ESSD- 2021-141 Author Responses

General response to the referees

We would like to thank the two anonymous referees for their kind and thorough comments on and suggestions for this manuscript. We appreciate that their constructive feedback has helped to improve the quality and clarity of the paper and its representation of the methane flus datasets we present. Our specific responses to each referee comment are presented below. A supplemental document with copies of updated figures is also attached.

General Comments (RC1):

In this work authors present a new dataset for boreal and arctic methane emissions from terrestrial and aquatic ecosystems. Given the methane-emitting potential of the boreal/arctic ecosystems, and their susceptibility to climate change, any work to improve our understanding and estimations of these methane fluxes is important. The paper is well written with excellent figures (albeit with some confusing color choices, as pointed out in the specific comments). My comments are relatively minor, the authors have done a great job here.

We would like to thank Referee #1 for taking the time to provide thoughtful and constructive comments on this manuscript. The suggestions help the text and figures read with more ease and we appreciated the feedback. Our responses to the specific comments are detailed below.

Specific Comments:

Line 47-49: You give the range of 211-402 Tg CH4 yr-1 here, and say that the large range is caused by differences in the top down and bottom up approaches to estimation. Is the 211 value the top-down estimate for the Arctic, and the 402 number the bottom-up estimate? It may be helpful to clarify where the 211-402 range comes from.

We have changed the text slightly for clarity. Previously, this range represented the low bounds of top down estimates and the high bounds of bottom up estimates. We now include the average bottom up and top down estimates for methane emissions from all global natural sources-

"Combined, CH4 emissions from northern ecosystems make up a significant but uncertain portion of fluxes from natural sources (232 to 367 Tg CH4 Yr-1 for the average bottom-up and top-down global estimates, respectively; Saunois et al. 2020)."

Line 62: I feel like the snippet "including CH_4 uptake" could use some explaining. Since this sentence is already long, consider adding a second short sentence to explain that some sites are net methane sinks.

We agree with the referee here and have included a separate sentence on methane uptake for clarity.

"…leading to a wide range of reported CH4 fluxes at the site level with differences of up to four orders of magnitude (Olefeldt et al. 2013; Wik et al. 2016a; Treat et al. 2018). Furthermore, drier terrestrial sites may drawdown, or uptake, CH4 out of the atmosphere (Treat et al. 2018)."

Line 72-73: The phrasing "colder soils in drier soils" is confusing to me, is this a typo?

We have fixed this typo so the sentence now reads "with typically colder conditions in drier soils and permafrost-dominated wetlands…"

Line 91-92: Do you mean here that deeper water columns limit ebullition release to the atmosphere because of bubble dissolution? Or that deeper water columns typically have colder sediments which leads to less ebullition? Please clarify.

We have added the following sentences to provide more information on the relationship between deeper water columns and ebullitions rates-

"Glacial and post-glacial waterbodies, on the other hand, have relatively low CH4 fluxes due to deeper water columns, which limit ebullition by creating cooler sediment temperatures and greater hydrostratic pressures for bubbles to overcome (Bastviken et al. 2004; DelSontro et al. 2016)."

Line 119: It would be helpful here to re-iterate the need for this new database. You talk about this in the first paragraph of the intro, so a quick tie-in to the importance of Arctic methane emissions and their large unknowns would emphasize how important your work is.

We changed the text to:

"The Boreal-Arctic region represents a potentially globally significant, but still highly unknown source of CH4. This dataset can be used to help constrain Boreal-Arctic flux estimates, compare field results, identify new research opportunities, or build and test models."

Line 142: How many cold season measurements do you have?

No cold season wetland methane flux measurements were collected in this database. The most recent synthesis of winter measurements is reported by Treat et al. 2018 in Global Change Biology (https://doi.org/10.1016/S0038-0717(02)00022-6).

Figure 3: I really like this figure, except it is not clear to me which columns go with which pH label. You have 4 labels but 3 columns of squares, so I could not figure out where the delineations were.

Here we wanted to highlight that acidic classes are on the very left, slightly acidic is on the transition from first to second box, neutral is transition from second to third box and alkaline is on the very right hand side. For clarity, we updated and simplified the figure to have acidic under the left most box, slightly acidic/neutral under the middle box, and Alkaline under the right most box.

Line 231-232: This is a little confusing because first you say they have the driest soils, and then say their soils are moist to wet.

We changed the sentence to the following to help clarify that permafrost bogs are the driest of the wetland classes, but still have relatively moist conditions compared to dry tundra and upland forests etc.

"Excess ground-ice and ice expansion often elevate Permafrost Bogs up to a few meters above their surroundings, and as such, they are ombrotrophic and relatively well drained (Fig. 3). Permafrost Bogs have moist to wet soil conditions, often with a water table that follows the base of the seasonally developing a thawed soil layer."

Line 251: Are rocklands a relatively small area compared with dry tundra? If so, it is logical to lump them together and assume their fluxes are similar. However, if rocklands are relatively large compared with dry tundra, you should note that lumping these two together could introduce significant uncertainty since rockland fluxes are unknown.

