Summary of major changes

In response to the reviewers' comments, in particularly Reviewer 1, we restructured, rewrote and extended the Study Area and Methods sections, while also establishing a clear terminology throughout the entire manuscript. In addition, we streamlined and shortened the Discussion sections. Below is a list of the major changes made during the revision, organized by manuscript sections.

Major changes in **Sect. 2. Study area.**

1. In response to Reviewers 1 and 2, we revised and restructured Section 2 (Study Area). The section is now divided into two new subsections:

   2.1 Climatic and environmental settings — largely new text; the new section includes information on peat-plateau formation, vegetation, and previous studies conducted at the sites.

   2.2 Sampling sites — original text revised to explain the rationale for pond selection in response to Reviewer 1 and to add information on non-thermokarst ponds in the area in response to Reviewer 2.

2. We removed the "best age estimate" for thermokarst pond formation from Table 3.

Major changes in **Sect. 3. Methods**.

3. Field sampling procedure (Sect. 3.1)
   In response to Reviewer 1, we clarified the field sampling procedure. Specifically, we detailed the locations of sampling within ponds, the protocol used for dissolved-gas sampling, and the storage conditions for water samples. Information on water temperature measurements was moved from the Study Area section to Sect. 3.1.
4. Analysis of gas concentrations (Sect. 3.3)
   In response to Reviewer 1, we added information on detection limits and the gas-chromatograph detectors used for analyses.
5. Dissolved CO_2 recalculation and corrections (Sect. 3.3; Supplementary material)
   Following the comment of Reviewer 1, we corrected the dissolved CO_2 values that were previously calculated from total DIC after acidification. We clarified this procedure in Sect. 3.3. The recalculation used in-situ pH and equilibrium constants adjusted to pond temperature. CO_2 values were changed for two ponds with pH > 5 (A3 and A8), decreasing by 9–42%. We updated the values and revised Fig. S9a in the Supplementary Material. The changes in the numbers do not alter the figure's main conclusion and are hard to spot when comparing the revised and the old figure.
6. Winter CH_4 storage and bottom flux calculation (Sects. 3.5–3.6)
   We reorganized the Methods section on flux calculation into two subsections: *Sect. 3.5 "CH_4 storage* in water and ice" and *Sect. 3.6 "Winter CH_4 bottom flux."*

In Sect. 3.5 we added the equations used to calculate CH_4 winter storage (Eq. 1) and CH_4 storage prior to freezing (Eq. 2). We clarified how the uncertainty of each term in Eqs. 1 and 2 is estimated and how these uncertainties combine to yield the final uncertainty using Gaussian error propagation.

In Sect. 3.6 we provided the defining equation (Eq. 3) for calculating the winter CH_4 bottom flux from the storage terms, and clarified the uncertainties associated with each term.

During the revision, we modified one substep in the winter bottom flux calculation by including dry peat and gas-bubble volumes in the ice-sample volume (Sect. 3.5). This changed the CH_4 volumetric concentrations, ice storage, and thus winter bottom fluxes by up to a few percent. Results, Figures 5 and 6 and Supplementary material were updated accordingly. These minor changes in the numbers did not affect the study conclusions and are barely visible in the figures.

We corrected the density value of frozen peat used in the calculations of both dry peat volume and headspace, which slightly changed the water-equivalent concentration of frozen peat in ponds A5 and A6. We clarified the density used in the Sect.3.5. We updated the values in Results and Figures 3 and 4 accordingly.

7. Thermokarst pond formation ages (Sect. 3.7)
   In response to Reviewer 2, we added Sect. 3.7 "Thermokarst pond formation ages," which describes how pond ages and developmental stages were determined.

Major changes in **Sect. 4. Results.**

8. In response to Reviewer 1, we clarified the order of months in Table 2 and Figure 1: "Note: months are presented in a non-chronological order to reflect the logical sequence of winter CH_4 accumulation."
9. Following the suggestion of Reviewer 1, we moved the CO_2 concentrations data from Sect. 4.1 to the Supplementary Material and added a reference to the Supplementary Material in Sect. 4.1.
10. We changed the Figure 3 boxplot for "Frozen peat" and updated the figure caption to state what the boxplot elements represent and how many samples were used to construct each boxplot (in response to Reviewer 1).
11. We corrected the inaccurate solubility value used for calculation of CH_4 in the first winter ice layers; this produced a slight change in the estimated storage prior to freezing. We updated the results accordingly, but the changes were very small and did not affect the winter CH_4 bottom flux values.
12. After the recalculations described above (bullet point 6), the relationship between winter CH_4 bottom flux and thermokarst pond formation age became just beyond the margin of becoming statistically significant ($p = 0.0048$). We updated Sect. 4.4 to reflect this result.

Major changes in **Sect. 5. Discussion.**

13. In response to the Review 1, we shortened the Discussion by removing several paragraphs from Sections 5.2 and 5.3 to improve clarity and achieve a better focus of the study.
14. In response to Reviewer 2, we revised the end of Section 5.4 to clarify and remove inaccurate wording from the discussion about an influence of Sphagnum on winter CH_4 bottom fluxes.

Major changes in **Supplementary material.**

15. We added explanatory text for each supplementary figure and table and added references to the sections in main text.
16. We included additional aerial images used to estimate thermokarst pond ages (Figs. S1–S3).
17. We added a section on CO_2 results in the Supplementary Material and updated Fig. S9 to reflect the methodological changes described above (bullet point 5).
18. We updated Fig. S10 to reflect the methodological changes described above (bullet point 11).
19. We updated the values in Table S2 because of the methodological revisions described above.

