don't forget that Temperature has direct impacts – the Arrhenius (exponential) relationship of productivity – including methane) to Temperature rises, provided nutrients are accessible.
c) the paper mentions plant stem transport as a pathway for egress to air (e.g. see Fig 6) but only comparatively briefly. (see comment about papyrus above.)
d) isotopic fractionation is passed over – but that's the best tool atmospheric chemistry has for differentiating between methane from fossil fuel leaks and methane from wetlands.
e) the introduction section is somewhat out of date and reads rather like an excerpt from a PhD thesis.

Specific comments
Abstract – very discursive. Should be made more concise and simultaneously more informative.
Line 55 needs s specific reference. This is a complicated number and it depends how you assess worming.
Lines 55-70- needs updating. Reads like a PhD introduction. See ref list below.
Line 84 – might be a good idea to add a paragraph about the difference between Tropical C4 and Boreal C3 wetlands. After all, this is a 'paper' and paper comes from 'papyrus'!

Line 93 – Anaerobic oxidation of methane is mentioned in various places (e.g.289, 336, 459) but each time it's only briefly passed over and comments tend to repeat earlier text….Maybe AOM deserves more attention in the paper, perhaps a paragraph of explanation.
L127 – Climate change impacts – essentially the 'meterological tropics' are expanding. Rainfall increases in a warmer world so wetlands grow, and Temperature rise by itself also drives methane emission…
L137 – glacier melting – well, actually ice caps as much as mountain glaciers. Also a significant part of the rise is thermal expansion.
L145 – 54-57% number - I'm not so sure, and it seems impossibly precise. Right now there is large expansion of wetlands going on in central Africa. The Tropical patterns are shifting. Also there is a lot more fertiliser and sewage flowing into tropical wetlands than there was 50 years ago. See for example Kayima et al. (listed below, and other papers)
L167 – need to mention just how different Tropical wetlands are from boreal. The world isn't just sphagnum.
Line 188-199 – maybe this should be followed by a paragraph on temperature directly impacting plant productivity and hence methane emissions. The manuscript has a very 'FLUXNET" view of the world – has little to say about Africa and S America.
Line 247 – also need to discuss transport in plant stems and trees (see Bastviken and Gauci papers in ref list.
L288-293 – the paper needs a coherent paragraph on AOM. L459 repeats what is said here.
L331-2 – good.
L531 – typo?? 'my"
L588 – the temperature impact is a knowledge gap – for example see Chadburn paper in fref list (12% increase per degree C warming).

Overall comments
The paper should be published after moderate revision.
I would suggest the authors should first go though the paper and shorten the existing text by about 30%. This will make it much tighter and better structured.
Then the authors should address the points of concern raised in review.

Listed below are some possible recent References to think about, and to pick and choose from….
(I'm not suggesting you cite more than a small number in the revised paper, but you do need to update on the global methane budget).

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Reviewer notes on 2024COMMSENV-23-2125A

The paper of Cui and co-workers provides an overview of the complex links between hydrological dynamics and methane emissions from wetlands. It is an actual topic that has been extensively studied over the last decade. Therefore, a critical review of the current knowledge is timely and it makes a valuable contribution to the field. The paper is well written and I really enjoyed reading the work.

Although the topic is relevant, there is some work to be done in order to clarify and better visualize how hydrological fluctuations specifically impact the microbiology, the wetland methane cycle. Let me emphasize that my expertise lies in the microbiology of the methane cycle, and I am not an expert on wetland hydrology. My review therefore focuses on the microbiological aspects of the manuscript.

A general point of feedback for the manuscript is that, overall, a bit more detail needs to be added to aid the reader into understanding the story without having to look up the references. More specifically, if "particular genes", "more sensitive" or "longer period" is mentioned, try to be more specific and mention the names of the genes, a percentage of increased sensitivity, the exact period in time. This improves the impact and understandability of the work. I have mentioned several instances under "general comments".

Furthermore, some microbial key processes and pathways are missing from the review. These points need to be addressed, and I have indicated these instances in the "general comments" section.

The figures provide a good overview of the current knowledge in the field, but they could be used more to their full potential to support the review. I have added the detailed comments regarding the figures at the end of the "General comments" section.

As I want to clearly mention before you read the suggestions, I mention several papers that could be relevant for your work. These are only recommendations and I do in no way want to give the impression that these papers should be cited. The choice to incorporate this work is entirely up to the authors.

General comments

The role of vegetation in hydrological fluctuations is addressed throughout the paper, and I agree that this is very relevant. A separate paragraph focusing on the roles of vegetation would, in my opinion, help to improve the structure of the review and to remove the repetitive parts on the roles of vegetations throughout the work. It would improve the structure and therefore the readability.

-The first paragraph (L52-L59) focuses on the contribution of methane to warming and the contribution of wetlands to methane emissions. I miss a reference to EDGAR (The Emissions Database for Global Atmospheric Research), which is relevant in this context.

-In the introduction the dynamics of wetland processes are extensively discussed. It is useful to shortly link this to atmospheric dynamics, since these are crucial in global methane dynamics. Such atmospheric dynamics (atmospheric sink) are e.g. mentioned in Dean et al. (2018) Methane feedbacks in the global climate system.

-For L85-86: The reference for alternative electron acceptors does not refer to the original work in the field. The original work: https://www.nature.com/articles/35036572 , https://pubmed.ncbi.nlm.nih.gov/16612380/ , https://pubmed.ncbi.nlm.nih.gov/20336137/ and https://www.science.org/doi/10.1126/science.1169984
Overall, for the paragraph (L78-86) I miss a discussion on how relevant this is for wetlands. For the "methane paradox" this is discussed well, but also for methane oxidation with alternative electron acceptors this could be linked to the natural occurrence of these compounds (nitrite, nitrate, oxidized iron, sulfate) in wetland ecosystems. This is needed to put it into the broader context of wetland fluxes under changing

hydrological dynamics, which is the objective of the paper.
It is important for a review article to put the case studies in a wider context.

-The paragraph L116-r132 focuses on the effects of global warming on amongst others coastal estuarine sediments, but important changes in chemical composition (and electron acceptors), vital for methane fluxes, are not yet discussed. Within this context the influx of sulphate as alternative electron acceptor is relevant as inducer of a potential methane sink. See: https://www.nature.com/articles/s41598-018-19505-9 for permafrost environments; and https://www.sciencedirect.com/science/article/pii/S0016703713001853?pes=vor for brackish coastal sediments and the role of iron. Also have a look at these processes, since they can largely affect methane fluxes of wetland ecosystems.

