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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 #1

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General Comments

This is an interesting manuscript which provides observational and inverse modeling results of methane storage during freezing of ponds and thermokarst lakes in a Siberian permafrost landscape. The overall work is a valuable contribution, and shows a large difference between CH4 emissions into the forming ice during pond freeze-up, apparently due to differences in permafrost degradation state around the ponds. Such permafrost degradation states are expected to change in a warming Arctic climate. The paper is well-written and generally easy to follow. It is suitable for publication in

Biogeosciences with minor revisions.

Specific Comments

In the abstract, lines 28-34 are a statement that advanced (degradation) state ponds show higher production rates of methane than from the “tundra landscape” at the study site. It’s difficult to tell precisely what the authors mean by this. Do the authors mean that the CH4 production in the lakes during freeze-up is faster/greater than from the same lakes in the summer/ice-free period, or do they mean that the CH4 production in the lakes during freeze-up is faster/greater than the CH4 emission from the entire local landscape (lakes + tundra). Or only the tundra? Are the authors trying to say that the lakes are a larger local source than the tundra? I think the authors mean the entire local landscape, as a similar but clearer statement is made in the conclusions (line 698).

Most of the units shown are those preferred by modelers rather than experimentalists; it would be helpful to provide the important flux values in units such as mg CH4 / m2 / day, at least occasionally, such as in the results. This aids in comparison with other papers in the literature.

I am most concerned with the extrapolations (explicit and implied), given the sampling method (13 sample ice blocks from 8 ponds and 1 lake, line 247). Previous work has shown high spatial variability of ebullition over single, similar lakes. Especially as the CH4 emission rates calculated for the study lakes was found to be so variable, the low number of samples is troubling. I wonder if we are seeing a sampling bias here? I understand well the difficulty of obtaining these samples. However, I think the authors should address, probably in the methods section, how they believe their sampling distribution is adequate for characterizing these ponds.

Line 720-724: The first sentence is true, but the second sentence does not logically follow from the first. Ponds make a significant contribution to the budget of methane in the atmosphere regardless of the freeze-thaw cycle. I suggest deleting the second

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sentence (“Ponds therefore make...”).

Line 737: I am not sure that the final statement in the conclusions that warmer winters may prolong the CH4 production period in ponds is well supported. Other studies (in different lakes) have shown that CH4 production and ebullition effectively shuts down well before lake freeze-up due to low, but above freezing, sediment temperatures. For example: Wik et al., JGR 2013, doi: 10.1002/jgrg.20103 Of course not all lakes are the same, but the statement seems too general as currently written.

Finally, because the lake/pond water and lake sediment during freeze-up period is cooler than in the ice-free summer period, the CH4 production in sediment is almost certainly lower during freeze-up, regardless of the lake-edge degradation state explored in this manuscript. It appears from the manuscript that this decrease in CH4 production is not observed in the study lakes, correct? This seems surprising to me, and this difference should be noted in the manuscript. One would expect that the methane trapped in ice during freeze-up and released at ice-out in the spring is likely being produced at lower rates than in the summer season. If possible, explain this surprising result. (To be specific, this is surprising because colder sediments produce less methane; e.g. Zeikus, J. G. and M. R. Winfrey (1976), Temperature limitation of methanogenesis in aquatic sediments.)

In my opinion, what is most important in this manuscript with respect to greenhouse gas releases from Arctic thermokarst lakes and ponds is that the degradation state of the permafrost surrounding thermokarst lakes may impact the magnitude of the burst of methane released from the lakes at spring thaw. This spring release has been difficult to model, and the model and results here may point to a way of doing that.

Technical Corrections

Section 5.1, should be broken up into paragraphs. Suggested break spots: line 577 “However, this study has demonstrated...” line 581: “Detailed investigations of the surface energy balance..” line 599: “The survey of waterbodies also revealed...”

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Line 22: “then inferred” should be “were then inferred”

Line 62: “the Lena River Delta more than” should be “the Lena River Delta, more than”

Figure 5: Could the authors draw figure 5 without black circles around the data points? This would make the color points appear less cluttered and make them easier to see.

Line 464: “are show in” should be “are shown in”

Interactive comment on Biogeosciences Discuss., 11, 11061, 2014.