

## ***Interactive comment on “The nitrogen, carbon and greenhouse gas budget of a grazed, cut and fertilised temperate grassland” by Stephanie K. Jones et al.***

**Stephanie K. Jones et al.**

stephanie.jones@sruc.ac.uk

Received and published: 14 October 2016

General comment; We would like to thank the referee for the comments.

Firstly, we accept the observation of the referee that the many of the measurements reported covered only for a part of the experimental period, and that some modelled and literature values were included. The data were collected during three different projects (GREENGRASS, CarboEurope and NitroEurope), each with different objectives. Therefore some gaps in the series of observations were unavoidable. Although some of the data has already been published, preceding publications focus on shorter timescales and narrower (often subannual) objectives. The novelty of this manuscript is indeed the aggregation of data to derive a total C and N budget and to present data

C1

on soil C and N stock over a long time period.

Secondly, we agree that by and large we cannot separate the effects of management and weather on C and N cycling processes. Our aim was firstly to characterise the overall C and N budgets for the site but then as a secondary objective provide commentary on clear relationships that were apparent between aspects of management and climate that were linked to the observed nutrient fluxes. Doing this over the long time period, with the typical variations in farm managements and climate, will provide a more robust interpretation of the key components one may want to pay attention to in future with improved measurements and modelling (for C: NEE leaching of DOC and harvest and for N; N<sub>2</sub> loss and NO<sub>3</sub> and DON leaching, which are difficult to measure). So there is an important message to the researchers and modellers to characterise and improve our understanding of these poorly described nutrient fluxes.

Considering the third point addressed by the referee, we appreciate that there is a spatial variability. However, spatial heterogeneity was not a specific focus of this manuscript (for the GHGs this is covered in Skiba et al. 2013 (Biogeosciences, 10, 1231–1241, 2013). The hollow position (wet area) which was not used for NO<sub>3</sub> leaching measurements only covered a small part of the field and was therefore avoided for N<sub>2</sub>O, CH<sub>4</sub> and soil measurements, but included in the footprint of the EC measurements.

We agree that the manuscript can be improved by shortening some parts. We have shortened the introduction section as well as paragraph 3.1 of the results section by omitting figure 2 and its description and some further parts of the result section (3.3). We have addressed all specific comments listed in the included PDF file.

specific comments;

Page 2, line 42: Better "stocks". Pools in plural suggests that different pools per element were assessed.

We replaced "pool" with "stocks"

C2

Page 3, line87: What is the source for this?

Increased N input through intensive N management will lead to a decrease in the C/N ratio of the soil resulting in increased nitrification and denitrification processes and thus N<sub>2</sub>O losses. Nitrogen fertilisation is also a key factor inhibiting CH<sub>4</sub> oxidation in soils. The NH<sub>4</sub><sup>+</sup> from the added fertiliser has an inhibitory effect on CH<sub>4</sub> oxidation, as it competes with CH<sub>4</sub> as a substrate of the enzyme methane monooxygenase (MMO). Mosier et al. (1991) reported an inhibition of CH<sub>4</sub> uptake on grassland by 41 % after the application of N fertilizer.

Page 3, line 99: It might be helpful to start with this part of the introduction, which contains the motivation to study grassland biogeochemistry. The paragraphs above should be shortened, e.g. very general sentences like the very first could be left out.

We agree with the reviewer and have changed the structure of the introduction and shortened some parts.

Page 4, line 127: quotation mark a typo?

Yes, this is a typo; Greengrass, EC EVK"-CT2001-00105 has been changed to Green-grass, EC EVK2-CT2001-00105

Page 4 line 134: "something" - should not this be a number (for temperature as well)

We apologise for this mistake, the actual standard deviation have been added (947 ± 234 mm, 9.0 ± 0.4 °C)

Page 4, line 136: common names are "perennial ryegrass" and "white clover"; "perenne" in scientific name should be lower case.

This has been corrected

Page 5, line 142: The size of the field should be given here already. What was the relief of the field (slope and hollow are mentioned later)?

### C3

The size of the grassland field (5.424 ha) has been added. The maximum gradient in the field is 2.5 %, so although not completely flat, the topography is only gently sloping. The field has a mean slope from NW to SE, with the steepest slope some 100 m to the NW of the EC flux tower. This information has been added.

Page 5, line 145: What was the grazing management? Continuous or rotational stocking? Animal numbers appear to change frequently (Figure 1a). Did animals receive any additional feed while on pasture? Was the field subdivided? It seems that in 2004 and 2005 spreading of organic manure occurred during grazing or was immediately followed by grazing.

The grazing management can be described as rotational grazing; The farmer moved animals from neighbouring fields to the Easter Bush field when the grass was high enough and then moved them to neighbouring fields again when the grass was too short at the Easter Bush field to allow the recovery and growth of the pasture plants after grazing. This represents a common management practice by farmers in this region. This information has now been added and "grazed continuously" replaced with "grazed rotationally". The field was not subdivided. The ewes got additional feed in spring (standard cake concentrate), this information has now been added. The slurry spreading was indeed followed shortly by grazing; the slurry was spread specifically for a measurement campaign.