We do not include Rockland area in our Dry Tundra flux estimates/simply scaling exercise. We also updated the text to clarify that there are no explicitly Rockland flux measurements in the dataset. There are five sites that are described as polar desert and also as high tundra, thus we included those sites with the Dry Tundra sites.

"No sites included in the database were described as Rocklands. There are five sites described as high polar desert or desert tundra, which were included as Dry Tundra sites."

Section 265: You do not mention what maps you use to determine whether lakes exist in glacial, yedoma, or peatland environments. Is this information reported in the literature for every site with methane measurements? If not, how do you determine this information for each lake?

Lake type determinations are based on information reported by the authors or in papers cited by the authors related to the study. In a handful of cases when the authors do not describe the lakes/lake sediments, we used the lake locations and yedoma/histel/histisol maps to determine the lake type. In a handful of cases, we could not determine the lake class type and left this field empty as an unknown. We have added this information to the text accordingly.

Line 386: Please add a period to end of sentence.

Added

Line 419-420: Does adding these constants affect your results?

We added the constant as it allows for uptake fluxes to be included in the models. Model residuals did not change much (<5%) when comparing models with and without this constant. This is also a standard practice for methane models, as seen in Turetsky et al. 2014 (https://doi.org/10.1111/gcb.12580), Treat et al. 2018 (https://doi.org/10.1016/S0038-0717(02)00022-6), Olefeldt et al. 2013 (https://doi.org/10.1111/gcb.13612), for example.

Line 434: I assume you mean "CH_4 flux" here? If so, please correct.

We added flux after CH4 on this line.

Line 455: It's a stretch to say these are evenly distributed. Sites are heavily concentrated in Alaska and the Quebec/Ontario border area, although you do have sites sprinkled in other places. I suggest changing the wording here so you do not use the word "evenly".

We have changed the sentence to the following: "Aquatic sites were distributed throughout the Boreal-Arctic region with a greater density of sites in Alaska and eastern Canada (Fig. 1b)."

Line 488: I suggest getting rid of commas to make it easier to read: "No other continuous variables were correlated with CH4 uptake; however, sites where shrubs were present had significantly higher…"

We made the suggested changes for this sentence.

Figure 10: Please increase the contrast between the circle colors by making the ebullition circles darker (as in the color scheme for Figure 11).

We changed the color of the ebullition circles to a darker shade of blue for better contrast.

Table 4: I would write out "Lakes – Diffusion" and "Lakes – Ebullition" in the top two rows, for clarity (assuming that is what D and E stand for?).

We replaced D and E with Diffusion and Ebullition, respectively.

Line 634-635: Sometimes you use "water body" and sometimes "waterbody". Please choose one and make it consistent throughout the paper.

We have changed all of the terms to "waterbody" or "waterbodies" for consistency.

Line 642-644: This sentence is confusing to me. Shouldn't having a broad range of depths make you MORE likely to see a depth effect than if you had a narrow range of depths? What makes your synthesis different from others such that you do not see these relationships? I also do not understand the second half of this sentence "…because it is likely that the temperature and depth influence is clearer over time and space, respectively, in each specific system." Do you mean that you do not have measurements across a range of temperatures (and spatial locations) within each given system? I assume this is the case, but then how did previous synthesis studies see these relationships?

We the changed the text of this section to the following to help clarify:

“The best model for ebullition contained waterbody surface area as a predictor and explained 21% of the variation in the fluxes. Previous synthesis efforts have linked ebullition fluxes to both temperature (Aben et al. 2017) and waterbody depth (Wik et al. 2016a). There are a few potential explanations as to why we did not find similar relationships between ebullition and temperature or waterbody depth. First, Aben et al. include global data that encompass sites across broad temperature ranges from the north to the tropics (2017). It is possible that the range of temperatures represented by our dataset is not wide enough to capture this relationship. It is also possible that the summary data collected, including average temperature and average flux over the ice-free season, are too coarse to show a relationship. It is likely that temperature and also depth influence is clearer over time and space in each respective waterbody and that a higher resolution of data would show these relationships. Regarding waterbody depth, it is also possible that in the absence of detailed surveys, estimated mean and max depths may be less reliable. It is also possible the effects of depth are confounded with surface area as the two metrics are highly correlated (SI Fig. 5). While this dataset represents one of the largest collections of ebullitive emissions from northern lakes so far, this emission pathway is still largely underrepresented and waterbody depth and temperature are not always reported with the flux estimates. Furthermore, we collected information on surface water temperature for this dataset because it was the most widely available temperature metric. Sediment temperature is a better metric to collect in hand with ebullition due to production and transport directly from the sediments (Wik et al. 2013; Aben et al. 2017). Future studies should work to report sediment temperature and water column temperature alongside their flux measurements.”

Figure 12: Having the bar graphs be a single shade of green is mildly confusing since shades of green also represent Relative Land Cover. I would suggest either making your bar graph colors match the legend based on their relative land cover or making them all a non-green color.

