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Interactive comment on "Seasonal dynamics of methane emissions from a subarctic fen in the Hudson Bay Lowlands" *by* K. L. Hanis et al.

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Thank you for the constructive comments. Please find below your comments and our responses. Line numbers given below refer to the original manuscript.

General Comments:

Reviewer: The manuscript is a good ïňĄt for Biogeosciences. However, several steps in the data processing need clariïňĄcation: The authors state that they measured methane ïňĆuxes over four seasons. From Fig. 1 it is evident that the coverage is different for each year. The authors should state what the data coverage is for each year and over which periods, since they compare different seasons for different years (2009 and 2011). Furthermore, the statistical analysis of 'all springs' seems to be

based on 2009 entirely (Tab.2, n=273 for 2009, n=311 for all four years). It seems doubtful that 38 data points from three additional years add substantial information, I suggest to exclude this from the analysis.

Response: We agree adding more detail of the measurements is informative. Thus, a new Table (now Table 1) has been inserted. The table contains columns that had been added to the original Table 1 in response to similar comments by Reviewer 1. The columns added are for melt, freeze-up, non-frozen and whole measurement periods and dates. Additional columns are included in the new Table 1; whole measurement, spring, growing, and fall measurement periods. Further, a new section has been added to the Results after 3.1 (Page 4548, Line 21) which summarizes this information as well as additional comments inserted in the Discussion 4.1 and 4.2. We also agree it is worthy to note that the 'All Springs' analysis was mainly (76%) represented by data for 2009. We believe it most straightforward to acknowledge to readers in the Results (3.6, Page 4552 line 21 - 22) and Discussion (4.1, Page 4554, lines 7 – 14 and 4.2, Page 4556, lines 26 - 28) that the majority of the data coverage for all springs was from 2009.

Reviewer: Some processing steps need a better explanation or re-phrasing: I do not have access to the 1969 paper by Tanner and Thurtell cited by the authors. I do not understand how the co-variances are rotated instead of doing the rotation before the computation of the co-variances (as e.g. Rebmann et al. 2012 in Aubinet et al.: Eddy Covariance).

Response: The Tanner and Thurtell 1969 report was the first broadly acknowledged description of these coordinate transforms. A link to the report is now included in the manuscript (http://www.dtic.mil/cgi-bin/GetTRDoc?Location=U2&doc=GetTRDoc.pdf&AD=AD0689487). For more information on their procedure, see Wilczak's description at http://acd.ucar.edu/∼tomkarl/tiltcorrection.pdf. Kaimal and Finnigan (1994) also describe this, but the Tanner and Thurtell reference is the prime reference. Briefly, this is a geometric calculation on the rotations, done to ensure that the average vertical velocity is zero. The geometry requires the use of the covariances of the three velocity components.

Reviewer: Similarily, I am confused by the frequency correction: An empirical approach of frequency correction is done by calculating the transfer function to correct for high frequency losses as the ratio of normalized co-spectra of the affected ïňĆux and the sensible heat ïňĆux (which is assumed to be not compromised by high frequency loss) (e.g. Foken et al. 2012 in Aubinet et al.: Eddy Covariance). The authors write they compared the spectra and derived a correction factor from this comparison. The reported values especially of the low-pump situation (12%) are low compared to the study by Detto et al. (2011), who report a substantial attenuation of the co-spectra and a correction factor of 45% using the undersized pump.

Response: Corrections for high frequency losses are highly dependent on many features of a closed-path flux system. Flow rate is important, and our system used dual pumps, whereas Detto et al. (2011) used a single pump. In addition, the plumbing configurations were not the same, and our experience has been that frequency loss can differ among systems to the extent that each system usually requires an independent frequency correction. Our 12% correction is based on real data at the site, and site conditions (i.e., typical length scales) can also be an important feature. Based on our empirical approach, we see no reason to adjust this correction to that given in the literature for a value that is different from that we determined.

