Response to reviewers 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 for their critical assessment and provide an answer on how we foresee to address the individual comments in a revised manuscript. Given that reviewer #1 already provided a full assessment during the access review phase we just want to restate again that we intend to provide the necessary changes in the revised manuscript as originally suggested with further amendments following the comments provided by reviewer #2 whom has raised has raised similar concerns and some others. Kindly find our response below. The reviewer's comment is stated first, followed by our response in *italic* font.

Reviewer #2:

The study presented here title "Memory effects on greenhouse gas emissions (CO2, N2O and CH4) following grassland renovation?" presents trace gas measurements from 5 years of a grazed and harvest pasture in Switzerland including a pasture restoration event. In general, this is a well written and worthwhile study. Few studies report all greenhouse gases, and even fewer for multiple years and covering infrequent management activities. I believe this to be of publication quality following consideration of my commentary below. I have separated my comments into major, moderate and minor/technical concerns based on importance and impact to the manuscript as I see it. I believe these can be dealt with by the authors and would further enhance the manuscript.

We thank the reviewer for this positive assessment and share the opinion of few studies reporting on multiple years of GHG exchange measurements of the three GHGs covering specific management activities.

Major concerns

1. CH4 fluxes: I have major concerns with the usage of the CH4 fluxes as presented in this manuscript. Firstly, while the authors present a comparison of N2O chamber and eddy covariance data (Figure 3), they do not for CH4. I believe this is likely as the comparison does not suggest any 1:1 relationship (based on my interpretation of Figure 4b). The authors then use this chamber data to derive annual CH4 fluxes for the years without EC data and assume to be comparable with the EC derived annual fluxes. From the data presented, I see no evidence to believe this to be the case (unlike N2O). Given the two chamber years suggest a small uptake of CH4, while the last three a release of CH4 coinciding with a difference in measurement methodology, I question whether the authors really believe these years are comparable. While the authors discuss these methodology differences in detail in the discussion section, and overall the contribution of CH4 to the GHG budget is small, I believe further attention needs to be given to this, and ideally the equivalent plot to figure 3b is presented for CH4. Based on the timing of management events (pasture restoration) and change in measurement methodology it could be easily interpreted as pasture restoration changes grassland CH4 exchange from an uptake to release.

These are indeed relevant points and surely, we do not want to give the impression that pasture restoration changes grassland CH4 exchange from an uptake to release as this can not be proven by the data presented in this study (see following response). We had preferred to show a similar comparison as given for N2O, however the methane concentrations measurements were not reliable in 2013 due to a flame ionization detector (FID) malfunction in the gas chromatograph.

*Overall, we also did not expect to find a similar relation between the methane flux measurements obtained by eddy covariance and chambers caused by the small magnitude of the fluxes measured. As stated in the original manuscript "*We calculated detection limits for the individual GHGs from our manual chambers following (Parkin et al., 2012). Detection limits were 0.34 ± 0.26 nmol m^{-2} s^{-1}, 0.05 ± 0.02 nmol m^{-2} s^{-1}, and 0.06 ± 0.06 µmol m^{-2} s^{-1} for CH_4, N_2O and CO_2, respectively, clearly indicating that methane fluxes measured by GHG chambers in 2010/2011 were on average -0.16 ± 0.16 nmol CH_4 m^{-2} s^{-1}, (see Table 2) and thus below the actual detection limit."

However, we did compare our eddy covariance methane flux values (methane fluxes fluctuating around 0 with an overall range of -40 up to +40 nmol CH4 m-2 s-1 (Figure 4 b)) with the values reported by (Felber et al., 2015) *from a similar grassland system in Western Switzerland.* (Felber et al., 2015) *have shown that such values measured by the EC technique represent a soil signal (Figure 6 in* Felber et al. 2015*).*

Following this, we agree that we should not have computed annual sums for the years 2010/2011 for methane and will remove these in the revised manuscript. Yet, we will remain with the numbers presented for methane in 2012 -2014. We want to stress again, that methane fluxes are of minor importance for the carbon and greenhouse gas budget of the site under the current management (see also our response to the following concern made by reviewer #2 on the influence of grazing animals on methane fluxes).

2. The impact of grazing needs further consideration. While harvesting is more common in this study, the impact of grazing needs further clarification and/or modification of the presented results. Firstly, it is unclear to me how the grazing off-take was estimated (please clarify), and whether the deposition of excreta C was included in the C balances. While I'm not familiar with sheep grazing, at least for cattle this can be in the order of one-third of consumption, and therefore not an insignificant component (especially for 2014, Parcel A with 1769.9 kg C ha-1 of grazing removal according to table S1) and requiring acknowledgement of how this is currently dealt with, or included in the C balance (e.g. Table 2).