We changed the colors of the bar graph to grey to reduce confusion with the relative land cover color.

Line 680 – Do you mean Midsize Peatland lakes here instead of Midsize Glacial lakes? According to Figure 13, Midsize Glacial lakes are well represented compared to their relative flux, whereas midsize peatland lakes are not.

Here we meant to say that Midsize Glacial Lake and Large Lake fluxes are not well represented across the Canadian Shield, despite their abundance. We have edited to text to reflect this.

Figure 14: As with Figure 12, it is confusing here to have shades of blue mean two different things: either ebullition/diffusion, or relative land cover. I would suggest using a different color scheme for one of these.

We changed the colors of the bar graph to grey and black for diffusion and ebullition, respectively, to reduce confusion with the relative land cover color.

Line 696-670: It is my understanding from the Wik et al. 2016 study that not only do you need 11/39 days for diffusive/ebullitive measurements (respectively), you also need these measurements in 3/11 depth-stratified locations (respectively). Please clarify this in your paper.

We added the following sentence to include the depth-stratified spatial sampling suggestions from Wik et al.

"Further, Wik et al. recommend that in addition to the number of sampling days, flux measurements should be distributed spatially across the waterbody using a depth-stratified approach included ~3 and ~11 locations for diffusion and ebullition, respectively (2016b). While we did not collect data on the number of sampling locations across each waterbody, it is likely that many of the average fluxes included the dataset also represent spatially under-sampled measurements."

Line 715: I do not understand what you mean by "split CH4-emitting ecosystem characteristics" here.

We have changed the sentence to "split **by** CH_4-emitting ecosystem characteristics"

Line 718: Is it fair to say that MAAT has important implications for future scaling efforts if MAAT only accounts for 3% of the variability (per Line 615)? How important could MAAT be if it accounts for such a small (even if statistically significant) amount of the variability?

While 3% looks like a relatively small amount of variation, and the majority of the variability is due to classes, the differences between classes can be large (from almost 0 to the ~150 mg CH4 m-1 d-1). So then even a minor influence of temperature suggest that individual classes might be relatively sensitive to temperature. For example, increasing MAAT from 10C to 15C for a small peatland lake increases the modeled flux from 35 mg CH4 m-1 d-1 to 50 mg CH4 m-1 d-1.

General Comments (RC2):

This paper describes data that combines terrestrial and aquatic CH_4 flux measurements and associated supporting information for northern boreal-arctic biomes. As the authors clearly state, methane-producing habitats are particularly abundant in this part of the world and current estimates of boreal and Arctic emissions are highly uncertain. This uncertainty is not unique to latitudes above 50â• °, but (not mentioned by the authors) it is arguably particularly important in this case given predictions and early evidence of increasing rates of emissions associated with rapid climate change.

A key 2-part feature of this paper is (a) its construction in tandem with a land cover data set and (b) the argument supported by preliminary analyses that land cover classes can be identified/assigned based on their CH_4 emission behaviors. The case for this argument is compelling for the boreal and Arctic region. But even if a researcher is skeptical about this argument, this dataset includes both flux data and the methane-relevant land cover data, thus providing the raw material for hypothesis testing as well as for cross-system comparisons and upscaling studies. The combination of terrestrial and aquatic fluxes and support data in the BAWLD-CH_4 dataset is a significant expansion of prior and separate terrestrial and aquatic data compilations, and when paired with the land cover data (BAWLD spatial data) it creates a 'one-stop shopping' resource for researchers studying high latitude GHG dynamics. I appreciate the clear delineation of what data are/are not included and potential limitations of the dataset. The limits of the current state of the data serve as useful raw material for ending the discussion with a section on directions for future research. There is much to like about this paper; it provides the best-available dataset for those interested in high latitude methane dynamics, which is a topic of substantial and rapidly increasing scientific interest, the paper is very well written, and it is thorough in the presentation and explanation of dataset assembly.

We would like to thank Referee #2 for taking the time to provide thoughtful and constructive comments on this manuscript. The suggested changes and comments have helped to strengthen the manuscript. Our responses to the specific comments are detailed below.

Most of my specific comments emphasize aquatic data issues due to my familiarity with these ecosystems. However, I have two general comments. First, following a preliminary reading of this manuscript, I mistakenly thought that this paper was discussing two complementary data sets: BAWLD and BAWLD-CH_4. I made this assumption because it makes sense that these datasets would be presented together given their construction, and also because a fair amount of text in this paper is dedicated to describing aspects of BAWLD (the land cover classes). Presumably the land cover categories are described in the Arctic Data Center (ADC) data publication, so could this part of the paper be streamlined to put the emphasis back on the CH_4 flux data?

We feel it is necessary to include the descriptions of the classes in this manuscript. While the descriptions section is lengthy, the definitions are important to explain why methane emissions are different and how we defined each class for the purpose of their mapping.