RC1: 'Comment on egusphere-2025-3059', Anonymous Referee #1, 09 Sep 2025

We thank the reviewer for taking the time to review our paper. The comments helped us to improve the manuscript. We fully restructured the sections on Study area and Methods and shorten the Discussion following the reviewer's recommendations. We also removed inaccurate wording. In general, there are only few studies that quantify wintertime CH_4 in thermokarst lakes, and even fewer from thermokarst ponds in permafrost peatlands (e.g., Kuhn et al., 2021). However, such data are very important to understand the annual CH_4 budget of such ponds which again is a prerequisite for upscaling to permafrost landscapes and beyond (e.g. Bastviken and Johnson, 2025). Our study provides over-winter CH_4 flux measurements from nine ponds in Northern Scandinavia. One of the study areas includes an eddy-covariance tower, which has been used to estimate the contribution of CH_4 accumulated in thermokarst ponds during winter to the annual budget. In addition to quantifying CH_4 wintertime flux and storage in thermokarst ponds, our study design linked to repeated aerial imagery allows us to estimate relationships between CH_4 production and thermokarst pond formation age.

In the following, we respond to the issues raised by the reviewer and indicate where we implemented changes in the revised manuscript. Reviewer comments appear in black, our responses appear in blue, and revised manuscript text appears in *blue italics*.

Comments

Revie of "Wintertime Production and Storage of Methane in Thermokarst Ponds of Subarctic Norway"

Here, Pismeniuk et al. quantified methane storage and emissions from several thermokarst peatland ponds during the ice covered period. By using chronosequences of thermokarst pond formation, they explained the observed rates and distinguished them according to vegetation types. The topic fist very well for Biogeosciences. However, at this stage, the manuscript contains several inaccuracies that need to be addressed. Please see my main comments below. The primary aim of the study is to quantify methane emissions and storage in ponds over time. I acknowledge the logistical challenges of sampling in remote locations, particularly given the number of ponds included. However, for the study it is necessary statistical strength to talk about ecosystem level replicates. For instance, in the case of pond A8, it is unclear why this very particular case was included. Please provide a stronger justification for its inclusion or consider removing it from the study. Regarding thermokarst formation, your results suggest a promising pattern. However, in several cases there are no replicates. For example, A6 represents a recently formed pond, but no comparative sites are provided, while A3 appears to present a similar issue. Therefore, you need to clearly explain the rationale behind your pond selection.

Our study does not really aim to draw conclusions at the ecosystem level (e.g. for the entire peat plateau complex), but we specifically focus on the ponds as potential CH_4 hotspots within the larger-scale ecosystem (e.g. Vonk et al., 2015; Kuhn et al., 2021). Our objectives are to (1) quantify

CH_4 winter bottom fluxes in various ponds across two permafrost peatland sites in Finnmark, (2) assess the contribution of wintertime cumulative CH_4 flux to the annual CH_4 budget at one of the sites, (3) identify the main factors causing differences in CH_4 winter bottom fluxes, and (4) explore the relationship between CH_4 winter bottom fluxes and the age of thermokarst pond formation. These objectives are now clearly stated in the Introduction.

Our data from the two peat plateaus show that methane bottom fluxes differ significantly between Iškoras and Áidejávri, which supports the conclusion that site-specific factors at least to some degree govern CH_4 production across different permafrost peatlands. We consider this an important finding as it complicates the upscaling of CH_4 fluxes to the ecosystem scale and beyond (Sect. 5.3, revised manuscript). For this reason, we do not draw any conclusion on fluxes on the ecosystem scale.

In the main study area (ponds A1–A6,) we include all available ponds formed from the same peat plateau segment, covering an area of 150 m × 100 m. The formation from the same peat plateau segment suggests that the submerged peat material has similar characteristics, while also the meteorological conditions and the hydrological regime are similar. This makes it possible evaluate the influence of pond formation age with limited confounding factors. The ponds A6 and A3 are the only available ponds of their respective ages. In the revised manuscript (Sect. 2.2), the rationale for selecting ponds A1–A6 reads: *"Six of the ponds (A1-A6) are located in a relatively small area of 150 m x 100 m in which all available ponds were sampled (referred to as "main study area" in Áidejávri in the following, see also Fig. S1). These ponds were all derived from the same peat plateau complex, suggesting similar environmental conditions and a shared origin of the submerged peat material. However, analysis of air photos (Sect. 3.7) revealed different formation ages of the ponds, spanning a chronosequence from 1 to 70 years, which allows us to compare wintertime CH_4 production with thermokarst pond formation age (Fig. S1)."*

Pond A8 is included because it differs in origin and characteristics from the thermokarst ponds. Unlike the thermokarst ponds in Áidejávri, A8 has an elongated shape and a markedly different pH. We interpret A8 as a remnant of a larger post-glacial water body that is partly transitioning to mire through sedge succession. Including pond A8 places the calculated CH_4 fluxes for thermokarst ponds in the main study area in a broader context and delivers value for future comparisons.

At the Iškoras peat plateau complex, we sampled two locations in the large central pond. This pond is situated in the footprint of the eddy-covariance tower operating at Iškoras. Pirk et al. (2024) disintegrated the fluxes of the eddy-covariance tower into different landcover types, one of which are "ponds". The studied pond is by far the largest in the footprint, so that the values for the "pond" class can be used as a proxy for cumulative summer fluxes of this pond. This puts us in the position to estimate the contribution of cumulative wintertime flux to the annual CH_4 budget (Sect. 5.2, revised manuscript). In general, there are fewer ponds in Iškoras compared to Áidejávri, as the total area of the peat plateau complex is much smaller. This makes it challenging to establish a

similar thermokarst pond chronosequence at Iškoras. During the fieldwork in March 2024, we tried to sample some of the younger ponds, but these proved to be very shallow, and sampling was unsuccessful.