Page 5, line 147: bracket missing

Instead of adding a bracket, the bracket before "Scottish Agricultural College" has been replaced by a semicolon.

Page 7, line 215: I do not think the "equals" sign should be used here, since 1 kg N cannot be equal to 1 kg C (even if its climate forcing effect was equal).

We agree that this sign is confusing. We therefore replaced the equal sign with "corresponds to".

### C4

Page 8, line 265: bracket missing

Missing bracket has been added

Page 9, line 287: where

Were has been changed to where

Page 11, line 365: At how many quadrats of which size, and at which intervals? 50 quadrates of 0.25 m<sup>2</sup> (0.5 m) were randomly thrown into the field to measure the species cover. The species composition has been measured once in 2002 and at monthly intervals in 2003. This information has been added.

Page 11, line 370: During which time period?

NEE was measured continuously from 1.January 2002 till 31.December 2010. This information has been added.

Page 12, line 384: The URL should probably be given in the reference list, or else in the text directly

The URL has been included in the text directly

Page 12, line 391: Is it legitimate to extrapolate from findings for forests to grasslands? Both vegetation types differ considerably in the proportion of photosynthetically active tissue to total tissue (and other aspects).

The Waring 1998 reference is indeed based on forest studies. The point is made more generally by Amthor (2000) and others, who show that there are strong theoretical constraints on this ratio, and that the estimate of 0.5 is reasonable. For example, Zhang et al (2009) estimate the variability across global biome types, and suggest a value of ~0.5 for grasslands. Note that the exact value assumed for this ratio is not critical to our results: the appearance of the pie chart would not change.

Amthor, Jeffrey S. 2000. 'The McCree-de Wit-Penning de Vries-Thornley

C5

Respiration Paradigms: 30 Years Later'. Annals of Botany 86 (1): 1–20. doi:10.1006/anbo.2000.1175. Zhang, Yangjian, Ming Xu, Hua Chen, and Jonathan Adams. 2009. 'Global Pattern of NPP to GPP Ratio Derived from MODIS Data: Effects of Ecosystem Type, Geographical Location and Climate'. Global Ecology and Biogeography 18 (3): 280–90. doi:10.1111/j.1466-8238.2008.00442.x.

Page 13, line 438: A 10 m grid with 100 points would cover roughly one hectare. How was this positioned in the pasture (with respect to slope, to areas potentially preferred by livestock, etc.)? Was the same grid used for sampling in 2004 and 2011?

The objective for the soil sampling grid was to cover the main footprint area of the site, rather than covering the entire field. We apologize that this information was missing and have now added it to the manuscript. The grid was positioned independently from slope and potentially preferred areas to avoid biased sampling. For the resampling in 2011 the same grid was used, but the transect was chosen two meters further to the NW in order not to meet the same places we already sampled and disturbed before.

Page 14, line 467: As I understand it, only confidence intervals for parameter means were calculated (while ANOVA should involve comparison of group means).

It is correct that CIs for individual parameters were calculated. The confidence intervals for group means (e.g. data presented in fig 5) were used to establish whether or not differences were significantly different from zero.

Page 14, line 472: Correlations between annual means of parameters are presented in the results. What type of correlation were these? Were the averages used in these correlations calculated by calendar year?

Correlations presented were positive linear correlations. Averages used in these correlations were calculated by calendar year.

Page 16, line 547: at various points in the results, coefficients of determination are given for sub-datasets from which certain years were omitted (e.g. L545-547), but no

C6

reason is given for excluding these particular years. If the reason is only that these years decrease  $R^2$ , this would not appear to be legitimate.

We agree with the referee's comment and decided not to include coefficients of determination for sub-datasets from which certain years were omitted.

Page 17, line 578: Means and standard deviation of the soil C and N concentrations (or stocks) both in 2004 and 2011 should be given in addition to the difference.

Mean and 95% CI of soil C and N concentrations are now included in the result section.  
Page 19, line 640: Which average? Per calendar year?

Annual NGHGE were highly correlated with annual NEE. This has been clarified in the manuscript

Page 20, line 655: Inhibition of biological N<sub>2</sub> fixation by high soil mineral N, and competitive exclusion of legumes are two different (and only indirectly linked) issues. Additionally, higher proportions of *T. repens* than observed here can persist even under comparable management intensity, so that height of N inputs alone probably cannot explain the virtual absence of legumes in this grassland.

We agree that the inhibition of biological N<sub>2</sub> fixation by high soil mineral N and the competitive exclusion of legumes are two different issues. This has been clarified in the text. The grass was sown as a grass mixture only and no clover was sown, which is the reason for the low clover content.