Second, given that a strength of this dataset is being able to link it to the BAWLD spatial dataset, it was surprising and disappointing to see several rows with missing or highly aggregated information on site location, including data records from papers written by co-authors on this effort. Often, many sites are lumped together and assigned the same very coarse-level lat/lon (e.g., 64, -148) despite including different types of ecosystems. This is disappointing, as it restricts future analyses that could have been done if and when new and better spatial data become available. I understand that nothing can be done about some of these records, but this is not the case for all records with vague/absent data.

We understand and sympathize with the frustrations presented here about missing data and aggregated data. For consistency, we only include data that can be accessed in the published papers and accompanying supplemental datasets. We believe this allows the reader to easily go back to the original papers and connect with the data. Furthermore, due to the large volume of data already present in this dataset, the time constraints on reaching out to every author for more detailed datasets was beyond the scope of this project.

Aquatic storage flux (lines 392-395)- Can you provide any more detail about determining storage fluxes? I think of this flux as being estimated by quantifying the mass of CH_4 under ice and then assuming that this mass (or some fraction of this mass) is lost to the atmosphere following ice out—because of mixing. Thus, I am not clear on how storage flux is being differentiated from spring mixing flux.

The storage flux represents emissions upon ice-out, including spring turnover. We have corrected the text-

"Storage/ice-out flux includes the annual release of CH_4 that accumulates within and under the ice over the winter and is released upon ice-melt and during spring turnover. Methodologically, thisalso includes estimates from ice bubble surveys (IBS). Our storage flux estimate does not include estimates of fall circulation fluxes, wherein CH_4 that is stored in the deep portion of the water column is released upon seasonal turnover of the water column (Karlsson et al. 2013; Sepulveda-Jauregui et al. 2015).

Section 3.3 figure citations- do you mean Fig. 10 (not fig 2.10) in this section? And re: fig. 10, while the 2 different blues are easily distinguishable in Fig. 9, they are less so in this figure."

We fixed the citations in this section to all be Fig. 10. We also changed this figure to make the colors contrast better with a darker blue color for ebullition.

Lines 248-249- I am also unaware of flux measurements taken from glacier surfaces, but there are now a handful of papers quantifying fluxes at glacial outflows or termini (of course including the Anthony et al. 2012 paper). It may be helpful to add in a sentence or two as to how these habitats were handled or why they were excluded given the increasing interest in glacial retreat and GHGs.

We found the following references related to this topic and added this line about glacial outflow and termini fluxes:

"Glaciers are assumed to have neutral CH_4 fluxes, however, to our knowledge there are no published studies with field data from the glacier surface. There are a handful of studies that highlight lateral CH_4 export and emission from glacial outflows and termini (Christiansen & Jørgensen, 2018; Burns et al. 2018; Lamarche-Gagnon et al. 2019), however due to both limited atmospheric flux measurements and information on the spatial distributions of termini features and difficulties in mapping their areas at the circumpolar scale, we did not included these fluxes. Fluxes from glacial outflows and streams are considered as riverine fluxes and our flux synthesis does not include riverine fluxes."

Line 376- There are several k models, beyond Cole and Caraco; is this the dominant model used in data sources? (why was it singled out?)

The Cole and Caraco model was the dominant model used, which is why we have singled it out here, but we changed the line to indicate that Cole and Caraco is just an example of one such model.

"Gas transfer velocity estimates are commonly calculated using equations (e.g. Cole and Caraco, 1998)."

Line 402- E.LOCATION refers to the location for ebullition only, and this should be specified here. But more importantly, it seems odd to have this category (1) only for ebullition and not also diffusion and (2) given that there is only one row corresponding to a single observation that reports an ebullition flux measured from the lake edge. In short, this field does not really provide much useful information, so is it worth including?

We added this column to help distinguish between ebullitive fluxes taken from just the edge or averaged across the entire lake due to strong evidence that ebullitive fluxes tend to be highest from the edge (Del Sontro et al. 2016, https://doi.org/10.1002/lol2.1007). We wanted to include these data for future scaling purposes to help partition between which estimates are whole lake estimates and which estimates are only from the edge. Most of the measurements ended up being whole lake measurements, but we still feel having this column is useful for the handful of edge only fluxes.

Line 410/Table 2- Very minor! “Dataset” is used in the Table, “Database” is in the csv

We have reached out to the Arctic Data Centre to fix the column titles to be “dataset”

Line 500/Fig 8- There may be a better way to report that shared letters denote no significant differences among categories within each comparison set in this figure.

We have clarified the language in this caption and the other bar graphs to help highlight that bar graphs with the same letter do not significantly differ.

Ex.- “…statistical differences among the categories are indicated by the letters (Sig), wherein bars with the same letters are not significantly different.”

Line 616-617- small suggestion: add in ‘spatial’ before differences to remind readers of this focus.

We agree and have added the word spatial to this sentence.

Line 646- depth may also not be a significant predictor because of unreliability of depths that were reported. In the absence of thorough surveys, depths are often estimated or measured at the center of the lake or where fluxes were measured, and thus may not be a reliable measure of lake mean or maximum depth.