L133-141 presents anthropogenic impacts on global wetlands. An important addition is the agricultural impact on nutrient cycles, mainly the increased input of nitrate(/nitrite) and sulphate in the context of the methane cycle: alternative electron acceptors. This is relevant to mention in this context.

L157-168 focuses on wetland vegetation and mentions the role of *Sphagnum*. However, the interaction between *Sphagnum* and aerobic methanotrophic bacteria, which functions as an important methane filter in these ecosystems, is not yet presented. This is a crucial process, which largely affects methane fluxes from northern wetlands/bogs. See for example https://www.nature.com/articles/ngeo939 and https://bg.copernicus.org/articles/9/47/2012/

L181-188: it is clear that rainfall affects wetlands, but from the paragraph it is not clear what the extent of the impact is. Could you say something on e.g. the area affected? This would help the readers to put the statement into perspective.

L190-200: this needs to be linked to the availability of electron acceptors, as is mentioned later on when describing anaerobic methane oxidation, and as is highlighted in figure 3 and 4. From this paragraph it, however, seems that it is WT steered, whereas the availability of electron acceptors is crucial too. I suggest to put this into perspective too.

L228-236 focuses on pulsed events and spring thaw. It is of added value to link this to organic matter build-up and thaw-induced metabolism, leading to these peak emissions. In this context ebullition is also highly relevant, and needs to be mentioned. See for example: https://pubmed.ncbi.nlm.nih.gov/17513268/

L290-294 mentions the input of oxygen-rich water and the transport of oxygen by plants. It is, however, not clear to me what the impact on methane fluxes can be. Has this been quantified in these studies and could you provide a range here? This would help to put the process into perspective.

L310-312: substrates directly used by methanogens are generally DOM and/or gaseous substrates (organic acids, H2, methylated amines etc), so “more utilized by methanogens” is a bit confusing. Rather: common substrates for methanogens are found in the DOM fraction?

L326-336: this is a very nice paragraph to indicate that chemical reactions and the formation of minerals can play a crucial role.

L338 mentions that a certain period of inundation can lead to complete depletion of TEAs. The reference describes a mesocosm study and is therefore not sufficient as an only reference to provide an environmentally relevant time period. Are there environmental references for this statement? Otherwise, I would clearly mention that environmental studies are lacking.

L363-365 mentions that freshwater rewetting for drained coastal wetlands depletes sulfate and leads to high methane emissions. The statement has to be more nuanced, since the study focuses on a disturbed wetland system, which largely affects peak methane emissions in the initial rewetting state. In addition, it is useful to quantify “high methane emissions” to provide a better context for the reader.

L455-462: I disagree with putting Ca. “Methylomirabilis oxyfera” with the aerobic methanotrophs, since they are dependent on nitrite from the environment. It is true that the organism uses oxygen via an intra-aerobic pathway, but the organism does not directly take up oxygen from the environment. Since you largely focus from an environmental perspective, this should still be seen as a nitrite-driven process.

L466-468 mentions a strong to weak resilience against wet-dry alteration. Could this be better quantified, so the reader gets an idea? The term “resilience” is not yet clear to me within the context.

L482 uses a reference for nitrate and nitrate dependent methane oxidizers, but focuses on ANME/sulfate reducers consortia. I would add more relevant references here (especially referring to the original work by e.g. Hinrichs and Boetius)

Conclusions and perspectives

(ii) it is useful to put the added value compared to “just” measuring the redox state of the sediment. Why is measuring electron pool renewal of added value to this? I agree, but it is useful to mention/discuss this shortly here.

(iii) within the context of the work this should, in my opinion, focus more on the seasonal activity of methane cycling microorganisms, focusing on fluxes. It is not yet clear why the community composition is relevant, since this has not been discussed before. Once again, I fully agree that unravelling the community composition is relevant as an additional tool to get to understand population dynamics, but… on the scale of a wetland methane cycle it might not be the first priority for investigation.

(iv) a minor comment: please mention/repeat the three transport pathways here to aid the reader.

(v) to what extent does the temporal variability need to be mapped? It is useful if the authors can provide a level of detail (Days? Weeks? Months?) to provide a supportive frame for the scientific field.

The figures

I think the figures can be updated/merged to better focus on the objective of the work: how hydrological fluctuations interfere with the microbial methane cycle.

Figure 1 shows the water table fluctuations in different wetlands. It provides useful reference material, but it would be more useful and insightful when this would be integrated with net methane efflux estimates/parameters from the FLUXNET-CH4 database.

Figure 2 shows seasonal water table fluctuations from an example study. This provides an example and I was wondering how relevant/representative this is for global wetlands, although I also understand that such detailed studies are limited. Could you discuss why this specific figure is relevant and representative for the review?

Figure 3 and 4 could be merged and this should have a more central role in the work. I would put the figure as a central piece of the review to illustrate the different microbial processes (now listed in figure 3) in the context of hydrological dynamics (shown in figure 3 and 4). An example of an integrated picture: https://www.sciencedirect.com/science/article/pii/S1369527411000439?via%3Dihub. What would be of added value is to e.g. show two scenarios: high water table and low water table, to illustrate the impact of hydrological fluctuations on the microbial methane cycle. This would better visualize and underline the objective of this work.

Regarding the terminology: The use of TEAs for alternative TEAs is confusing. Oxygen is also a terminal electron acceptor. Rather mention “alternative electron acceptors” instead of using TEAs.

