

Interactive comment on “Frozen ponds: production and storage of methane during the Arctic winter in a lowland tundra landscape in northern Siberia, Lena River Delta” by M. Langer et al.

Anonymous Referee #2

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Langer et al presents a CH₄ production during the ice growth period from smaller tundra ponds by sampling ice and analyzing the samples for CH₄. Water column temperature measurements (not presented) have been used to interpolate the rate of ice growth in some ponds. They find at the CH₄ concentration in ponds ice increase exponentially with pond depth and that shallower ponds tend to have a lower total CH₄ storage in their ice column. In addition, they claim the net CH₄ release is related to stable versus unstable (degrading) permafrost conditions.

The manuscript feels rushed and I find that the authors has interpreted their data be-

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yond what the data can justifiable support. For example, no data is presented in regards to the thermal state of the permafrost and no proof of degradation is presented. The manuscript is frustrating to read in its current form as new information and study sites keep appearing, definitions and descriptions are given late, no temperatures are provided (which is the foundation for their analyses!), it is missing clarity at several places including figures, I am lacking an integration between the methane and non-methane aspects (especially in the introduction), and the introduction, discussion and conclusion feel unbalanced compared to the overall theme (results) of the paper. The manuscript reads scattered and is missing a coherent flow where information build upon information in a logical order. I get the feeling there were three people who large contributed to the writing, but the three individual “packages” had not been wrapped into one before submission to the journal. The manuscript provide data of value to the scientific community, but I think the authors need to scale back their ambitions and focus on the data they collected. Some detailed comments are given below.

– Introduction: The bubble paragraph is rather long and it is a jumping the style of writing. It also does not integrate well with the rest of the chapter. Do you need to describe all bubble types in detail?

Define what you mean by pond, and initial stage and late stage pond. I'm missing that in the introduction. Nothing about in in section 1, but its mentioned in the abstract.

P11066, L4-10: “These polygonal structures are present in different stages of degradation. Initial degradation often leads to the accumulation of surface water, either in the depressed polygon centers (intra-polygonal ponds) or along the troughs between the polygon rims above the ice-wedges (ice-wedge ponds) (Wetterich et al., 2008; Helbig et al., 2013; Negandhi et al., 2013).... Both intra-polygonal ponds and ice-wedge ponds are usually very shallow, with water depths ranging from just a few centimeters to a few tens of centimeters.”

Typically ponds in the center of low-centered polygons are not formed due to degrada-

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tion, but rather the opposite (ice wedge growth that pushes the soil, e.g. rims, up). See Lachenbruch 1962 for details.

The authors have not described their particular study ponds in detail. Based on the very general description, which certainly is not specific to their study ponds, it is impossible to clearly understand what types of ponds they are investigating and whether or not their geomorphological assessment is realistic.

A general pond description is not given until page 11067. It is OK, especially with fig 2, but it is too late in the manuscript.

P 11067, L12: Figures should be referred to in order. Fig 2b is discussed before Fig 2a.

L 15-16: "ISPs can be interconnected with other ISPs or with larger waterbodies, but the individual polygon shape is still preserved." Grouping ice wedge ponds and polygon center ponds into ISP makes this statement confusing. Fig 2 does effectively show the interconnectedness between ice wedge ponds, but certainly not for polygon center ponds (Fig 2 says they are ASP). Therefore, it is difficult to envision how connected pond of the latter still maintain the individual polygon shape (especially by looking at fig 2c).

P11068, L7-8: "The pond was transitional between ISPs and ASPs in its level of degradation." What do the authors mean here?

L5-7: "The first temperature profile was from an intra-polygonal pond, based on measurements from four temperature sensors over a depth of 0.4 m." Be specific. At what depths were the temp sensors? When was the water depth 0.4 m? I would think you see some variability in water level over the summer season. Same concerns regarding the other ponds.

P11067-11068: No discussion at all about the different types of bubbles.

P 11070, L27-28: " Thus, the obtained ice profiles were analyzed for occurrence of C5886

bubble layers that were related to air pressure changes." During modeling or measurements?

P11071, L11-12: "...the net CH₄ production rate and effective bubble cross-section can be inferred by fitting the model to measured CH₄ concentration profiles." Please clarify. Profiles of what? The ice via the collected ice samples?

L16-17: "The model was fitted to the measured CH₄ profiles using a non-linear fitting routine provided by MATLAB." Same issue here. The measured CH₄ profiles form the ice samples?

Again, no mention about the classification of bubble types, which were extensively discussed in the introduction.

Fig 3: Only water bodies <1500 m² are represented, but on page 11067 (L8) you write that you studies ponds up to 10,000 m². Why this inconsistency? Is Fig 3 then really representative of your other results and vice versa??

Fig 4: What is the depth of each pond?