We changed this section of text also based on referee 1’s suggestion. The text now reads:

“The best model for ebullition contained waterbody surface area as a predictor and explained 21% of the variation in the fluxes. Previous synthesis efforts have linked ebullition fluxes to both temperature (Aben et al. 2017) and waterbody depth (Wik et al. 2016a). There are a few potential explanations as to why we did not find similar relationships between ebullition and temperature or waterbody depth. First, Aben et al. include global data that encompass sites across broad temperature ranges from the north to the tropics (2017). It is possible that the range of temperatures represented by our dataset is not wide enough to capture this relationship. It is also possible that the summary data collected, including average temperature and average flux over the ice-free season, are too coarse to show a relationship. It is likely that temperature and also depth influence is clearer over time and space in each respective waterbody and that a higher resolution of data would show these relationships. Regarding waterbody depth, it is also possible that in the absence of detailed surveys, estimated mean and max depths may be less reliable. It is also possible the effects of depth are confounded with surface area as the two metrics are highly correlated (SI Fig. 5). While this dataset represents one of the largest collections of ebullitive emissions from northern lakes so far, this emission pathway is still largely underrepresented and waterbody depth and temperature are not always reported with the flux estimates. Furthermore, we collected information on surface water temperature for this dataset because it was the most widely available temperature metric. Sediment temperature is a better metric to collect in hand with ebullition due to production and transport directly from the sediments (Aben et al. 2017; Wik et al. 2013). Future studies should work to report sediment temperature and water column temperature alongside their flux measurements."

BAWLD_CH4_Aquatic.csv and BAWLD_CH4_Terrestrial.csv contain data drawn from papers that are not cited in the body of the manuscript, and thus are not cited in the reference section of the manuscript. I was also. Unable to find a list of citations for data sources for these files on the ADC dataset web page. It seems appropriate to include this information somewhere- perhaps as a supplement (and apologies if this information was overlooked).

Thank you for pointing this out. We reached out to the topical editor from ESSD for this paper and they recommended adding a list of all of the papers in the dataset and their DOIs to a table in the appendix at the end of the paper. We have followed this recommendation accordingly and also added a column with DOI's to the actual dataset csv files.

Very minor grammatical details-

Line 74- 'like' means similar to, whereas I think you are saying that marshes and fens are actual examples of graminoid-dominated wetlands (long way of saying- use 'such as' instead of 'like')

We changed like to "such as"

Lines 77-80- this is a run-on sentence. Can it be divided into two at the point of the second 'which'?

We split the long sentence into the following two sentences:

"Methane fluxes are typically highest from graminoid-dominant wetlands such as marshes and fens which are frequently inundated. Inundation, in turn, enhances primary productivity (Ström et al. 2012), creates a soil habitat conducive to CH4-producing microbes (Woodcroft et al. 2018), and facilitates transport CH4 through aerenchymatous roots and stems (Chanton et al. 1993; Ström and Christensen, 2007)."

ESSD- 2021-141 Supplemental Author Responses – updated figures

Figure related comments (RC1):

Figure 3: I really like this figure, except it is not clear to me which columns go with which pH label. You have 4 labels but 3 columns of squares, so I could not figure out where the delineations were.

Here we wanted to highlight that acidic classes are on the very left, slightly acidic is on the transition from first to second box, neutral is transition from second to third box and alkaline is on the very right hand side. For clarity, we updated and simplified the figure to have acidic under the left most box, slightly acidic/neutral under the middle box, and Alkaline under the right most box

