

The impacts of drainage, nutrient status and management practice on the full carbon balance of grasslands on organic soils in a maritime temperate zone

Please find below a list of all final changes to manuscript as suggested by all referees and the editor as well as further explanation to some of the comments made.

We wish to thank all the anonymous referees and Prof. Lars Elsgaard and the editor Prof. Georg Wohlfahrt for their supportive and thorough comments which helped us improve the manuscript.

From Ref #1

P5560 line 9: Consider “Presently” instead of “Nowadays”, and on the same line delete “s” from areas.

DONE

P5572 lines 18-20: It is slightly confusing to state that the “highest monthly NEE value . . .” is a negative number. Consider re-wording to “highest monthly CO₂ uptake . . .”

DONE

P5574 lines 19-21: The mean values are higher in Year 2 not in year 1 – but I would say that both years are not significantly different (Fig 8 and Table 4).

Done. Data has been re-checked and is correct but indeed the sentence is not correct. We propose to change the sentence in the text to: “Annual CH₄-C emissions were not significantly different between years with 1.3 ± 1.09 g CH₄-C m⁻² yr⁻¹ in Year 1 and 1.4 ± 1.1 g CH₄-C m⁻² yr⁻¹ in Year 2 (Table 4).”

P5575 lines 17, 18, 22: Consider replacing “total C flux” with “waterborne C flux” or “fluvial C flux”.

DONE

Section 3 Results overall: In some cases the variability of a result is indicated by a number in brackets () - e.g. 265 (27) - and in other cases as +/- - e.g. 1.3 ± 1.09 . The caption to Table 4 explains the meaning of the () values. If the others are the same, then be consistent.

DONE

P5576 line 16: Checking the values from Table 4 the NECB for Site ad (Year 1) should be 358 not 342 as stated here and listed in Table 4. The others are correct.

An error occurred in the mean NECB for Site Ad (extra cell included in the spread sheet) and 358 is indeed correct. However, the SD was correct. Edits have been done both in the text and Table 4.

P5577 line 2: use “among sites” instead of “at each site”

DONE

P5577 line 20: add “s” to year

DONE

P5579 line 16 and 23: replace “between” with “among”

DONE

P5579 lines 22 & 23: Change “explain the variation in Reco between sites” to “explain the differences in Reco”.

DONE

P5587 line 29 and P5588 Line 1: It’s a small point but to avoid confusion with the publication date of the IPCC report which is 2014, change the first part of this sentence to: As per the updated IPCC guidance, a 90% . . . [correct citation appears at end of sentence]

DONE

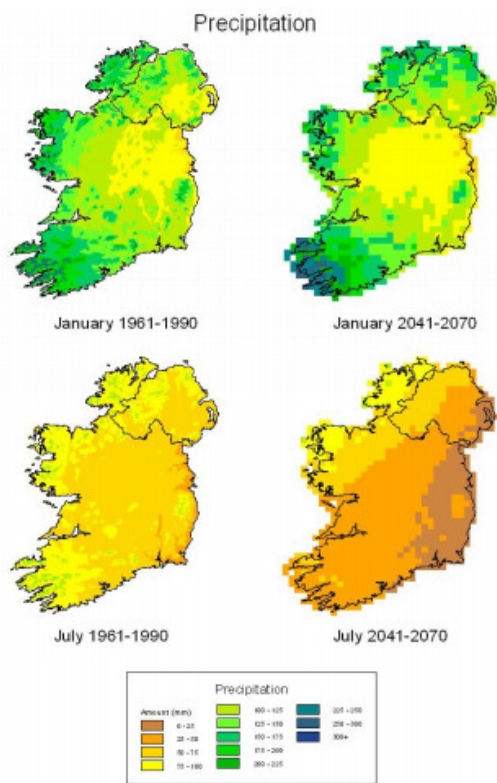
P5588 line 15: Similar to above change phrase to “IPCC 2013 Wetland Supplement”

DONE

From Ref #2

1) At various places it seems too ambitious to disentangle the large number of site edaphic, various management activities, climate and biotic effects on GHG fluxes in this study. Focusing on the main differences among sites (as listed in the title) would be beneficial in my view. Specifically, I find the climate gradient to be too small and doubt that the effect of the very small differences in climate could be related to and conclusively explain differences in the GHG fluxes. At least, I suggest rewording ‘climate effects’ to ‘weather effects’ and remove the idea of investigating climate effects in the hypotheses.

The two main sites (rich and poor) are located in two different geographical locations with respect to climate and in relation to precipitation in particular as seen during the monitoring years. These are considered two distinct climatic regions in Ireland, not only currently but also in terms of climate change predictions (see map below). This could be critical for ‘rewetting purposes’. PPF is also a critical factor in NEE and therefore need to be included. We agree to use the word ‘weather effects’ in the results but we suggest to leave the information and comparison as this is important in the bigger context in terms of locations of these sites (with a view of rewetting).



