

Reply to the editor:

We like to thank the editor for the very constructive comments on our manuscript which strongly enhanced the quality and improved the comprehensibility of our study. We carefully considered all comments of the editor. The answers are highlighted in bold and changes done in the manuscript are marked in italic.

p. 1; l. 30 - 34: In the present study, you have not measured tundra landscape emission rates. Including terrestrial (ground) flux into this comparison makes it obviously different than pond flux (as you know, water logging is a critical factor for CH₄ production). So I think this comparison weakens your argument. Your data provides a comparison between different types of ponds (cf the points you are making in the conclusions). Your data also provide an argument that we should take into account early winter net CH₄ production because they are not negligible. You need to find a way to compare your results to summer pond flux, not global tundra flux.

Summer time CH₄ fluxes from ponds are not available for the study site. It might be also not correct to use summer time CH₄ fluxes of ponds from other sites for a direct comparison, since fluxes rates are most likely strongly determined by regional characteristics such as sediment composition and overall thermal conditions. Thus, we have decided to exclude this point completely from the abstract. In addition, it is not a major result of our study. Nevertheless, we believe that it is valuable to provide the gross scale summer time CH₄ emission as a reference value in the discussion. Discussion changed to:
The maximum summertime CH₄ emission rates per square meter from the average tundra landscape on Samoylov Island are of the order of $5 \times 10^{-8} \text{ mol m}^{-2} \text{ s}^{-1}$ ($60 \text{ mgCH}_4 \text{ m}^{-2} \text{ d}^{-1}$) (Sachs et al., 2008; Wille et al., 2008). These average landscape CH₄ emission rates were obtained by eddy covariance measurements with typical footprint areas of several hundreds of square meters including ponds and vegetated tundra soils. Thus, these measurements are not directly comparable to the production rates of individual ponds inferred by this study. Nevertheless, the eddy covariance measurements provide a reference value which allows to assess the strength of CH₄ production in ponds relative to a landscape scale CH₄ emission rate. Under this consideration, the early winter net CH₄ production rates per square meter from ASPs are about five times larger than the maximum summertime landscape scale CH₄ emissions per square meter. Considering that ponds occupy about 10% of the tundra landscape, this stresses the importance of ponds and the freezing period to the local carbon cycle.

p. 1; l. 50: using "the" instead of "this" is preferable, since when you say "this old carbon pool" you assume all C trapped in pmf (mentioned in previous sentence) is old, and I don't think it's the case (its a mixture of different pools, and it depends on how pmf was formed, after or during deposition)

We agree with the editor and changed the wording accordingly.

p. 1, l. 54: Please fill up the missing info

Done.

p. 2: l. 83: form

Corrected.

p. 2: l. 146: I think here is missing a general sentence on the type of deposits (loess? yedoma?)

We agree with the editor and extended the site description:

The Island is mainly characterized by a Holocene cryogenic soil complex that is largely characterized by the typical micro-relief of polygonal patterned ground formed by frost cracking and subsequent ice-wedge formation (Lachenbruch, 1962). The polygonal structures usually consist of depressed, water-saturated centers surrounded by elevated rims. The soil in the polygonal centers usually consists of sandy peat while the elevated rims are usually covered with a dry moss layer underlain by wet sandy peat soils and massive ice wedges (Kutzbach et al., 2004; Zubrzycki et al., 2012).

p. 2; l. 102: not sure if however is the correct link word here / provide the range as we have no clue

Statement changed to:

The sizes of freeze-out bubbles are reported to range between micrometers to millimeters at natural freezing rates of the order of millimeters per day (Lipp et al., 1987; Yoshimura et al., 2008).

p. 2; l. 108-109: I understand that it's because shallow means later in winter when CH₄ production is slower, is that the case/explanation brought by Phelps? This needs to be mentioned explicitly. Or is it increasing exponentially deeper in the ice because of the cumulation of CH₄ both from new production and from ice-degassing?? Either here (if Phelps paper brings explanations) or in the discussion (based on your results) you need to talk more explicitly about the reason for this exponential shape.