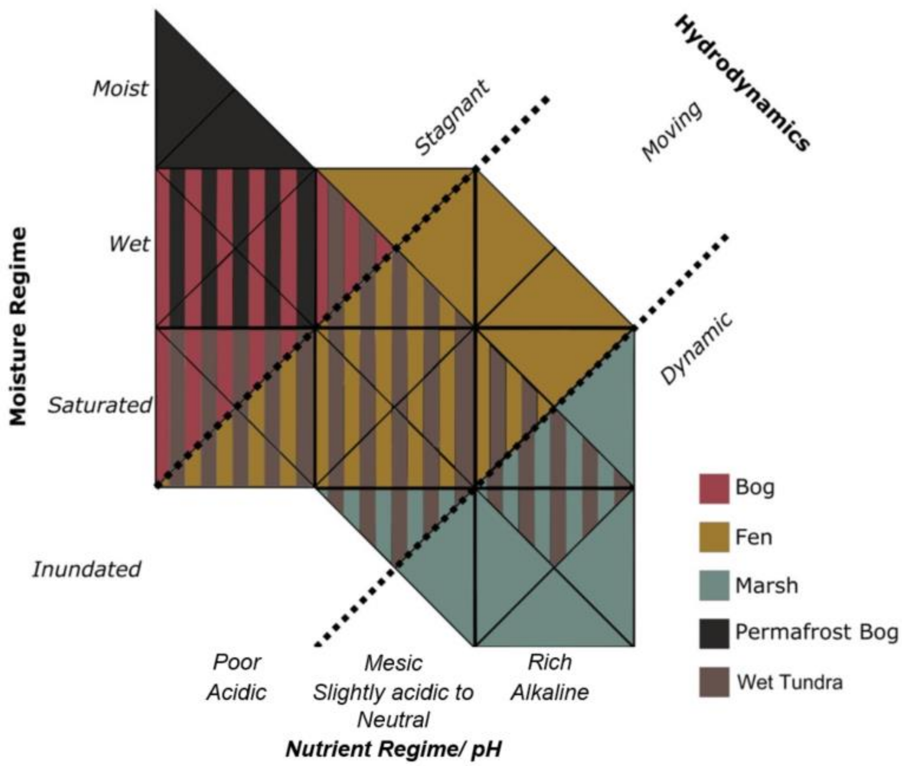

Figure 1. Definitions of the five wetland classes in BAWLD along axes of moisture regime and nutrient regime.

Figure 10: Please increase the contrast between the circle colors by making the ebullition circles darker (as in the color scheme for Figure 11).

We changed the color of the ebullition circles to a darker shade of blue for better contrast

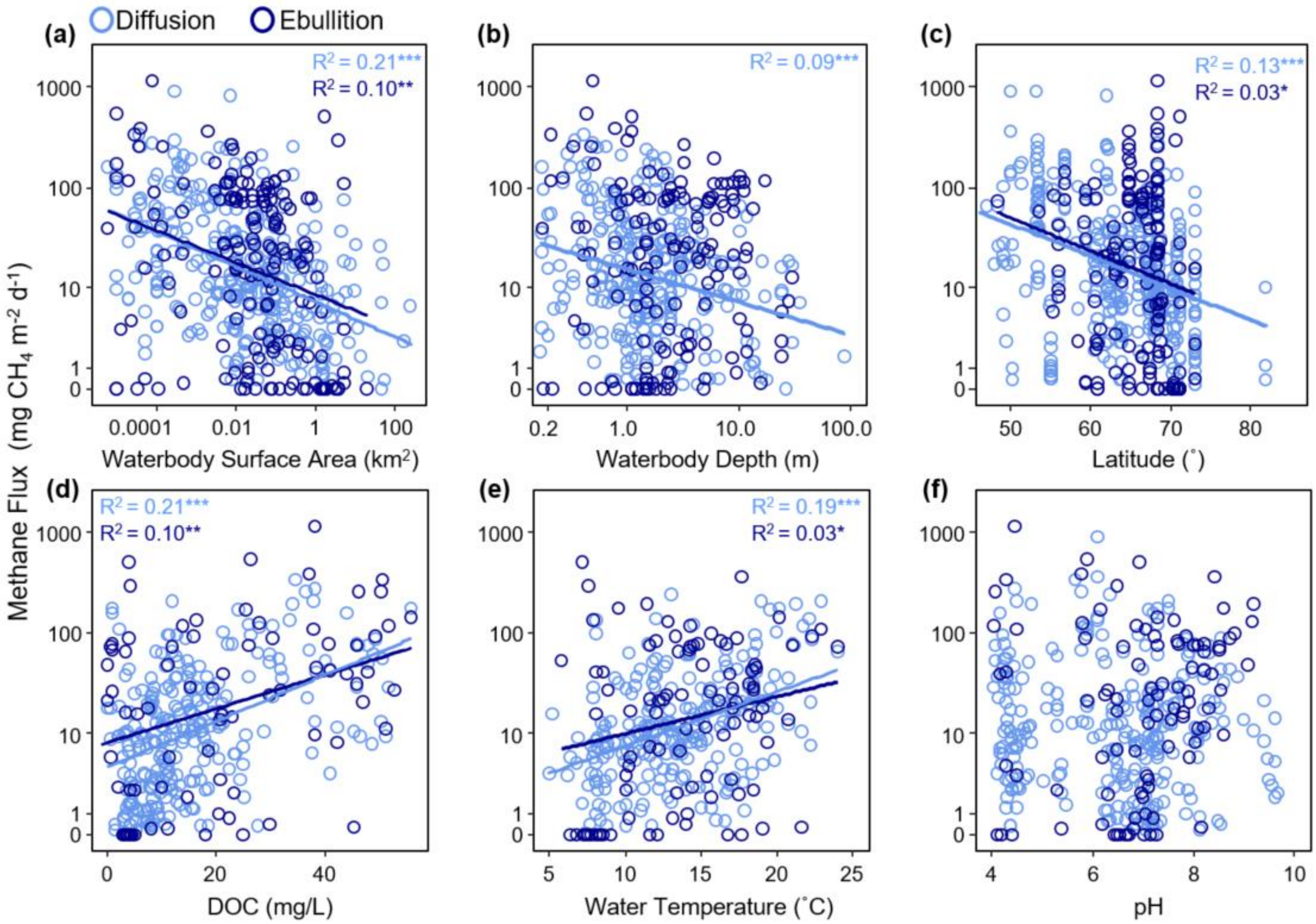

Figure 2. Relationships between site-averaged ice-free diffusive and ebullitive CH_4 fluxes (note the log scale) and environmental variables. Environmental variables include surface area, waterbody depth, latitude, dissolved organic carbon (DOC) concentration, water temperature, and pH. Regression lines and R-square values are shown for significant relationships. Log diffusive CH_4 flux was linearly related to surface area, depth, latitude, water temperature, and DOC. Log ebullitive fluxes were linearly related to surface area, latitude, DOC, and water temperature. * P <0.05. ** P < 0.01. *** P < 0.001.