Reviewer: Most studies analyzing environmental controls on methane emissions ïňĄnd nonlinear functions, e.g. between soil temperature and/or water table level. The authors exclusively use Pearson product-moment correlation (which assumes a linear relationship) and linear regressions to determine relationships between environmental variables and the ïňĆuxes. Especially the temperature response (expressed as slope of linear function) and the gap-ïňĄlling model performance can be biased by this, which should be tested. Furthermore, most of the environmental controls are co-varying with

each other and the measured ïňĆuxes and should be analyzed stepwise (e.g. use temperature normalized ïňĆuxes for a correlation with WT as in Rinne et al. (2007)).

Response: The relations presented in Figure 5 show linear rather than exponential functions. Furthermore, visual inspection of relations for the environmental variables on methane also showed linear functions thus the reason for using Pearson product-moment analysis. We have included in the Methods (2.4, Page 4546, lines 27 - 29) that visual inspection of relations indicated linear rather than exponential functions. Furthermore, the Discussion (4.2, Page 4557, lines 4 - 6) has been amended to indicate our finding of linear relations was different from that of Rinne et al. (2007). In development of the manuscript for the initial submission, we extensively debated if results of multiple linear regression analysis provided sufficient informative insight to be included. In light of the comments, we re-evaluated and extended that analysis. We split the data set into two sets for multiple linear regression analysis based on presence of WT height being above or below 16.61.m.a.s.l. (5 cm above mean sedge peat surface). WT was used to split the data set because emissions were very low above that value thus leading to non-linear response to WT. Stepwise multiple linear regression analysis resulted in a good model predicting daily methane emissions for when WT was below 16.61 m.a.s.l. with hollow temperature at 60cm, WT and air temperature accounting for 64, 6 and 6% in variation of measured values (total r2 = 0.76). A model was not possible for when WT was greater than 16.61 m.a.s.l. because no variables met the criteria for inclusion in the analysis. Nevertheless, flux values were very low averaging 16 nmol CH4 m-2 s-1 (0.17 kg C ha-1 d-1). This analysis has now been included in the Methods (2.4, Page 4547, after line 16), Results (3.6, Page 4554, after line 4) and Discussion (4.2, Page 4558, after line 27) and new figure (Fig. 6).

SpeciïňĄc comments:

Reviewer: Title: Since the focus of the paper is especially on the shoulder seasons, this could be mentioned in the title.

Response: In preparing the manuscript for the original submission we did try variations of the title that included wording of shoulder season as well as spring and fall, and melt and freeze-up. We decided on the current title because it was simple and 'season' captures spring, growing and fall periods.

Reviewer: p. 4542 l.17-18: Is there more information about the active layer depth, spatial and/or temporal? Especially for the discussion and comparison with other sites, it would be a helpful information. 1.5m seems to be large compared to values from Siberia or Greenland.

Response: The active layer depth was measured periodically for hummock, sedge-lawn and hollow landscape units using a 1.5 m frost probe. By July we were able to penetrate the full 1.5 m of the probe into the ground at each landscape unit and therefore speculate the active layer exceeds this depth over all cover types at this fen. We feel that our half hourly soil temperature measurements to a depth of 60 cm (discussed in 2.3, Page 4545, lines 11 – 15) are able to adequately capture changes in the active layer depth during melt and freeze-up periods for different landscape units as well as integrated over all cover types at this fen. The active layer was relatively deep compared to that published for studies in Siberia and Greenland perhaps because of standing and pool water at this fen. We find that where there is standing water in landscapes in this region, active layer depths are always very deep (> 1.5 m).

Reviewer: p. 4544, l.26 - p4545 l.5: EC measurements under these conditions are challenging. However, as mentioned above, a table recording periods of measurements with data coverage would be helpful to evaluate the results.

Response: Please see response in 'General Comments' section.

Reviewer: p. 4546, l1-l.7: As mentioned above, these steps need clariïňĄcation.

Response: Please see response in 'General Comments' section.

Reviewer: p. 4546, l.17-18: What is the reason to use mid-day NEE ïňĆuxes at

all? The data coverage of the NEE ïňĆuxes seems to be much better than those of methane. Why do the authors not use it to partition the ïňĆuxes in GPP and Reco? These dynamics would be more informative when analyzed together with the methane emissions. p. 4546 l.27-l.29: What is the reason to correlate the methane ïňĆuxes with NEEMD (see above) and especially with PAR?

