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# *Interactive comment on* "Open system pingos as hotspots for sub-permafrost methane emission in Svalbard" *by* Andrew Jonathan Hodson et al.

**Anonymous Referee #2**

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The manuscript 'Open system pingos as hotspots for sub-permafrost methane emission in Svalbard' by Hodson and co-workers report concentrations of dissolved ions and gases as well as water and methane stable isotope signatures in spring water of four pingos in the Adventdalen valley, Spitsbergen. The springs were sampled several times between 2014 and 2017. The authors use the obtained data to derive information on the sources of the sampled ground water and of the detected methane, which is present in surprisingly high concentrations. Furthermore, they estimate the contribution of the spring water methane to total atmospheric methane fluxes from the Adventdalen valley.

The topic of the paper is of high relevance and I read it with great interest. It's concise and well written, but it could be improved by adding some background information.



Furthermore, the discussion is in large parts very speculative due to the lack of data.

The development of an open system pingo should be explained in more detail. It was unclear to me, how liquid water may find it's way through the permafrost. What is the temperature of the permafrost? Are these open system pingos particularly developing above marine sediments? What is the difference to a 'normal' pingo? It's particular difficult to understand since the cited reference (L57) is not given in the list of references.

Furthermore, more background information should be given on the geology of the study sites, including the geology of the surrounding mountains that may affect the composition of the spring water. Is there a connection between the springs and fresh melt water (as suggested in lines 275ff)? Is there a talik below the river and might this be connected to the springs? Furthermore, more background information on the potential source of the methane in the spring water should be given. The authors differentiate biogenic and geogenic sources. However, it should be made more clear which geogenic sources might be present, gas hydrates or natural gas from deep deposits? Is there information on these sources in the region, and what is the carbon stable isotope signature of these sources? Concerning the biogenic source, it should be explained why high methane concentrations in marine waters are expected. Generally, no methane is produced as long as sulfate is present. In contrast, methane, e.g. from gas hydrates is oxidized with sulfate as electron acceptor.

The discussion concerning the methane sources is rather speculative due to a lack of data. The carbon stable isotope signature is only a weak indicator for differentiating geogenic and biogenic methane sources. If only the carbon stable isotope signatures of methane are available and no delta D or concentrations of further hydrocarbons, as in this manuscript, no differentiation between geogenic and biogenic sources is possible. E.g., gas hydrates may have carbon stable isotope signatures between about -40 and -70‰ a range covering the whole values given in this manuscript.

But the weakest part of the discussion is the part on the pingos as methane emission hotspots. The authors derive spring water fluxes from an unpublished study on Adventdalen's groundwater system, add unpublished data on methane concentrations in a 'neighboring lake', which contributes about 1/3 to the total flux estimate and assume that 100% of the methane in the water will be emitted to the atmosphere. Estimating methane fluxes from water concentrations comes along with high uncertainties. It might be possible for pond, lake or sea water. However, in soils, bacteria will likely oxidize a large fraction of the methane as soon as oxygen is available. Hence, methane fluxes will likely be much lower. To derive meaningful data on methane fluxes from soil surfaces, emission measurements should be conducted. Furthermore, there seems to be a mistake in the calculation of the land fluxes from Adventdalen valley using the Pirk et al. (2017) paper and the active layer fluxes seems twice as high (see specific comments) as given in this manuscript. In this case the relative contribution of the sub-permafrost fluids is reduced by 50%.

Concluding, I suggest changing the title of the manuscripts, since it indeed does not measure methane emissions. Furthermore, I would downplay the calculations of methane emissions and more clearly consider their uncertainties. The authors mention that it is only a 'crude' estimate, which is correct. In this case, this crude estimate should not be in the focus of the manuscript by mentioning it in the title and elaborating it over more than half of the discussion. The authors may dicuss the emissions in a more qualitative way and also include information about the abundance of such springs, if available.

Finally, the reference list needs attention.