In the revised manuscript, we have rearranged the Study Area section and now include a more detailed description of the study site and the rationale behind the pond selection (Sect. 2.2, Sampling sites, revised manuscript). We also added a section on thermokarst age evaluation in Methods and Supplementary material (Sect. 3.7, Fig. S1-3, revised manuscript).

The protocol for estimating CH_4 storage in the ponds is confusing, as it relies on arbitrary or insufficiently justified assumptions when summing the different ice and water layers collected from each pond. It is unclear what you mean by the 5% uncertainty in relation to the headspace method and storage in the water column, please provide a clear explanation and justification. Similarly, the arguments for including uncertainties related to peat are not clearly presented and require clarification.

We thank the reviewer for this comment! To provide more clarity, we have completely rearranged the Methods section on flux calculation, creating two subsections Sect. 3.5 "*CH_4 storage in water and ice*" and Sect. 3.6 "*Winter CH_4 bottom flux.*" In Sect. 3.5 we added the equations used to calculate the CH_4 winter storage (Eq. 1, revised manuscript) and the CH_4 storage prior to freezing (Eq. 2, revised manuscript). Based on these equations, we clarify how the uncertainty of each individual term in Eqs. 1 and 2 is estimated and how these uncertainties combine to a final uncertainty using Gaussian error propagation.

We state and justify a 5% uncertainty for CH_4 concentrations measured by the headspace method. This value is based on published error estimates for dissolved gases at low pH (Koschorreck et al., 2021); we assume the relative error for CH_4 is the same or lower than that reported for CO_2 under comparable conditions. We also clarify the treatment of uncertainties associated with frozen peat (Sect. 3.5, revised manuscript): *"For the frozen peat samples, when only a single sample was available, we applied the average relative uncertainty from the deepest ice layers in other ponds."*

In Sect. 3.6 of the revised manuscript, we again provide the defining equation (Eq. 3) for calculating the winter CH_4 bottom flux from the storage terms and describe the uncertainties associated with each of the terms. We then calculate the uncertainty of the winter CH_4 bottom flux using Gaussian error propagation.

Several methodological sections highlight potential problems with your core sampling procedure and the way the overall balance was calculated. Typically, storage estimates begin from the onset of ice cover and are calculated forward through the ice-covered period. In your study, however, you assume the end of the 2023–2024 ice period based on measurements from the beginning of the 2024–2025 ice period. This reversed logic is highly questionable. Please justify why the study design started in the opposite direction (you acknowledge it in the discussion but still is not enough

to consider a good selection, expand it and use literature to discuss about it). And would recommend to sort it properly, in Figure 2 or Table 2 you are sorting in a way that March measurements are later, which is not the case.

To derive the winter CH_4 bottom flux for the 2023–2024 season, we need to estimate the CH_4 storage in the pond water just before the onset of freezing in fall 2023. However, these measurements are unfortunately not available to us, as it is logistically very challenging to be out in the field exactly at the time of freeze-up for each of the ponds. For this reason, we used the CH_4 concentrations in the very first winter ice layer, as sampled in March 2024, which provides a fingerprint of the composition of dissolved gases at the time of freeze-up when the pond becomes decoupled from the atmosphere. However, we acknowledge that this approach introduces uncertainty, as it is not clear how fast the ice thickened and whether our "first winter ice" sample does not also contain layers with e.g. ebullition bubbles released after freeze-up. To cross-check the obtained values, we returned in September 2024 and re-sampled the ponds to obtain typical CH_4 concentrations during fall. After adjusting the dissolved CH_4 concentrations to 0 °C, the comparison confirms (Fig. S10) that, in most cases, the differences between the first-formed-ice concentrations and the September concentrations are relatively small, which suggests that they indeed provide an adequate estimate for the CH_4 concentrations prior to freezing. However, in a few cases the differences were much larger which we account for in our uncertainty analysis (see Sect.3.6, Sect. 4.3. Sect 5.1 in the revised manuscript). In case of larger differences, we assume very large uncertainties of up to 100% for the pre-freeze CH_4 storage, but since the absolute values are small compared to the winter storage, these uncertainties do not strongly contribute to the final uncertainty in the winter bottom flux. In the revised version, we have clarified this procedure in both the Methods (Sect. 3.6, revised manuscript) and the Discussion (Sect. 5.1, revised manuscript).

Concerning Fig. 2 and Table 2, we consider it most important to show the order of magnitude of the CH_4 increase from September to March rather than the strict chronological order of sampling. Instead, we clarify this in the figure caption: *"Note that the months are presented in non-chronological order to reflect the logical sequence of the winter CH_4 accumulation."*

Regarding the sampling campaigns of dissolved gas in water and ice cores measurements, I have several questions. Because, measurements were very limited at the beginning of ice cover in October 2024, and those from September 2024 appear very superficial. Please clarify why dissolved gas samples were collected at only 0.1 m depth in September, and were sampling was conducted (in the center?) of the pond, and why not bottom samples were collected? Please explain the rationale for being selective in October 2024, why were some ponds sampled while others were not? The table showing pond properties is questionable not sampling them, as not all sites were included in the final sampling. Finally, how many samples were collected per site, only one ice core per pond? And water samples per point?

Due to logistical constraints (not all ponds were reachable due to thin ice) and poor weather, we could not sample all ponds in October 2024. We present the October data to show how quickly dissolved CH_4 storage increases with formation of the first ice on the ponds. We consider these data useful to the research community.

