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Interactive comment on "Shifting environmental controls on CH_4 fluxes in a sub-boreal peatland" *by* T. G. Pypker et al.

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RESPONSES TO REVIEWER #2

General comments. Pypker et al. do detailed measurements of NEE and CH4 using flux tower measurements from a fen at Seney NWR. They are able to look at correlations between NEE and CH4 and whether higher plant productivity translates into higher CH4 fluxes, an interesting question. The authors begin to explore this question but face some difficulties given the auto-correlation between productivity, CH4 and environmental variables. The authors are able to draw some conclusions from these analyses, but might benefit from using some further aggregation of the data (or multiple seasons of data) to draw stronger conclusions. The authors find relationships between NEE and CH4 emissions, as well as between CH4 emissions, soil tempera-

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tures, and water table levels. They also find that relationships between productivity and CH4 emission seems to differ depending on environmental factors, a really neat bit of insight that could use a bit of additional exploration.

As a suggestion, an analysis of the lag time between high CO2 uptake (high productivity) and high CH4 fluxes might be useful. The current linear analysis seems to indicate an instantaneous (or at least daily) conversion of recently fixed CO2 into CH4 emissions; from a mechanistic perspective, I would be curious to see whether that conversion really operates on a daily time scale as this study would suggest, or whether it is better represented by a lag of hours to several days.

RESPONSE: Our initial approach was to look for a lag between CO2 uptake and CH4 fluxes. However, the strong autocorrelation results in an ever improving Pearsons correlation coefficient with longer lag times. For this reason, we looked into shifting controls between periods with little CO2 uptake and periods with high CO2 uptake. By separating the analysis between these two periods, we can look into periods when CO2 uptake is highly correlated with CH4 fluxes and when CO2 uptake is not.

The authors are a little loose with some of their terminology, especially regarding the word 'changes.' Often they aren't showing changes, but rather presenting fluxes. Similarly, the authors use of 'priming' doesn't seem to be in line with the more common and technical use of the term within the soils community.

RESPONSE: Thank-you for commenting on this error. We have altered the text to correctly describe the correlations between NEE, soil temperature and the CH4 efflux. Furthermore, we have removed the word "priming" and replace it with more appropriate terms.

Two methodological concerns: I understand why the authors removed negative nighttime CO2 fluxes, but it seems problematic to remove negative CH4 fluxes without cause. Net CH4 oxidation is a possibility in these ecosystems especially at low water table levels. Please provide justification for this approach as it seems like it could

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possibly bias results towards higher net CH4 fluxes.

RESPONSE: Although we removed negative data, it was more because they were outliers than anything else. Although methane oxidation can occur in the unsaturated zone, it doesn't make sense to have strong variation between positive and negative fluxes on a half-hourly timescale unless CH4 emissions are dominated by ebullition. All the negative nighttime CH4 fluxes are removed when the data is despiked because of rapid changes from positive to negative. However, we do not wish to remove rapid changes in the positive direction because of possible ebullition events. Because the negative CH4 fluxes were statistical outliers, they were removed. We will alter the text to make this change more clear.

Secondly, the authors only consider linear relationships between CH4 emissions and environmental variables. While the range in CH4 fluxes is relatively small, some of the relationships still appear to be exponential, as is common in chamber flux literature (Figure 4a, Fig. 7c, 7d). In order for the regressions to be valid, the linear regression residuals need to be normally distributed and this may not be the case if the relationship is really exponential. The authors need to look at the residuals from the regressions for normality and also compare to log-normalized CH4 fluxes, or clearly state that they have already done this in the methods and that the use of untransformed (or transformed) data in the regressions is appropriate.

RESPONSE: The reviewer makes a valid point. Figure 4a was already represented using an exponential function and therefore, was not changed. For figures 7c and 7d, the data is normally distributed and the variances of the residual are equally distributed. If log transformed, there is only minor improvement in the R2 (<0.02). However, when a log transformation is used, the variance of the residuals is not equal when NEE is positive. Because the residual are equal when using non-transformed data and the fact that the residuals do not have equal variance for one of the figures (7d) when the data is log – transformed, we proceeded with non-transformed data. We have modified the text to clearly state our reasoning for using non-transformed CH4 data.

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A remaining question after reading the paper was the role of ebullition in these seasonal flux measurements. Is there any way to tell what percentage of fluxes were due to ebullition? While this is likely outside the scope of this paper, it is an interesting question to acknowledge, especially because it has some implications within the manuscript as far as relationship to water table levels.

RESPONSE: Yes, ebullition would be very interesting to pursue. It would be a full, separate paper to try and tease out ebullition events from the EC data. We now acknowledge ebullition in the discussion paper, but we feel it is outside the scope of this paper.

Specific comments:

Section 2.1. Please include peatland type and site location including coordinates. Some of the information included doesn't seem particularly relevant.

RESPONSE: The peatland type is included in the description (poor fen). We have added the coordinates (46ïĆř 19' N and 86ïĆř 03' W). We would prefer to leave in the full description of the site as the region has been modified by humans.