2) Further related to the above comment, it remains unclear what the relative importance of the individual drivers and what the main controls really are. The conclusion section states ‘NEE estimations were driven mainly by local climate, soil fertility, water table level and potentially soil organic matter quality. These attributes are in turn intimately

linked to past and current management practices in terms of drainage duration and intensity and inputs.' This broad conclusion provides little insight into the main drivers of NEE and other fluxes. I suggest a more quantitative multivariate statistical analysis of the various controls in addition to the currently primarily descriptive nature of the analysis if the goal is to identify the main drivers within the complex interaction effects from the various controls.

This is indeed a good point as our objective is to understand better the drivers but the two locations and only one water table gradient at the nutrient poor site would not be sufficient to carry out a full multivariate statistical analysis. We do provide some information as to important drivers of NECB which also helps stratify the LUC further for reporting purposes.

3) There is a discrepancy between the level of the main goal of this study (: :to support a progression towards the Tier 2 reporting level in Ireland by producing emission factors (EFs) [and NECB] for typical organic soils under grassland) and the detailed mechanistic level in results and discussion. As one example, is it necessary to show and discuss the relationship between LAI and vegetation height in Fig 1 when aiming for estimates of EF and NECB? I suggest that this detailed (but admittedly valuable) information (other examples are listed below) could be moved into the supplementary part. Furthermore, the detailed presentation of results and discussion of individual component fluxes is in general well written (i.e. no redundancies, repetition, etc) but in my view more adequate for a paper focusing on the dynamics of the individual components. As I understand, here, the individual components are being connected to a bigger picture with a higher level study goal. For that purpose I suggest that the text should be adjusted/shortened at various places. I have provided some examples further below. Furthermore, since the main goal is to provide EFs, why not present them in the result section? Currently, there is no information on EFs in any Table/Figure/or results section text, while they are discussed in detail in section 4.5.

This point raises the issue of reporting scientific evidence which has a direct practical application, in this case, to be used by reporting bodies in Ireland. While the impetus of this study was to fill the gap in terms of Irish EFs for grasslands on organic soils, the main remit was to understand the dynamics of individual factors.

We felt that as the manuscript was already long, the addition of an EF table was not warranted as the EFs (Irish specific) were described and discussed in the text. Our study results lead to the computation of EFs and are therefore presented as such in the discussion (this is similar to how GWP is often treated and such presentation of the calculation of EF has been replicated in similar papers, e.g. Elsgaard et al.2012).

The presentation of EFs in the results section with a full table (such as in Petersen et al. 2012) would be warranted in the case of a larger number of sites. As Referee #1 has pointed out, if we were to repeat this study at a few more well-selected sites and for a longer period, the emissions database would be sufficient then to establish a country-specific Tier 2 table for EF.

In order to respond to this comment further, we have edited the introduction so that the objectives of the analysis are clearly stated and the manuscript is not seen as purely a calculation of EF:

*Elsgaard, L., Gorres, C.-M., Hoffmann, C. C., Blicher-Mathiesen, G., Schelde, K., and Petersen, S. O.: Net ecosystem exchange of CO₂ and carbon balance for eight temperate organic soils under agricultural management, *Agriculture, Ecosystems & Environment*, 162, 52-67, 2012.*

Petersen, S. O., Hoffmann, C. C., Schafer, C.-M., Blicher-Mathiesen, G., Elsgaard, L., Kristensen, K., Larsen, S. E., Torp, S. B., and Greve, M. H.: Annual emissions of CH₄ and N₂O, and ecosystem respiration, from eight organic soils in Western Denmark managed by agriculture, Biogeosciences 9, 403-422, 2012.

Specific comments (we understand the pages and lines to correspond to version 2 manuscript submitted 24 March in a word format.

Pg 1, L14ff: Define methane (CH₄) and nitrous oxide (N₂O) the first time and then stick to their abbreviations.

We believed that we followed the journal rules that define a word first time and then use the abbreviation. This applies to the main text, not the abstract. Perhaps the editorial team can advise on this: if the words appears in the abstract, it should be in full (abbreviated in brackets) and thereafter used the abbreviation only.

Pg 1, L17: remove 'NEE' inside the bracket, or reword to e.g. 'NEE = 233 g C m⁻¹yr⁻¹). The same applies to L 27 and 29.

NEE removed L17. We don't see 'NEE' in L 27 and L29 and feel NECB is required in the brackets L29.

Pg 1. L19 why not give actual years instead of Year 1 and 2?

As stated in the M&M, the monitoring year correspond to the period 1 April to March and therefore not a calendar year. As Beetz et al suggested, the exact period used for deriving annual estimates is critical and therefore should be stated as such.

