Response to reviewers and handling editors comments from 8th January 2021 on our manuscript "Memory effects on greenhouse gas emissions (CO2, N2O and CH4) following grassland restoration?" by Lutz Merbold et al.

We thank both reviewers and the handling editor for the final positive evaluation of our manuscript. We adjusted the manuscript as requested. Similar to the previous exchange, in the following sections, the reviewer/editor's comments are stated first, followed by our response in *italic* font.

Reviewer #1:

Having initially reviewed an earlier version of this manuscript, I am pleased to see most of my concerns have been addressed and alleviated (aside from a couple of outstanding points described below). Within this review, I was also asked to comment on the methodological concerns highlighted by the second reviewer, which have not been modified in the current revision of the manuscript. I am of the belief that the manuscript as it currently stands is sufficiently sound methodologically and suitable for publication after addressing a few minor points. Below I have firstly commented on the methodology concerns of the second reviewer, then concluded with a few minor points requiring the authors to address.

Comment on Methodology

The primary concerns of the second reviewer were the use of a running median as a gap-filling method for N2O, and the lack of uncertainties presented. In general, I am in agreement with the author's response on most matters. Given the main purpose of this study was to calculate and present annual GHG exchange from a managed pasture a complete time series of N2O data was a required. As the authors describe, N2O gap-filling is rapidly advancing field with many approaches, but as yet there is no generally accepted and standardised method. A running median approach (particularly for the EC measurements) is as valid as any other method, and certainly appropriate within the scope of this study. Having said that, I find gap-filling of the chamber N2O fluxes with their limited data coverage, (particularly as a single measured is considered representative of a day) rather tenuous, especially given the log-normal distribution of fluxes as noted by the second reviewer. Nonetheless, it is a suitable option to provide an annual estimate of N2O emissions, but this limitation should be commented on within the manuscript where these results are presented.

The lack of uncertainties presented in this study is in line with other recent GHG studies. Certainly, an uncertainty estimate could be applied to the CO2 fluxes where there are well-established methods, and maybe methane, but there are very few papers where uncertainty associated with annual N2O estimates is calculated . Accordingly, given the purpose of this study (annual GHG balances), inclusion of uncertainty to some components and not others prevents an uncertainty estimate for the GHG balance and thus are of marginal benefit. Therefore, I see the results as presented acceptable and in line with current practice.

As mentioned before, we are grateful for the positive evaluation.

Minor Comments and Corrections

• Lines 302-304: I think it would be appropriate to add the % of flux values rejected due to r2 < 0.8 (this would also be in line with the second reviewers comment #3).

We added the link to the paper Imer et a. 2013 where the chamber fluxes of 2010 and 2011 were first presented. In addition, we updated Table 1 – Data availability. This now shows the sampling days as well as the HQ data, i.e. how many data points were above the detection limit for both CH4 and N2O in the years 2010, 2011 and 2013. While there were no changes to N2O, we needed to adjust the number of HQ values for CH4, which does not change the conclusion of the paper but rather further confirm that the 2010 and 2011 values can not be used for GWP calculation.

• Lines 468-470 (and Table 2): for 2010 and 2011 where an annual methane flux is not available, and the annual N2O flux is calculated from very limited data, I question the validity of presenting annual GWP values here for these years. Suggest revising this sentence, and at worst including the disclaimer than 2010 and 2011 are incomplete budgets. (Also, note that the value in line 469 of -387 is now -385 in Table 2).

We added the following sentence in the revised manuscript: As an important note, due to the limited data availability for the years 2010 and 2011, the budgets of these years are likely incomplete. Furthermore, we adjusted Table 2 by similarly highlighting the careful interpretation of the 2010/2011 budgets for N2O.

• Line 565: My initial review highlighted the need to acknowledge the role of enteric methane, which the authors have now done, but I believe there to be a calculation error here. I believe their calculation should be as follows:

1. 404 g CH4 head-1 day-1 * 4.04 head ha-1 * 30 days / 1000 g kg-1 = 48.96 kg CH4 ha-1
2. 48.96 kg CH4 ha-1 * 12/16 = 36.72 kg CH4-C ha-1
3. 36.72 kg CH4-C ha-1 = 3.67 g CH4-C m-2

This is a value is an order of magnitude larger than the 0.407 g CH4-C m-2 stated in the manuscript. This should be corrected, but does not affect the manuscript in any significant manner.

This is correct, thanks for pointing this out. We adjusted this value in the text.

As an aside, the authors could not follow my initial calculations as presented in my first review, and my apologies, I also made a mistake, and they should have been as follows:
1. 1769.9 kg C ha-1 * 3% = 53.1 kg CH4-C ha-1 (proportion of grazed pasture returned as CH4 assuming 3% is returned as methane)
2. 53.1 kg CH4-C ha-1 = 5.31 g CH4-C m-2

This value would be broadly similar to the above calculations by the authors, and thus I find their calculation suitable.

Thank you for clarifying and indeed the magnitude is similar to our estimate.

• Line 597: The start to this paragraph feels like it should be included in the previous paragraph. Revise and correct if needed.

Done

• Table 4: please check the Parcel A and Parcel B labelling for Fertilizer and Harvest lines – these still appear incorrectly labelled.

Corrected

Comments by Sara Vicca – Handling Editor

Dear authors,

The two reviewers have now seen your revised manuscript. Despite some disagreement about the methodology, the first reviewer did not provide further comments and indicated that the manuscript could be accepted for publication.
I asked also for the opinion of the other reviewer regarding the methodology and also verified this myself. I agree with the second reviewer that your approach is suitable for the scope of the study.

Thank you for also evaluating the methodology and the positive assessment.

The second reviewer provided some minor suggestions that I would like you to consider before accepting your manuscript for publication. I agree that for 2010 and 2011, the limitations for the GWP calculation need to be clarified in the results section and in Table 2. Note also that the legend of Table 2 was also not fully visible in the uploaded pdf.

We have checked Table 2and adjusted this and similarly we clarified the limitations for GWP calculation in the year 2010 and 2011 in the text. See also our response to the reviewer. We have further corrected