In September, we collected water samples at 0.1 m depth because the ponds are shallow and well mixed prior to first-ice formation, as supported by measured temperature profiles (Fig. S4). Samples are taken at the pond center, which we specified in the main text: "*The exact positions were determined from the aerial imagery (Sect. 3.7), generally in the central area of each pond, and located by differential GPS in the field.*" (Sect. 3.1, revised manuscript).

We clarify the number of samples in Sect. 3.1 of the revised manuscript: one ice core per pond was collected. Generally, we split each core into three subsamples and used these subsamples for analysis which allowed us to evaluate the uncertainty in the CH_4 content of the ice. Water samples were taken from the same location where the ice core was taken. In winter, this was done at exactly the coring location after drilling a first hole through the ice. In this case, we took two samples where the remaining, unfrozen water column was deep enough, and only one sample when only a thin water layer was present. In September, the samples were taken from the shore at the approximate location of the winter coring in the center of the pond. Since the ponds are generally small and the water column is well-mixed during ice-free conditions (as suggested by the temperature measurements, see above), we consider this an adequate procedure.

Another critical part is the sampling procedure for dissolved gas and DOC measurements which also requires clarification. For example: (i) How much vacuum was created in the 12 mL vials prior to filling? Why was shaking performed for 5 minutes? This seems excessive, and the friction and hand-warm inside the syringe could have increased the temperature, thereby affecting gas solubility and Henry's law values. (ii) If acid was added directly into disposable syringes, this could have damaged the syringes and caused leaks. Were syringes replaced for each measurement? Did you check for potential sample interferences or leaks? If not, I strongly recommend verifying this in the laboratory. (iii)

We used a well-established sampling protocol for the dissolved gases, which has been used in our research group for many years, and which is documented in peer-reviewed articles, e.g.

1. Knutson, J. K., Clayer, F., Dörsch, P., Westermann, S., de Wit, H. A. Water chemistry and greenhouse gas concentrations in waterbodies of a thawing permafrost peatland complex in northern Norway, Biogeosciences, 22, 3899–3914, https://doi.org/10.5194/bg-22-3899-2025, 2025.
2. Eiler, A., Valiente Parra, N., Andersen, T., Hessen, D. O., Allesson, L. Drivers and variability of CO_2: O2 saturation along a gradient from boreal to Arctic lakes. Scientific Reports, 12(1), 18989–10, https://doi.org/10.1038/s41598-022-23705-9, 2022.

3. Wei, J., Fontaine, L., Valiente, N., Dörsch, P., Hessen, D. O., Eiler, A. Trajectories of freshwater microbial genomics and greenhouse gas saturation upon glacial retreat. Nature Communications, 14(1), 3234–12, https://doi.org/10.1038/s41467-023-38806-w, 2023.

Before applying the method in the field, we tested the equipment and procedures to rule out potential leaks, interferences, and sample damage. In particular, we followed the protocol described by Knutson et al. (2025) (who quantified summer gas concentrations and water chemistry at the Iškoras site) to maintain methodological consistency between different studies. In the revised version, we clarify the procedure in Sect. 3.1: "*Immediately after bringing the water samples to the surface, dissolved gases were extracted from a subsample on-site using the acidified headspace method (Åberg and Wallin, 2014) following the protocol of Knutson et al., (2025). 30 mL of water was collected into a 60 ml disposable syringe equipped with a 3-way valve and 20 mL headspace with ambient air was created. The samples were acidified with 0.6 mL of 3 % HCl to achieve a pH < 2, so that the dissolved inorganic carbon (DIC) was completely released as CO_2 into the headspace. To reach the equilibrium, the syringe was shaken for 1 min, followed by a 30 s rest and this sequence was repeated three times (Knutson et al., 2025). The headspace gas was transferred to a Helium (He) washed and evacuated 12 mL septum vials (Chromacol, remaining pressure 4-6 mbar).*"

We do not think that heating of the sample during equilibration was a major concern in our case, as the main sampling in March 2024 was conducted at freezing temperatures. Furthermore, we made sure to not touch the syringe with a warm hand (at all sampling dates), and we hold the plunger while shaking. The syringe is not insulated, so any frictional heating is likely to dissipate rapidly to the colder environment and not result in a major temperature change. Furthermore, the CH_4 solubility changes by only ~3% per 1°C, so that a small degree of warming could even be tolerated as it is negligible compared to other sources of uncertainty in our analysis.

Please note that Falcon tubes are known to leach DOC. Did you test whether this influenced your results? Filtration through 0.45 µm is unlikely to remove all bacteria, which could result in DOC depletion if samples were stored for too long. How long were DOC samples stored prior to analysis? (iv)

Using Falcon tubes and 0.45 µm filtration for DOC analysis is a standard practice in many published studies (e.g. Feng et al., 2020, Carlsen et al., 2025, Racasa et al., 2026). In addition, we tested for DOC leaching from the Falcon tubes and did not find any. For this purpose, we compared the results from lake samples stored in Falcon tubes with those stored in glass tubes, as well as with controls with deionized water; these comparisons did not show any detectable leaching. We filter samples through 0.45 µm filters, which remove most bacteria, and we store samples cold or frozen to minimize microbial activity. In our protocol, unfrozen samples are kept dark at 4 °C for no more than 7 days. The combination of filtering and short-time storage at cold temperatures inhibits bacterial growth or alteration of DOC. In the revised manuscript, we added the storage conditions and maximum storage time to the Methods (Sect. 3.1, revised manuscript).