Furthermore, the authors state they did not detect any CH4 release with grazing (lines 432-433). Using the example of Parcel A in 2014, which was primarily grazed by cattle, and assuming _3% was converted and released as CH4 (e.g. Felber et al. (2016)), 53.1 kg C ha-1 would have been emitted from the grazers as CH4, which when converted to g CO2-eq m-2 calculated to 240 g CO2-eq m-2 or much larger than the 55 g CO2-eq m-2 reported in table. If this was not detected, then I suggest the authors reconsider how grazing related CH4 is dealt with in this manuscript given they are reporting ecosystem scale GHG budgets.

Indeed, methane emissions from grazing animals need to be considered in annual budgets of methane and carbon. We argue that these are already accounted for in our data. What needs to be noted is that grazing intensity was extremely low and only lasted for few days in the specific years (2010, 2011, 2014). Also, most of the grazing were sheep, and cattle were only present in 2014 in Parcel A for less than four weeks in total at an average stocking rate of 4.04 heads per hectare. Thus, the reviewer's statement that Parcel A was primarily grazed by cattle in 2014 may be misleading.

We are aware of the 3% assumption and while this approach could be taken, we were not able to follow the numbers presented by the reviewer. Possibly some additional explanation could be provided on how the values given were derived.

At the same time, we propose another approximation for methane emissions from enteric fermentation from cattle as follows and in relation to the study by Felber et al. (2015). Felber et al. *reported an average of 404 g CH4 per head per day in a table summarizing different. Taking this value and given the cattle occupied Parcel A (2.2 ha) for about four weeks with an average stocking rate of 4.04 heads per hectare (average of 12.5 and 5.3 for 2.2 hectares) our calculations are as follows.*

*Emissions for enteric methane = 404 g CH4/head/day * 4.04 head/ha * 30 days / 1000 to derive kg)*

The total CH4 emissions calculated are thus 48.96 kg CH4 per ha. When we convert this to C, we derive emissions of 4.07 kg CH4-C per ha. This would be the value we expect also to see with the EC flux tower under perfect conditions with a non-movable point source. Unfortunately, such perfect conditions aren't reality and we may not have captured all of these emissions due to shifts in wind direction, *changes in turbulence as well as the actual animal movement. Also, as indicated by Felber et al. (2015) distance from the cow to the EC tower determines how much methane one measures with the EC tower. Moreover, 4 kg CH4-C are of minimal influence for both the C budget as well as for the GHG budget of the site (see Table 4). The proposed way forward is to add this information on the issue of grazing in the results section, ie the calculation provided here in a first step and secondly highlight that methane remains of minor importance at this site for both the C as well as the GHG budget, even if we were adding another 4 kg CH4-C in the year 2014.*

To clarify on how the grazing removal was estimated and dealt with in the budget please see the following explanation. Grazing removal was quantified experimentally by having areas in both parcels from which the animals were excluded. At the end of each grazing period, the grass in the enclosures was cut similar to the approach taken when estimating harvests with subsequent laboratory analysis for C and N. Grazing is included in the harvest in Table 4, as this is a removal of biomass from the system. We agree that we had not included the return of nutrients via excreta (approx. 32% C, (Felber et al., 2016)*) and will include this in the revised budget calculations for both C and N. This adjustment does not change the key results of the paper presented.*

Moderate Concerns
3. The focus (or perhaps title?) of this manuscript needs sharpening. The title indicates a focus on pasture restoration which is matched by the abstract, yet much attention is given to methodological considerations. Specific goal (ii) states "briefly compare two different measurement techniques" however the first two-thirds of the discussion (i.e. not briefly) comments on this aspect! While important and noteworthy, either change the title/abstract, or return the primary focus of the discussion to management effects. Additionally, goal (iii) is not really explored in this manuscript – perhaps combine with goal (i)?

Thank you for this suggestion. In the revised manuscript we will combine goals (i) and (iii) and suggest shortening the discussion on the methodological aspects and give with this more attention to the primary goal of the study.

4. Providing a partial N budget provides little useful information. Including individual components is beneficial, but to sum them up as an incomplete "budget" is not. If the authors choose to retain the N budget, please include some further context including some ballpark estimates of the remaining components to aid interpretation.

Agreed. Since we intend to keep the N budget, we will add information by including ballpark estimates on ie. ammonia emissions, N deposition etc. in the revised manuscript. We will also include the necessary information on the origin of these estimates in the revised methodology section.

5. While N2O flux gap filling is difficult, the use of running medians may be problematic, and especially for gaps occurring during pulse emissions (e.g. the restoration period/fertiliser applications). The authors should comment on limitations of this approach, especially in the absence of any uncertainties (which I accept is rarely done in N2O flux studies so do not see them as a requirement here).

