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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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Authors' note: We would like to take this opportunity to thank the two reviewers for providing very constructive comments, which have improved our manuscript. We appreciate their efforts and attention to detail when reviewing our submission. For our response document, the reviewer comment (–) is first and the authors' response is below.

L. Kutzbach (Referee) lars.kutzbach@zmaw.de Received and published: 23 March 2014

General Comments –This manuscript reports interesting data on land-atmosphere fluxes of CH4 from a high-arctic study area. The presented dataset comprises 5 years

of CH4 flux chamber measurements at an arctic desert site, 3 years of CH4 flux chamber measurements at an adjacent wetland site, and 1 year of eddy covariance measurements at the same wetland site. Such an extensive CH4 flux dataset on from two nearby high-arctic landscape units is very valuable for the scientific field of the arctic biogeochemistry. Particularly relevant is the identification of the stable sink function of arctic desert for atmospheric CH4 which have not been shown so clearly before. The applied methods appear sound, and the results of high quality. The manuscript is very well written, and I have not found any orthographic mistakes. The tables and figures are well designed and have a high information density. The supplementary material is also very useful. I have only few specific and technical comments which I list below. I recommend the manuscript of Emmerton et al. for publication in Biogeosciences after minor revisions.

Specific comments –Page 1677, line 14: I suggest adding inserting "pronounced" or "considerable" before "diurnal patterns. People often underestimate radiation variability during polar day.

Yes, agreed, diurnal patterns are certainly stronger than most realize. We have inserted "pronounced" before "diurnal patterns" on line 14 of page 1677.

–Page 1678, line 7: I suggest inserting "other" before "closed path detectors". The TDL is also a closed path analyser.

Thank you! We have inserted "other" before "closed path detectors" on line 7 of page 1678.

–Page 1679, lines 15-16: Please specify how long samples had to be stored before GC analysis. Has it been checked how stable the CH4 concentrations were over time in the bottles?

We have performed repeated sampling experiments on samples collected into our glass bottles and stoppers. We found non-detectable differences in concentrations, rel-

ative to initial sampling, up to approximately 6 months later. Therefore we are confident that our analyzed samples, which were stored at most for 60 days, are representative of concentrations at the time of collection.

–Page 1679, lines 24-25: Have you applied some approach of quality screening? I recommend not using the coefficient of determination as an indicator of chamber flux quality. R2 inherently will be always lower for small fluxes than for high fluxes. RMSE would be a more suitable statistical measure to evaluate the quality of a chamber flux measurement. We have given an expanded explanation on this topic here: Kutzbach et al. (2007). Biogeosciences 4: page 1019.

Yes, we quality screened the results of each chamber measurement from each site between 2008 and 2012. The vast majority of all chamber measurements showed strong linear changes in CH4 concentrations over the deployment period. We now report the RMSE to evaluate the quality of chamber flux regressions. The previous sentence, "The coefficients of determination were typically strong for both desert and wetland measurements (r2 = 0.9$\pm$0.2)." has been replaced with, "Regression estimates typically fit well to observed CH4 masses in both desert (mean observed$\pm$RMSE; 11.19$\pm$0.45 μg CH4; n=101) and wetland (13.23$\pm$0.47 μg; n=66) chambers." We also added to end of the sentence previous ending on line 25, "and RMSE's were used to assess regression performance (Kutzbach et al., 2011)."

–Page 1679, lines 27-28: I would add that the EC measurements supported this assumption (supplementary material).

Yes, agreed, thank you. We have updated this line as ". . .would be little diurnal variation in FCH4 (supported by EC measurements; see section 3.2)."

–Page 1680, first paragraph: Please provide some more information about the location of the tower with respect to the area of interest (at the eastern edge of the wetland) and about wind/footprint climatology. How often did the wind blow from a suitable direction? Have you filtered the time series for wind direction? Page 1680, second paragraph:

Have you considered applying the Burba correction for the CO2 fluxes?

Re: information about the location and wind/footprint→On line 8 of page 1680, we inserted, "The wetland EC tower was positioned just outside the western margin of the wetland, leeward of the prevailing wind. Winds originated from prevailing 82% of all half-hour measurements and 90% of all fluxes originated from within the wetland footprint using the Kljun et al. (2004) model."

Re: frequency of correct wind direction, filtering of data for poor wind direction→This issue was addressed in section 2.2.3, page 1680, lines 23-24; we further inserted "(17.8% of all half-hour fluxes)" after "wetland" and before "," on line 24 of page 1680 to address the frequency of poor wind direction and the fact we removed these fluxes.

