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Interactive comment on "The effect of warm summer 2012 on seasonal and annual methane dynamics in adjacent small lakes on the ice-free margin of Greenland" *by* Sarah B. Cadieux et al.

Anonymous Referee #1

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Summary

The manuscript describes a study on dissolved CH4 concentration in five arctic shallow lakes located in Greenland. They used here five data sets (from summer 2012 to winter 2014) on the Southwest ice-free margin of Greenland. The aim of the study was focused on the effect of one high warming event occurred in summer 2012 on CH4 concentration profiles and compared it with subsequent years (2013 and 2014). The study of CH4 dynamics in lakes is a topic of broad scientific interest as lakes represent an important source of this gas to the atmosphere.

I recognize that it is a difficult task to study lakes in these extreme environments, and data coming from them are therefore valuable. The manuscript is not very clear in

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demonstrating how the warm 2012 summer influenced CH4 dynamics in these lakes. Even, this study shows minor effects of the 2012 warm summer on CH4 dynamics (showed in Figure 7), and it is very difficult to correlate the minor effects to any particular phenomena (showed in Figure 8).

Likewise, the authors should always make clear when data have been previously published. I was surprised that several data in Tables, Figures (Figure 6 and 7) and Map (Figure 2) are the same (or at least very similar) than those reported in another manuscript from the same authors (Cadieux et al. 2016); and no reference is made to that previous study (and/or indicated in those tables and figures). I also want to point out, that there are strong similarities in the DOC and pH data presented in Table 1 and Table 2 (for DOC) for open-water conditions 2012 in this manuscript and data presented in Table 1 for open water conditions in 2013 from Cadieux et al. (2016).

The manuscript is well written, although some sections are not totally mature yet and therefore the manuscript lacks a clear focus and structure. I think that some of the analysis are speculative and/or over-interpreted and numerous issues in the method section must be better addressed.

Specific comments

The introduction contains mixed statements related to temperature effects on CH4 production/oxidation/storage in the water column (e.g. temperature dependencies on CH4 production is described in two sentences in second and fourth paragraphs). I would recommend reorganizing the ideas to improve the introduction flow (which should go from general to specific).

Likewise, it is necessary to carefully review the literature to avoid controversial statements like the authors indicate at the end of the introduction “This work provides the first measurements of dissolved CH4 concentrations under both open-water and ice-covered conditions for consecutive years in small, Arctic lakes”. From the literature that I know (and for sure I am missing a vast amount of studies), there are previous studies

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or multiyear dissolved CH4 concentrations in water column, in similar latitudes. Some of these previous works measured dissolved CH4 concentration through and over several years. I suggest some readings: Kaankala et al. (2006), Bellido et al. (2009), Karlsson et al. (2013), Greene et al. (2014), Miettinen et al. (2015), Tan et al. (2015), among others.

The description of the methods is the most important section to understand what the authors did. This section has to be improved substantially. Firstly, I found a number of cases in which devices or sample preparation are not full described (e.g. electronic submersible pump, total organic carbon analyzer, passive diffusion bags PDBs, HCl concentration, dilution correction for CH4 measurements). Secondly, littoral sediment CH4 bubble sampling method (used in this manuscript) is a very unspecific method. While in Cadieux et al. (2016) the method was used in combination with the isotopic analysis (isotopic values are helping to understand CH4 dynamics), in this manuscript, values of CH4 are given without determining the volume of sediment samples (as commented in the method section). Therefore, what is the point to include very speculative values of CH4 concentration from the littoral. Thirdly, I consider it would be necessary to describe briefly the methods, even if they are previously described (Cadieux et al. 2016), to avoid excessive self-citation and tedious reading. Finally, the statistical analyses need to be clarified. Some of them does not make sense, as written, and specific information is required to understand how data analysis was made e.g., mean/median temperature and CH4, profile values, seasonal, sectional.

Through the results and discussion section some Figures are used to explain variations and significant differences between lakes. In data from Figure 7, it is impossible to note the range reported in surface waters and depth axes are missing in some sub-figures (making impossible to see clearly the depth profile). Moreover, in data analysis from Figure 8 (wrongly named Figure 7 in Page 9, Line 307), it is impossible to see when CH4 vs. DO and CH4 vs. T are related or not. Likewise, some discussion sections are not well focused on the main issue and over interpret results. Some examples are:

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i) “The competition for substrates favors sulfur reduction (SR) and methanogenesis typically does not occur until SO42- is exhausted and SR rates have decreased (Lovely & Klung 1983, Lovely & Klung 1986, Scholten et al., 2002, Ward & Winfrey 1985). However, EVV Upper lake did not have the lowest concentrations of CH4 in the water column, suggesting there was sufficient reduced carbon substrates to fuel both SR and methanogenesis. Therefore, while aquatic chemistry in the water 320 column may be a factor influencing CH4 production, it alone is insufficient to explain the variation in CH4 concentrations observed lake-to-lake, as well as seasonally and annually.”

ii) all section “6.3 Effects of temperature on CH4”, and

iii) you don’t have thorough information on the ice phenology to indicate that “Our results suggest that changes in the duration of seasonal ice cover will, in turn, result in changes in inventories of under-ice CH4. As the duration of ice cover decreases, the amount of CH4 stored under ice 455 cover will likely decrease due to the shorter time for accumulation, potentially reducing the amounts of CH4 emitted during ice-breakup and spring overturn.”. I think, the results are not reliable to support such statements.

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Tan, Z., Zhuang, Q., Water Anthony, K.: Modeling methane emissions from arctic lakes: Model development and site-level study, J. Adv. Model. Earth Syst., 07, doi:10.1002/2014MS000344., 2015. (note: for sure to develop the models, they used multi-year dissolved CH4 concentration data).

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