Beetz, S., Liebersbach, H., Glatzel, S., Jurasinski, G., Buczek, U., and Höper, H.: Effects of land use intensity on the full greenhouse gas balance in an Atlantic peat bog, Biogeosciences, 10, 1067-1082, 2013.

Pg 1. L32: 'were also significant factors which impacted: : :'

Replaced 'are' with 'were' and 'impact' with 'impacted'

Pg 5 L 18: Greenhouse gas

Replaced 'measurements' with 'sampling' and replaced 'Gas' with 'gas'

Pg 9, L18ff: The seasonal dynamics of PPFD are well understood and the lengthy description of its standard features therefore not needed here.

We understand this comment pertains to Line 38 of page 9. However, we feel it is reasonable to explain the seasonal dynamics of PPFD for comparison with other 'temperate' sites.

Pg 10, L14: The logic order in the results should be 1. Weather, 2. Biomass, 3-5. CO₂, CH₄ and N₂O fluxes. The current order of the GHG fluxes is interrupted by the biomass section.

We had initially followed this suggested 'typical' order but felt that since photosynthesis and biomass are closely link, the results would 'flow' better together.

Pg 10, L 23ff and several other places: 'The relationship between observed and predicted GPP fluxes was good' – what does 'good mean? Avoid qualitative terms in the result section and instead provide parameters describing the goodness of fit.

While Figure 4 shows clearly the 'goodness of fit' on a 1:1 line, we edited the text and added the r² value between observed (data not used in the model analysis) and predicted GPP fluxes. "Validation of the model showed a strong agreement between modelled versus measured GPP fluxes (Fig. 4) and independent test data (r² = 0.86 at both Sites A and B)."

... "As with GPP, the relationship between observed and modelled R_{eco} was generally strong (Fig. 4) ($r^2 = 0.63$ at Site A and $r^2 = 0.54$ at Site B using independent test data)."

Pg 10, L 27-28: Move speculative content from result into discussion section

These results follow from the analysis of measured data with which statistically and physiologically based response models were built. We do not believe this is speculative content.

Pg 11, L 39ff: Is the information on the biomass N export relevant to the main study objectives? I suggest moving it to the supplementary section.

We felt this information gave support to evidence-based policy that such research is aimed at. If the editor feels this is superfluous, we can delete it.

Pg 13, L 3-10: This section could be moved into the discussion

We believe this information belongs to the 'results' section despite being written in a discussion way.

Pg 13, L12ff: Most of section 3.7 (i.e. L12-22) should be moved into the method section

We feel presenting this information in one location brings the reader more quickly to the point. This type of presentation has been used elsewhere in Biogeosciences papers (e.g. Skiba et al. 2013)

Skiba, U., Jones, S. K., Drewer, J., Helfter, C., Anderson, M., Dinsmore, K., McKenzie, R., Nemitz, E., and Sutton, M. A.: Comparison of soil greenhouse gas fluxes from extensive and intensive grazing in a temperate maritime climate, Biogeosciences, 10, 1231-1241, 2013.

Pg 15, L 10ff: The authors relate GPP to aboveground biomass here but ignore that belowground biomass production can account for a substantial portion of GPP. Is there any information on differences in belowground C allocation and production available? If not at least acknowledge and adjust the discussion accordingly.

We agree with this point. It is well known that GPP also relates to belowground biomass as well as aboveground biomass. It is also known that WT levels would impact on both. But since we did not measure belowground biomass and no data is available, we believe this information would be superfluous information to an already lengthy manuscript.

Pg 17, L7: change 'emissions' to 'fluxes'

Done

Pg 17, L38-40: Provide reference for this statement.

Does the referee mean reference for the fact that it is lower than typical grasslands over peat (there are a lot references for this) or similar to nutrient rich shallow drained. In the latter case, we can add Drösler et al 2013 (already in reference lists) and Jacobs et al 2003 (cit. in IPCC). In the case of the latter, over 15 references are used to calculate the default N2O Emission Factor for drained nutrient rich temperate grasslands over peat. In both cases the reference to the IPCC default EF was deemed sufficient as reference for this statement.

Jacobs, A. F. G., Ronda, R. J., and Holtslag, A. A. M.: Water vapour and carbon dioxide fluxes over bog vegetation, Agricultural and Forest Meteorology, 116, 103-112, 2003.

Pg 18, L14: End the sentence with a period (full stop).

Done

Pg 20, L31-35: Fertilization events were not included (pg 12, L27-28) in this study, thus the EF for N2O might have been underestimated.

We agree with re-iterating this information in this paragraph.

Pg 21, 32ff: Please provide clear take-home messages in the conclusion section, rather than another discussion section.

We feel the take home messages are in the abstract and that the conclusion is usually seen as an opening of the discussion onto a wider context. As such it performs this objective.

