Author’s response to referee comments

Author’s responses are prefixed with AR and given in bold blue font at the end of each comment.

I. Referee #1

General comments:

The paper by Helfter et al. presents a long-term study of carbon flux exchange at a peatland site. With their study, Helfter at al make an important contribution in advancing our knowledge of land-atmosphere carbon exchange, in particular as peatland sites are generally underrepresented in carbon-exchange studies. The paper is well written and results are presented in a clear way.

AR: We thank referee #1 for the positive and supportive assessment of the manuscript as a whole.

One point caused a bit of confusion. Section 3.3 discusses the effects of dry periods on CO2 exchange. The drought in 2013 was the biggest but is not mentioned in the beginning and in the tables where droughts are discussed. You have to read carefully to understand that it belongs to the drought assessment and it is not obvious for the reader how it fits to the rest. I understand that the 2013 drought shows different patterns and the authors offer plausible explanations, why. However, to obtain an overview about the different droughts and their impacts it would be better if 2013 is mentioned together with the others at the beginning of section 3.3 and maybe also in table 2.

AR: We appreciate the point raised by the referee regarding the 2013 dry spell. However, in the interest of readability, we feel that the structure of section 3.3 offers the best compromise between presenting the notable dry spells and discussing their attributes. An entry for 2013 was made in Table 2 as recommended by referee #1 (see below for details of modified table 2).

Table 2: Water table drainage rates and maximum water table depths (WTD) observed during the summer dry spells of 2008 and 2010. The time lag is the number of days elapsed between the start of the dry period and the onset of a response from the ecosystem respiration (R_{eco}); the time lag was determined by optimising the polynomial fit between R_{eco} and WTD. The minimum value of R_{eco} for each dry spell and the water table depth corresponding to each minimum value of R_{eco} were calculated using a second degree polynomial regression functions between R_{eco} and WTD. No parabolic relationship was observed in 2013 between R_{eco} and WTD; for this reason, time lag, minimum R_{eco} and WTD for minimum R_{eco} could not be calculated.

Period	Drainage rate [cm day^{-1}]	Maximum WTD [cm]	Time lag [days]	Minimum R_{eco} [μmol m^{-2} s^{-1}]	WTD for minimum R_{eco} [cm]	Mean T_{air} [° C]	Wind direction [°]
05-29/05/2008	1.2	20.4	2	0.03	1.5	10.1	70
22/07-01/08/2008	3.0	19.1	3	2.31	4.5	16.1	100
15-26/05/2010	1.6	30.7	2	1.05	15.6	12.9	181
09-24/06/2010	2.0	36.1	0	1.58	12.5	13.0	176
21/07-08/08/2010	2.0	22.1	5	2.01	2.9	11.4	191

26/05-06/09/2013	1.4	48.5	-	-	-	14.5	222

Specific comments:

1. P14985, l20: "Reco and GPP ... respond similarly, although not necessarily with the same magnitude, to extreme events such as drought": This effect can also be reproduced with global vegetation models (Zscheischler et al. 2014).

AR: We thank the referee for pointing out this relevant publication which has now been cited.

2. P14989, l9: "University of Jena": should be "Max Planck Institute for Biogeochemistry, Jena".

AR: Change made.

3. P14990, l14: define what the values denote after plus-minus sign the first time it appears.

AR: Definition given. The sentence now reads:
"During the study period 2002-2013, the site received a mean annual precipitation of 1018 mm ± 166 mm (± values denote standard deviation)."

4. P14993l, l7: C missing in the unit? Consider transforming μmolm−2 s−1 into gC m-2 s-1 for better comparison with the other units in the manuscript.

AR: In line with referee #2's specific comment for P14993 L5-6, the sentence fragment "the magnitude of the respiration term was the same at the end of the first and the beginning of the second cycle (2.5 μmol m^{-2} s^{-1})" was deleted due to inconsistencies with the data presented. This deletion has however no impact on data interpretation and discussion. As a result of this deletion, the change of unit proposed by referee #1 is no longer applicable.

II. Referee #2

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 phenology 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 labelling).

AR: We thank referee #2 for the positive general comments on our manuscript. The issues regarding captions and axes labels are addressed in the specific comments section below.

Specific comments:

5. P14982 L2: change to carbon (C) and thereafter in the ms.

AR: Change made.

6. P14982 L8: Move "since 2002" to the start of the sentence.

AR: Change made.

7. P14983 L7: please state what emissions scenario is used by IPCC in regard to 3 °C increase.

AR: Scenario A1B explicitly stated. The sentence now reads:

"UK peatlands are predicted to become a net source of carbon in response to climate change (Worrall *et al.*, 2007), with climate models predicting a rise in global temperature of ca. 3^{o} C between 1980-1999 and 2100 (IPCC, 2007; scenario A1B which considers a balanced distribution between fossil fuel intensive and non-fossil fuel energy sources)."

