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Interactive comment on "Thermokarst-lake methanogenesis along a complete talik profile" *by* J. K. Heslop et al.

Anonymous Referee #2

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In this manuscript the authors present data of talik CH4 production potentials on different depth along a 590cm sediment core. They also determined the CH4 production potentials on ice-rich yedoma permafrost soils. The results nicely show that the CH4 production potential was the highest in the organic-rich surface sediments but also in the recently thawed permafrost in the bottom of the talik. Low or no production of CH4 was found in sediments being thawed for longer periods of time and in permafrost soils.

The study is timely and it covers a topic that we know very little about. We know that thermokarst lakes are hotspots of CH4 but we know very little about where the CH4 is produced and what is the substrate consumed. This manuscript hence adds new and valuable knowledge to the scientific community. Although the manuscript contains is rather technical sound and feels quite long it still reads easily.

Methods: The method section is rather long and detailed but reads quite easily. The sampling, preparations and the incubations seems to be preformed correctly. Something for the authors to think of is if parts of the method can be written shorter? For example; the measurements of the magnetic susceptibility is very detailed (and rather long) described. It is however not clear to the reader why these measurements are important. It is mentioned in the results, but the discussion is not based on this data and no conclusions are drawn from these results?

Results, discussion and conclusions: The authors discuss/conclude that most the production of CH4 occurs in the organic C rich mud. The main reason for high CH4 production is hence relative high OM content. This is rational and not surprising, as also stated by the authors. Most interesting is however the questions that these result rise:

What is the role of allochthonous vs. autochthonous C sources (briefly discussed on L8 – p4881)? Fig. 6 show that CH4 production normalized per unit Corg also is the highest in the surface sediments (which consists of both allochthonous vs. autochthonous C). Is this only due to that recently deposited is more labile to methanogens and/or is there also a priming/fertilizing effect? What would for example happen if for example autochthonous C was mixed into the incubations of permafrost soils? The authors further touch this at L16-24 (p4884) where they discuss if the high CH4 potentials in Vault Lake is due that the sediment is a mix of biolabile OM, Holocene aged OC and in lake primary produced C.

Another interesting dissuasion is the role of modern and/or ancient microbes for CH4 production (see L17 and forward on p 4885). It is possible that the authors would have found a different result when incubating the permafrost soil if microbes from the surface sediments were inoculated.

Although the authors are careful to draw conclusions (which is good) and this paper leaves many interesting questions unanswered, this paper will still be a valuable contri-

bution to the scientific discussion. The new questions raised are sometimes the most important conclusion.

Minor: L9 p4876: Mean depth is not a result of this study and already mentioned in section 2.1 L25 p4878: Maybe put brackets around “R”? L25 p4884: This section is hard to follow, especially since it refers to the next section.

Interactive comment on Biogeosciences Discuss., 12, 4865, 2015.