Figure 12: Having the bar graphs be a single shade of green is mildly confusing since shades of green also represent Relative Land Cover. I would suggest either making your bar graph colors match the legend based on their relative land cover or making them all a non-green color.

We changed the colors of the bar graph to grey to reduce confusion with the relative land cover color.

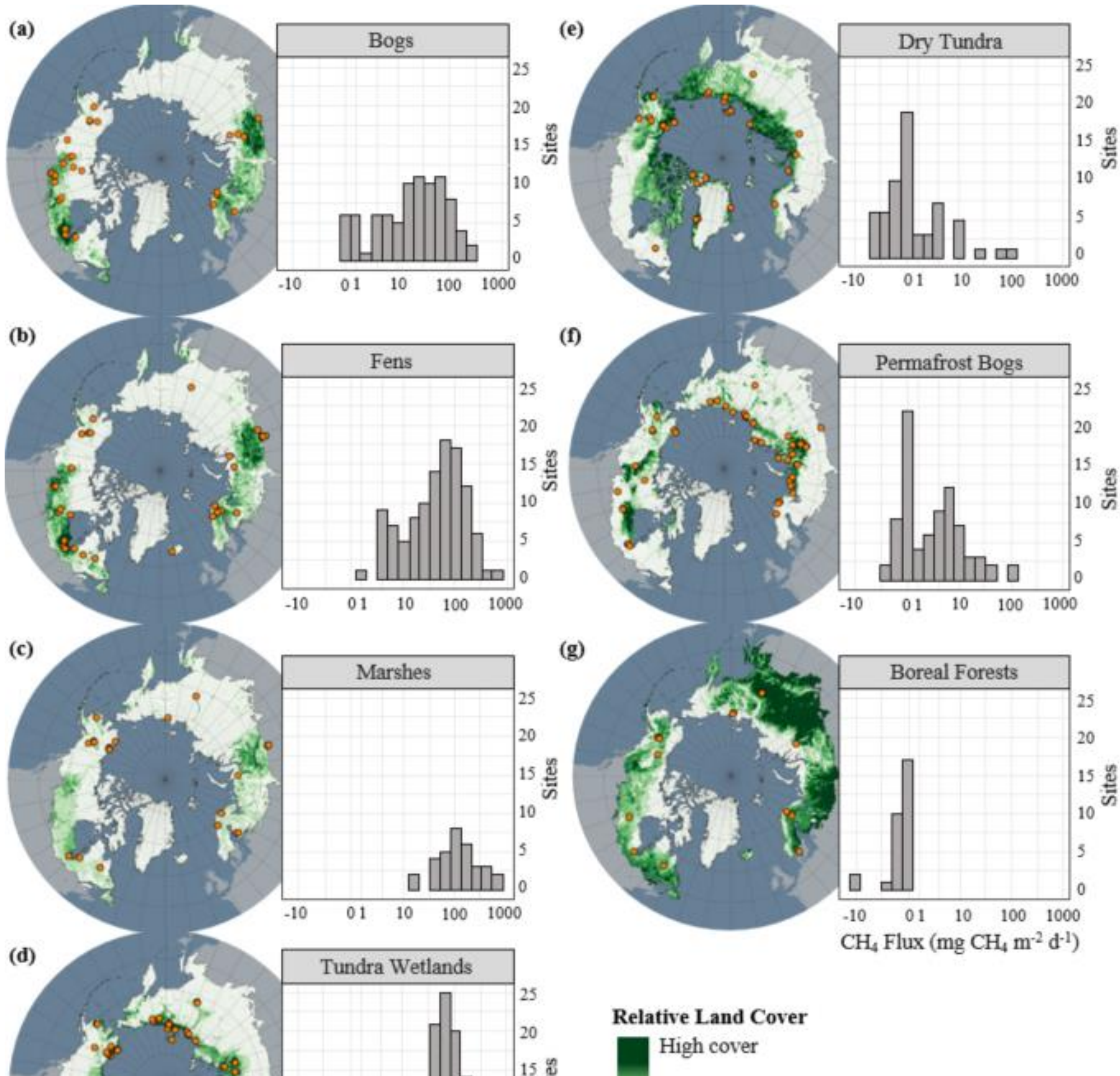

Figure 3. Geographical distribution and flux frequencies and for each terrestrial class. Relative land cover for each type is represented in green on the map. Site locations are represented by orange circles. Note the log scale for CH_4 flux. Land cover distributions from Olefeldt et al. 2021. Histograms of non-transformed flux data can be found in the SI Fig. 3.