Response: Mid-day NEE and PAR were used to encompass the effect of light intensity on plant photosynthetic activity and transpiration because it has been shown related to plant-mediated transport fluxes of rhizosphere gases such as methane (Joabsson et al 1999, http://www.sciencedirect.com/science/article/ pii/S0169534799016493). We have now included this rationale in the Introduction (Page 4540, lines 3 -10). We extensively examined modelling R and GPP. The majority of night atmospheric conditions were stable. This made estimating R and GPP difficult. Nevertheless we will invest a great deal of time to continue these efforts in hope (but not guaranteed) of preparation of a manuscript focusing on NEE, R and GPP of the study years presented here as well as two previous years where CO2 and not CH4 fluxes were determined.

Reviewer: p. 4547 l.17-l.25: When comparing different years, it is essential that the budgets were calculated for the same period of time each year. It is not clear if that was done here. Also, what is the difference between gap-ïňĄlling procedure 1 and 2?

Response: We standardized estimation of cumulative fluxes based on the period when mean daily air temperature reached > 0°C and when it was consistently < 0°C. This has been clarified in the Methods (Page 4547, lines 17 – 25). To clarify the three gap-filling procedures, Procedure 1 generated mean daily values from measured 30-minute periods in a day and then linearly interpolated fluxes across non measurement days. Procedure 2 differed in that linear interpolation of missing 30-minute values in a day was first done, then a daily average taken, then linear interpolation for missing measurement days was done. The procedures differ basically with the later trying to more fully capture diurnal variation since night-time (PAR < 10 μmol m-2 s-1) data capture of FCH4 was 0 – 4% of all potential night-time half hour periods. We have now included this rationale in the Methods (Page 4547, Lines 20 - 25), and replaced the text in the Discussion (Page 4560, Line 19 – 28) to reflect differences between the two gap-filling methods focussing on diurnal variation capture.

Reviewer: p. 4548 l.1-l.20: Since one focus of the paper is on the shoulder seasons, more information about the winter weather conditions - if available for e.g. winter 2008/2009 - could help to interpret emissions in the subsequent spring.

Response: In preparation of the original manuscript we did consider including winter months in Table 1 of the submitted manuscript. However, we opted to not include them because the monthly mean temperature and precipitation were similar for all years except 2009/2010 whereas those for the measurement periods reported varied considerably. Winter of 2009/2010 was warmer than the 1971 – 2000 Climate Normal for Churchill, MB measured at the Environment Canada meteorological station. However, we did not capture spring methane fluxes in 2010 so this information is not imperative. In light of the comment we have included in text of the Results (3.1, Page 4548, Lines 7 - 10) the air temperature measurements for December through April of 2008/2009 to assist in interpreting spring melt emissions of 2009. The winter of 2008/2009 was normal when compared to the 1971 – 2000 Climate Normal for Churchill, MB measured at the Environment Canada meteorological station. Precipitation was not successfully measured at our fen site or at the Environment Canada station for the winter of 2008/2009 and therefore cannot be included.

Reviewer: p. 4550, l. 11-l.14: What about the temperature at lower depth? Do all temperatures converge to 0 or only air temperature and temperature at 5cm? I think it is worth mentioning (and discussing), especially when comparing to the Mastepanov paper, that at the Greenland site, the soil temperature at 5cm (10cm, 15cm) depth decreased to -4 which is much colder than in this reported study.

Response: All temperatures to 60 cm depth converged to 0°C at the end of the fall measurement period and our average winter soil temperature from 0 – 60 cm was

-2°C from October to April (as described in our response to Reviewer 1). This has been clarified in the Results (page 4550, lines 11 – 15). The Greenland fen studied in Mastepanov et al (2008) was shown in a photograph in the Supplementary Information. At freeze-up the site appeared to be a continuous fen with no standing water above the peat, unlike our site which has a hummock and hollow topography and had standing water at the time of freeze-up. Inundation at our site allowed for a continuous ice layer to form above the peat, creating a physical barrier for gas exchange with the atmosphere. The Greenland fen may have been saturated but as it froze cracks in the soil could allow for emission bursts. Additionally, as the peat frost moved upwards and the surface froze downward, methane could be produced in the trapped unfrozen peat layer and eventually forced to move through the aerenchyma of sedges out to the atmosphere (Kim et al 2007: http://onlinelibrary.wiley.com/doi/10.1111/j.1600-0889.2006.00233.x/abstract?deniedAccessCustomisedMessage=&userIsAuthenticated=fals We agree that a comparison of physical and environmental conditions leading to emission bursts at our fen and the Greenland fen is noteworthy and is now included in the Discussion 4.1 following Page 4554, line 25.

