Authors response to comments from Reviewer 3

General comments

Peat extraction is a major land use category of peatlands in some countries, and the recent emission factor of the IPCC 2013 Wetland Supplement is indeed based on only a few studies mainly on boreal sites. Therefore, new emission data on peatlands managed for extraction is very welcome and fits well into the scope of Biogeosciences. However, there are some methodological issues which need to be addressed before the manuscript can be published.

Response: We thank Reviewer 3 for the positive review of the manuscript and for the helpful comments and suggestions.

1) The methods section is very brief and needs to be extended.

2.5.1 Field measurements:

• How often did you measure Reco and GPP at each measurement date?

Response: Reco: 2-4 measurements per collar per measurement date. NEE (light): 3- 8 measurements per collar per measurement date. This has been added to the text.

• Did you ensure that the maximum PPFD was reached at each measurement date to avoid an extrapolation beyond measured values?

Response: Yes. The measurements were carried out so as to cover the full range of PPFD on a given day (see response below).

• Did you measure at different PPFD levels by shading or at different times of the day?

Response: Measurements were carried out between 8 am and 6 pm in the summer and 9am and 3pm in the winter and covered the full range of PPFD on a given day. Artificial shading was used early in the morning to obtain low PPFD levels (<100 µmol m^{-2} s^{-1}). This has been added to the text.

• For the NEE measurement: were the chambers also cooled (e.g. by icepacks) and the temperature measured inside the chamber to avoid more artificial conditions than necessary?

Response: Yes, the chambers were cooled with a cooling system. The system involves the continual pumping of iced water (from submersion of ice packs/bottles) from a container through a hose pipe into a small car radiator located in the chamber and back to the container via a second hose pipe. Two fans located in the chamber ensure that the cooler air is mixed within the chamber. Air temperature was measured in the chamber continually. The setup is described in detail by (Alm et al., 2007).

• What kind of chambers have been used for NEE measurements?

Response: The same polycarbonate chambers (60 x 60 x 33 cm) were used (as described on P7499, L18-19 in the ms). The following information has been added;

"At the DP sites, net ecosystem exchange (NEE) was measured under a range of ambient light levels (PPFD; μmol m^{-2} s^{-1}) prior to R_{eco} measurements with the same polycarbonate chambers described above."

• How is the light transmissivity of these chambers (usually, it does not reach 100%) and was this accounted for when modelling GPP? It should be included in the GPP model as otherwise GPP might be underestimated.

Response: Accounting for the light transmissibility of the chambers is valid if the PPFD sensor is located external to the chambers (e.g. Beyer and Höper, 2015; Beyer et al., 2015). Our PPFD sensors are located within the chamber, so the PPFD recorded during each NEE measurement period is the "attenuated" value (our chambers attenuate light transmissibility by ~12%). When modelling GPP, we used the relationship between fluxes (estimated GPP = measured NEE - measured Reco) and the PPFD values from inside the chamber to produce light response curves.

The GPP models were then used with the PPFD time series recorded by the external PPFD sensors on the weather stations to reconstruct the annual CO2 balance.

The light response curves are only valid for the range of PPFD values recorded during NEE measurements. This does mean that the highest PPFD value recorded during flux measurements (from inside the chamber) is always likely to be around 12% less than the actual PPFD value (measured by the weather station). However, we feel that this results in minimal underestimation of GPP as (a) it impacts on a very small number of hourly fluxes (<0.1%; i.e. number of occasions in the year where PPFD values recorded by the weather station > than the maximum observed PPFD value in the chamber) and (b) the plots are light saturated at PPFD >1000 μmol m^{-2} s^{-1}, so the difference in NEE at PPFD values >2200 μmol m^{-2} s^{-1} is likely to be minor.

• Why didn't you chose a site under ongoing (or recently ceased) industrial extraction?

Response: A recently ceased extraction site was chosen (IP6), however, for the remainder it was not possible to establish monitoring sites for either logistical or equipment security reasons. Indeed, the decision to locate the sites on abandoned areas with limited access has proven to be sensible given that equipment (weather station batteries, solar panels etc.) at site IP6 have been stolen on a number of occasions.

2.5.2 Flux calculation

• "GPP was calculated as NEE minus Reco": Which value of Reco was used; the nearest value in time or the one calculated by the model? If there was only one Reco measurement per measurement date, using the actual measured value could potentially induce some uncertainty as the time lag between the Reco measurement and the first GPP measurement is not clear and as there is probably a strong temperature-dependent diurnal variability of Reco.

