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Interactive comment on "The net exchange of methane with high Arctic landscapes during the summer growing season" *by* C. A. Emmerton et al.

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General comments

In short, it is a very good manuscript, with solid and valuable data, clear results and well written discussion. I recommend it for publication with minor changes, which I suggest and discuss below. Despite a couple of them being critical, they do not impugn the value of the manuscript and do not change its main conclusions.

Discussion and specific comments

The manuscript represent a very valuable study of CH4 fluxes at a remote high-Arctic site. Three closely related datasets are reported: static chamber measurements of CH4 exchange at an Arctic desert site (2+2 plots, 5 summer seasons, 4-7 measurements per season); static chamber measurements of CH4 exchange at a wetland site (4 plots, 3 summer seasons, 4-7 measurements per season); and EC measurements of CH4 exchange at a wetland site (1 spring+summer season, almost continuous dataset).

Despite a limited number of measurements, both in time and in space, net CH4 fixation at polar desert site seems consistent and convincing. This dataset is quite unique and very valuable both per se and for estimating, modeling and predicting any wider Arctic CH4 balance. I agree with the authors' discussion and conclusions upon this dataset and have no doubts it deserves the publication.

The dataset for wetland chamber measurements has, in my view, relatively smaller value. The constrained marginally location of the plots, small number of replicates, apparently expressed interannual variability of the fluxes, especially in the second part of a growing season (when only two years of measurements are available) make the interpretation of the data very hard and unsure. The measurements in Artcic wetlands are much more common than those in Arctic desert; as the authors note, many published studies report higher CH4 fluxes comparing with the ones found in the current studies. Also, to my knowledge, a more common seasonal pattern of CH4 fluxes (a positive flux starting soon after snowmelt and peaking sometime in the middle of the growing season) differs from the one found in the current study (Fig.3). EC measurements (Fig. 4a) do fit this "classical" pattern, which supports the idea that the wetland plots are representative only for a very specific marginal location, affected by a local stream flow (page 1883, line 19). Saying this, I do not argue against including wetland chamber measurements into the paper; the authors might consider some wording in the abstract (lines 9-10) acknowledging that the numbers for wetland are less certain that those for desert. By the way, line 7 (the abstract) gives a wrong impression as well: "we made static chamber measurements at both locations over ïňĄve growing seasons" does not fit with the rest of the manuscript where only three years of measurements for wetland location are presented. I would suggest the authors to read the abstract again, critically thinking what would they really want to flag in this publication.

The EC dataset is indeed very clear and valuable. The (relatively) new Li-7700 open-path CH4 analyzer is really appealing to be used for studies in remote Arctic; it is implemented by a number of teams, however published works are still very limited (see for example Sturtevant et al, Biogeosciences, 9, 1423–1440, 2012; Sturtevant and Oechel, Global Change Biology, 19, 2853-2866, 2013.) One of the issues that are not clear for the moment (and is very essential for applications in high latitudes) is how much the instrument heating can affect the measurements (Burba et al, Global Change Biology, 14, 1854-1876, 2008). This effect has been studied for Li-7500 and corrections have been developed. Li-7700 is much bigger, and what kind of artifact can it produce, is not clear yet. This might be mentioned in the discussion. Otherwise the flux pattern (Fig.4a) seem convincing, despite gaps in the data and 43.8% data removal due to quality issues (Page 1680 line 28).

What – in my view – has a very questionable value and dilutes the otherwise high scientific value of the manuscript, is the modeling of 2010 and 2011 CH4 fluxes using empirical equation (1), built on 2012 data (page 1684, Fig.5b). What is the point of such "prediction" of the previous years fluxes, which can not be examined anyhow? What do those "predicted" fluxes show? What message in this work do they have? For me it is just a speculation, which has high probability to be a way off. Very limited number of studies in Arctic included multiyear CH4 flux measurements, and in most of them interannual variability was found to be essential. Empirical models, developed for one specific season, did not work for the next (or previous) one – see for example Sachs et al., JGR:Biogeosciences, 113, G00A03, 2008; Bäckstrand et al., JGR:Biogeosciences, 113, G03026, 2008; Mastepanov et al., Biogeosciences, 10, 5139-5158, 2013. I would suggest removing Fig5b and the related text from the manuscript.

Fig.2 is also questionable. What is "2008–2012 mean methane flux"? For the desert I can agree, that the fluxes (Fig.3) seem consistent both within and between the five studied seasons. So the concept "mean ± SE" is applicable. But for wetland I totally disagree. What one can say about 2008–2012 mean, if no data is available for 2008 and 2009? If the data from 2010 truncates in the middle of the season, when the fluxes might be expected to get higher? I would suggest removing this figure, or considering its substantial change. As I wrote before, the dataset from the desert (5 years, no clear seasonal pattern, no clear interannual variation, representative for a large area) is qualitatively different from the dataset from the wetland (3 years, pronounced seasonal pattern, pronounced interannual variation, representative for the marginal part). I see no correct way of comparing them and no real need to do this.

Equations (2) and (3); "If we assume no net storage of CH_4 in the wetland over a growing season" (line 8 page 1685): Are there any reasons to assume this? Mastepanov et al. (Nature, 2008; Biogeosciences, 2013) argue that accumulation of substantial amounts of subsurface CH_4 during a growing season and its release during soil freezing might be a common feature in permafrost environments.

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Interactive Comment