8. 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.

AR: We fully agree with this comment. The sentence has now been changed to:

"Whilst chamber approaches provide useful methods for comparing sites/treatments, the scale of measurement and potential spatial heterogeneity, mean that upscaling chamber-derived fluxes to larger land surface areas can be problematic."

9. P14984 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 (Gažovic et al. 2013) for inclusion in your analysis.

AR: A 12-year record for Degerö Stormyr has since been published and the manuscript has been updated to reflect this recent dataset.

The Gažovic paper is indeed interesting but the 2 years presented are highly contrasting (dry and wet) and might not be representative of longer term trends. For this reason, we have decided not to include it in our comparison.

10. 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.

AR: The sentence has been modified accordingly:

"Although less well understood and modelled than GPP, R_{eco} plays a major role in ecosystem C exchange dynamics and sink strength, and increases in R_{eco} have been shown to turn a sink of C into a source (Lund *et al.*, 2012)."

11. P14986 L14-15: Any particular reason as to why the 1995-96 dataset was not included?

AR: For the purpose of this analysis, we chose to concentrate on the most recent continuous dataset and decided not to include the 1995-96 data for this reason.

12. 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).

AR: The question of whether the peatland studied can be considered intact and, as such, compared to truly intact sites is interesting. The site was indeed drained in the past, albeit > 100 years ago, but the potential long-lasting legacy of drainage can unfortunately not be assessed on the basis of this study alone. Grazing is minimal, especially in the footprint of the EC tower, and we therefore assumed that its influence can be considered negligible. The emphasis of this

manuscript was on the presentation of a reasonably long, continuous dataset and the study of inter-annual variability. The other peatlands cited offer an interesting broader context for our temperate site even if like-for-like comparison cannot be claimed. Considering the differences in management, climate and latitudes, we feel that the comparison of the peatlands presented in this paper highlights noteworthy similarities in ecosystem drivers.

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

AR: The description of the meteorological instrumentation has been rewritten and now includes details of the instrumentation used:

"In addition to eddy-covariance measurements, the site is equipped with a Campbell Scientific 23X datalogger for the automated acquisition of a comprehensive suite of meteorological parameters which include net radiation (Skye instruments SKS1110), PAR (Skye instruments SKP215), air temperature (fine wire type-E thermocouple), air pressure (Vaisala PTB101C), wind speed and direction (Gill Instruments WindSonic), soil water content (Campbell Scientific CS616 TDR probes), soil temperature (Campbell Scientific 107 thermistors at 10 cm, 20 cm, 30 cm, and 40 cm), rainfall (tipping bucket rain gauge) and, since April 2007, water table depth (Druck PDCR 1830)."

14. 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.

AR: We feel that changing the sign of water table values to positive ones for aboveground levels would affect the readability of the document and would therefore like to abstain from carrying out the requested change. Throughout the document, we refer to the term water table depth rather than level, which, combined with the negative values the requested change would give rise to, could be interpreted as an elevation above the surface (i.e. double negation "negative depth").

15. P14992 L16: What do you mean by "available"?

AR: We have now clarified in the text that water table measurements began in April 2007. The sentence now reads:

"Throughout most years and most seasons Auchencorth Moss can be considered a wet site, with mean water table depth (WTD) 3.5 ± 6.8 cm and monthly range -3.8 cm (flooded; negative values denote water table levels above the peat surface) to +36 cm (April 2007 to December 2013; no WT measurements prior to April 2007).".

16. P14992 L19: Do you have any idea as to why the WT drops so quickly? Lack of water holding vegetation? Bulk density or pore size?

AR: We do not know what causes such rapid drops in water table levels but it would be interesting to investigate this in future.

17. 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.

AR: We are grateful to the referee for spotting this inconsistency. As already noted in our response to the comments of referee #1, the sentence fragment "the magnitude of the respiration term was the same at the end of the first and the beginning of the second cycle (2.5 µmol m^{-2} s^{-1})" was deleted. This deletion has however no impact on data interpretation and discussion.

18. P14994 L15: typo, change to "steady".

AR: Typo corrected.

19. 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.

AR: We are grateful to the referee for bringing this highly relevant paper to our attention. We have incorporated the relationship between WT and LAI reported by Renou-Wilson et al. (2014) into our discussion.

The original sentence was extended and now reads:
"This has previously been shown to be important at other sites, particularly in moss species (Aurela et al., 2009, Lafleur et al., 2003, van der Molen et al., 2011); **furthermore, a negative linear relationship between leaf area index (LAI) and WTD has been reported for a grassland established on drained organic soil in Ireland (Renou-Wilson, Barry et al. 2014) which illustrates the effect of water availability on graminoid productivity. It must however be noted that the WTD range in the Renou-Wilson (2014) study was significantly deeper (typically 20 cm to 60 cm below peat surface) than at our study site**."