Minor comments

L45: activity of methanogens → sensitivity of methanogens / response of methanogens
L92-93: "and indirectly affect plant traits" is not clear to me. Could you clarify this?
L97: "remains lacking"→ "is lacking"
L116: "the rising"→ "rising"
L220: soil properties → could you specify these soil properties? What is the mechanism of these lags? This is not clear from the text
L265: reference 121 focuses on groundwater. Is there a more relevant study that supports this statement?
L270: suggestion to change "determines" into → largely determines
L287: "wetlands" → "wetland soils"
L300: "aerobic CH4 production" should be "CH4 production in oxic zones/water columns". (aerobic methane production refers to the process associated with (bacterio)chlorophyll metabolism and photosynthesis)
L391: "as the electron acceptor"→ "as the terminal electron acceptor"
L440: "the community structure of methanogens"→ "the community structure of methanogenic populations"
L470 mentions "particular methane monooxygenase genes". Please mention the specific genes here.
L485: please also include the original work here for nitrite driven AOM: https://www.nature.com/articles/nature08883
L497-499: check formulation of the sentence. I suggest "However, oxic/anoxic alterations, which occur in oxygen minimum zones, may create suitable living conditions for anaerobic methanotrophs.
L511-513: check formulation. Methane fluxes are the <u>result of the balance</u> between methanogenic and methanotrophic processes in soils, sediments and water columns.
L538-539: Please also have a look at the work of Aben and co-workers: https://www.nature.com/articles/s41467-017-01535-y

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have spent significant effort in revising their manuscript to address my comments, and those of R2. I particularly appreciate the deletion of lots of text to slim down the draft. I have two minor comments on this revision, as follows below, then believe the paper will be acceptable for publication, and well read and widely cited.

1) In the revised figure one, the legend says "Note: positive values of the water table represent distance below the ground surface and negative values represent surface flooding."

This might not be an issue, but I do wonder; swamps, rice, and mangroves all skew way up above zero into positives. According to your legend this suggests that these ecosystems all suffer quite extreme water table drawdown. But I would assume these are the ecosystems most prone to surface flooding. Are your axes correct here, or is it just a case that negative numbers should be below the zero intercept?

Response: Thank you for the suggestions. After carefully rechecking the data, we have confirmed that the data and the axes are correct. The potential misunderstanding regarding the overall water table trend appears to stem from the aggregation of water table data from different sites within each wetland category. To avoid confusion, we have modified this Fig. 1 as shown below. In the updated Fig. 1, we present the density distribution of water table data for each wetland category. For example, water table data in rice paddies are mainly distributed above the surface, whereas in swamp ecosystems, they are indeed mainly below the surface but concentrated near the surface. Regarding mangrove ecosystems, we have decided not to include them due to the limited representation in the FLUXNET database (only one site: HK-MPM).

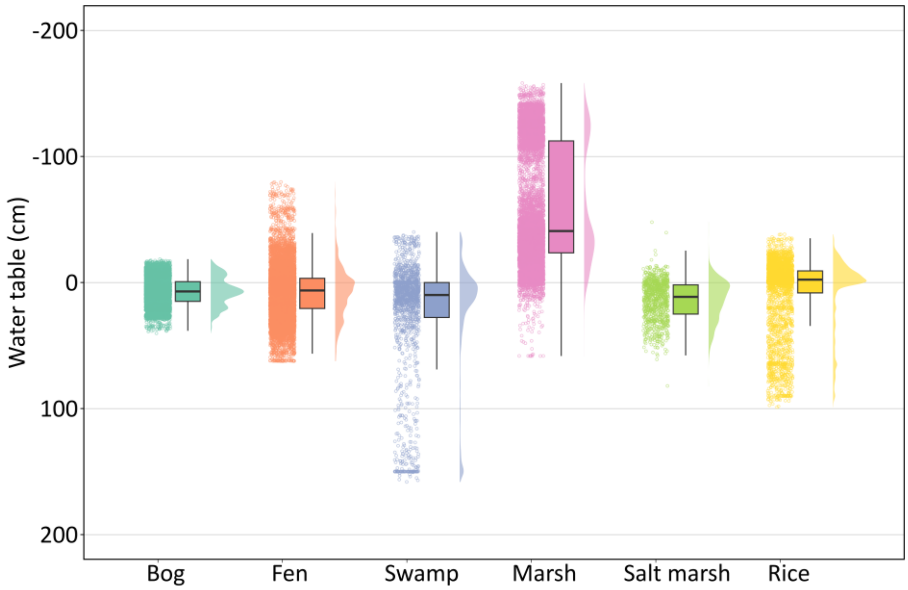

Fig. 1. Water table fluctuations in six wetland types.

2)Original comment: L165. This figure comes unannounced. So far, my understanding was that this was a descriptive review paper, but here we find new figures. I would suggest adding a few sentences in the intro/abstract to highlight that there is data analysis/extraction as part of the paper, and you can refer the reader to the supporting information where the methods are.

Response: Thank you for the suggestions. Combined with the Editor's suggestions, we chose not to perform statistical analyses on the data. The updated Fig. 1 retains only the raw water table data across various wetland types, with the intention of offering readers an intuitive understanding of water table fluctuation phenomenon.

New comment: This is fine, but the FluxNet data still comes totally unannounced. Ideally, mention in your abstract that you synthesise data, because currently you just say that you "review". I would also suggest adding a few words around L181 (in the track changes document) to again mention that you synthesise/collate FluxNet data.

Response: Thank you for the suggestions. We have added the relative description in Lines 39-40 of Abstract and Lines 163-165 of Section 2.2.

Reviewer #2 (Remarks to the Author):

The manuscript has been carefully amended to take into account the comments made by the referees. Overall, from a rapid read-through, the work now looks substantially improved. Section 8 now has a much clearer exposition of plant-mediated transport of methane. The paper is a valuable contribution, reviewing a complex topic, and should now be published.

I only have very minor comments now.

1. It would help the flow of the first few paragraphs if the references were renumbered in sequence. It's just the first 10 or so that have become muddled, after a couple of deletions. Just restore a couple of the many new papers to fill the gaps and then number refs sequentially through to line 74. After that it doesn't matter. Ref 6 (Dean et al) is important and should be restored.

Response: Thank you for the suggestions. In fact, we submitted two versions with one is the tracked-changes version and the other one is the clean version. In the clean version, the references are updated and in the right order. Additionally, we have cited Dean et al. (2018) in Line 55 now.

2. Ref 12 (Schaefer) is good but not now up to date: they thought the 13C shift was primarily driven by agriculture, whereas more recent papers like Basu et al, Lan et al, and Nisbet et al primarily focus on wetlands. Maybe keep ref 12 but replace ref 8 (nisbet general paper) with Nisbet 2023 which makes the wetland point much more strongly. Nisbet, E.G., et al. (2023) Atmospheric methane: Comparison between methane's record in 2006–2022 and during glacial terminations. Global Biogeochemical Cycles 37: e2023GB007875.

Response: Thank you for the suggestion. We have removed the work of Nisbet et al. (2021) (Nisbet's general paper) and cited the work of Nisbet et al. (2023) in Line 57.