Response: The Reco value that was used was always the value closest in time to the NEE measurement. In winter time, diurnal variation in the soil temperature was very small

and approximately two Reco flux measurements per plot were taken. In summer, when diurnal changes in soil temperature were very pronounced up to four Reco flux measurements per plot were carried out.

2.5.3 Modelling

• Modelling GPP should be included in this sub-chapter

• Why was this specific GPP model chosen, and not a Michaelis-Menten type model, which is frequently used for GPP?

Response: The basic form of the GPP model used in this study (see Eq. 4) is a Michaelis-Menten type model, which has been used to describe the saturating response of photosynthesis to PPFD in numerous studies (e.g. Tuittila et al., 1999; Byrne et al., 2005; Laine et al., 2006). The Levenberg-Marquardt algorithm described in the manuscript is a multiple non-linear regression technique used to derive model parameters and associated standard errors. The text has been amended as follows;

"GPP was related to PPFD using the Michaelis–Menten type relationship that describes the saturating response of photosynthesis to light (Tuittila et al., 1999). GPP model coefficients and associated standard errors were estimated using the Levenberg-Marquardt multiple non-linear regression technique (IBM SPSS Statistics for Windows, Version 21.0. Armonk, NY, USA)."

• How was "plot-specific LAI" modelled?

Response: This was described on P7499, L3-12

"However, at the DP sites a vegetation component is present and in order to incorporate the seasonal dynamics of the plants into CO_2-C exchange models, the leaf area index (LAI) was estimated for each of the collars. This involved accounting for the green photosynthetic area of all vascular plants (leaves and stems) within the collar at monthly intervals. In short, the number of leaves and stems were counted from five subplots (8 x 8cm) within each collar. The size (length, width) of the leaves was measured from sample plants outside the collars. The LAI was then calculated by multiplying the estimated number of leaves by an area estimate of the leaf. Moss and lichen % cover was estimated at the same time. Species-specific model curves were applied to describe the phenological dynamics of the vegetation of each collar, and the models (vascular plants and moss) were summed to produce a plot-specific LAI. For a detailed description of the method see Wilson et al. (2007)."

2) My major concern, however, are the NEE results of the GP sites. The vast majority of the measured fluxes (Figure 3) show an uptake of CO2, especially at site DP1, but all annual balances (Table 2) show a net release. How do you explain these results?

In my opinion, there are several possibilities:

• The measurements themselves are biased: but as they were regular and rather frequent, I would suggest that this is not the reason for the surprising results.

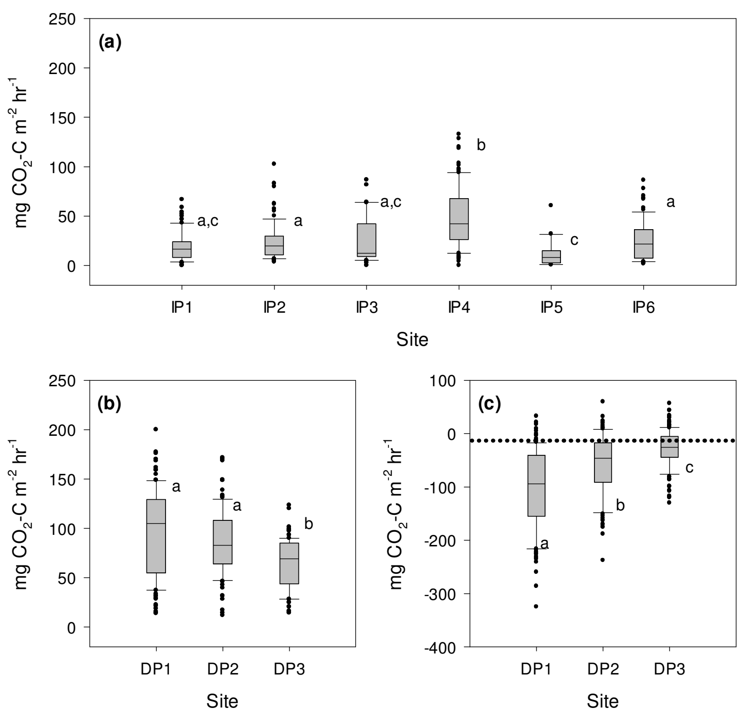
• There are problems with either the Reco or the GPP model. Here, I would recommend checking the following issues:

• Is there extrapolation beyond the range of measured temperature and PPFD data?