Page 20, line 681: NUE of different studies using either forage or animal product as the basis for calculating cannot be compared directly at all. This is not a matter of grazing vs. cutting, but of the trophic level that is considered in the evaluation of NUE. This whole paragraph should be rewritten taking this into account.

We agree that the inclusion of grazing adds a new trophic level and the NUE in the grass itself is not lowered due to grazing, but the sum of NUE of harvest and a lower NUE for grazing leads to a total lower annual NUE. We have now clarified this in the

C7

text.

Page 21, line 689: See my comment above: It would appear that other factors apart from fertilization limit competitive strength of legumes at this site.

Yes, the main reason was that no clover was sown. Furthermore low pH and wet conditions are not favourable for clover, both factors were present at the Easter Bush field.

Page 21, line 692: Was it not stated that fertilization stayed within recommended maximum levels? (L670f)

Yes, the fertilisation stayed within the recommended maximum levels. WE have changed "avoidance to over fertilisation" to "reduction of fertiliser input".

Page 22, line 722: For better comparison, g N m<sup>-1</sup> y<sup>-1</sup> should be used as unit here as well.

The units have been changed from kg N ha<sup>-1</sup> y<sup>-1</sup> to g N m<sup>-1</sup> y<sup>-1</sup>

Page 23, line 776: Table 6 shows nine years (although 2010 is only the mean of the other eight), and it seems that only four of them exceed 3.0%.

This has been corrected in the manuscript. N<sub>2</sub>O fluxes were measured in 2010, therefore the N<sub>2</sub>O emission factor in 2010 is not the mean of the other eight.

Page 23, line 780: Can this be generalized over all Scottish soils?

Yes, generally Scottish soils are high in SOC, see "Buckingham,S., Rees,R.M. & Watson,C.A. 2013. Issues and pressures facing the future of soil carbon stocks with particular emphasis on Scottish soils. The Journal of Agricultural Science, 152, 699-715." We added this reference in the text.

Page 24, line 813: missing space

The missing space has been added

C8

Page 29, line 955: missing space

The missing space has been added

Page 34, line 1185: Wrong alphabetical order

The wrong alphabetical order has been corrected

Page 35, line 1254: duplicate

The duplicate reference has been deleted

Page 37, line 1333: Initials before name.

The initials have been moved after the name.

Page 42, line 1539 (table 1): Correct term would be "stocking rates". Abbreviation LSU should be explained here.

"Livestock densities" has been changed to "stocking rates" and the abbreviation LSU has been explained (with asterisk)

Page 42, line 1545 (table 2): volumetric or gravimetric content?

Values are given for volumetric soil water content (% by volume). This information has been added.

Page 42, line 1546 (table 2): Based on which temperatures?

The plant growing season begins and ends with periods of consecutive days where daily temperatures average more than 5C without any five-day spells of temperatures below 5C. This information has been included in table 2. For some reason some of the values for the length of growing season were not correct; values have now been corrected.

Page 42, line 1552 (table 3): "how much for measurements?" - is something missing here?

C9

We apologise for the missing value and have now included the systematic uncertainty value for measuring leaching of 10%.

Page 43, table 4: Why was this not modelled, as for the other times where no measurement data were available? (I did not find this stated in the methods section).

Wet N deposition fluxes 2002-2009 are measured values and dry N deposition fluxes for 2002-2009 were modelled using measured concentration values. Neither of these data were available for 2010 when we originally wrote the manuscript and therefore averages were used. But in the meantime those data are available and are now included in table 4. We apologise for this mistake. Missing C and N leaching values for 2010 have also been modelled and are now included in table 4.

Page 45, table 5: Since this is only the mean of 2002-2009, it should not be included here.

N2O fluxes were measured till November 2010 and therefore 2010 data are not a mean of 2002-2009. We corrected the information in the method section, where it was stated that N2O fluxes were only measured till December 2009. Originally we were not planning to include 2010 data in the manuscript, we apologise for the confusion.

Page 46, figure caption 1: "Stocking density" would be more accurate here; LSU should be explained; Letters for sub-figures a, b, c, are not quite correct; "input and export" should be replaced by probably be added to the y axis as well.

We have now changed "Live stock density" to "Stocking density", explained the term LSU and corrected the letters for sub-figures.

Page 46, figure caption 2: Volumetric or gravimetric soil water content? Given that only annual averages are presented for the measured values, this Figure might not be necessary.

Volumetric soil water content was measured, which is now stated in the figure capture. We agree that we don't discuss inter-annual variations of C and N fluxes/concentrations

C10

and that therefore this figure is not needed.

Page 46, figure caption 4: Figures 3 and 4 essentially duplicate information contained in Tables 4 and 5; regarding the division into grazing-only vs. grazing-and-cutting years, please see my general comments

We think that Figure 3 and 4 are valuable to investigate the influence of different managements on the C and N budget and therefore prefer to keep these figures in the manuscript.

Page 46, figure caption 5: Abbreviation NEE should be explained here

We have now included the explanation of the abbreviation NEE in the figure capture.

---

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-221, 2016.