3. The paper still has a strongly 'northern' flavour, but now does think harder about the tropics...I'd still encourage the authors to deepen the discussion of tropical wetlands somewhat, though I agree the literature is much thinner than for wetlands in Europe and North America.

Response: Thank you for the suggestion. We have tried to added more discussion on tropical wetlands in Lines 127-130, 234-235, 258-259, and 632-633.

Reviewer #3 (Remarks to the Author):

Dear authors,

The submitted manuscript addresses a very complex topic and provides a nice overview of the complex links between hydrological dynamics and methane emissions from wetlands. It is an actual topic that has been extensively studied over the last decade. Therefore, a critical review of the current knowledge is timely and it makes a valuable contribution to the field. The paper is well written and I really enjoyed reading the work.

Although the topic is relevant, there is some work to be done in order to clarify and better visualize how hydrological fluctuations specifically impact the microbiology, the wetland methane cycle. Let me emphasize that my expertise lies in the microbiology of the methane cycle, and I am not an expert on wetland hydrology. My review therefore focuses on the microbiological aspects of the manuscript. I hope that my feedback is useful for you, and that it can improve the manuscript.

A general point of feedback for the manuscript is that, overall, a bit more detail needs to be added to aid the reader into understanding the story without having to look up the references. More specifically, if "particular genes", "more sensitive" or "longer period" is mentioned, try to be more specific and mention the names of the genes, a percentage of increased sensitivity, the exact period in time. This improves the impact and understandability of the work. I have mentioned several instances under "general comments" in the uploaded document with all my comments.

Response: Thank you for the suggestions. We have supplemented more details to the previously ambiguous parts, especially concerning the microbiology part in Lines 435, 487-488, 489-490, 514-518, 520-521, 552-553, 560, and 565.

Furthermore, some microbial key processes and pathways are missing from the review. These points need to be addressed, and I have indicated these instances in the "general comments" section.

Response: Thank you for the suggestions. We have added more processes in wetlands that can influence methane-related biogeochemical processes, such as organic matter buildup and thaw-induced metabolism in Lines 344-347, changes in alternative electron acceptors (*e.g.,* increased sulfate from sea level rise or saltwater rewetting, increased nitrate/nitrite from agricultural runoff and leaching) in Lines 394-399 and 445-453, and the interaction between *Sphagnum* and aerobic methanotrophic bacteria in Lines 529-534.

The figures provide a good overview of the current knowledge in the field, but they could be used more to their full potential to support the review. I have added the detailed comments regarding the figures at the end of the "General comments" section.

Response: Thank you for the suggestions. We have updated Fig. 1, 2, 3, and 4 based on your and other reviewers' specific comments.

As I want to clearly mention before you read the suggestions, I mention several papers that could be relevant for your work. These are only recommendations and I do in no way want to give the impression that these papers should be cited. The choice to incorporate this work is entirely up to the authors.

You can find my general and detailed comments, as well as the comments listed here in the attachment file "

Response: Thank you for the suggestions. After careful review, we have added or updated references, such as Lines 55, 87-88, 288, 538, and 541.

General comments

The role of vegetation in hydrological fluctuations is addressed throughout the paper, and I agree that this is very relevant. A separate paragraph focusing on the roles of vegetation would, in my opinion, help to improve the structure of the review and to remove the repetitive parts on the roles of vegetations throughout the work. It would improve the structure and therefore the readability.

Response: Thank you for the suggestion. After a thorough discussion with all co-authors, we have decided to keep the original structure. In fact, the structural logic of this work is that we first described the prevalence and patterns of hydrological fluctuations in wetlands (Section 2), and then presented the uncertainty of methane fluxes under these fluctuations (Section 3, phenomenon description only). Subsequently, we explored how hydrological fluctuations affect methane fluxes through different pathways:

- Section 4 examined their impact on soil redox conditions that determine the occurrence of both methanogenesis and methanotrophy;
- Section 5 examined their impact on methane production, focusing only on the abiotic route;

- Section 6 examined their impact on methane oxidation, focusing only on the abiotic route;
- Section 7 examined their impact on methane cycling microorganisms, including Section 7.1-methanogens and Section 7.2-methanotrophs;
- Section 8 examined their impact on methane transport.

This means that our discussion is centered around different stages of methane cycle. Given the complexity of vegetation's role in the methane cycle—via carbon substrate supply, oxygen input, and plant-mediated methane transport—it is challenging to confine its effects to a single section.

-The first paragraph (L52-L59) focuses on the contribution of methane to warming and the contribution of wetlands to methane emissions. I miss a reference to EDGAR (The Emissions Database for Global Atmospheric Research), which is relevant in this context.

Response: Thank you for the suggestion. We have added the reference in Line 55.

-In the introduction the dynamics of wetland processes are extensively discussed. It is useful to shortly link this to atmospheric dynamics, since these are crucial in global methane dynamics. Such atmospheric dynamics (atmospheric sink) are e.g. mentioned in Dean et al. (2018) Methane feedbacks in the global climate system.

Response: Thank you for the suggestion. We have linked wetland process dynamics to atmospheric methane dynamics in Lines 73-74.

-For L85-86: The reference for alternative electron acceptors does not refer to the original work in the field.

The original work: https://www.nature.com/articles/35036572, https://pubmed.ncbi.nlm.nih.gov/16612380/, https://pubmed.ncbi.nlm.nih.gov/20336137/,

and https://www.science.org/doi/10.1126/science.1169984

Response: Thank you for the suggestions. We have added correct references in Lines 87-88.

Overall, for the paragraph (L78-86) I miss a discussion on how relevant this is for wetlands. For the "methane paradox" this is discussed well, but also for methane oxidation with alternative electron acceptors this could be linked to the natural occurrence of these compounds (nitrite, nitrate, oxidized iron, sulfate) in wetland ecosystems. This is needed to put it into the broader context of wetland fluxes under changing hydrological dynamics, which is the objective of the paper.

It is important for a review article to put the case studies in a wider context.

Response: Thank you for the suggestions. We have tried to link them more to wetland ecosystems. The revision can be found in Line 88-90.