Figure related comments (RC2):

Figure 14: As with Figure 12, it is confusing here to have shades of blue mean two different things: either ebullition/diffusion, or relative land cover. I would suggest using a different color scheme for one of these.

We changed the colors of the bar graph to grey and black for diffusion and ebullition, respectively, to reduce confusion with the relative land cover color.

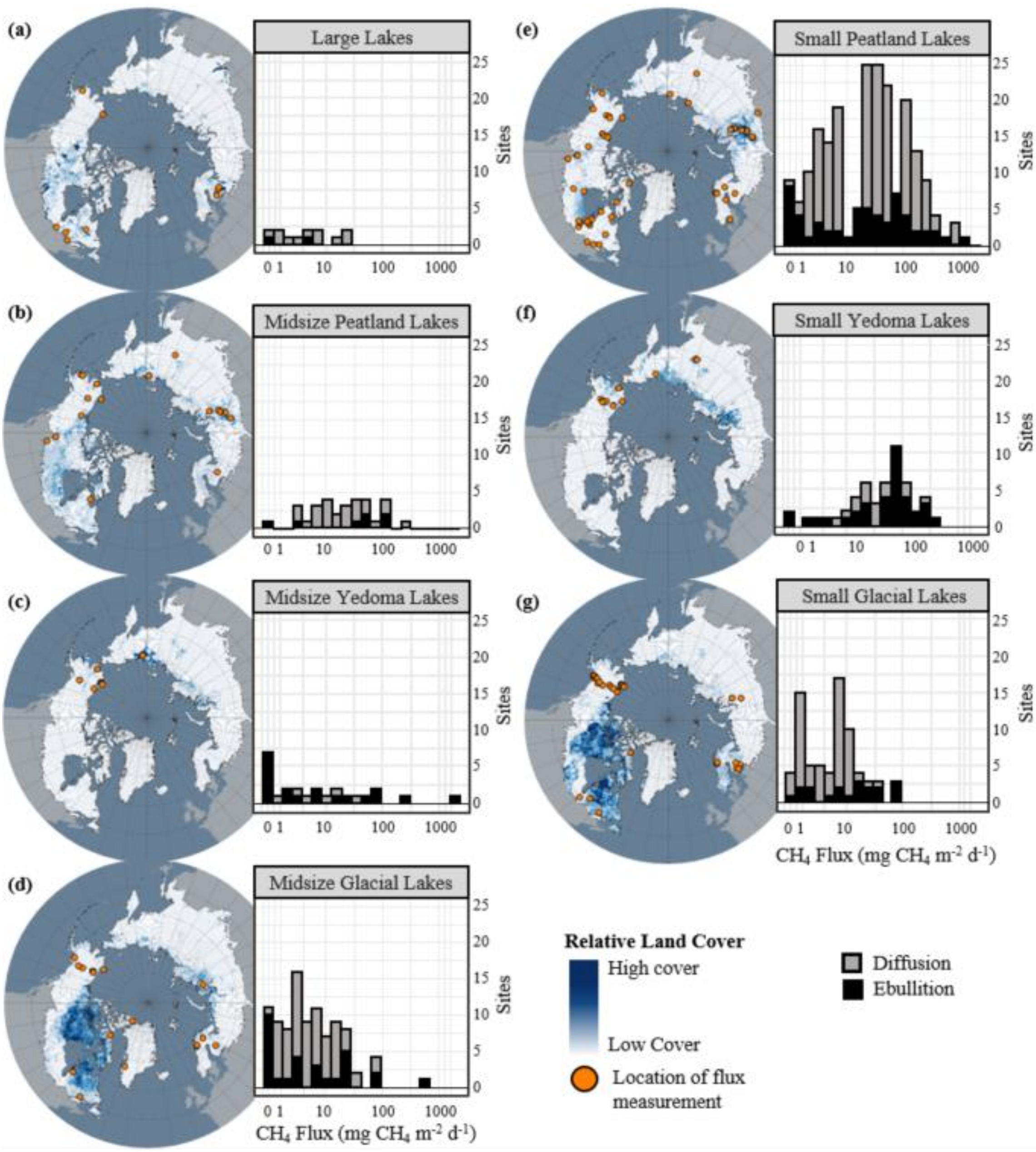

Figure 4. Flux frequencies and geographical distribution for each aquatic class. Relative land cover for each class type is represented in blue on the map. Site locations are represented by orange circles. Note the log scale for CH_4 flux. Land cover distributions from Olefeldt et al. 2021. Histograms of non-transformed flux data are shown in SI Fig. 4.

Section 3.3 figure citations- do you mean Fig. 10 (not fig 2.10) in this section? And re: fig. 10, while the 2 different blues are easily distinguishable in Fig. 9, they are less so in this figure.”

We fixed the citations in this section to all be Fig. 10. We also changed this figure to make the colors contrast better with a darker blue color for ebullition (see Figure 10 under RC1 comments).