I do not consider your reported CO_2 values from dissolved gas samples to be valid, since total inorganic carbon was not determined. Without this measurement, the reported CO_2 concentrations cannot be considered representative of the actual conditions in the water (you added acid and no alkalinity was measured), or you need to expand the calculation of Appelo and Postma, 1993. (v) the type of GC detector is not clear, and also you must provide the detection limit for the gases. Also, I do not see the point to include CO_2 and N2O in the study since the study is focused on CH_4 (N2O is mentioned in the methodology but not used in the results or discussion). (vi) the O2 is not clear how did you measure and which device was used for it. (vii) Again the mixing of the ice samples in the jars was for 1 hour to equilibrate headspace, the remaining oxygen in the ice could be used to oxidize the methane stored in the ice. Still I do not understand why you have such long periods of mixing.

We agree with the reviewer that CO_2 concentrations and CO_2:CH_4 ratios were not the main focus of our study. However, we believe these data can provide potentially important context for pond classification and serve as future reference, e.g. in modeling studies on pond greenhouse gas balances. Therefore, we have moved the dissolved CO_2 results to a new Section 2.1 in the Supplementary Materials. In the Results section of the main paper, we only provide a short reference to Supplementary Section 2.1.

We thank the reviewer for the comment on the inorganic carbon which helped us correct the CO_2 values for ponds with pH > 5. We use the acidified headspace method of Åberg and Wallin (2014) for inorganic carbon. After acidification the sample pH is 2 and dissolved inorganic carbon (DIC) is released completely as CO_2 into the headspace; we now state this clearly in the Methods section of the revised manuscript (Sect. 3.3). We also corrected the dissolved CO_2 values calculated from total DIC after acidification, using in situ pH and equilibrium constants adjusted to pond temperature following Eq. 12 in Åberg and Wallin (2014). When the pH is below 5, this correction is negligible. However, for the two ponds with pH > 5 (A3 and A8) we recalculated dissolved CO_2 and include those values in the revised Supplementary Section 2.1 (Fig. S9a, revised supplementary). The recalculated CO_2 concentrations for these two ponds are 1.1–1.7 times smaller (a decrease of 9–42%) than the uncorrected values. As Fig. S9a uses a logarithmic scale, these changes are hardly visible, and the main conclusion from the figure remains unchanged.

In September, we measured O_2 together with the other gases to characterize conditions prior to freezing; this is now reported in the Methods (Sect. 3.3, revised manuscript). Furthermore, we now specify the GC setup in the Methods (Sect. 3.3, revised manuscript): "*CH_4 was measured with a flame-ionization detector (FID; detection limit 0.1 ppm). CO_2, O2 and N_2 were measured with a thermal-conductivity detector (TCD; detection limits 10 ppm for CO_2 and 100 ppm for O_2 and N_2).*" We removed N_2O as an operational characteristic of the gas chromatograph in Sect. 3.3.

To avoid oxidation during headspace extraction, we flushed the ice samples with He prior to melting. We reviewed the relevant literature on methane oxidation rates to evaluate potential impacts on the resulting CH_4 concentrations. Reported oxidation rates in similar subarctic and

boreal surface waters are in the range 0.0007–0.05 µmol CH_4 L^{-1} $hour^{-1}$ (Matveev et al., 2018; Kankaala et al., 2006). Our measured CH_4 concentrations in the ice samples vary from 0.1 to 1258 µmol CH_4 L^{-1} w.e. Assuming the maximum reported CH_4 oxidation rate (0.05 µmol L^{-1} h^{-1}), oxidation during 1-hour equilibration would correspond to 50% of our smallest measured concentration (0.1 µmol L^{-1}) and ≤ 0.004% of our maximum measured concentration (1258 µmol L^{-1}). For the vast majority of samples, this error source is negligible. In addition, the lowest concentrations (typically in the first-formed ice layer where the relative error of oxidation would be highest) contribute very little to the total ice CH_4 storage. For this reason, we do not think that oxidation during equilibration can meaningfully alter CH_4 concentrations and thus affect the calculated ice storage and winter bottom-flux estimates.

Figure 2 are showing some error bars, what is this and how they were estimated, it is not clear in the text. Please sort it properly, March at the beginning.

Regarding Figure 2 and Table 2, as explained above, we consider it most important to show the strong CH_4 increase from September to March, rather than the exact chronological order. However, we clearly state this now in in the figure caption: *"Note that the months are presented in non-chronological order to reflect the logical sequence of the winter CH_4 accumulation."* Error bars represent standard deviations of multiple samples (n = 3–12) collected from different depths in March and October, as well as replicate samples taken in September from the same depth. We have specified this in the capture of Figure 2**.**

Figure 3 is a boxplot, so please add the number of data used to construct them, and the meaning of the whiskers, boxes and lines and circles.

We thank the reviewer for the comment. We have corrected Figure 3 and revised the caption to specify the number of data points used. The revised caption now reads: "*Figure 3. Box plots illustrating methane (CH_4) concentrations in distinct ice types (ice types from Boereboom et al., 2012 with adjustments): 1 – Superimposed ice (n = 20), 2 – Clear ice (n = 31), 3 – Methane ebullition bubbles (n = 9), 4 – Spherical and nut–shaped bubbles (n = 10), 5 – Elongated bubbles (12), 6 – Mixed bubbles (n=38). Boxes show the interquartile range (25th–75th percentiles), the line indicates the mean, and whiskers extend to the min and max values. For Frozen peat (7) with n = 2, only mean, min and max are shown. CH_4 concentrations are reported on a water-equivalent (w.e.) basis".*

The discussion and conclusion sections are highly repetitive. I recommend condensing them and reformulating after the methodology and results have been corrected or modified in response to my previous comments.