We agree with the editor that this point required further explanation. Therefore, we have expanded this point in the introduction and discussion:

The storage of CH₄ within the ice cover of shallow Alaskan lakes has been investigated by Phelps et al. (1998). They found that CH₄ concentrations were very low in the upper part of the ice cover, but increased rapidly with depth. This behavior was explained by supersaturation of dissolved gases due to the shrinking volume of available water underneath the growing ice cover. In addition, the concentration of dissolved CH₄ was observed to increase at the water-ice interface over the winter period. However, it remains unclear whether this CH₄ accumulation in the shrinking water column was caused by freeze-degassing of CH₄, ongoing CH₄ production during ice cover formation, or a combination of both.

p. 3; l. 115: Water depth is not the correct criteria to classify thermokarst lakes from ponds. I am sure you have used other criteria but the formulation here is unclear. I am assuming you are not in a region where kettle lakes are present (cause they would also be of the same depth range as thk)

We agree with the editor that lake depth is not a valid classification criteria to distinguish thermokarst lakes from other waterbodies. In fact, thermokarst lakes were identified based on multiple geomorphological criteria. Thus we modified the concerning paragraph to:

These deeper waterbodies are usually larger with horizontal extent ranging from about 50 m to several hundreds of meters. Therefore, these waterbodies are classified as lakes. At the study site these lakes occur either in the form of oxbow lakes or thermokarst lakes which can be distinguished according to multiple geomorphological indicators such as shape and location. Oxbow lakes are excluded from this study. ISPs, ASPs, and thermokarst lakes can be part of an evolutionary process of permafrost degradation so that transitional forms between these waterbody types exist.

p. 3; l. 222: HOBO is making a large suite of loggers: please provide the name of specific one you used

Changed:

The temperatures were recorded using water temperature loggers (Onset, HOBO Pro v2 with an accuracy of better than $\pm 0.5^{\circ}$ C) positioned along a metal wire hanging down from a small buoy anchored in the middle of each pond.

p 3; l. 230: at

Corrected

p. 4; l. 236: deployed

Corrected

p. 4; l. 230-240: rephrase (the metal wire is not floating, it's the buoy that maintains the wire vertical...)

Rephrased to:

... all sensors were hold in place relative to the water surface by the floating buoy.

p. 5; l. 369: I think at some point you could cut this paragraph into 2, maybe here?

Done.

p.5; l. 376: this needs to be defined: is it the sum of the bubble area across the whole ice matrix?? You need to explain what is the use of such term. It is very important variable in your work, yet it remains vague

The definition of the term effective bubble cross section is given in the paragraph before. However, we extended this definition to:

The effective bubble cross-section was calculated as horizontal area occupied by bubbles of an infinitesimal thin horizontal ice cover slices with an area of 1 m².

p. 5; l. 380-381: The CH₄ concentrations I see in the Supplement info of Abnizova et al. is 0.07 to 249 µgCH₄/L (249 being measured only once; most data being below 1), which corresponds, to 0.004 to 15.6 µM, and this is not corresponding to this range providing here... Please acknowledge where this range comes from. Moreover, this range does not seem right. It is much too low! and lower than the DL of the GC provided in method section! (5×10^{-5})

The range provided here originates from the dataset provided by Abnizova et al. (2012). However, we have missed to explain that the given range between the 6% and 94% quantile of the entire dataset. This filtering was done in order to exclude very low and very high CH₄ concentrations which rarely occur. It was the intention to test the model for the most significant range of CH₄ concentrations. However, we follow the remark of the editor changed the range to the entire range provided by Abnizova et al. (2012). Furthermore, Abnizova et al. (2012) used a different GC so that the dataset is not limited by the detection limited used in this study. Please note that the values given by Abnizova et al. (2012) are given in µgC/L which changes the final range a bit. We have also tested the model with the new range of initial values. However, the maximum initial CH₄ concentration is naturally limited by CH₄ concentrations observed in the upper most centimeters of the ice cover. The initial concentration of CH₄ in water can not exceed a certain limit which would not fit the observed concentrations in the ice cover. This limitation only exists for the profiles ISP2 and ASP2 which show generally very low CH₄ concentrations. We have added the following to the discussion section:

The results were also found to be very robust against uncertainties in the initial CH₄ concentration within the water column, prior to the onset of freezing. Except for the ponds ISP2 and ASP2, sensitivity tests over the entire range of possible initial CH₄ concentrations (1×10^{-9} and 1×10^{-5} mol m⁻³ see Sect. 3.4) were not found to affected the modeled magnitudes of CH₄ production and effective bubble cross-section. For the ponds ISP2 and ASP2 the model produced consistent results with initial CH₄ concentrations ranging between 1×10^{-9} and 1×10^{-7} mol m⁻³. This limited range relates to the generally very low CH₄ concentrations found in these ponds. Higher initial CH₄ concentrations would require methane decomposition instead of production to reproduce the observed CH₄ concentration profiles.

p. 5; l. 384: from?