-The paragraph L116-r132 focuses on the effects of global warming on amongst others coastal estuarine sediments, but important changes in chemical composition (and electron acceptors), vital for methane fluxes, are not yet discussed. Within this context the influx of sulphate as alternative electron acceptor is relevant as inducer of a potential methane sink. See: https://www.nature.com/articles/s41598-018-19505-9 for permafrost environments; and https://www.sciencedirect.com/science/article/pii/S0016703713001853?pes=vor for brackish coastal sediments and the role of iron. Also have a look at these processes, since they can largely affect methane fluxes of wetland ecosystems.

Response: Thank you for the suggestions. In fact, in Section 2.1-"Prevalence of hydrological fluctuations", we focused on the phenomenon of prevalent hydrological fluctuations in wetlands. The discussion regarding how changes in the availability of electron acceptors induced by hydrological fluctuations impact CH_4-related biogeochemical processes is mainly presented in Section 5.2-"Alternative terminal electron acceptor" and Section 6-"Effects of hydrological fluctuations on methane oxidation". With this, the effects of changes in sulfate availability caused by sea level rise on CH_4 oxidation is added in Lines 445-450 of Section 6.

L133-141 presents anthropogenic impacts on global wetlands. An important addition is the agricultural impact on nutrient cycles, mainly the increased input of nitrate(/nitrite) and sulphate in the context of the methane cycle: alternative electron acceptors. This is relevant to mention in this context.

Response: Thank you for the suggestions. As mentioned in the previous response, we chose to add the relevant content in Lines 447-453 of Section 6.

L157-168 focuses on wetland vegetation and mentions the role of Sphagnum. However, the interaction between Sphagnum and aerobic methanotrophic bacteria, which functions as an important methane filter in these ecosystems, is not yet presented. This is a crucial process, which largely affects methane fluxes from northern wetlands/bogs. See for example https://www.nature.com/articles/ngeo939 and https://bg.copernicus.org/articles/9/47/2012/

Response: Thank you for the suggestions. We have added the relevant content in Lines 529-534 of Section 7.2-Methanotrophs.

L181-188: it is clear that rainfall affects wetlands, but from the paragraph it is not

clear what the extent of the impact is. Could you say something on e.g. the area affected? This would help the readers to put the statement into perspective.

Response: Thank you for the suggestion. We have added the discussion about effects of climate change on wetland area loss in Lines 127-132 of Section 2.1.

References:

1) Xi, Yi, et al. "Future impacts of climate change on inland Ramsar wetlands." Nature Climate Change 11.1 (2021): 45-51.
2) Xu, Donghui, et al. "Climate change will reduce North American inland wetland areas and disrupt their seasonal regimes." Nature Communications 15.1 (2024): 2438.

L190-200: this needs to be linked to the availability of electron acceptors, as is mentioned later on when describing anaerobic methane oxidation, and as is highlighted in figure 3 and 4. From this paragraph it, however, seems that it is WT steered, whereas the availability of electron acceptors is crucial too. I suggest to put this into perspective too.

Response: Thank you for the suggestions. In fact, in Section 3-Uncertainty of methane fluxes under hydrological fluctuations, we only described the phenomenon of methane flux uncertainty caused by hydrological fluctuations. In Sections 4-8, we specifically explained how hydrological fluctuations impact methane cycling through various pathways. In particular, in Section 5.2-Alternative terminal electron acceptor and Section 6-Effects of hydrological fluctuations on methane oxidation, we respectively illustrated how changes in electron acceptor availability caused by hydrological fluctuations affect methane production and anaerobic methane oxidation. Therefore, we recommend maintaining a descriptive approach in Section 3, focusing on the phenomenon of methane uncertainty under hydrological fluctuations.

L228-236 focuses on pulsed events and spring thaw. It is of added value to link this to organic matter buildup and thaw-induced metabolism, leading to these peak emissions. In this context ebullition is also highly relevant, and needs to be mentioned. See for example: https://pubmed.ncbi.nlm.nih.gov/17513268/

Response: Thank you for the suggestions. Regarding organic matter buildup and thaw-induced metabolism due to hydrological fluctuations, we have added the relevant discussion in Lines 344-347 of Section 5.1-Substrate availability. In addition, we have included ebullition and its contribution to methane flux uncertainty in Lines 241 and 245-246 of Section 3-Uncertainty of methane fluxes under hydrological fluctuations.

L290-294 mentions the input of oxygen-rich water and the transport of oxygen by plants. It is, however, not clear to me what the impact on methane fluxes can be. Has this been quantified in these studies and could you provide a range here? This would help to put the process into perspective.

Response: Thank you for the suggestions. After carefully reviewing the current literature, exist environmental studies appear to be insufficient to quantify the effects of these individual dynamic processes on methane fluxes. Our primary aim was to highlight that these processes could alter soil redox conditions, thereby complicating methane flux assessment in wetlands.

L310-312: substrates directly used by methanogens are generally DOM and/or gaseous substrates (organic acids, H2, methylated amines etc), so "more utilized by methanogens" is a bit confusing. Rather: common substrates for methanogens are found in the DOM fraction?

Response: We agree. The revision can be found in Lines 330-332.

L326-336: this is a very nice paragraph to indicate that chemical reactions and the formation of minerals can play a crucial role.

Response: Thanks.

L338 mentions that a certain period of inundation can lead to complete depletion of TEAs. The reference describes a mesocosm study and is therefore not sufficient as an only reference to provide an environmentally relevant time period. Are there environmental references for this statement? Otherwise, I would clearly mention that environmental studies are lacking.

Response: Thank you for the suggestion. We have clarified the lack of environmental studies in Lines 366-367.

L363-365 mentions that freshwater rewetting for drained coastal wetlands depletes sulfate and leads to high methane emissions. The statement has to be more nuanced, since the study focuses on a disturbed wetland system, which largely affects peak methane emissions in the initial rewetting state. In addition, it is useful to quantify "high methane emissions" to provide a better context for the reader.

Response: Thank you for the suggestion. We have changed the original reference to the recent work of Petersen et al. (2023), which compared methane emissions between saltwater and freshwater rewetting. They observed negligible methane emissions from saltwater-flooded soils compared to freshwater-flooded ones emitting around 303 Mg yr^{-1}. One main reason of this difference is the active sulfate reduction facilitated by high sulfate concentrations in saltwater, although differences in water levels and substrate availability cannot be ignored. The revision can be found in Lines 394-399.

<u>References:</u>

1) Petersen, Susan Guldberg G., Erik Kristensen, and Cintia O. Quintana. "Greenhouse gas

emissions from agricultural land before and after permanent flooding with seawater or freshwater." Estuaries and Coasts 46.6 (2023): 1459-1474.