We agree with the reviewer that especially the Discussion section needed to be streamlined and shortened. We have completely rewritten the Methods section for clarity and revised Results and Discussion accordingly. Furthermore, in response to the reviewer's comment, we shortened the Discussion, removing some of the less focused discussion points from Sections 5.2 and 5.3.

In addition, Figure 6 is not sufficiently supported by the results and appears to present data in a casual way. Please rework this figure to ensure that it is consistent with, and properly supported by, your findings.

Fig. 6 shows the winter CH_4 bottom fluxes (i.e. the same values as in Fig. 5) for the main study area in Áidejávri, plotted against the formation age of the thermokarst ponds. Furthermore, we provide an assessment of the succession stage of the ponds in the figure. In response to the reviewer's comment, we have revised the Methods section to add clear information on how the two latter quantities (age and succession stage) were obtained (Section 3.7, revised manuscript). Furthermore, we added horizontal error bars to indicate the uncertainty in the timing of thermokarst pond formation, based on historical and drone aerial imagery (as detailed in Sect. 3.7, revised manuscript).

References:

Bastviken, D., Johnson, M.S. Future methane emissions from lakes and reservoirs. Nature Water, 3, 1397–1410. https://doi.org/10.1038/s44221-025-00532-6, 2025.

Boereboom, T., Depoorter, M., Coppens, S., Tison, J.-L. Gas properties of winter lake ice in Northern Sweden: implication for carbon gas release, Biogeosciences, 9, 827–838, https://doi.org/10.5194/bg-9-827-2012, 2012.

Carlsen, E. C. L., Wei, J., Lejzerowicz, F., Trier Kjær, S., Westermann, S., Hessen, D. O., Dörsch, P., Eiler, A. Redox determines greenhouse gas production kinetics and metabolic traits in water-saturated thawing permafrost peat. ISME Communications, 5(1), ycaf009. https://doi.org/10.1093/ismeco/ycaf009, 2025.

Feng, L., An, Y., Xu, J., Li, X., Jiang, B., Liao, Y. Biochemical evolution of dissolved organic matter during snow metamorphism across the ablation season for a glacier on the central Tibetan Plateau. Scientific Reports,10 (1), 6123. https://doi.org/10.1038/s41598-020-62851-w, 2020.

Kankaala, P., Huotari, J., Peltomaa, E., Saloranta, T., Ojala, A. Methanotrophic activity in relation to methane efflux and total heterotrophic bacterial production in a stratified, humic, boreal lake. Limnology and Oceanography, 51(2), 1195–1204. https://doi.org/10.4319/lo.2006.51.2.1195, 2006

Knutson, J. K., Clayer, F., Dörsch, P., Westermann, S., de Wit, H. A. Water chemistry and greenhouse gas concentrations in waterbodies of a thawing permafrost peatland complex in northern Norway, Biogeosciences, 22, 3899–3914, https://doi.org/10.5194/bg-22-3899-2025, 2025.

Koschorreck, M., Prairie, Y. T., Kim, J., Marcé, R. Technical note: CO_2 is not like CH_4 – limits of and corrections to the headspace method to analyse p CO_2 in fresh water, Biogeosciences, 18, 1619–1627, https://doi.org/10.5194/bg-18-1619-2021, 2021.

Kuhn, M. A., Varner, R. K., Bastviken, D., Crill, P., MacIntyre, S., Turetsky, M., Walter Anthony, K., McGuire, A. D., and Olefeldt, D. BAWLD-CH_4: a comprehensive dataset of methane fluxes from boreal and arctic ecosystems, Earth System Science Data, 13, 5151–5189, https://doi.org/10.5194/essd-13-5151-2021, 2021.

Matveev, A., Laurion, I., Vincent, W. F. Methane and carbon dioxide emissions from thermokarst lakes on mineral soils. Arctic Science, 4(4), 584–604. https://doi.org/10.1139/as-2017-0047, 2018

Pirk, N., Aalstad, K., Mannerfelt, E. S., Clayer, F., De Wit, H., Christiansen, C. T., Althuizen, I., Lee, H., Westermann, S. Disaggregating the Carbon Exchange of Degrading Permafrost Peatlands Using Bayesian Deep Learning, Geophysical Research Letters, 51, e2024GL109283, https://doi.org/10.1029/2024GL109283, 2024.

Racasa, E. D., Jenner, A.-K., Saban, R., Kienzler, J., Batistel, C., Choo, S., Wang, M., Gräwe, U., Böttcher, M. E., Janssen, M. Characterization of Submarine Groundwater Discharge in Front of a Rewetted Coastal Peatland. Estuaries and Coasts, 49(1), 12. https://doi.org/10.1007/s12237-025-01607-z, 2026

Vonk, J. E., Tank, S. E., Bowden, W. B., Laurion, I., Vincent, W. F., Alekseychik, P., Amyot, M., Billet, M. F., Canário, J., Cory, R. M., Deshpande, B. N., Helbig, M., Jammet, M., Karlsson, J., Larouche, J., MacMillan, G., Rautio, M., Walter Anthony, K. M., Wickland, K. P. Reviews and syntheses: Effects of permafrost thaw on Arctic aquatic ecosystems, Biogeosciences, 12, 7129–7167, https://doi.org/10.5194/bg-12-7129-2015, 2015.

RC2: 'Comment on egusphere-2025-3059', Anonymous Referee #2, 28 Oct 2025

We thank the reviewer for taking the time to read and review our paper. The comments helped us to improve the manuscript and clarify inaccuracies. We restructured and clarified the information on pond formation and properties both in Study area (Sect. 2) and Methods sections (Sect. 3), following the reviewer's recommendations.