Corrected.

p. 5; l. 339: uniform along time over winter, which means uniform in the ice core?? can you please clarify?

Rephrased and clarified to:

Because of the very stable temperature conditions in shallow sediments during the freezing period from October through February we assumed constant CH₄ production and oxidation rates. Furthermore, other factors controlling CH₄ production such as sediment composition are assumed to remain constant. We also assumed a uniform enrichment of methane in the water column beneath the ice cover. A uniform distribution of dissolved CH₄ in the shrinking water column is considered a reasonable guess for the investigated very shallow waterbodies albeit concentration gradients are reported for deeper lakes. However, increased CH₄ enrichment at the bottom of the ponds would lead to underestimated net

CH₄ production in the model calculations.

p. 5; l. 341-345: I understand that you need to make assumptions at some point for modelling, but production and consumption is not only dependant on temperature, but also on substrate: could substrate be changing over time in autumn through winter (?)

We agree with the editor and included an statement that substrate composition is assumed to remain constant for modeling (please see above).

Moreover, you seem to have data on sediment temperature (?) but unfortunately they are not presented (a point raised by both Reviewers, either concerning the data set or the discussion of the fact that higher methane production at lower T is unlikely). I think you need to defend this point better in the discussion and for this, such data could help.

Temperature measurements at the water-sediment interface are available for different ponds. These measurements show a slight temperature change from about 2 °C to 0 °C during the freezing period. Assuming a standard Q₁₀ relation between CH₄ production and temperature (Q₁₀ = 3), this would lead to a change in CH₄ production of about a factor of 1.3. Considering that only differences of orders of magnitudes are discusses in this study, a constant production rate seems to be appropriate. We believe that the manuscript should keep its focus on CH₄ concentrations and modeled production rates. Therefore, we would like to avoid an additional figure that would introduce a new aspect of thermal dynamics which is not in the focus of this study. The temperature plot would show a slow cooling from about 2 to 0°C at all ponds as already shown in Boike et al. (2013). We have added the following paragraph:

Very stable temperature conditions with slowly decreasing temperatures from about 2 to 0° C were observed at the bottom of shallow ponds and lakes during the freezing period from October through February at the study site (Boike et al., 2013). Assuming a standard Q₁₀ relation between CH₄ production and temperature (Q₁₀ = 3), a maximum change in net CH₄ production of about a factor of 1.3 can be expected (Van Hulzen et al., 1999). Thus, we assumed constant CH₄ production and oxidation rates during the freezing period. Furthermore, other factors controlling CH₄ production such as sediment composition are assumed to remain constant during the freezing period.

p.5; l. 348-349: you mean increased CH₄ concentration?? (confusing with enrichment underneath the ice)

Changed.

p. 5; l. 353-354: add support from the literature?

Done.

p. 6; l.388-389: This is unclear; a GC measure a gaseous concentration that is provided from a headspace of a container/vial/bottle. Why considering here the sample size and headspace volume? I am guessing you are converting back to ice concentrations but this is unclear to readers.

When you thawed your ice cubes in the Nalgene bottle, the gas contained in the bubbles + melted ice became in equilibrium with the nitrogen headspace, and the ratio of water to air is requested to correct for gas displacement and calculate the original concentration in the ice bubbles knowing the water volume. You mentionned that headspace is about half the 1L bottle.

This needs to be clarified (but kept simple of course).

The editor is right, we have converted the detection limit to CH₄ concentrations in ice. We have rephrased the sentence to:

The detection limit of the used GC setup was at about 1 ppm which would equate to about 2×10^{-3} mol m⁻³ for CH₄ concentrations in ice samples assuming a head space volume of about 0.5 l.

p. 6; l. 443: is this the thickness of the layer or the position of the layer from top?

Information added.

p. 6; l. 446-447: The next sentence seems to suggest that this layer is linked to photosynthetic activity (?); what is the gas composition in this layer? is this very poor in CH₄?

The statement on increased bubble concentrations along moss stems documents an observation. These bottom bubble layers were excluded from the CH₄ concentration measurements. These bubbles were often interconnected so that it is very likely that they have lost their original CH₄ content during sampling. Thus, we must limit the interpretation of this observation to the discussion section where we provide possible explanation related to photosynthesis. However, in the frame of this study this point remains speculative. Please note that we have added the following statement to the discussion:

Since it was likely that the ice samples from these highly porous layers have lost their original gas content during sampling, they were excluded from the CH₄ concentration measurements. Thus, the impact of mosses on the net CH₄ production and storage in the ice cover remains unclear.

p.6; l. 510: I am seeing only 2 data points that are above in Fig. 5 (top left) one from a large and one from a small waterbody; are these what you are referring to?? I am not sure to understand this sentence. This needs to be clarified.