20. 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?

AR: We have expanded the discussion slightly to suggest that the single point water table measurement might not be a universal proxy at our site. The following sentence was added to the discussion:
"The parabolic trends were especially strong during the two first dry spells of 2010 (15/05-09/06/2010 and 10/06-10/07/2010) during which the prevailing wind direction was South. The WT measurements might not be representative of the entire flux footprint which could perhaps explain the markedly different trends observed in 2008 when wind was blowing from the East."

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

AR: The length of the growing season used in this study was indeed defined in terms of a temperature threshold. This has now been clarified in the text with the sentence:
"The start of the growing season was defined as the first day of the year when mean diurnal air temperature exceeded 5 °C for 5 consecutive days. Conversely, the end of the growing season was defined as the first day of the year when mean diurnal air temperature fell below 5 °C for 5 consecutive days."

22. 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.

AR: The drivers and controls of NEE at our site have not yet been fully constrained but the following sentences were added at the end of the summary section in a bid to offer a qualitative outlook on possible future scenarios:
"The large inter-annual variability of NEE observed to date makes future trends difficult to predict and quantify. Changes in seasonal hydro-meteorological conditions, especially changes in precipitation patterns and intensity, could however be pivotal for the CO_2 cycling of this peatland. Drier summers could lead to a reduction in net CO2 uptake but this could be offset by milder temperatures, particularly in winter time, and longer growing seasons. Mean annual temperatures at the study site have risen by 0.019 °C yr^{-1} since 1961, which could, in theory, benefit C uptake by the peatland in the long-term since NEE was found to be closely linked to the length of the growing season."

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

AR: Description of criterion added. The caption of table 4 now reads:
"Table 4: Annual minimum, maximum and mean values of NEE at several long-term peatland monitoring sites in the Northern hemisphere. LGS and LDS are the length of growing and dormant season respectively, and subscripts GS and DS denote growing and dormant season. **The length of the growing season for the study site Auchencorth Moss was bounded by the first and last day**

for which mean daily air temperatures exceeded 5 °C for 5 consecutive days. For the other sites, LGS was estimated from data available in the respective articles.”

The Gažovic et al. 2013 paper, although highly interesting, was not included in our manuscript for the reason stated under point 9.

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

AR: R2 values have been added to Figure 7 and 8 as requested.