In the following, we respond to the issues raised by the reviewer and indicate where we implement changes in the revised manuscript. Reviewer comments appear in black, our responses appear in blue, and the revised manuscript text appears in *blue italics*.

This is an interesting study that highlights the importance of thermokarst peatland ponds as sources of overwinter methane. Measurements and their uncertainties were carefully conducted and explored with respect to a variety of pond metrics and water and ice characteristics. The results are comprehensively presented with a fulsome discussion that places these results in context of annual methane budgets and with other circumpolar lakes and ponds.

I only have a few small editorial questions/suggestions:

Line 63. Is it possible that some of these shallow ponds are ephemeral and satellite imagery may show 'dry' terrain at certain times of year/different years, which might confound aging? Either way, I suggest a summary of how the age of ponds was determined be included here in the main text as there appears to be several lines of evidence used for the age evaluation. Additional details such as dates of aerial imagery, etc. could perhaps be given in the Supplemental Materials.

We thank the reviewer for the suggestion. We reorganized the Methods in the revised manuscript and added a new Sect. 3.7, "*Thermokarst pond formation ages*." We also added Figures S1–S3 in the revised Supplementary Materials that show all aerial images used for age estimation, with corresponding references in the main text.

Our aerial-image analysis (Sect. 3.7, revised manuscript) shows that once water accumulates in the thermokarst depressions, the ponds do not dry out intermittently. However, we do not have arial images available before the 1950s and between 1955 (1958 for Áidejávri) and 2003, so we cannot track annual changes for these periods. For the ponds A7 and A8, the water surface area is large and remains nearly unchanged across the available imagery, so the drying of such large waterbodies is unlikely. The Iškoras pond was relatively small in 1955 but had expanded markedly by 2003 when it became hydrologically connected with the surrounding mire. For the ponds formed before 2003, our age uncertainty is large. For the ponds formed during last 10 years (by 2023) we have good aerial image coverage from drone surveys. In our study we interpret pond formation age as driven primarily by permafrost collapse (Sect. 3.7, revised manuscript). So, even if a newly formed depression briefly held little or no water, this will not affect age of permafrost collapse and depression formation.

The new Sect. 3.7. on thermokarst pond formation age reads:

"The thermokarst pond formation age was defined as the time of collapse of the peat plateau section, which typically coincided with water accumulation; however, there can be exceptions with the accumulation of water starting several years after collapse, as observed in the case of pond A4 (Fig. S1). We used 2023 as the baseline year for the age estimation. The thermokarst pond formation age was determined using geo-referenced historical aerial images (1955, 1958, 2003 and 2013) from the Norwegian Mapping Authority, as well as drone images (2015, 2020 and 2023) obtained from the Drone Infrastructure Lab, University of Oslo. The earliest available photographs are from 1955 for Iškoras (Kartverket survey WF-688 H-13) and 1958 for Áidejávri (Norgeibilder.no, 2025). The ortho-rectified drone imagery was processed following Martin et al. (2021) and has a ground resolution of 3 cm (2023), 5 cm (2020), and 10 cm (2015). The estimation of formation age was limited by the available imagery. No imagery from before the 1950s was available, and there is a gap between the 1950s and 2003. For the oldest pond types, which were already present in 1955 at Iškoras and 1958 at Áidejávri, it is not possible to constrain formation age prior to 1955.

Based on the area change, we classified pond developmental stages as stable, expanding, or overgrowing. If the net area change from 2015 to 2023 (including both permafrost degradation and succession) was less than 15%, ponds were identified as "stable". If the pond was formed or its area increased by more than 15% since 2015, it was named "expanding". If the pond area decreased by more than 15% because of vegetation succession, it was classified as "overgrowing". Succession by Sphagnum or sedges was identified based on the predominant species observed in aerial imagery and validated during fieldwork."

Line 76. Again, it wasn't immediately clear that ice formation timing was monitored using several methods. Please clarify in the main text.

The start of ice formation in 2023 was determined from both meteorological data and Sentinel-2 satellite imagery available for October 2023. We reorganized Methods Sect. 3.6, "*Winter CH_4 bottom flux*," to describe how we estimate the time interval between the start of ice formation and the sampling day in March 2024, Δt. *"Δt was estimated using meteorological data and Sentinel-2 satellite imagery available for October 2023. Initial ice formation at Iškoras started between October 1 and 2, 2023, coinciding with the first substantial drop in air temperature (Fig. S5). In Áidejávri, the onset of ice growth occurred between October 6 and 13 as indicated by temperature data from the meteorological station Sihccajavri and Sentinel-2 satellite imagery (Fig. S6)*"

All data supporting our Δt estimation appear in the revised Supplementary Material (Fig. S5-6).

Line 94. Give some detail about non-thermokarst pond formation processes in this region.

Most lakes of non-thermokarst origin in this area formed after deglaciation and are remnants of larger post-glacial water bodies. We reorganized the Study area section in the revised manuscript into two subsections: 2.1 "*Climatic and environmental settings*" and 2.2 "*Sampling sites*". In Sect.

2.1 we now provide a broader context on peatland development. In Sect. 2.2 we explain why pond A8 is classified as a non-thermokarst pond and give details on its formation.

The information on the non-thermokarst pond A8 in the revised manuscript reads: "*Finally, we sampled pond A8, located north of the main study area, which is being slowly overgrown by sedges (Figs. S2c-d). Unlike the thermokarst ponds in the area, A8 has an elongated shape, and a strongly different pH compared to the thermokarst ponds (Sect. 4.1). For these reasons, we rather interpret A8 as a remnant of a larger post-glacial water body which has partly transitioned into a mire than a thermokarst pond. A8 is therefore referred to as "non-thermokarst" pond in the following."*

Line 299. w.e. ?