L455-462: I disagree with putting Ca. "Methylomirabilis oxyfera" with the aerobic methanotrophs, since they are dependent on nitrite from the environment. It is true that the organism uses oxygen via an intra-aerobic pathway, but the organism does not directly take up oxygen from the environment. Since you largely focus from an environmental perspective, this should still be seen as a nitrite-driven process.

Response: Thank you for the suggestion. We have removed it from the aerobic methanotrophs.

L466-468 mentions a strong to weak resilience against wet-dry alteration. Could this be better quantified, so the reader gets an idea? The term "resilience" is not yet clear to me within the context.

Response: Thank you for the suggestion. In the context of this section, "resilience" refers to the response of methanotrophic activity, abundance, and composition to hydrological disturbances. In the work of Ho et al. (2016), the abundance of type Ia was least affected by dry-wet cycles compared to other methanotrophs. In contrast, the abundance of type II and type Ib decreased by about two orders of magnitude following frequent dry-wet cycles, but the former gradually recovered over time while the latter kept declining. The revision can be found in Lines 514-518.

References:

1) Ho, Adrian, et al. "Recurrence and frequency of disturbance have cumulative effect on methanotrophic activity, abundance, and community structure." Frontiers in microbiology 6 (2016): 1493.

L482 uses a reference for nitrate and nitrate dependent methane oxidizers, but focuses on ANME/sulfate reducers consortia. I would add more relevant references here (especially referring to the original work by e.g. Hinrichs and Boetius)

Response: Thank you for the suggestion. We have added more relevant references in Line 538.

References:

1) Hinrichs, K-U., and Antje Boetius. "The anaerobic oxidation of methane: new insights in microbial ecology and biogeochemistry." Ocean margin systems (2002): 457-477.
2) Knittel, Katrin, and Antje Boetius. "Anaerobic oxidation of methane: progress with an unknown process." Annual review of microbiology 63 (2009): 311-334.
3) Michaelis, Walter, et al. "Microbial reefs in the Black Sea fueled by anaerobic oxidation of methane." Science 297.5583 (2002): 1013-1015.

4) Orphan, V. J., et al. "Comparative analysis of methane-oxidizing archaea and sulfate-reducing bacteria in anoxic marine sediments." Applied and Environmental Microbiology 67.4 (2001): 1922-1934.
5) Orphan, V. J., et al. "Multiple archaeal groups mediate methane oxidation in anoxic cold seep sediments." Proceedings of the National Academy of Sciences 99.11 (2002): 7663-7668.
6) Schreiber, Lars, et al. "Identification of the dominant sulfate-reducing bacterial partner of anaerobic methanotrophs of the ANME-2 clade." Environmental microbiology 12.8 (2010): 2327-2340.

Conclusions and perspectives

(ii) it is useful to put the added value compared to “just” measuring the redox state of the sediment. Why is measuring electron pool renewal of added value to this? I agree, but it is useful to mention/discuss this shortly here.

Response: Thank you for the suggestion. We have briefly discussed this in Lines 641-645. The characterization of electron acceptor pools, combined with soil redox state characterization, helps reveal which biogeochemical processes are occurring in wetlands. Simple redox potential measurements may fail to capture local heterogeneity, such as aerobic and anaerobic microsites.

(iii) within the context of the work this should, in my opinion, focus more on the seasonal activity of methane cycling microorganisms, focusing on fluxes. It is not yet clear why the community composition is relevant, since this has not been discussed before. Once again, I fully agree that unravelling the community composition is relevant as an additional tool to get to understand population dynamics, but… on the scale of a wetland methane cycle it might not be the first priority for investigation.

Response: We agree that more research is needed to explore the seasonal activity of methane cycling microorganisms to better understand seasonal variation patterns and microbial driving mechanisms of CH_4 fluxes. The revision can be found in Lines 646-649.

(iv) a minor comment: please mention/repeat the three transport pathways here to aid the reader.

Response: Thank you for the suggestion. We have added the three transport pathways in Lines 652-653.

(v) to what extent does the temporal variability need to be mapped? It is useful if the authors can provide a level of detail (Days? Weeks? Months?) to provide a supportive frame for the scientific field.

Response: Thank you for the suggestion. We have added the suggested temporal variability level (such as daily and seasonal) in Lines 663.

The figures I think the figures can be updated/merged to better focus on the objective of the work: how hydrological fluctuations interfere with the microbial methane cycle.

Figure 1 shows the water table fluctuations in different wetlands. It provides useful reference material, but it would be more useful and insightful when this would be integrated with net methane efflux estimates/parameters from the FLUXNET-CH4 database.

Response: Thank you for the suggestion. We have incorporated methane flux data in new Fig. 3. Notably, based on the editorial advice not to perform statistical analyses in the last review, we therefore only present the data in Fig. 3, as well as in Fig. 1 and 2.

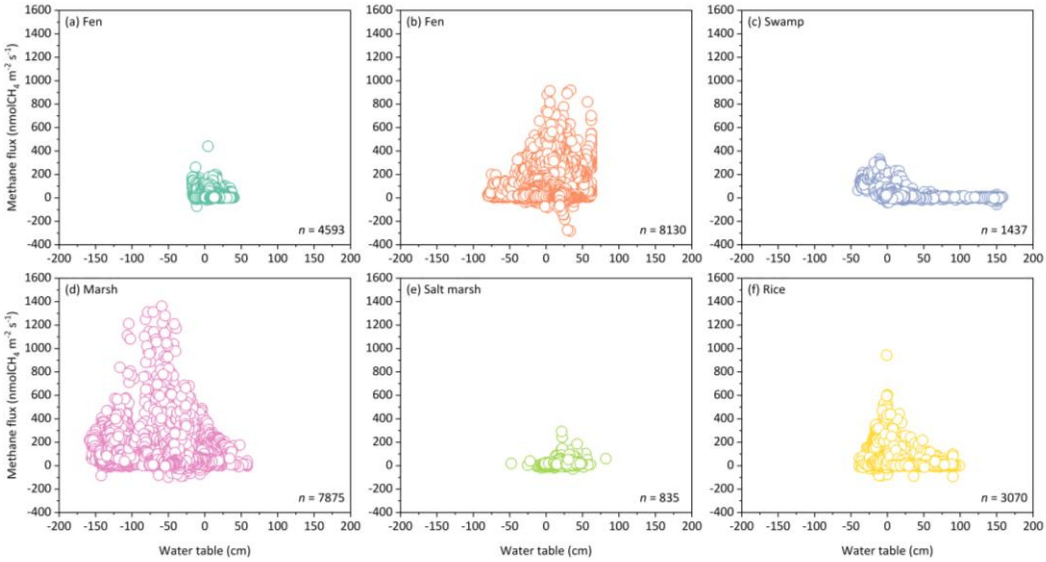

Fig. 3. CH_4 fluxes at different water tables in six wetland types.