Tables/Figures

Adjust the table format to the Journal style.

We took care to use the format requested for the Journal and the editorial team ‘adjusted’ the tables. We will check these upon type-setting of final revised manuscript.

Figure 1, 4 6,7,10 could be moved into the supplementary part

Fig 6 has been removed. Fig 10 appears now earlier in the Results (new Fig 6).

From Ref. #3

Dear Lars,

We are very grateful for your in-depth review of our manuscript and your comments are extremely helpful and no doubt will improve our manuscript.

General comments

It is not clear from the methods description how many times each collar was visited for CO₂ measurements during 8 am to 6 pm on the individual measurement days. This information on data coverage is important and must be included.

-Daily data coverage is indeed very important and in all studies there is a trade-off between spatial (many plots and sites) and temporal coverage (many measurements and visits). We have included additional information in this respect by editing the M&M and including the number of measurements per visit which varied between 2 and 9 (see comment below).

It seems somewhat unfortunate to pool all data from each site to derive models for GPP and Reco. This reduces the information on spatial variability, e.g., among the deep and shallow sites. The Reco models applied at site A and B are very different and apparently without any exponential term for temperature response? I think a model like eq.2 usually comes with an exponential term? Note also that parameter b appears twice in eq.2?

-Agreed. In pooling the data, we do reduce some information, particularly in regard to the response sensitivities at the various sites. However, we found that by pooling the data, we were able to considerably improve the goodness of fit of the models, as we were now able to incorporate a much wider range in environmental variables (e.g. LAI and WT). We feel that approach is vindicated by good r² values and a satisfactory 1:1 fit between observed and modelled data, and with the independent test data. We feel that this then provides for a much more robust estimation of annual CO₂-C balances at these sites.

Equation 2 was incorrectly written in the manuscript. The correct equation which was used for the model is now included:

$$\text{Reco} = (a + (b \times \text{WT})) \times (\exp(c \times ((1/T_{\text{ref}} - T_0) - 1/(T_{5\text{cm}} - T_0))))$$

A treatment of uncertainties in the annual C balances is missing. How was the effect of uncertainty in modelled response parameters addressed? It seems only spatial uncertainty was addressed (by running the models for individual collars and presenting data as mean and standard deviation). Generally, details on reported variability are missing or unclear; I have provided suggestions below.

-Uncertainty in the annual estimates of GPP and Reco has now been addressed by summing up the maximum and minimum standard errors associated with each of the model parameters (following the methodology used in Drösler 2005 and Elsgaard et al. 2012). The largest deviation from the mean was used as an approximate SE estimate of GPP and Reco as applied in Elsgaard et al. 2012. As NEE is not directly modelled, uncertainty in the annual NEE estimate was calculated following the law of error propagation as the square root of the sum of the squared standard errors of GPP and R_{eco}. This approach to uncertainty in the annual estimates is now described in the M&M and the results shown in Table 4.

Detailed comments

Abstract comments:

Introduce abbreviation GHG at first mention (line 4, rather than line 9)

Line 13 and 14: confusing to report C per m² and N₂O per ha; I suggest to change units and unify area

Line 13: spell out net ecosystem exchange, NEE, at this first mention in abstract

Line 13: in information like (NEE 233 g C m⁻² yr⁻¹) I suggest to introduce a comma for clarity, i.e., (NEE, 233 g C m⁻² yr⁻¹). This applies throughout the manuscript.

Line 16: suggest not to use '+' at occasional instances

Abstract responses:

Introduction of abbreviations are now following editorial rules (both abstract and main text should stand alone and therefore abbreviations are spelt out upon first mentioning in each.

L 13 units changed so that all results are now in g m⁻²

L13 advice from editorial team will be sought for preferred way to show information (NEE, 233 g C m⁻² yr⁻¹) or (NEE: 233 g C m⁻² yr⁻¹) or (NEE 233 g C m⁻² yr⁻¹)

L16: all occasional "+" have been removed in front of positive values.

Introduction comments:

Line 20 (p 5559): maybe use 'year' rather than 'annum'

Line 12-13 (p 5561): I guess biomass C export should also be included here in the elements mentioned for calculation of NECB?

Introduction responses:

L20 (5559) 'annum' was replaced by 'year'

L12 (5561) 'as well as biomass C export,' added to the sentence.

Material & Methods comments:

Line 8 (p 5562): (annual runoff, c. 586 mm) rather than just (c. 586 mm)

Line 22 (p 5562): IPCC citation should be 2014 rather than 2013

*Line 9 (p 5563): *Holcus lanatus**

Line 16 (p 5563): '...each collar...' change to '...each collar for GHG measurements (see below)...

