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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.

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We thank the reviewer for the comments and agree with some, but not all of the criticisms raised. There are also some good suggestions made that could improve the paper. Here we reply to the general comments raised, rather than the specific ones.

The referee comments about the lack of data and the speculation that materialises in the Discussion. We wish to point out that there is a necessity to speculate to some degree on account of the great lack of studies pertaining to fluid flows beneath continuous permafrost. van der Ploeg (2012) describe it as "the fierce data scarcity of subpermafrost groundwater systems". We are therefore describing an important, yet largely unknown fluid migration pathway. Our system is in fact data-rich by comparison

to other sub-permafrost environments, but we still lack the kind of access enjoyed by researchers studying methane dynamics in wetlands, the active layer or other near-surface environments.

The referee asks for a better description of the formation of open system pingos and also the hydrogeology of the valley. We point out that these processes form the basis of Hornum et al's submission to The Cryosphere, presently available as a Discussion paper (Hornum et al, 2020) and cited in the field site section of our paper (line 82). The characteristics and relationships between isostatic uplift, fluid flow and the geology are discussed at length by Hornum et al, and we can therefore make the links between these papers better for the reader.

We regret the mistake with the reference given on line 57. It is given as Hodson et al (2018) and should in fact be Hodson et al (2019), as given in the reference list. The paper describes open system pingo formation at length.

There is no talik beneath the river. It is a good suggestion to include this information in a revised version of the paper.

More background on biogenic and geogenic methane is requested and it is asked whether the geogenic methane is likely to be hydrate-derived or natural gas from depth. We are able to comment upon this given our earlier work on the thermodynamic stability of gas hydrates in the region (Betlem et al, 2018). This clearly show that hydrates are no longer stable in the valley bottoms, yet they might persist in the mountains. However, the issue opens a can of worms, because the rate of hydrate dissolution and the fate of the gas produced is unclear. Therefore, since hydrate is not a source of CH_4 but a store, we do not see the need to write much about this. The issue is covered to some extent by Hornum et al (2020) though, and we can allude to this and Betlem et al (2018) if deemed necessary.

The reviewer asks whether there is information on the geogenic sources, which we are happy to enhance following a recent publication. However, we already discuss Huq et al (2017) on p11 and 12, who makes it clear that: i) a mixture of both biogenic and geogenic methane exists beneath the permafrost, but ii) only biogenic methane is found immediately beneath the permafrost in the aquifer that provides runoff to the pingos.

The reviewer raises a good point about our assumed links between biogenic methane and pore water flow through marine sediments being unlikely on account of there usually being abundant SO4 in these sediments to enable sulphate reduction to outcompete methanogenesis. We made the point because Cl correlates with methane concentrations. We now think other processes could cause this and so we can make the necessary changes.

The reviewer implies that our discussion about methane provenance is weak because it is entirely based upon 13C-CH4. The reviewer then implies that we cannot discriminate between geogenic methane, biogenic methane and hydrates. But hydrates are not a source of methane – they are a transient store of either geogenic or biogenic methane (or a mixture of the two). Our paper therefore seeks to present the following logical narrative that we believe is entirely robust:

a) The source is either geogenic or biogenic. It doesn't matter if it was from a hydrate or not (although this is clearly an interesting question that we are able to address through modelling)

b) The 13C-CH4 can rule out geogenic when the values are low, and we have many low values that fall outside this "geogenic range".

c) An earlier study of methane in pore spaces conducted at our site uses CH4 concentrations, 13C-CH4 and the presence of other hydrocarbons to establish the relative abundance of biogenic versus geogenic methane from the surface down to ca. 900m (Huq et al, 2017). Our figure 4 presents these data down to 500 m. This work does not require delta-D because the presence of other hydrocarbons is a reliable indicator of geogenic CH4 in the region (Huq et al, 2017).

d) The above study shows that there is a methane source in an aquifer immediately below the permafrost that is largely biogenic. Geogenic methane is found at greater depths (> 300 m: see Fig 4) and might not be able to migrate upwards due to the geology of the site

e) The biogenic methane inferred from Huq's study was also found by a mining company, who reported a salty groundwater body with a 13C-CH4 range (-48.9 ‰ to -52.9‰ almost identical to that found at our nearby pingos sites (ie River Pingo and Innerhytte Pingo: -49.7 ‰ to -57.8‰ as in Table 2). Their reported salt content was 1500 mg/L, which is also almost identical to that found at these two pingos (1380 – 1540 mg/L: Table 1).

f) At the other two pingo sites, the 13C-CH4 values either lie within the same range as the above, or are too low to be geogenic.

g) We therefore conclude that there is no evidence for geogenic methane in our springs.

The reviewer suggests that the weakest part of our manuscripts is where we explore the emission fluxes. We tend to agree but feel that their potential significance should still be addressed. We don't mind attempting it qualitatively, as proposed. We thought about this earlier, but then decided that a quantitative argument would be a reasonable demand from any likely reviewer. However, we do not agree with some of the criticisms directed towards our emission estimates, and wish to make the following points in defence of our work:

i) Methane consumption in soils "will likely oxidize a large fraction of the methane as soon as oxygen is available". We point out that soils are frozen for much of the year, yet the springs we study are constantly discharging. Furthermore, we seldom find the springs infiltrating into soils. More often, the springs erode turbulent channels through impermeable marine clays. Such flows are more conducive to rapid degassing to the atmosphere and therefore little methanotrophy. However, at pingos where lakes form upon their summit then methanotrophy is more likely. The impact of this process is already discussed (line 310 onwards) and accounted for in our estimates.

ii) “To derive meaningful data on methane fluxes from soil surfaces, emission measurements should be conducted.” We do not wish to derive such data. We are studying point sources of methane that by-pass the soil environment. Maybe we could include photographs of the sites to clarify this?

iii) “The authors derive spring water fluxes from an unpublished study on Adventdalen’s groundwater system. . .” This study is Hornum et al, as cited and therefore available to the reader as a discussion paper. It provides a lot of necessary back ground data, as requested above.

iv) “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.” The reviewer continues in their specific comments with: “typically...a...seasonal budget of around 2gC m-2 for the summer thaw season. . .” This is a quote from the Pirk et al paper. Unfortunately, the quote refers to the median of two sites: Adventdalen and Zackenberg (in Greenland). The emissions from Zackenberg are greater than those from Adventdalen. Quick scrutiny of Figure 5 in Pirk et al (2017) clearly shows that all of the median values at Adventdalen lie below 2. It is therefore hard to justify using 2 gC m-2 y-1 as a spatially representative value. For this reason, I digitised Figure 5 in Pirk et al and determined the minimum and maximum median values from their three year study. These values were used to produce the range of likely emissions from wetlands for comparison with our emission estimates from springs. This range is presented in Table 3.

v) “Furthermore, the winter fluxes are not considered in these estimates, which might be as high as the summer fluxes (see Zona et al., 2016).” The Pirk et al (2017) study does infact include the freeze-up processes that were emphasised by the Zona et al (2016) study. After this period, the (late) winter emissions in Adventdalen have not

been studied much, although Pirk et al (2016) did some pre-melt chamber work one April/May and found great suppression of the methane flux by icing layers. Where such layers were less prolific (in Zackenberg again), the winter fluxes were one to two orders of magnitude lower that those before the end of freeze up.

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