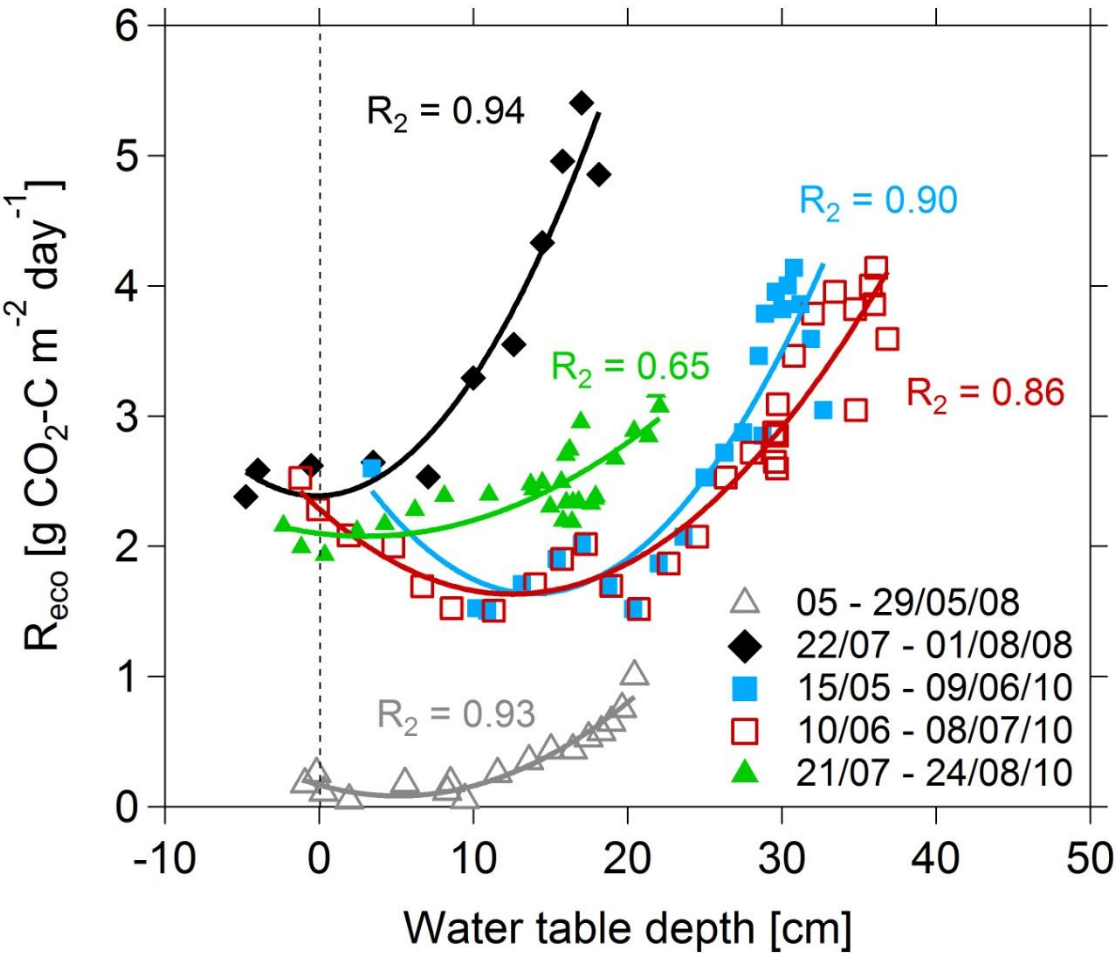

Figure 7: Daily ecosystem respiration as a function of water table depth during five dry spells (two in summer 2008 and three in 2010).

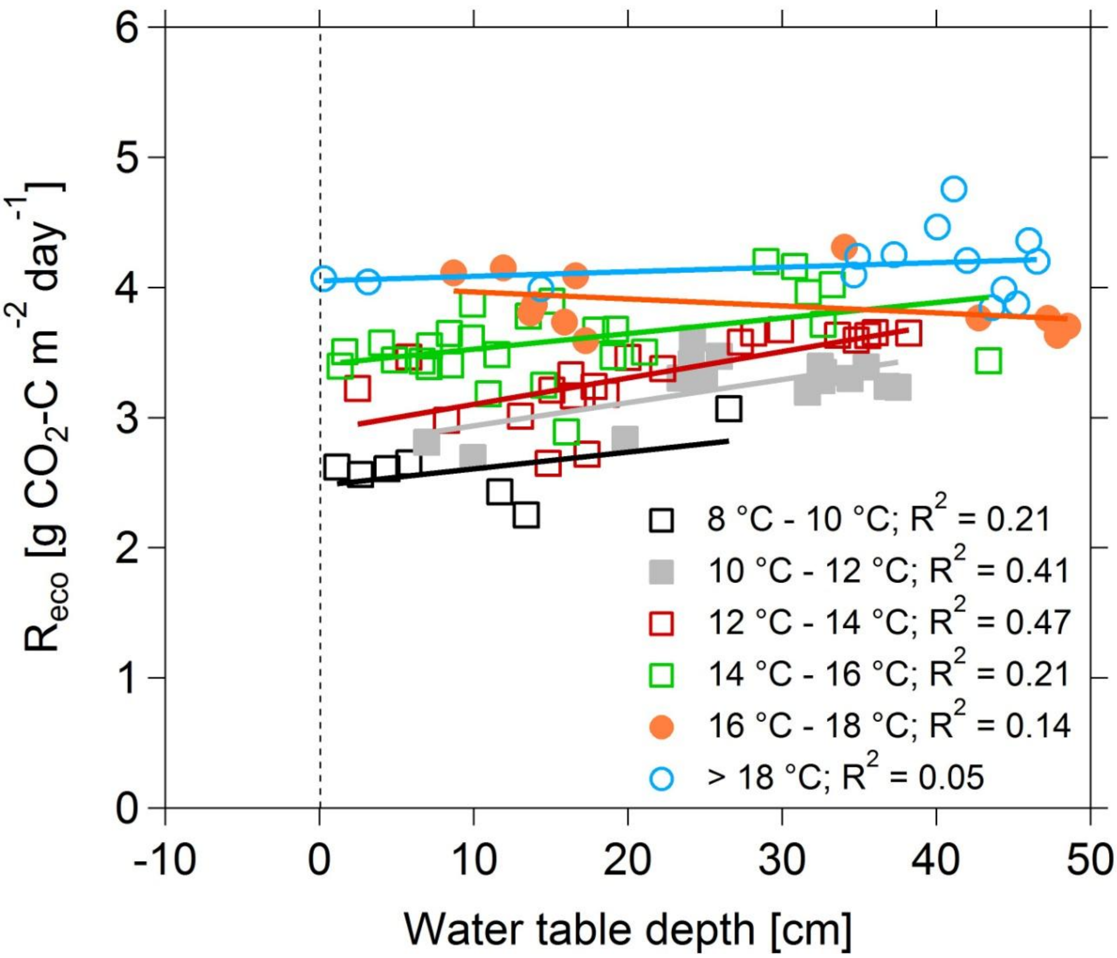

Figure 8: Ecosystem respiration as a function of water table depth and air temperature (daily means for May to September 2013).