Line 1 (p 5564): define PPFd at first mention here (rather than p 5564, line 23); is PPFd used here as equivalent to PAR then PAR can be replaced (p 5563, line 26)

Line 2-4 (p 5564): why are linear regressions used to develop site specific relationships, when data in Fig. 1 are shown with fitted cubic regressions? Indeed, it seems inappropriate to use linear regressions through the origin to fit to data in Fig. 1?

Line 5-9 (p 5564): it seems these 5 line rather belong to the next subheading on 'Greenhouse gas measurements'

Line 11-12 (p 5564): (n = 7 for site A_a, n = 5 for site A_s)

Line 21-24 (p 5564): so you had two series of soil temperature recordings at 5 and 10 cm?

Line 1 (p 5565): '...chamber method (Alm et al., 2007), between 8 a.m. and 6 p.m.' change to '...chamber method (Alm et al., 2007). Measurements were done between 8 a.m. and 6 p.m.'

Line 1-2 (p 5565): no need to define NEE again and no need to give units of PPFd again

Line 9 (p 5565): (PP Systems. UK). Change to (PP Systems, UK).

Line 22-28 (p 5565): I appreciate the consideration of low fluxes which are valid even though r^2 is not high. Indeed r^2 is not a good quality indicator of robust fluxes (see e.g. Görres et al. (2014) *Agriculture, Ecosystems and Environment* 186: 64–76.)

Line 4 (p 5566): stick to 'fortnightly' rather than 'biweekly' (which can also mean twice a week)

Line 7-9 (p 5566): 'Four 50mL samples were withdrawn into 60mL polypropylene syringes from the chamber headspace at 10 min intervals over a 40 min period and then injected...' This is a little ambiguous as samples taken over a 40 min period at intervals of 10 min would amount to 5, rather than 4, samples. Does this imply that no $t=0$ measurements were taken? Please clarify.

Line 9 (p 5566): I guess you used Exetainers®; this could be specified as these come with stoppers known to withstand storage of sampled gases

Line 22-25 (p 5566): what is the reason for being more strict on $r > 0.90$ when it comes to CH₄ and N₂O fluxes? The situation is analogous to the CO₂ fluxes, and you risks to discard a number of valid, but low fluxes.

Line 22 (p 5567): do you mean (see Sect. 2.2) ?

Line 1: delete 'in the soil'

Line 2: WT already defined; just use WT

Eq. 2 (p 5568): parameter occurs twice? Change T to T_{5cm}

Line 6 (p 5568): delete degree sign in front of K

Line 12 (p 5569): NEE already defined

Material & Methods responses:

L8 (5562): text inserted: (annual runoff, c.586 mm)

L22 (5562) IPCC citations updated throughout to 2014

L9 (5563) small cap for 'lanatus'

L16 (5563) 'for GHG measurements' added

L1 (5564) PFFD is defined and used instead of PAR.

L2 (5564) the LAI data was analysed using a linear model with a quadratic term (because the height is the only term squared, this polynomial regression still qualified as a linear model). Polynomial replaces 'linear' for clarity in the text.

L5-9 (5564) Lines shifted to next paragraph

L11 (5564) text edited ($n = 7$ for site A_d, $n = 5$ for site A_s)

L21-24 (5564) yes, the weather station recorded soil temp at both 5 and 10 at each site.

L1 (5565) text edited '...chamber method (Alm et al., 2007). Measurements were done between 8 a.m. and 6 p.m. (2 to 9 measurements per collar per day)'

L1 (5565) abbreviation used and units removed

L9 (5565) (PP Systems, UK)

L22-28 (5565). reference to support this point is acknowledged and included.

L4 (5566) text edited 'fortnightly' instead of 'biweekly'

L7-9 (5566). There was no measurement at $t=0$ and this is now clarified in the text.

L22-25 (5566). The same criteria as for CO₂ applied here. Discarded fluxes were not necessarily low fluxes but were obviously non-linear (due to leakages or other errors during measurement). Text edited.

L22 (5567) changed to Section 2.2

L1 (5568) deleted 'in the soil'

L2 (5568) WT used as already defined

Eq. 2 corrected

L6 (5568) degree sign removed

L12 (5568) NEE used

Results comments:

Line 23-44 (p 5570): 'In Year 2, Site A received similar to Year 1, above long-term average precipitation (1193 mm) driven by high values during June and July, September and October, and December and January. Both the mean...' suggest to rephrase to 'In Year 2, Site A again received higher precipitation than the long-term average (1193 mm) driven by high values during June and July, September and October, and December and January. However, both the mean...'

Line 5-6 (p 5571). It seems from Fig. 2 that cumulative Y1 and Y2 PPDF are very similar, so is your statement correct?

Line 18-20 (p 5571). The model coefficients for R-eco cannot be compared as they are part of two very different models, so rephrase this sentence.

