

Interactive comment on “The impacts of drainage, nutrient status and management practice on the full carbon balance of grasslands on organic soils in a maritime temperate zone” by F. Renou-Wilson et al.

Anonymous Referee #2

Received and published: 6 May 2014

This study is a comprehensive effort to provide emission factors (EF) and estimates of the net ecosystem carbon balance (NECB) by accounting for exchanges of the most important greenhouse gases and lateral fluvial and biomass carbon exports. The authors identify the lack of such data existing for extensive organic soils and thus provide a very valuable contribution. The methodology used is based on standard techniques, well described, and sound. The results are convincing and well discussed with adequate references to the literature. Overall, I recommend that this manuscript is worthy of publication after addressing following comments outlined below:

C1431

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.

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.

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 com-

C1432

ponents. 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.

Specific comments:

Page (Pg), Line (L):

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

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

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

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

Pg 5 L 18: Greenhouse gas

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

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.

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.

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

C1433

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.

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

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

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.

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

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

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

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.

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

Tables/Figures

Adjust the table format to the Journal style.

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

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C1434