Figure 2 shows seasonal water table fluctuations from an example study. This provides an example and I was wondering how relevant/representative this is for global wetlands, although I also understand that such detailed studies are limited. Could you discuss why this specific figure is relevant and representative for the review?

Response: Thank you for the suggestion. As we discussed in Section 3, subjected to typical periodical and atypical dramatic hydrological fluctuations, methane fluxes readily exhibit nonmonotonicity, lagged response, and pulsation. Furthermore, under climate change, rising temperatures may override the influence of water table on methane emissions, especially at high latitudes, by affecting the reaction rates of methane-related biogeochemical processes. We believe Fig. 2 could be a good example showing two typical seasonal hydrological fluctuation patterns. Despite both locations being in the Northern Hemisphere, they exhibit opposite water table trends during the summer period (Fig. 2a&2b). However, the methane emission patterns at these two locations do not necessarily mirror the fluctuations in water tables, but rather align with temperature variations (Fig. 2c&2d). This further complicates the relationship between methane flux and water table, highlighting its strong dependence on the specific wetland environment. We have added relevant content in Lines 253-262.

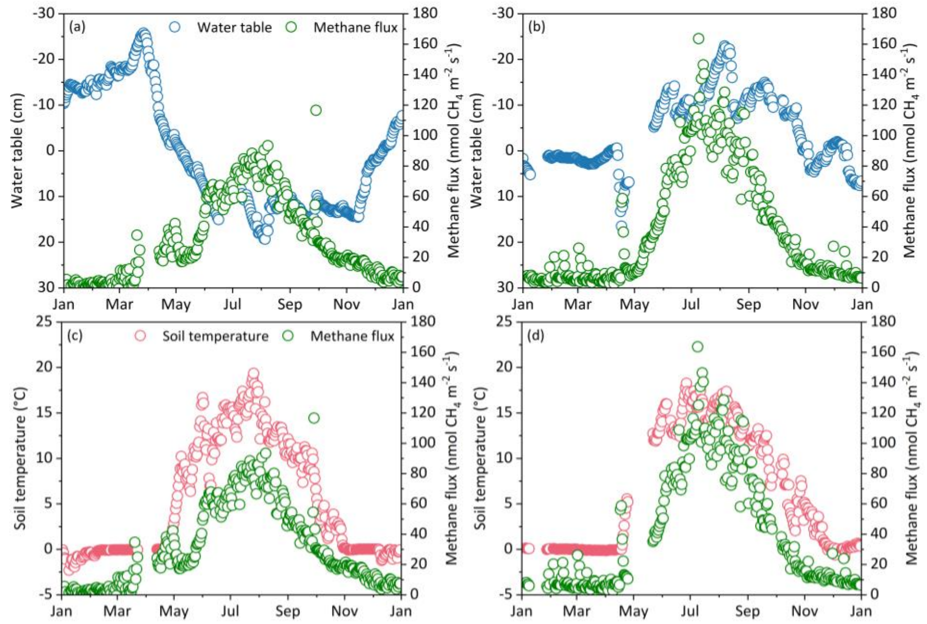

Fig. 2. Seasonal water table fluctuations and corresponding CH_4 emission patterns at the SE-Deg wetland site (Västerbotten, Sweden) in 2016 (a) and at the FI-Sii wetland site (Pirkanmaa, Finland) in 2013 (b), alongside seasonal soil temperature fluctuations and corresponding CH_4 emission patterns at the same sites in panels (c) and (d), respectively.

Figure 3 and 4 could be merged and this should have a more central role in the work. I would put the figure as a central piece of the review to illustrate the different microbial processes (now listed in figure 3) in the context of hydrological dynamics (shown in figure 3 and 4). An example of an integrated picture: https://www.sciencedirect.com/science/article/pii/S1369527411000439?via%3Dihub. What would be of added value is to e.g. show two scenarios: high water table and low water table, to illustrate the impact of hydrological fluctuations on the microbial methane cycle. This would better visualize and underline the objective of this work.

Regarding the terminology: The use of TEAs for alternative TEAs is confusing. Oxygen is also a terminal electron acceptor. Rather mention "alternative electron acceptors" instead of using TEAs.

Response: Thank you for the suggestion. We have merged the original Fig. 3 and Fig. 4 into a new Fig. 4 as shown below. We have also added the description of this new figure in Lines 615-627.