Line 26-27 (p 5571): this qualitative statement could easily be backed up by statistics

Line 4-7 (p 5572): Again, statistics of bias is recommended

Line 27 (p 5572): change 'a-1' to 'yr-1'

Line 5-8 (p 5574): Clarify. '...the highest amount at 265 (27) kg N ha⁻¹ yr⁻¹ compared to 107 (45) and 80 (12) kg N ha⁻¹ yr⁻¹ in Site Ad and Site As. N biomass exports were 17 to 19% lower in Year 2 in Site Ad and As respectively.'

E.g.,:

'...the highest amount at 265 (± 27) kg N ha⁻¹ yr⁻¹ compared to 107 (± 45) and 80 (± 12) kg N ha⁻¹ yr⁻¹ in Site Ad and Site As (data are mean ± standard deviation with n = 7 for Site Ad and n = 5 for site As). Compared to Year 1, N biomass exports were 17 to 19% lower in Year 2 in Site Ad and As respectively.'

Line 12-13 (p 5574): I see no high emission in November ?

Line 19-21 (p 5574): reading this sentence is rather confusing: 'Annual CH₄-C emissions differed between years with higher values (1.3±1.09 g CH₄-C m⁻² yr⁻¹) observed in Year 1 and lower values (1.4±1.1 g CH₄-C m⁻² yr⁻¹) in Year 2 (Table 4).' How is it 1.3 ends up as a higher value than 1.4? I would rather say the values are identical. Also, again, the basis of the used statistics on variability should be clearly stated.

Line 24-25 (p 5574): keep consistency and cite as Year 1 and 2

Line 7 (p 5575): delete '.' in parentheses

Line 8 (p 5575): I suggest to use same area unit as for other fluxes, i.e., 0.16 g N₂O-N ha⁻¹ yr⁻¹

Line 18 and 19 (p 5575): specify that percentages refers to total fluvial C fluxes

Line 15-17 (p 5576): restructure this sentence to more clearly indicate what the basis for variation shown in parentheses represents. This is highly unclear as the numbers are very different from the uncertainties shown in Table 4 ? Also note, that 342 g C m⁻² yr⁻¹ probably should be 358 g C m⁻² yr⁻¹ ? Finally, 'g' is presently omitted in line 16.

Results responses:

L22 (5570) Text edited "In Year 2, Site A again received higher precipitation than the long-term average (1193 mm) driven by high values during June and July, September and October, and December and January. However, both the mean..."

L5 (5571). Data re-checked and statement corrected "There was no significant difference between Year 1 and Year 2 at Site A but PPDF values were consistently higher in Year 2 during the period June-December, except for July."

L18 (5571) Correct, Reco models differ between sites and therefore cannot be compared. Text edited: "The relationships between GPP and the environmental variables differed between the study sites as demonstrated by the different model coefficients derived for equations (1) given in Table 3."

L26-27 (5574) Text edited to include statistical analysis. Validation of the model showed a strong agreement between modelled versus measured GPP fluxes (Fig. 4) and independent test data ($r^2 = 0.86$ at both Sites A and B).

L4 (5572) text edited "As with GPP, the relationship between observed and modelled R_{eco} was generally strong (Fig. 4) ($r^2 = 0.63$ at Site A and $r^2 = 0.54$ at Site B using independent test data)."
L27 (5572) 'a-1' changed to 'yr-1'
L5-8 (5574) edited text as suggested. '...the highest amount at 265 (± 27) kg N ha⁻¹ yr⁻¹ compared to 107 (± 45) and 80 (± 12) kg N ha⁻¹ yr⁻¹ in Site Ad and Site As (data are mean \pm standard deviation with $n = 7$ for Site Ad and $n = 5$ for site As). Compared to Year 1, N biomass exports were 17 to 19% lower in Year 2 in Site Ad and As respectively.'
L12 (5574). The previous sentence states that high emissions are recorded during the summer months. We just wanted to add the information that both months of April and Nov show high fluxes (as high as say July) but not the 'highest'.
L19 (5574) (see Ref. 1) Data has been re-checked and is correct but indeed the sentence is not correct. We propose to change the sentence in the text to: "Annual CH₄-C emissions were not significantly different between years with 1.3 \pm 1.09 g CH₄-C m⁻² yr⁻¹ in Year 1 and 1.4 \pm 1.1 g CH₄-C m⁻² yr⁻¹ in Year 2 (Table 4)."
L24 (5574) text changed to Year 1 added for consistency.
L7 (5575) dot deleted in parentheses
L8 (5575) changed units and text: 0.16 g N₂O-N m⁻² yr⁻¹ and kept m⁻² for consistency (not ha)
L18 (5575). As noted by Ref #1, 'total waterborne C fluxes' or 'total fluvial C fluxes' were added to the various percentage for clarity purpose.
L15 (5576) 342 changed to 358 (see Ref 1 same comment). Numbers in the sentence are however the same as in Table 4. Uncertainties based on NECB for each collar as not all same uncertainty associated with all the terms.
L16 (5576) 'g' added

