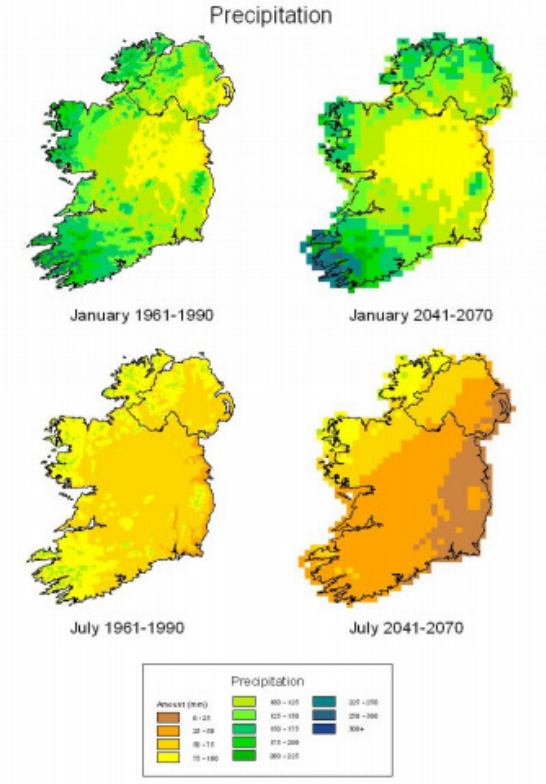
**Dear Referee #2**
**We are grateful for your comprehensive comments and we are happy to perform some of the following corrections as suggested. We have explained our reasoning behind not changing other aspects that you kindly proposed.**

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'. PPFD 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 suggest to edit the introduction so that the manuscript is not seen as purely a calculation of EF. For example :**
"This study ~~aims~~ helps to support a progression towards the Tier 2 reporting level in Ireland by producing emission factors (EFs) for $CO_2$, $CH_4$, $N_2O$ and DOC for typical organic soils under grassland."

*Elsgaard, L., Gorres, C.-M., Hoffmann, C. C., Blicher-Mathiesen, G., Schelde, K., and Petersen, S. O.: Net ecosystem exchange of $CO_2$ 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 $CH_4$ and $N_2O$, 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 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 corresponds to the period 1 April 2011 to March 2013 and therefore not a calendar year which is why we don't call them 2011 and 2012 would be confusing. 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., Buczko, 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
**Removed 'measurements' 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 inititally followed this suggested 'typical' order but felt that since photosynthesis and biomass are closely link, the results would 'flow' better together. We have no problem moving it if the editor feels this is required.**

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.**

"The model predictions **agreed well** the observed GPP fluxes (Fig. 4) as was the accuracy of predictions based on the independent test data that were employed for model validation (**$r^2$ = 0.86 at both sites**). However, higher variation was evident in the predicted GPP fluxes at Site B, particularly at low to medium flux rates (1000 - 3000 mg $CO_2$ $m^{-2}$ $hr^{-1}$), **despite the residuals being normally distributed.**

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 results as a whole 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
**There are indeed various view points with regards to the presentation of budgets and other 'calculated' variables. We feel presenting this information in one location brings the reader to the point quicker. 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 N20 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 as they saw fit (i.e. we didn't have anything to do with the re-formatting of the table).**

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

**We feel these figures are all necessary to the main objective of the paper as discussed earlier.**