Rephrased to:

Two of the four outliers that do not fit into the general exponential behavior revealed very high CH₄ concentrations of up to 0.08 mol m (Fig. 5). The other two outliers only showed moderately increased concentrations of about 0.003 mol m⁻³. All outliers were found relatively close to the top of the ice cover and three of four outliers were observed at thermokarst lakes with surface areas larger than 104 m².

p. 7; Fig.3: the use of a back slash could suggest that you are talking about pond to lake ratio (even if obviously not), so maybe you can use "or" between pond and lake instead?

This comment applies throughout the ms

Changed.

p. 7; l. 617: refer to figure at the end of sentence?

Done.

p. 8; Fig:5: ice sample?

Changed.

p. 8;l. 601: see comment on Fig. 6. This is indeed a depth, not a thickness as indicated in Y-axis title.

Changed accordingly for the entire manuscript.

p. 8;l. 605.606: check english meaning. What process are you referring to?

Sentence changed to:

The consistency between these two studies suggests that both freeze-degassing of CH₄ and CH₄ storage within the ice cover generally occurs in shallow lakes.

p. 9; Fig. 6: Measurement

Corrected.

p. 9; Fig. 6: I think Y-axis identification should be ice depth not ice cover thickness. You did not measure CH₄ as the ice thickness was "growing" but you took CH₄ measurements from different depths in the ice.

When we report data on a water column, we say water depth, not water thickness... Anyway in the text you do use ice depth correctly.

We agree with the editor and changed the wording accordingly.

p. 9; Fig. 6: Does it mean ASP ponds were 1.2 or 0.9 m deep but ice grew only to about 0.6 meter? or it means you did not sample ice lower than 0.6m below the ice surface? It's possible that I would get the answer by reading once more the ms but nevertheless it's always better that figure captions are self-explanatory.

We agree with the editor and added the following explanation to the figure caption:

Sampling was limited to a maximum ice cover depth of about 60 cm due to the length of the used guide bar of the chainsaw.

p. 9; Fig. 7: why is this error bar thicker?

The graphical error was corrected.

p. 9; l. 632-634: But I noted that values provided at line 380 above is super low, if this is what you are referring to = between 2×10^{-9} and 7×10^{-7} mol/m³ = between 2×10^{-6} and 7×10^{-4} μM! (reported as coming from Abnizova et al. 2012, while in this ref. values are = 0.07 - 249 μg/L = 0.0044 to 15.6 μM. There is confusion here. How is this affecting your robustness tests AND absolute values of net production rates? Nevertheless, when I use 0.0025 mol/m³ as a maximal concentration in ice bubbles, and convert it to aqueous concentration (in water at 0.1degC, just above freezing point), I get 0.9 μM which makes sense to my knowledge (I found 0.02 to 25μM at my field site on Bylot).

We agree with the reviewer and have corrected the range of possible initial CH₄ concentrations. We have tested the model with the new range and found the most results to be very robust. However, at two ponds it was not possible the test for the entire range since the generally low CH₄ concentrations would not fit to very high initial values so that the model would calculate negative production rates. A corresponding paragraph has been added to the discussion (please see answer above).

p. 10; l. 657-661: Our ice-wedge ponds indeed are featuring erosion and thermokarst subsidence, but are variably shallow (few cm to <1.5m) and elongated such as your icewedge ponds. I don't think you should compare them to your ASPs as they are morphologically very different and it brings confusion (there is already enough confusion about pond classification!).

In our case, in addition to thermokarstic ice-wedge pond class, we combined the 2 other types (your intra-polygonal class + your merged ponds) into the "polygonal ponds" class based on the fact that neither were showing erosion and both were having cyanobacterial mats (sink in CO₂). In our case, the merged ponds were not showing erosion features.

Maybe you can distinguish your types, when comparing with other studies, based on erosional features (slumping) as you do below? In other words, time stage is rather more complex and not a good point of comparison. Of course, the same applies to next sentence (i.e. avoid this confusing comparison of my polygonal ponds to your ISPs; they do not correspond). I suggest that you remove lines 654-661 completely. It's enough that you mention that your values compare well to mine globally. It takes nothing away from the following sentence were you say that YOUR results provide evidence that the marked difference in net CH₄ production rates between different ponds types are likely to be due to fundamental differences in biogeochemical processes resulting from active thermal erosion.