Discussion comments

Line 11 (p 5579): for clarity: (30% more in Year 1 and 35% more in Year 2 compared to site As)
Line 14 (p 5582): 'maybe' change to 'may be'
Line 28-29 (p 5582): '...temperate climate, emitted only in very small amounts when the mean annual water table was around - 23 cm.' suggest to write '...temperate climate, and that CH₄ was emitted only in very small amounts when the mean annual water table was around - 23 cm.'
These data fits well with a number of recent compilations, e.g., Audet et al. (2013) *Ecological Indicators* 34, 548-559.
Line 4 (p 5583): N₂O
Line 8 (p 5583): '(closer to the IPCC default values for nutrient rich shallow drained).' This is a little insider style: Include mentioning the soil type .
Line 3-4 (p 5586): subheading: '4.5 Implications for reporting and climate change mitigation strategies CO₂, CH₄, N₂O and DOC emission factors' ; please rephrase for clarity
Line 16-17 (p 5586): suggest not to use '+' at occasional instances

Discussion responses:

L11 (5579) text changed as suggested to (30% more in Year 1 and 35% more in Year 2 compared to site As).
L14 (5582) 'maybe' changed to 'may be'
L 28 (5582) changed text as suggested: '...temperate climate and that CH₄ was emitted only in very small amounts when the mean annual water table was around - 23 cm.'
L4 (5583) changed N₂O
L8 (5583) rephrased to '(closer to the IPCC Tier 1 default values for shallow drained nutrient organic soils)'
L3 (5586). During the transfer to pdf, the first heading was 'merged' with the second sub-heading. It should read:

“4.5 Implications for reporting and climate change mitigation strategies”

Then next line:

CO₂, CH₄, N₂O and DOC emission factors

This could be in effect 4.5.1 but since there is no 4.5.2 we felt this sub-heading is sufficient. The editorial team could advise here perhaps.

Tables comments:

Table 1: Table heading: change ‘...from both locations...’ to ‘...from the two research locations...’

The data entries are not aligned with the years; move all data down one row

Table 2: State the data source of Table 2, so methods can be evaluated. E.g., related to LOI-to-OM conversion and OM-to-C conversion if C was not measured directly

NH₄-N rather than NH₄N or is typing intended to be NH₄-N?

Table 3: heading: insert ‘and’ between (GPP) and ecosystem respiration. Put Reco in parentheses.

What about T₀ from eq. 2? Was that modelled or assumed to a fixed value; please specify. Further, in the text r²= 0.85 is mentioned for GPP model at Site B (p 5571, line 26) – should this be the correct R² to include in the Table (rather than 0.72) ?

Table 4: As done for NECB, the other parameters should be spelled out. State the basis for the SD and SE values, i.e., specify n and that they represent the spatial uncertainty. NECB Site Ad sums to 358 rather than 342.

Table responses:

Table 1: changed text to “from two research locations”. Figures were aligned and will be checked upon typesetting.

Table 2: C and N were directly measured and the instrument stated as a footnote. Ammonium was measured and is usually abbreviated NH₄-N. Typesetting gives a wrong impression of ‘negative’. This will be double checked after new typesetting of paper.

Table 3: inserted ‘and’ between (GPP) and ecosystem respiration and put Reco in parentheses. T₀ is set at 227.13K. This has been added to the text. r²= 0.85 is the correct value for GPP model at Site B (p 5571, line 26) and was corrected in Table 3.

Table 4: 242 changed to 358.

Parameters have been spelt out. The uncertainty has now changed and is fully explained in the text and we feel that it is not necessary to include it again here for each parameter as the caption is already lengthy.

Figures comments:

Figure 2: panel identifier (a) is missing from upper panel. Note umol rather than μmol on y-axis, panel A. Caption: delete ‘(a)’ in front of PPF_D. Maybe write PPF_D in full text in the figure caption

Figure 3: specify what error bars in panel a represent. At what depth were VMC measured and are the low contents in May believed to be real? Maybe you could have included precipitation data to substantiate the dynamics.

Figure 4: I suggest to spell out Reco and GPP in the figure caption. Has the accordance between data and 1:1 lines been tested? And just a detail, h-1 is used in caption whereas hr-1 is used on axes.

Figure 5: amend y-axes to 'CO₂ flux (g CO₂-C m⁻²). Figure caption: state the basis for standard deviations (e.g., standard deviation of means for CO₂ fluxes for all collars at a site, i.e., with n = 7 for Site Ad etc...)

Figure 6: I suggest to delete this figure

Figure 7: Again, indicated source of variation.