• Regarding the GPP model: I doubt whether one "general model" and especially one (equation 4) not including the LAI could predict correctly the NEE for the whole year. Therefore, pooling data of several measurement campaigns might be an alternative if the LAI is not included.

• Given the obvious discrepancy between measured NEE values and modelled sums I would strongly suggest that the authors check their model by e.g. a cross-validation approach (leaving one measurement date out at a time and trying to predict both Reco, GPP and NEE by the remaining data).

Response: Reviewer 2 has also pointed out this discrepancy. The caption to Fig. 3 should have included the following text ***"(c) net ecosystem exchange (NEE; mg CO_2-C m^{-2} hr^{-1}) when PPFD>1000 µmol m^{-2} s^{-1} at sites DP1-3".*** **This has now been added. The letters denoting differences between fluxes were also lost during the uploading process and are now presented below.**



3) Measuring emissions from burning peat is a valuable addition to the manuscript. However, I'm not sure whether these numbers are to be used for Lfire (Wetlands Supplement) – is fire an issue for non-vegetated peat extraction sites in Ireland and the UK? Otherwise, wouldn't be burning of peat reported in the energy sector (and how is it done now if there are no

numbers available)? These issues should be made clearer especially for those not familiar with reporting methodologies.

Response: Emissions associated with the burning of peat are reported under the Energy sector. The peat burning EFs in this study (Table 3) are primarily to be utilised for Lfire (i.e. on site). Fires do occur on non-vegetated sites in ROI and the UK, particularly in very dry years. For clarity, the following text has been added to the introduction:

"Emissions associated with off-site peat combustion are reported under the Energy sector and are not considered further here."

Specific comments

Abstract

I have recently experienced some discussions during which the relatively low EFs for peat extraction sites (at least compared to agriculture) tended to raise the rather questionable opinion that peat extraction is a climate-friendly activity in peatlands. Therefore, it would be helpful to clearly include a statement on the system boundaries of your study, especially as you included peat burning but no horticulture.

Response: We clearly state the boundaries of our study in the discussion section 4.7 but have added the following text in the abstract:

"Drainage related methane (CH_4) and nitrous oxide (N_2O) emissions, as well as CO_2-C emissions associated with the off-site decomposition of horticultural peat were not included."

Results

Both WT and VMC enter the Reco-equations without any additional model parameter. That suggests that a) there is no optimum water level or moisture for respiration which I would expect to exist and b) respiration is highest at that highest VMC, i.e. at saturation which is rather surprising.

Could you comment on this?

Response: Both points made by the Reviewer above are valid. The Reco models in this study are controlled by soil temperature. While the addition of WT and VMC improved the performance of the models at some of the sites, the improvement was only slight. We feel that this is due to the fairly narrow range of WT/VMC values recorded over the course of the 12 month study (e.g. the range in VMC values at DP3 only ranged between 56-64%). Therefore, optimum WT /VMC levels for respiration may not have been encountered. The Reco models used here are only valid for the data that was measured over the course of the study at each site and should not be extrapolated beyond the range of that data.

Discussion

Generally, I do not really understand why the emissions are lower than in other studies: Your study areas are relatively warm with mild winters (at least compared to boreal sites), the physical environment is with maximum soil temperatures of 28°C not too extreme, and at

least some of the sites are characterised by rather narrow CN ratios and sub-neutral pH-values. How is the WT compared to previous studies?
Response: The WT levels are our sites are similar to the other studies in Fig. 7 (where a WT is reported).

In my opinion, the choice of sites might be a reason why the emissions are lower than in other studies: Easily degradable organic matter would have been already gone, while during abstraction there would have been also less decomposed "fresh" peat at the surface during certain periods of time. Furthermore, your chose "unvegetated microsites" for the measurements, which suggests that parts of the peatlands are already re-vegetated. Probably, these microsites are unvegetated for a reason (peat quality, water repellency,...), and these conditions might also limit microbial activity.
Response: The CO2 emissions from our sites are slightly higher than those reported for Fenno-Scandia but lower than emissions from Canadian sites. We believe that this is due to the peat end-use requirement in Canada (i.e. horticultural peat). As the Reviewer has stated, this latter peat is likely to be more fibric, less decomposed and produce higher CO2 emissions. In contrast, the residual peat at our IP sites and at the majority of Fenno-Scandia sites is utilised for energy production as it is more decomposed. We have discussed this at length in section 4.4.