Reviewer: p. 4552, l. 24-l.26 and p. 4553 l.10-11: I interpret these correlations as a result of the seasonal and/or diurnal co-variation of WT, NEE, PAR and CH4 ïňĆuxes with no clear explanatory power. The authors should focus on temperature as explanatory variable.

Response: Initially, we only examined temperature and methane relations in drafting the manuscript. Other colleagues reading a previous draft of the manuscript suggested including WT and NEE. We believe including these provided a better assessment of possible variables related to methane emissions.

Reviewer: p. 4553 l.17-25: As mentioned above, the relationships could be non-linear which should be tested especially with regard on the slope estimates. The same could be true for responses to change in water table level.

Response: We examined linear and non-linear relationship. The relationships were best described by a linear model to a threshold of the independent variable. Non-linear relationships had a lower R2 value than the linear model. Thus, we used linear models to describe the relationships over the non-threshold portion of the data sets.

Reviewer: p. 4556 l.10-l.12: Rinne et al. (2007) report a thaw burst (contributing 3% to the annual emissions) in a Finnish peatland without permafrost but winter air temperatures below 0 and peat temperatures in 35cm depth of ca. 0. These contrasting results show that other factors apart from temperature still play a role, as e.g. changes in biogeochemical cycling in the soil (see e.g. Sachs et al. 2008, JGR and references within).

Response: In the study by Rinne et al (2007), soil temperature remained at or above 0 °C and water table was above the peat surface for the duration of winter despite winter air temperatures below 0°C. Therefore a surface ice layer was likely present that insulated the peat and dampened the effects of air temperature. This would allow for decomposition and methanogenesis within the peat at slow rates over winter which could be why a spring emission burst was reported. Temperatures less than 0°C are required to completely freeze soil water (Kozlowski 2004: http://www.sciencedirect.com/science/article/pii/S0165232X03001198). The average soil temperature from 0 – 60 cm depth from October to April was lower and -2°C in our study. Within the referred paragraph we now explain how air temperature could affect soil temperature and thus biogeochemical cycling over winter in both non-permafrost and permafrost sites. It nowfocuses on the differences that soil temperature and permafrost makes rather than the difference air temperature.

Reviewer: p. 4556 l.28: As the authors discuss later, the stronger 'response' in spring is probably due to additional substrate for methanogenesis. I think this is much more important to discuss than listing the different temperatures used in other studies. My opinion on the choice of a temperature for analyzing landscape-scale emissions is that it depends on a) available temperature measurements and b) site conditions. Similar

to the choice of temperature for NEE partitioning, the best correlation does not necessarily imply a mechanistic relationship but is purely empirical (e.g. Reichstein et al. 2005, GBC). Comparison between sites are thus hampered.

Response: We agree with the Reviewer that soil temperature measurements are empirical and in light of this have decided to remove the individual site details from this paragraph and now simply acknowledge that although temperature measurements at specific depths may correlate well with FCH4 over an entire measurement period, the magnitude of the temperature effect on FCH4 can change throughout the year.

Reviewer: p. 4560 l.19-l.22: This should be rephrased: How do the authors know that one method underestimates the budget without producing an independent estimate? Theoretically, the other gap-ïňĄlling methods could overestimate the budget in 2008 and 2009 and underestimate in 2010.

Response: The text within this section has now been rephrased to discuss differences between gap-filling methods 1 and 2 as they reflect diurnal variation capture. We agree with the Reviewer that use of the words, underestimation and overestimation, are misleading without having produced an independent estimate and therefore their use has been removed.

Interactive comment on Biogeosciences Discuss., 10, 4539, 2013.