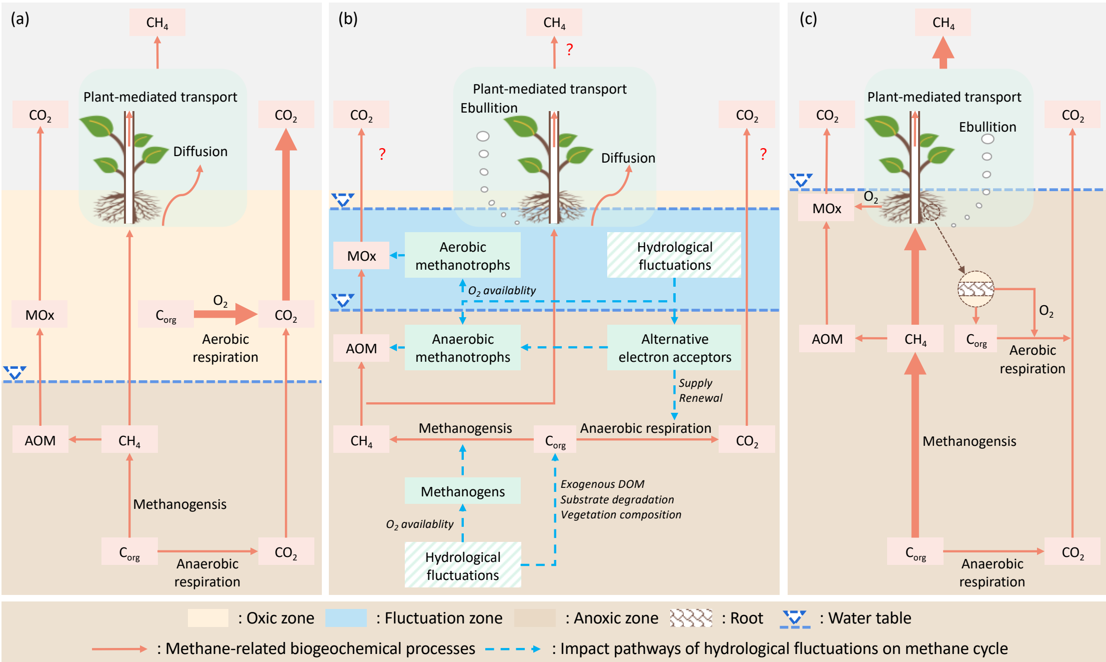

Fig. 4. Conceptualization of the biogeochemical CH_4 cycle in wetlands under different hydrological scenarios, including: (a) prolonged drought, (b) hydrological fluctuations, and (c) prolonged flooding. Aerobic respiration tends to be more active in oxic zones during prolonged drought, while methanogenesis is more prevalent in anoxic zones during prolonged flooding. Under hydrological fluctuations, CH_4-related biogeochemical processes become more complex due to changes in soil redox state, substrate availability, the pool of alternative electron acceptors, contribution of different CH_4 transport routes, and the community structure and activity of methanogens and methanotrophs. Note: rhizosphere oxygen is particularly critical for aerobic processes under prolonged flooding conditions and is therefore highlighted in Fig. 4c.

Minor comments

L45: activity of methanogens → sensitivity of methanogens / response of methanogens

Response: Thank you for the suggestion. We suggest keeping "activity" since community structure and activity is the real thing controlling methane production, however, the resistance and recovery is also one important topic here, we understand your suggestion to change "activity" to "sensitivity", but sensitivity might only cover their behavior in handling environmental stress, but not their activity without those stress (*e.g.,* with high WT).

L92-93: "and indirectly affect plant traits" is not clear to me. Could you clarify this?

Response: Climate warming can indirectly impact methane cycling through its effects on plant traits. This is mainly because plants can transport O_2 (electron acceptors) and provide carbon substrates (electron donors) to the rhizosphere. Generally, plants with high productivity exhibit more developed root systems and underground biomass, leading to increased carbon input into the rhizosphere. Further, plant biomass and root porosity can positively affect O_2 transport. We have briefly clarified this in Lines 96-97.

References:

1) Sutton-Grier, Ariana E., and J. Patrick Megonigal. "Plant species traits regulate methane production in freshwater wetland soils." Soil Biology and Biochemistry 43.2 (2011): 413-420.
2) Noyce, Genevieve L., and J. Patrick Megonigal. "Biogeochemical and plant trait mechanisms drive enhanced methane emissions in response to whole-ecosystem warming." Biogeosciences 18.8 (2021): 2449-2463.

L97: "remains lacking" → "is lacking"

Response: Thank you for the suggestion. We have revised it in Line 101.

L116: "the rising" → "rising"

Response: Thank you for the suggestion. We have removed "the".

L220: soil properties → could you specify these soil properties? What is the mechanism of these lags? This is not clear from the text

Response: Thank you for the suggestion. In fact, the temporal lag can be influenced by multiple soil properties, such as availability of alternative electron acceptors, soil texture, and initial moisture. For example, after water table rise, the replenished electron acceptor pool can compete with methanogens for electron donors, thereby inhibiting methane production. In addition, coarse-textured wetland soils tend to retain some air-filled pores during the initial stages of rewetting. If the soil is already saturated, increasing water table may not immediately enhance methane production. We have briefly clarified this in Lines 231-232.

Reference:

1) Zhu, Xiaoyan, et al. "Effects of water regimes on methane emissions in peatland and gley marsh." Vadose Zone Journal 17.1 (2018): 1-7.

L265: reference 121 focuses on groundwater. Is there a more relevant study that supports this statement?

Response: Thank you for the suggestion. We have updated the reference in Line 288.

Reference:

2) Bridgham, Scott D., et al. "Methane emissions from wetlands: biogeochemical, microbial, and modeling perspectives from local to global scales." Global change biology 19.5 (2013):

1325-1346.

L270: suggestion to change "determines" into → largely determines

Response: Thank you for the suggestion. We have revised it in Line 293.

L287: "wetlands" → "wetland soils"

Response: Thank you for the suggestion. We have revised it in Line 309.

L300: "aerobic CH4 production" should be "CH4 production in oxic zones/water columns". (aerobic methane production refers to the process associated with (bacterio)chlorophyll metabolism and photosynthesis)

Response: Thank you for the suggestion. We have revised it in Lines 322-323.

L391: "as the electron acceptor" → "as the terminal electron acceptor"

Response: Thank you for the suggestion. We have revised it in Line 427.

L440: "the community structure of methanogens" → "the community structure of methanogenic populations"

Response: Thank you for the suggestion. We have revised it in Line 486.

L470 mentions "particular methane monooxygenase genes". Please mention the specific genes here.

Response: Thanks. This was a spelling mistake. We have corrected it as "*pmoA* gene encoding the β-subunit of the particulate methane monooxygenase enzyme (pMMO)" in Lines 520-521.

L485: please also include the original work here for nitrite driven AOM: https://www.nature.com/articles/nature08883

Response: Thank you for the suggestion. We have included this reference in Line 541.

L497-499: check formulation of the sentence. I suggest "However, oxic/anoxic alterations, which occur in oxygen minimum zones, may create suitable living conditions for anaerobic methanotrophs.

Response: Thank you for the suggestion. We have adjusted the formulation of this sentence in Lines 556-558.

L511-513: check formulation. Methane fluxes are the result of the balance between methanogenic and methanotrophic processes in soils, sediments and water columns.

Response: Thank you for the suggestion. We have revised Lines 571-574.

L538-539: Please also have a look at the work of Aben and co-workers: https://www.nature.com/articles/s41467-017-01535-y

Response: Thank you for the suggestion. We have cited the work of Aben and co-workers and briefly introduced the potential impact of global warming on ebullition events in Lines 600-602.