In regard to the "unvegetated microsites", areas around the periphery of the industrial sites may be vegetated, as they are often close to a seed source, while the remainder can remain largely bare and unvegetated for decades after the cessation of peat extraction. Even where a seed source is available plant establishment and survival are made more difficult by the edaphic conditions that may exist in the upper layers of the peat surface. As the reviewer has mentioned the lack of plant colonisation could be due to an unsuitable nutrient status (Wind-Mulder et al., 1996) but could also be caused by the instability of the peat surface (Campbell et al., 2002), water table fluctuations (Price, 1997), high evaporative losses (Waddington and Price, 2000) and high peat temperatures in mid-summer (Waddington and Warner, 2001).

Are there any obvious differences between the vegetated and non-vegetated sites?
Response: In the ROI sites, peat type would appear to be an obvious difference between DP and IP sites, however this division does not hold up when the UK sites are included.

Effects of drainage level: To my understanding, the effect of the WT on single fluxes (i.e. the Reco dynamics) and the effect on the general emission level shouldn't be mixed up. While at the scale of single fluxes, effects of the WT might be obscured by a co-variance between WT and temperature, or the activity of the vegetation, the general height of the emission might indeed be influenced by the WT (which seems not need to be the case in the study). However, at the scale of single fluxed, I do not think that concluding that there is generally (nearly) no effect of the WT is not valid unless fluxes from all sites are combined into one model.

Response: We do not think that we state that there is no effect of WT on CO2 emissions at the site level. We have stated that, based on our results, soil temperature at 5 cm depth is the strongest determinant of fluxes but that WT depth (and VMC) also play a role in some sites. Clearly, there is a certain element of co-variance between soil temperature and WT at play. However, the Reviewer is correct to point out that the effect of WT on the general emission level is different. Our sites are all drained to varying degrees (deeper than -20cm) and as such the emissions that we have measured are all a function of the drainage. While we did not find a close relationship between annual CO2-C and any of the WT parameters (e.g. mean, max or min) across the sites, this is likely to change if rewetted sites (WT shallower than -20cm), for example, were included in the analysis. We have added the following text in the Discussion;
"Given that all the sites are drained to a similar depth (Fig. 1), the variation in emissions appeared to be controlled largely by differences in soil temperatures between the sites (Fig. 6)."

How do you differentiate between areas influenced by domestic peat cutting and otherwise disturbed peatlands with similar WT or vegetation which not used for agriculture or forestry? To do so, you would probably need to define a zone of influence. You briefly mention this problem in the discussion section, and I agree that there will be a problem with the activity data. Do you see any way forward to identify domestic peat cutting areas?
Response: Activity data for domestic peat cutting is highly problematic for both jurisdictions. In the ROI, there are potentially 600,000 ha of peatlands (~30% of the total peatland area) affected to some degree by domestic peat extraction (Malone and O'Connell, 2009). We have added the following text to the discussion of activity data;
"Determining to what degree that peatlands have been affected by domestic peat extraction and how far those impacts extend into the main peatland area are obvious challenges facing future research. The use of remote sensing platforms could provide high resolution data that will be able to differentiate between domestic peat extraction and other disturbed peatlands. In particular, the use of Unmanned Aerial Vehicles (i.e. drones), which have been used to map individual peatlands at a very high resolution (e.g. Knoth et al., 2013) offer considerable potential for more detailed mapping of domestic peatlands at the national scale."

Tables and Figures
The tables and figures are generally of good quality.
Table 1: Please include the WT and the vegetation at the DP sites.
Response: WT values are already presented in Fig. 1. We have added vegetation to Table 1 as suggested.

Figure 2: I don't think this figure is really necessary. If you should chose to keep it, please use percentages instead of absolute counts as due to the different lengths of the study periods the sites are hard to compare by absolute counts. In this case, please add the range of temperatures at which measurements took place.

Response: We feel that this figure is important as it clearly shows which sites are "extreme" in terms of soil temperature and allows for comparison between the sites. We have made the changes as suggested (i.e. percentages and range of temperatures).

Overall, the manuscript is clear and well-written, but, in some cases, uses IPCC-related jargon. Therefore, I would suggest to have the manuscript read by a scientist not familiar with National inventories or reporting issues. Similarly, the discussion should focus a bit stronger on those results interesting for scientists not involved with emission reporting.

Response: We have deleted IPCC jargon text at
P7495, L15-16, 19, 20
P7514, L15-18
We feel that there is a good balance in the discussion as it is. The discussion is composed of seven sections, five of which are devoted to non-emission reporting results.

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