The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-11-AC1, 2020

The Cryosphere Discussions

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Interactive comment

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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Received and published: 30 March 2020

We thank the author of these comments and note that they raise an interesting point about the diffusion-based approach to flux calculation. Below we address the reason we did not employ such an approach in our paper. However, before we do so, we also point out that we have already used it for one of our sites where the local conditions make this approach entirely appropriate (i.e. Lagoon Pingo: See Hodson et al, 2018 in the reference list).

First, our manuscript acknowledges that it "crudely" estimates the methane release from pingo waters to the atmosphere. It does so by assuming that all waters achieve equilibrium with the atmosphere due to turbulence and freezing effects. These are

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discussed further below because both these effects render the diffusion modelling approach unsuitable.

Secondly, the more sophisticated diffusion approach is almost impossible to employ, because it requires environmental data (wind speed, temperature etc) that are extremely variable.

Therefore, in defense of our approach: A) Pingo waters are not similar to open ocean, shallow lakes or river waters, because: i) Often pingo springs are shallow (<5cm deep), turbulent and sometimes gas-saturated flows that are unlikely to behave like deeper surface waters in lakes and the ocean surface. ii) Seasonality strongly affects water bodies of springs (extent and depth of water body). In summer, some of the pingos develop shallow pools and slow moving springs where the diffusion modelling approach is certainly appropriate. This applies to the two sites at Lagoon Pingo for example (hence our earlier manuscript in Frontiers). However, for the rest of the year, the flows discharge from cracks in an ice lid and form an icing that is constantly changing. iii) Freezing and hydrostatic pressure forces pingo waters through the ice lid and into the atmosphere. Sudden, turbulent drainage events occur throughout much of the year (say 8 – 9 months). These turbulent flows conditions are not at all similar to those where the Wanninkhof equations that underpin the diffusion approach may be applied iv) Ice formed in methane rich waters during freezing is generally quite reduced in methane (Langer et al 2015, Biogeosciences, 12, 977–990, doi:10.5194/bg-12-977-2015). In other words, exclusion of the dissolved gas from the ice during freezing increases the concentration of methane in the residual water and thus enhances the emission into the atmosphere. This is not east to accommodate into the diffusion modelling approach

B) Not all pingo springs have been identified or their flows, or methane content, properly quantified. High exposure environments near pingo summits (high wind speeds) means that predicted values are likely to be substantially underestimated since wind speeds are not well known for these sites (See Wanninkhof 2014)

C) Supersaturated waters may not behave as predicted from sub-saturated ocean waters. In some cases (e.g. Innerhytte Pingo) ebullition is the dominant process at times (ie not diffusion), which is why the samples exceed the solubility limit here. A more appropriate flux method here could involve the use of chambers, however, these would be extremely difficult to employ, and add a further methodology to hamper comparison.

D) Turbulent flow from pingo springs are likely to mean that diffusion-based approach is not appropriate for this system.

We would be happy to improve our paper by discussing these issues and regret not making the methods and their justification clearer.

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