Section 2.4: Justification of removal of negative CH4 fluxes. Please include % of filled CH4 and CO2 data.

RESPONSE: We will in include the % of the CH4 and CO2 fluxes that have been filled.

Section 2.5: Have you compared statistics to analyses with log-transformed CH4 fluxes? Please look at the normality of the regression residuals to determine whether this is appropriate and state whether this is necessary or not.

RESPONSE: Yes, as stated in a previous response, the residuals are normal distributed and the variance is equal when using non transformed data. Therefore, we did not transform the data.

Section 4.1. p. 11769 line 8: unclear how these numbers (5-50 mg m-2 d-1) were

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chosen and results were arrived at (17.6 – 18.6 g m-2 yr), especially because the authors include little data for the remaining year.

RESPONSE: We do not have data for the rest of the year. In the initial draft we assumed 5-50 mg m-2 d-1. Alternatively, we could produce a "minimum estimate" based upon other research where fluxes ranged from 1 to 10 mg per day outside the measurement period. This would truly be a low estimate as fluxes were still between 30 and 50 mg CH4 m-2 d-1 at the end of September. We have now adjusted our estimates to assume a flux between 1 and 10 mg per day outside the measurement period. The text has been corrected to reflect this.

Section 4.2: Another reason that the Q10 values from this study may have been lower than some previous values may have been because of the temperature range studied (often times higher Q10 values occur around 0C).

RESPONSE: I believe that Q10 values tend to decrease with lower temperatures (e.g. Slater, 1906). If there are papers showing the opposite effect, please provide the reference as should read them.

Section 4.3: loose terminology. Again, changes in CH4 efflux are not shown in figure 7. Also, observed results are not actually "priming" in the ecosystem/soil sense of the word priming because the manuscript presents no evidence for additional decomposition of substrate caused by the labile C input from photosynthesis (priming). Rather, the authors seem to be referring to the correlation between high rates of photosynthesis and high rates CH4 fluxes (perhaps a coupling?).

RESPONSE: We have revised this section to remove both the use of the words "changing" and "priming"

Secondly, the argument is that mean daily soil temperature exerts a larger influence on CH4 emissions when NEE is positive than when it is negative. However, the authors don't present sufficient data to assess this. They are relying on the r2 statistic of the re-

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lationship and do not show the slopes of the lines and whether these differ significantly, which would be the true test of this argument. Please revise.

RESPONSE: The relationship between NEE and CH4 is significant when NEE is negative (the slope is significantly different from zero). In contrast, when NEE is positive, the relationship between NEE and CH4 is not significant (slope is not different from zero). This then suggests that NEE is only correlated with the CH4 efflux when NEE is negative. When comparing soil temperature to CH4 efflux, CH4 efflux is significantly correlated to mean daily soil temperature. The slope of the lines are not statistically different, but the R2 increases when relating mean daily soil temperature to CH4efflux during periods when NEE is positive. This suggests the mean daily soil temperature explains more of the variation in CH4 when NEE is positive than when NEE is negative.

Secondly, this relationship appears to be exponential. Perhaps a log-transformation would be appropriate. RESPONSE: I agree that it does visually look exponential, however, the data is normal and the variance of the residuals are statistically acceptable when using a linear regression. In fact, if the data is log transformed (natural log), the residuals no longer have equal variance when NEE is positive. If a log base 10 is used to transform the data, the residuals no longer have equal variance for when NEE is positive and negative.

Finally, there are clearly many more measurements of CH4 emissions during negative NEE and also more scatter that could be due to differences in other environmental conditions as well as accumulated C. Can that be taken into consideration at all?

RESPONSE: Yes, there is more scatter and this suggests that other variables are influencing the net daily CH4 efflux. We used the AIC and BIC selection protocol to select the best model. The addition of other variables that we measured did not significantly improve the model. Therefore, they were excluded. If another variable had improved the model sufficiently, then we would have included it in the model. Yes, there is larger scatter, but the suite of variables used does not explain all the differences. However,

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this does not change the fact that NEE is not significantly correlated to the CH4 efflux when NEE is positive. There is more to explore on the “shifting environmental controls” of the CH4 efflux, but other parameters will need to be measured.

Conclusions: some of the sentences are rather vague and overall, the conclusion section isn’t especially insightful. For example, “when daily NEE was positive, the correlation between mean daily soil temperature at 20cm depth increased”. Correlation with what?

RESPONSE: We have revised the conclusion.

Figure 2, Figure 3: Changes in net ecosystem CO2 exchange. These aren’t anomalies or changes in fluxes, they’re flux rates shown over time. Revise.

RESPONSE: This has been revised.

Figure 7c, 7d. I’d be curious to know whether the slopes of these lines differ significantly and whether the variance improves if the analysis is done using log-transformed CH4 fluxes.

RESPONSE: Answered in earlier responses.

Interactive comment on Biogeosciences Discuss., 10, 11757, 2013.