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Interactive comment on "Drivers of long-term variability in CO_2 net ecosystem exchange in a temperate peatland" *by* C. Helfter et al.

Anonymous Referee #2

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General comments

In this manuscript, Helfter et al. present results from a long-term CO2 monitoring study at an ombrotrophic peatland in Scotland. CO2 fluxes have been measured continuously at the site since 2002 and the authors here report 11 years NEE data (with the exception of 2011 due to equipment failure). The study site is one of only a small number of peatlands globally where NEE has been continuously monitored for more than 2 years and, as such, is a very valuable addition to our knowledge of carbon cycling in these ecosystems. The authors have examined the impact of changes in meteorology over the same time period on Gross Primary Production (GPP) and ecosystem respiration (RECO). They provide evidence to suggest that the size of annual NEE is predicated to a large extent by the preceding winter weather, particularly on the phenol-

ogy of graminoids. They also focus on RECO dynamics during a number of dry periods and highlight the influence of water table position in determining the magnitude of CO2 fluxes. The inclusion of comparative data from other long term monitoring sites is very useful and the authors are able to put the results from this study into context with the other studies. The manuscript is well written and concise. The Tables and Figures are clearly understood (although I have some very minor issues, detailed below, in regard to captions and axis labeling).

Specific comments P14982 L2: change to carbon (C) and thereafter in the ms L8: Move "since 2002" to the start of the sentence P14983 L7: please state what emissions scenario is used by IPCC in regard to 3°C increase P14984 L2-5: I would tend to disagree here. EC towers are far from infallible – there is often extensive gap filling required (as in this study). Studies by (Schrier-Uijl et al. 2010) and (Laine et al. 2006) for example have shown that annual estimates of CO2 between EC towers and chambers can be quite comparable. L7: The criteria for inclusion as a long term study is stated as great than 3 years here, yet in Table 4, Degerö Stormyr is a 2 year study. You could also consider the 2 year study by (Gažovič et al. 2013) for inclusion in your analysis. P14985 L18: "60L26-28: "RECO. . .. .has been shown to turn a sink of C into a source" – given that RECO is ever present, I suspect that the authors mean that "an increase" in Reco can switch the system to a source. Please amend for clarity.

P14986 L14-15: Any particular reason as to why the 1995-96 data set was not included?

P14987 L3-23: I would have some reservations about this site as an example of an intact peatland (it is compared with intact sites in Table 4). Clearly, as the authors have described, it has been subject to some modifications in the past (drainage), and indeed may still be modified (livestock grazing). However, CO2 dynamics do seem somewhat similar to the other sites (although CH4 may be another matter). I would be interested to hear the author's opinions (not necessarily for inclusion in the ms but for my own interest).

P14988 L10-12: Please provide manufacturers details of the weather station etc.

P14991 L8: Please change all WT values in the text, Tables and Figures so that WT values below the surface are negative and WT above the surface is positive.

P14992 L16: What do you mean by “available”? L19: Do you have any idea as to why the WT drops so quickly? Lack of water holding vegetation? Bulk density or pore size?

P14993 L5-6: To my eyes, the respiration at the end of the first period is 4 and decreases to 2.5 following the rainfall event (i.e. start of the 2nd period). L8: The initial WT was also deeper in the first period as well.

P14994 L15: typo, change to “steady”

P14995 L27: (Renou-Wilson et al. 2014) found a nice relationship between LAI and WTD in a drained peatland that might also support your argument here.

P14997 L1-13: These seem like very small initial drops in WT level. Would this really have stressed the plants so that autotrophic respiration rates would decrease? The parabolic model seems appropriate for the dry periods in red and blue in Fig. 7 but I would suggest that on the others it is highly subjective. Interestingly, the latter all display an initial WT closer to the surface. Maybe mosses are more stressed when the WT gets deeper.

P14998 L15: What were the criteria for determining length of growing season? A temperature threshold?

P15001 L1: It would useful if the authors could expand the discussion to delve more deeply into the implications of climate change for this peatland, based on the results presented in this ms.

Table 4 Describe criteria used to define growing season length. Consider adding (Gažovič et al. 2013).

Figures 7 and 8 Consider adding r2 to each curve/line

References Gažovič, M., I. Forbrich, D. F. Jager, L. Kutzbach, C. Wille, and M. Wilmking. 2013. Hydrology-driven ecosystem respiration determines the carbon balance of a boreal peatland. Science of the Total Environment 463–464:675-682. Laine, A., M. Sottocornola, G. Kiely, K. A. Byrne, D. Wilson, and E.-S. Tuittila. 2006. Estimating net ecosystem exchange in a patterned ecosystem: Example from blanket bog. Agricultural and Forest Meteorology 138:231-243. Renou-Wilson, F., C. Barry, C. Müller, and D. Wilson. 2014. The impacts of drainage, nutrient status and management practice on the full carbon balance of grasslands on organic soils in a maritime temperate zone. Biogeosciences 11:4361-4379. Schrier-Uijl, A. P., P. S. Kroon, A. Hensen, P. A. Leffelaar, F. Berendse, and E. M. Veenendaal. 2010. Comparison of chamber and eddy covariance-based CO2 and CH4 emission estimates in a heterogeneous grass ecosystem on peat. Agricultural and Forest Meteorology 150:825-831.

Interactive comment on Biogeosciences Discuss., 11, 14981, 2014.