I would add explicitly the role of organic matter availability. Did you find difference in the amount of OM in your pond types? (in terms of DOM or in sediments?)

By the way Rautio et al. 2011 is a review and do not present such evidence if I recall correctly. You would rather cite a paper from other arctic regions.

We have followed the suggestion of the editor and have removed the comparison contained lines 654 to 661. The reference to Rautio et al. 2011 has been removed.

As a note, in fact, if the difference is quite large in Laurion et al. 2010 data series (2007), with median POL = 0.2 and median ice-wedge (RUN) of 1.8 mmol/m²/d (0.05 and 0.45 if I apply a correction factor for the improper use of wind-based model estimations...), the difference between these 2 types gets smaller when using a larger data set (N=129): respectively 0.6 and 1.9 (or 0.14 and 0.46 corrected values).

We thank the editor for this additional explanation.

p. 10; l. 669: bottom?

Corrected.

p. 10; l. 671: why referring to this study??

Changed. This reference has been removed in the context of changes related to a comment on page 6 and one additional comment on pond vegetation later.

p. 10; l. 672: do you mean methanotrophic?? But it would not be necessary as you already say "CH₄-oxidizing bacteria".

Deleted.

p. 10; l. 677: there is nothing in this section supporting this sentence...

Wrong reference to section has been deleted.

p. 10; l. 685-687: One important aspect to mention is the method used to estimate diffusive summer fluxes as the choice of models (wind-based, ones taking into account heat exchange) or methods (tower, chamber, models) can largely affect the estimations! At least mention how Sachs and Wille obtained their estimations (I think from Eddie tower right?). I found in average 4 times lower chamber flux values than wind based modeled values... And tower estimations integrate over a certain area over the landscape (including ground) so they are not fully comparable to discrete measurements over a pond. I think this comparison is biased. It needs at best further description and justification.

We agree with the editor. This paragraph has been rewritten and further explanations have been added:
The maximum summertime CH₄ emission rates per square meter from the average tundra landscape on Samoylov Island are of the order of $5 \times 10^{-8} \text{ mol m}^{-2} \text{ s}^{-1}$ (60 mgCH₄ m⁻² d⁻¹) (Sachs et al., 2008; Wille et al., 2008). These average landscape CH₄ emission rates were obtained by eddy covariance measurements with typical footprint areas of several hundreds of square meters including ponds and vegetated tundra soils. Thus, these measurements are not directly comparable to the production rates of individual ponds inferred by this study. Nevertheless, the eddy covariance measurements provide a reference value which allows to assess the strength of CH₄ production in ponds relative to a landscape scale CH₄ emission rate. Under this consideration, the early winter net CH₄ production rates per square meter from ASPs are about five times larger than the maximum summertime landscape scale CH₄ emissions per square meter. Considering that ponds occupy about 10% of the tundra landscape, this stresses the importance of ponds and the freezing period to the local carbon cycle.

p. 6; l. 389: I think this needs to be toned down

Sentence rephrased to:

Even during the freezing period small waterbodies can be hotspots of CH₄ production in a tundra landscape.

p. 11; l. 687: I don't think supplement info is cited separately

Supplement reference has been removed.

Additional comments of the editor:

Line 110: "They also found that the CH₄ stored in the ice cover was largely released into the atmosphere during spring melt, and that the amount of CH₄ emitted in spring equated to half of the total annual CH₄ emissions from the lake."

The first part of the sentence is obvious (and implicit in second part) and should be removed: "that the CH₄ stored in the ice cover was largely released into the atmosphere during spring melt, and"

We do not agree. The sentence contains two different informations (a) that the stored CH₄ in the ice cover was released to the atmosphere and not remains within the waterbody. This information is clearly not inherent in the statement (b) that the spring time emission equated to half of the total annual CH₄ emissions. In order to clarify, we have split the sentence into two.

At line 159, you mention that there are plenty of *Limprichtia revolvens* (which has lot of synonyms, like *Scorpidium revolvens* or *Drepanocladus revolvens*) in the tundra ponds. But then at line 670, you talk about *Scorpidium scorpioides* and their influence to methane emissions. Are these two info connected? (but why then mentioning one moss in the beginning and then talking about another not previously mentioned, especially that it's important sp).

Note that we have changed the site description for consistency. Furthermore, we have modified the discussion in order to point out that this is only a side aspect of the study.

Line 191: Verb tenses needs to be homogenized in "We were able to loosely distinguish three types of ponds within the study area" and "On the basis of morphology we distinguish..."

Sentence deleted.