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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.

**Anonymous Referee #2**

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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 temperatures, and water table levels. They also find that relationships between productivity and CH4 emission seems to differ depending on environ-



mental 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.

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.

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 possibly bias results towards higher net CH4 fluxes.

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.

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.

Specific comments:

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

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

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.

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

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).

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?). 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 relationship 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. Secondly, this relationship appears to be exponential. Perhaps a log-transformation would be appropriate. Finally, there are clearly many more measurements of CH4 emissions during negative NEE and also more scatter that could be do to differences in other environmental conditions as well as accumulated C. Can that be taken into consideration at all?

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?

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.

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.

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