Re: Burba correction→we used an enclosed-path LI-7200 IRGA to collect CO2 flux data for the 2012 growing season (page 1680, Lines 10-11); this sensor is minimally affected by electronics-related heating of the measurement path and density effects from radiation, so the Burba correction is generally not applied to this instrument.

–Page 1683, lines 7-9: This is unclear for me: Why multiple correlation for the relationship between CH4 consumption and soil temperature? Have you used several temperatures at several depths?

Thank you to the reviewer for catching this error. This is a simple correlation. We have updated the text on lines 7-9 of page 1683 to read, "simple correlation".

–Page 1684, Eq. (1). The parameters in this empirical equation should have units. These would also clarify which units FCH4 and FCO2 have.

We have added units to the parameters in Equation 1.

–Page 1686, lines 19-23: Please make clearer in these sentences that you consider here soils that take up CH4 from the atmosphere. Methanotrophic activity behave differently in wetter soils. In these, often the highest methanotrophic activity is found in the upper part of the saturated soil zone.

Yes, methanotrophic activity often increases near the interface of saturated and unsaturated zones in wet systems. However, the context of these two sentences centers around dry soils and how methanotrophy is affected as these soils get increasingly moister. Whalen and Reeburgh (1996) show how methanotrophy decreases past moderate 20% soil moisture (v/v), likely due to air-filled pores being replaced by water-filled pores. Here, we are interested in the general trend in how methanotrophy changes as dry soils become wetter, rather than the activity of methanotrophs in saturated or near-saturated conditions. We propose here to simply remind the reader that we are discussing dry soils in this context by adding "dry" after "within" and before "soils" on line 19 on page 1686.

–Page 1687, lines 5-6: I do not think that these results can tell you so much about methogenic activity. It could be that the methanotrophic activity is just so efficient that the produced CH4 is anyway consumed, no matter if CH4 production is smaller of bigger.

Yes, the reviewer does make a good point here. We have removed the sentence, "These results also suggested that there was no enhancement of methanogen activity in association with vegetation cover." from the manuscript.

–Page 1688, line 10: I think that "even" is not appropriate. I would expect that the temperature effect on CH4 processes is probably particularly pronounced in cold environments.

Sure, we have updated this sentence to, "Temperature influences CH4 production and emission from wetlands in cold environments (van Huissteden et al., 2005)."

–Page 1689, second paragraph: The mineral soil horizons and also the through-flow nature of this wetland could lead to higher availability of electron acceptors as nitrate or Fe(II). It might be interesting to include this potential explanation in the discussion. In this context, you can have also a look to the following two papers: [Zona et al. (2009) Global Biogeochemical Cycles 23] and [Lipson et al. Biogeosciences 9].

This is an interesting point that we have added to our manuscript. We added after "...microbial activity." On line 18 page 1689, "Further, the shallow mineral soils and flow-through nature of our wetland may have made strong oxidizing species more available for microbial communities and thus restricting methanogen activity (Lipson et al., 2012)."

Technical Comments –Page 1688, line 16: Insert "at" before "wetlands"

Done, thank you.

–Page 1688, line 28: Correct "Eriophorum"

Done, thank you.

M. Mastepanov (Referee) mikhail.mastepanov@nateko.lu.se Received and published: 24 March 2014

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' discus-

sion 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 Arctic 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.

We agree with the reviewer that the reader should have some guidance here on the strength of numbers. We have appended ";n=27" and ";n=18" after the mean$\pm$SE results for the desert and wetland chambers in the abstract.

–By the way, line 7 (the abstract) gives a wrong impression as well: "we made static chamber measurements at both locations over n ÌÍAve 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.

We have revised the portion of this sentence in the abstract to now read, ". . ., we made static chamber measurements over five and three growing seasons at the desert and

wetland, respectively, and eddy. . .". We hope this change will reduce confusion.

–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.

We have contacted LI-COR to confirm our interpretation of the LI-7700 literature that addresses this issue. Below is a response from their LI-7700 expert regarding the issue of heating:

"The old LI-7500 had to maintain temperature control of some parts of the electronics inside the head at +30C, so effectively it was heated. Also, the heated air was circulating through the spars between top and bottom parts of the head to provide purging of CO2 and H2O from inside the electronics. As a result, the measurement path of the old LI-7500 in winter was surrounded with warm elements. The LI-7700 is not heated, and no warm air is circulated around the measuring path. The exception is when the bottom mirror is intentionally programmed to heat to avoid dew formation [from the authors: this was not programmed to do so at our dry site]. This is typically small, and time period is short. More so, we were concerned with heating when designing LI-7700, and have tested it under unrealistically high heating. We injected 1000 Wm$-$2 of heat into mirror and warmed it 17 C above ambient in cold winter period in 2008. Please see Figure 10 in McDermitt et al., 2010."