'...grazing events...' change to '...simulated grazing events (cuttings)...'

y-axis: put cm day⁻¹ in parentheses

Figure 8: amend y-axis to read: 'CH₄ flux (mg CH₄ m⁻² h⁻¹)'

Figure 9: cf comment to Fig. 8 and note 'μ' should be used on y-axis. Further the source and amount of fertilizer could be mentioned in the caption

Figure 10: spell out LAI in caption and indicate units on x-axis. Please provide some info on the nature of the points in Fig. 10. I guess each collar is represented 2 times (Year 1 and Year 2) to give 10 and 14 points. But what about LAI; when are the measurement done, or are the data a seasonal average? Please specify.

Figures responses:

Figure 2: The panel identifier (a) was not missing from the upper panel in the original and must have been lost during the upload. μmol on y-axis, panel (a) was added. Caption: deleted '(a)' in front of PPFD. PPFD has been written in full text in the figure caption.

Figure 3: error bars in panel (a) were specified in addition to the depth that VMC was measured at. We believe that the low VMC contents measured in May are the response to a very dry spell from mid April/early May (caused by a combination of low RF and increased temperatures). This is perhaps more clearly seen in the figures below, which show hourly RF (mm), and where T5CM and VMC data (recorded at 10 min intervals) are overlain. The low VMC values observed for this period are supported by WT data from the site (not shown), which showed that WT levels in early May were deeper than we were able to detect with our experimental set up. We agree that precipitation data would add useful information to this graph; unfortunately the sensor logging rainfall at our site malfunctioned in late December. We do not feel that it would be useful to provide rainfall data in such an incomplete form for the manuscript but are happy to incorporate it here in our response to your comment.

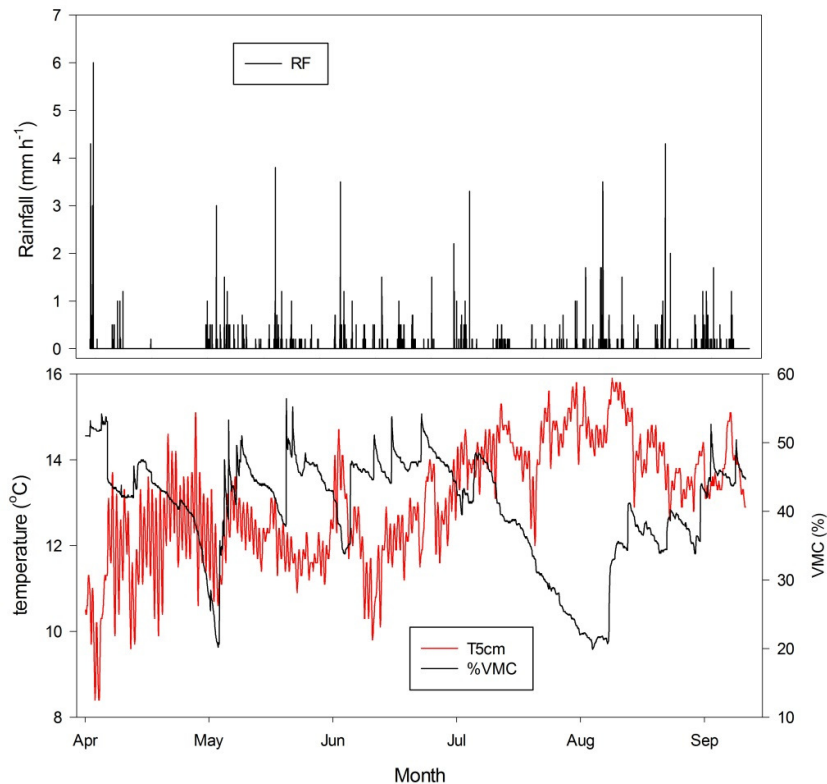


Figure 4: Reco and GPP has been spelt out in the figure caption. Accordance between data and 1:1 lines is explained in the text. h-1 is now used instead of hr-1.

Figure 5: amended y-axes to 'CO2 flux (g CO2-C m-2). Edited what standard deviations are.

Figure 6: Fig 6 has been removed.

New Figure 7: added source of variation and changed text to "...simulated grazing events (cuttings)..." and put cm day-1 in parentheses

Figure 8: amended y-axis to read: 'CH4 flux (mg CH4 m-2 h-1)'

Figure 9: edited μ on y-axis.

Information related to fertilisation event was added in the caption

Figure 10: LAI spelt in caption and units on x-axis. New information in caption pertaining to the nature of the points. 'Mean annual' added to LAI and Year 1 and 2 combined. Information regarding when and how LAI was measured is in the text and repetition in this caption unnecessary. This is now Fig. 6 in the revised manuscript.

Thank you.

Dr Florence Renou-Wilson