Specific comments:

L31: This quote does not fit here very well, better cite particular studies that are 'quantifying the release of methane from the active layer during summer thaw' and not a general review on the permafrost carbon feedback.

L57: This reference is not given in the list of references



L138 ff: How were gas pressures measured in the vials and which CH4 solubility was assumed?

L152: Please give the standards used for methane and CO2 $\delta$13C measurements.

L214: Excess CO2 seems to correlate with methane concentration not it's variation.

L225f: What means 'overlaps closely'?

L239ff: The last part of this paragraph belongs to the discussion.

L258ff: The 'distal' samples not only seem to be different but they very obviously are.

L265ff: I find this paragraph confusing and the conclusions not convincing. It is indeed counterintuitive to expect that the influence of marine waters are higher the farther one comes from the sea. Furthermore, this conclusion is only supported if a part of the dataset (distal samples) is omitted from the analysis, but there is no justification given to do so. Furthermore, it is unclear why the system is more diluted downstream by fresh groundwater from snow and ice melt. I understand, also from Fig. 4 that the sampled water originates from below the permafrost. In this case, the up-valley sites should be more influenced by melt water.

L286ff: The explanation of the variability in CH4 stable isotopes is unclear. Why should CH4 oxidation preferentially take place while the fluids are trapped below an ice lid and not during it's transport to the surface or after surface thaw? To oxidize methane, an electron acceptor is needed, the respective microorganisms and liquid water but not stagnant water. And what might be the electron acceptor for methane oxidation? The fluids seem mostly oxygen free and low in sulfate.

L310: What means 'favorable thermodynamic conditions' in this context? Favorable for which process?

L317ff: I understood from L286ff that the springs are frozen in winter. Please clarify.

L 331FF. Please give the reference for 'this paper'. Furthermore, clarify to which paper the newly introduced data from the ‘neighboring lake’ belong.

L345: Pirk et al., 2017?

L347f: This calculation neglects aerobic methane oxidation, which might oxidize up to 100% of the methane before it is released into the atmosphere. Hence, the flux assumption from the springs is the upper limit of methane fluxes from the springs.

L363: Pirk et al. (2017 not 2018) report ‘typically . . . a . . . seasonal budget of around 2 gC m-2’ (not 1 g C m-2) for the summer thaw season (1st June to 30th September) in Adventdalen. According to my calculation the annual flux from 4.7 km2 would then be about 12,600 kg methane yr-1 (not 6,040 kg methane yr-1). Furthermore, the winter fluxes are not considered in these estimates, which might be as high as the summer fluxes (see Zona et al., 2016).

L375f: The meaning of this sentence (‘The sensitivity. . .’) is unclear.

L376ff: I cannot follow this calculation. Where does the number 50 L sec-1 come from? What is the Adventdalen terrestrial methane flux? In addition, why compare the total annual runoff of Adventdalen with the groundwater flux of 50 L- sec-1? The authors are aware that the methane concentration in surface melt water is several orders of magnitude lower than what they found in the springs with sub-permafrost fluids. This comparison is without meaning.

Table 2: Please also differentiate the “distal” samples from the River Bed Pingo

Fig. 2 is difficult to read. Please give references for the published pore water data and please use units that make a comparison with the data in the tables possible (e.g. mg L-1 not $\mu$L mL-1)

Cited references:

Pirk N., Mastepanov M., Loez-Blanco E., Christensen L.H., Christiansen H.H., Hansen B.U., Lund M., Parmentier F.J.W. et al. (2017) Toward a statistical description of



methane emissions from arctic wetlands. Ambio, 46, S70-S80.

Zona D., Gioli B., Commane R., Lindaas J., Wofsy S.C., Miller C.E., Dinardo S.J., Dengel S. et al. (2016) Cold season emissions dominate the Arctic tundra methane budget. Proceedings of the National Academy of Sciences of the United States of America, 113, 40-45.

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