We express CH_4 concentrations measured in ice samples as water-equivalent (w.e.) concentrations. We clarified the abbreviation "w.e." in the section where it first appears.

Line 334. This statement is unclear without skipping ahead. Perhaps briefly state here how methane storage in ice is related to thermokarst pond formation age.

We thank the reviewer for this suggestion and agree that the original statement was unclear. The relationship between the thermokarst pond formation age and wintertime CH_4 bottom flux are assessed for the main study area (A1–A6) and are now first presented in Results Sect. 4.4. We discuss this relationship more broadly (including additional ponds) in Discussion Sect. 5.4. For clarity, we have removed the passage on pond age from the Results subsection on CH_4 ice and water storages (addressed only in Sects. 4.4 and 5.4 in the revised manuscript).

Line 378. Recommend this statement be changed to "the relationship between pond age and bottom flux is not statistically significant ($p = 0.07$)."

During the revision process, we changed one of the substeps in the calculation of the winter bottom fluxes, i.e. we included the dry peat and gas bubble volumes in the ice sample volume calculation (Sect. 3.5, revised manuscript), which changed the CH_4 volumetric concentrations and thus winter bottom fluxes by up to a few percent. In particular, for the pond A5, the winter bottom flux was reduced from 29 ± 4.3 to 26 ± 4.2 mg CH_4-C m-2 d-1 following this correction. Prior to this correction, the statistical test between winter bottom fluxes and pond ages yielded $p = 0.07$, i.e. just above the threshold to being statistically significant (i.e. marginally non-significant). After recalculation, we re-evaluated the correlation between pond age and wintertime CH_4 bottom flux and obtained $p = 0.0048$, which is just below the threshold to be statistically significant (with the revised numbers for A5 being the most important factor for this change in significance level). In the revised manuscript (Sect. 4.4) we therefore report: *"The relationship between CH_4 winter bottom flux and the age of thermokarst pond formation was statistically significant by a narrow margin ($p = 0.0048$), although the limited number of ponds makes the statistical evaluation challenging."*

Lines 549-552. Are these moss mats on the margins of the ponds and not below the water? If so, how would aerated peat layers or endophytic methanotrophs on the margins of the ponds limit pond bottom fluxes of methane as measured in this study?

We thank the reviewer for this question. The formulation in the submitted manuscript was indeed misleading. Sphagnum mosses occurs both submerged in the Iškoras pond and on the surface. We now state in Sect. 2.2 that submerged Sphagnum was found at the Isk-1 sampling site. In the Discussion section 5.4 we note that during ice-free conditions submerged Sphagnum can reduce methane bottom fluxes via oxidation mediated by symbiotic or endophytic methanotrophic bacteria (Raghoebarsing et al., 2005; Parmentier et al., 2011). However, under ice cover the water column is generally anoxic, so Sphagnum-associated methane oxidation is likely negligible.

In the revised manuscript, we now address the issue at the end of Sect. 5.4: "*The old, stable thermokarst ponds at both Iškoras and Áidejávri (Isk-1 and -2 and A7), exhibited low winter CH_4 bottom flux (< 15 mg CH_4-C m^{-2} d^{-1}). At Iškoras, submerged Sphagnum was observed at the bottom of the sampling location Isk-1. In the ice-free season, submerged Sphagnum can reduce CH_4 bottom flux via oxidation mediated by symbiotic, endophytic methanotrophic bacteria (Raghoebarsing et al., 2005, Parmentier et al., 2011). In winter, however, the water column is predominantly anoxic, so Sphagnum-associated CH_4 oxidation is likely not a controlling factor. The low winter CH_4 bottom flux in Iškoras and A7 could indirectly be linked to pond age as Sphagnum succession marks the late stages of thermokarst pond development (Magnusson et al., 2020).*"

References:

Magnússon, R. Í., Limpens, J., Van Huissteden, J., Kleijn, D., Maximov, T. C., Rotbarth, R., Sass-Klaassen, U., Heijmans, M. M. P. D. Rapid Vegetation Succession and Coupled Permafrost Dynamics in Arctic Thaw Ponds in the Siberian Lowland Tundra, JGR Biogeosciences, 125, e2019JG005618, https://doi.org/10.1029/2019JG005618, 2020.

Martin, L. C. P., Nitzbon, J., Scheer, J., Aas, K. S., Eiken, T., Langer, M., Filhol, S., Etzelmüller, B., Westermann, S. Lateral thermokarst patterns in permafrost peat plateaus in northern Norway, The Cryosphere, 15, 3423–3442, https://doi.org/10.5194/tc-15-3423-2021, 2021.

Parmentier, F. J. W., Van Huissteden, J., Kip, N., Op Den Camp, H. J. M., Jetten, M. S. M., Maximov, T. C., Dolman, A. J. The role of endophytic methane-oxidizing bacteria in submerged Sphagnum; in determining methane emissions of Northeastern Siberian tundra, Biogeosciences, 8, 1267–1278, https://doi.org/10.5194/bg-8-1267-2011, 2011.

Raghoebarsing, A. A., Smolders, A. J. P., Schmid, M. C., Rijpstra, W. I. C., Wolters-Arts, M., Derksen, J., Jetten, M. S. M., Schouten, S., Sinninghe Damsté, J. S., Lamers, L. P. M., Roelofs, J. G. M., Op Den Camp, H. J. M., Strous, M. Methanotrophic symbionts provide carbon for photosynthesis in peat bogs, Nature, 436, 1153–1156, https://doi.org/10.1038/nature03802, 2005.