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Biogeosciences Discussions

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Interactive Comment

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

Anonymous Referee #2

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General comment
The manuscript by Hanis et al. is reporting on methane emissions in a subarctic fen underlaid by continuous permafrost with special focus on spring thaw and autumn freeze up. The data reported are from the Hudson Bay Lowlands, the second largest peatland complex in the world, from which few studies about methane fluxes were reported so far. Methane fluxes were measured with eddy covariance during four years (2008-2011) with varying data coverage. During spring melt no spring burst is observed, although it is reported from other boreal and arctic sites. In autumn, no squeezing effect as reported by Mastepanov et al. (2009) is observed, but few bursts in one year. During the growing season, methane fluxes followed a seasonal pattern except for one year, when a cold front and high rainfall events caused a sudden decrease.

The manuscript is a good fit for Biogeosciences. However, several steps in the data processing need clarification:

- The authors state that they measured methane fluxes 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.
- 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). 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 flux and the sensible heat flux (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.
- Most studies analyzing environmental controls on methane emissions find non-linear functions, e.g. between soil temperature and/or water table level. The

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authors exclusively use Pearson product-moment correlation (which assumes a linear relationship) and linear regressions to determine relationships between environmental variables and the fluxes. Especially the temperature response (expressed as slope of linear function) and the gap-filling 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 fluxes and should be analyzed stepwise (e.g. use temperature normalized fluxes for a correlation with WT as in Rinne et al. (2007)).

Specific comments:

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

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.

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.

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

p. 4546, l.17-18: What is the reason to use mid-day NEE fluxes at all? The data coverage of the NEE fluxes seems to be much better than those of methane. Why do the authors not use it to partition the fluxes in GPP and Reco? These dynamics would be more informative when analyzed together with the methane emissions. p. 4546 l. 27-l29: What is the reason to correlate the methane fluxes with NEE$_{M}D$ (see above) and especially with PAR?

p. 4547 l.17-25: 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-filling procedure 1 and 2?

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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.
p. 4550, l. 11-l14: 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.
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 fluxes with no clear explanatory power. The authors should focus on temperature as explanatory variable.
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.
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).
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.
p. 4560 l19-l.22: This should be rephrased: How do the authors know that one method underestimates the budget without producing an independent estimate? Theoretically,

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the other gap-filling methods could overestimate the budget in 2008 and 2009 and underestimate in 2010.

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