McDermitt et al. (2010) clearly showed that ambient sensible heat fluxes were similar to sensible heat fluxes measured when heat was injected into the measurement path. Therefore we do not believe that heating of the LI-7700 measurement path at our high Arctic location was a concern for our CH4 fluxes.

–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).

Thank you. . .we agree that the pattern seems convincing, especially with support from other measurements.

–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.

We do fully understand the concerns of the reviewer and we do understand the highly variable nature of methane in space and in time. The original intent of this modeling exercise was to attempt to provide more reasonable CH4 fluxes (relative to the underestimate-prone wetland margin chambers) during years when an EC tower was not available (2010, 2011). It was clear in 2012 that EC tower measurements may be as much as a magnitude larger than the chamber fluxes. Our model did a reasonably good job at predicting CH4 fluxes based on soil temperatures and CO2 fluxes trends

during the 2012 season. We do concede that this model is not ideal, however we are confident that these modeled fluxes are at least closer to true integrated fluxes over the wetland, compared to the chambers located at the margin.

Further discussion is clearly needed in the manuscript on this topic, so we have inserted at the end of line 25 on page 1684, "FCH4 can vary substantially in space and in time in wetland ecosystems, so our model is used here with caution. The model serves to bridge the considerable gap between chamber measurements taken at the wetland margin and landscape-integrating EC measurements, which are closer to the true wetland CH4 fluxes."

–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$\pm$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.

We agree with the reviewer that "2008-12 mean methane flux" does mislead the reader to think that this comparison is a paired analysis and that the measurements from both sites have the same value. We propose to remove "2008-2012" from the caption and simply state, "Comparison of mean methane fluxes (FCH4; $\pm$1se) measured in chambers. . .". The strength of the SE can be reconciled with the values of n included in the caption. The original intent here was to guide the reader into understanding that these two landscapes are quite different. Although our 2010 samples did not capture 10 days of the wetter period compared to the other years (which would presumably lower wetland fluxes more towards 0), the difference between the wetland chambers and the desert chambers are still significant when 2010 is included and still illustrate large site differences well. Further, the EC measurements show that the differences between sites are even larger than what we observed using the chambers. Although we

collected fewer chamber samples from the wetland, we still collected 2/3 of the number of desert measurements and we sufficiently sampled the two hydrological "seasons" over the three-year period, so we are confident that the mean±1SE we present are representative from that location and can be compared to desert results. We do see value in Figure 2 as a general overview of conditions between the two sites that anchor the reader for the remainder of the manuscript, even though error around the wetland mean may be large.

–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.

Please see our previous response. Yes, the reviewer is correct in stating that the wetland is more variable both within and between seasons compared to the desert site, however it is unlikely that our measurements in the wetland are of so little power that the two sites have statistically similar CH4 fluxes from their soils. Again the main point of figure 2 is to show the reader that these sites are quite different from each other and represent very different CH4 conditions at the landscape level and are controlled by different environmental factors.

–Equations (2) and (3); "If we assume no net storage of CH4 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 CH4 during a growing season and its release during soil freezing might be a common feature in permafrost environments.

Yes, we are aware of the interesting findings at Zackenburg and did consider these bursts to be a possibility at our site. There are two main reasons why we suspect that these bursts are less frequent or strong at our site. First, the organic layer of

our wetland was less than 10cm, compared to the ∼40cm deep peat at Zackenburg. CH4 emissions at Zackenburg were also over 30x stronger than our site. Therefore, we suspect that there is much less opportunity for CH4 to be generated, stored or released from this site as a large burst. Second, we have unpublished data showing eddy covariance CO2 fluxes into early October of one season, and it did not show a detectable large burst of CO2 at any point in autumn, rather a low-level, extended release of CO2 to the atmosphere. Mastepanov et al. 2013 state that observed CH4 bursts at Zackenberg were accompanied by concurrent CO2 bursts. We also did not see a CH4 burst during a CO2 burst in the early spring in 2012.

However it is clear this should be discussed further in the manuscript. We have now added at the end of the sentence on line 17 of page 1685: "Net storage of CH4 in wetland soils during the growing season was clearly shown via burst events in autumn at another high Arctic location (Mastepanov et al., 2008). However, we suspect these events were less important at our site because of a substantially thinner organic layer (see section 4.2), lack of a measurable CH4 burst in spring, and the absence of an autumn CO2 burst at our site (unpublished data), which is often coincident with CH4 burst events (Mastepanov et al., 2013)."

Interactive comment on Biogeosciences Discuss., 11, 1673, 2014.