Fig 4 and P11072, L12-onwards: For the purpose of finding an average ice growth rate, I do not find it effective to compute a linear average based upon all three ponds, especially if the onset of ice growth varies between each pond (see Fig 4a). Instead, compute the linear regression for each pond or normalize the onset of the start of ice growth. As presented now, you are not presenting average ice growth rate, but rather the average start and end of freeze amongst ponds.

Why the difference in pond-to-pond variability between the two winters?

P11072, L26-30 and onwards: It is unclear what periods the authors are referring to. I suggest highlighting the different periods in Fig 4.

P11073: "During the field campaign clear differences in snow cover thickness of about 20 to 30 cm were observed between the ponds." Unclearly written. Was snow 20 cm

at one place and 30 at the other, or?? Which ponds had deeper snow? If you are to mention/discuss snow cover, then provide details or simply do not discuss it at all.

P11073, L5: Did you study these lakes? If not, write so. And what does “larger” entitle?

P 11073, L21: Bottom of what? Clarity please.

P11073: The paragraph describing bubbles needs a supporting figure that shows the bubble distribution, especially if this bubble distribution is important to the rest of the paper.

P11073, L10: ISP2 and ISP3 pond?? This is the first time ISP 2 and 3 pond are mentioned. There is only an ISP1 pond in Fig 4.

L16-18: “The consistent occurrence of these thin bubble layers in similar depths and different ponds indicates a formation related to air pressure changes.” Needs clarity. Are all these three ponds of similar water depth? Looking at Fig 4, there is quite a difference in both onset of ice formation and timing of complete freeze-up, eg bubbles at the same depth between ponds may not represented the same date.

L6-27: Lack of consistency. Use either the bottom of the pond (sediment-water inter-phase) as the reference or use the top of the water (ice) surface as the reference. As written, it is unclear at places.

P11074, L23-24: What does “close to the bottom” mean? In cm.

P 11074, L24-26: “The lowest CH₄ concentrations were recorded in the ice columns from large thermokarst lakes. In these lakes only the uppermost part of the ice cover was sampled relative to the maximum lake depths.” Isn’t there a bias introduced to Fig 4 and the results if the larger lakes only had the top part of the ice cover analyzed for CH₄? If only the top ice was sampled at larger (what does “larger” mean??) lakes, then you need to be careful in how you refer to the results.

P11075: How ponds were divided into different categories (ISP versus ASP) is a defi-

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inition that should be provided earlier, not wait until this time. Also, the wording is a bit funny, making it read like a circular statement.

L 7-8: “The maximum CH₄ concentrations measured in the ISP1, ISP3, and ASP3 samples were about one order of magnitude higher than those from the other profiles.” What does “other” refer to? If it is “larger lakes”, then I do not find the claim justified as the “larger lakes” only had the top ice measured for CH₄. Why were not the entire ice column measured for CH₄ in the larger lakes?

Fig 6: Is each sub-figure supposed to be an individual pond?? Or are they all “ASP 3”??

Fig 7: Suddenly there is an ASP4 pond. It is frustrating to read the manuscript when new ponds and new results keep appearing without the pond being introduced and described.

P11076-11077: A larger portion of the discussion is devoted to future changes. Nowhere does the manuscript address any future changes and studies of future conditions. The manuscript addresses the current system and the variability in ice growth rate and CH₄ production during the ice growth period of differing ponds. I find it a stretch to devote a large portion of the discussion to future changes and conditions. It comes across as hand-waving. A large portion of this first section in the discussion reads as a literature review too. Focus on the results in this manuscript!

P11079, L1-2: Poorly written. Should state “uniformly distributed CH₄ concentrations and a constant rate of bubble accumulation, could also affect the simulated net CH₄ production rates.”

P11079-11080: “This is half an order of magnitude less than the winter net CH₄ production rates from ASPs...” No sediment temperatures were provided. The study did not include the entire winter. I find it inappropriate to refer to this study as representing “winter net CH₄ production”

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P11080, L12-13: Why does the CH₄ increases exponentially with depth?

L15-16: “..in ponds showing signs of erosion in the surrounding permafrost..” What does “surrounding” refer too? Lateral/pond sides? Below pond etc?

L20-21: “..Ponds therefore make a significant contribution to the greenhouse gas emission budget of the tundra” This is just thrown out from nowhere. Add a justification how important its contribution is to the rest of the tundra if you are writing “significant contribution”.

L23-24: “thermal state and stability of the permafrost” Nowhere are we presented with proof of degradation (change in soil (permafrost) temperature over time etc). How can you then state that these ponds are degrading?

L25-26: “..show signs of thermal erosion in the surrounding permafrost..” Again, the authors are making statements that are not supported by data.

P11081, L4-6: This conclusion is not supported by any data in this manuscript. Remove.

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