This is a very relevant point made by the reviewer. The method chosen here, follows the approach taken by Hoertnagl et al. (2018) and whom identified the running median being the most appropriate method to use if either large amounts of original data are available (ie as provided by the EC method) and/or if it is likely that the majority of N2O pulses have been covered by ie chamber measurements. Certainly, there are other options to fill N2O flux measurements and these were highlighted for instance in Nemitz et al. (2019) or Mishurov and Kiely (2011). Particularly, Nemitz et al. (2019) suggests linear interpolation for short gaps and daily averages to fill other gaps. For very long gaps more sophisticated and complex approaches such as machine learning tools are suggested.
Given that we aimed at deriving an annual budget which is relatively conservative we chose the running median approach. First of all, this way we are less likely to overestimate N2O emissions compared to ie the daily average approach. Linear interpolation would also have led to an overestimation of N2O

emissions particularly for the years 2010 and 2011 with few data points. Certainly, we see the lowest influence of gap filling errors for the years with EC measurements, whereas there may be a larger bias for the year with chamber measurements. Based on our 5-year observation period that indicated N2O emissions peaks during the growing season only and following fertilization events primarily (except 2012), we are confident that we covered the majority of these peaks during the years 2010 and 2011 when only chamber measurements are available. Nevertheless, and in line with comments made by reviewer #1 as well as our response to these comments during the access review phase, we intend to include additional gap-filling method estimates in the revised version of the manuscript as supplementary information.

Minor/Technical Concerns
Lines 33-34: grazing is listed as both a regular and sporadic management activity. Please clarify which it is.

We apologize for the mislead in wording and will rephrase as follows: "Grazing is a typical management activity in such intensive grassland. At our site, we observe grazing with either sheep or cattle for few days at the beginning or end of most years."

Line 37: Missing the word "out" (or similar) after "carried".

Done

Lines 86-89: Why did you hypothesis continuous losses of CO2? Several studies (e.g. (Rutledge et al., 2017; Ammann et al., 2020, etc) show CO2 uptake in restoration and later years.

Thank you for pointing this out. Actually, we had the hypothesis of increased CO2 uptake already in the manuscript (L. 89-90). We reworded these lines as follows: Prior to our measurements we hypothesized short-term losses of CO_2 after restoration and more continuous losses of primarily N_2O following dramatic managements events such as ploughing occurring at irregular time intervals. We further hypothesized an increased carbon uptake strength compared to the pre-ploughing years.

Lines 89-90: If you expect CO2 losses (as per the above point), why would you expect a C gain? Please adjust this and align with the previous sentence to clarify your hypothesis.

See our comment to the previous remark made by the reviewer.

Line 108: Do you mean CH4 emissions from the land or the grazers? In fact, this point needs clarity throughout the manuscript – are the grazers included within the system boundary, and therefore their emissions?

We actually refer to both, land emissions/uptake as well as CH4 emissions from grazers. In terms of system boundaries, these are set to the ecosystem here, thus we account for the GHG emissions made by grazers (CH4 from enteric fermentation, as well as CH4 and N2O from excreta). Given that stocking rate was low and the actual time of grazing short we expected little effects of grazing on the budget while still aiming at being inclusive as we wanted to include all the management activities occurring in this field with some having a clear influence on GHG flux measurement, while others may not. We further included the offtake due to grazing in the budget calculations and revise the existing budgets by accounting for the returns of nutrient to the pasture via excreta deposition.

Lines 123-127: this sentence is very clunky – suggest reviewing.

We are not sure what the reviewer refers to here as these are two sentences in the original manuscript. However, in order to increase the flow of reading the suggested lines will be adjusted as follows in the revised manuscript. "The study by Hörtnagl et al. (2018) further elaborated the variation in management intensity and related variations in GHG exchange across sites, stressing the need for more *case studies based on continuous GHG observations to improve existing knowledge and close remaining knowledge gaps. To complete the picture on factors impacting ecosystem GHG exchange, irregular occurring events such as dry spells or extraordinary wet periods can further lead to enhanced or reduced GHG emissions (Chen et al., 2016; Hartmann and Niklaus, 2012; Hopkins and Del Prado, 2007; Mudge et al., 2011; Wolf et al., 2013)"*

Line 130: "adaptations" should be "adaptation" (no "s").

Done

Line 137: "respectively" is not needed – please delete.

Done

Lines 232-234: If an LI-7500 (rather than LI-7500A) was the self-heating correction applied?

That was an oversight and we added the A.

Lines 241-249: It was unclear to me what QA/QC procedures were applied to the raw (10/20Hz) and which to the 30-minute data. I suggest improving the clarity here.

We rephrased this section by clearly distinguishing between raw data and raw time series (high frequency) and specifically state when we refer to 30-minute data.

Line 248: what was considered the physically plausible range? Please include this information.

Done

Line 280: Order of words: "no longer closed" should be "closed no longer".

Done

Line 314: Remove the word "Up"

Done

Line 413: Insert the word "and" between "(Figure 1c)" and "temperatures".

Done

Lines 477-478: I think the before and after restoration periods should be separated. I don't believe averaging the two periods to be fair as part of the purpose of restoration is to improve growth, and therefore modification of CO2 exchange should also be expected.

This may be a misunderstanding. We clearly differentiate between periods as indicated in the original mansuscript under sections 3.3. CO2 exchange and N2O exchange as well as under section 3.4.

Line 480: According to Table 2, CH4 emissions for 2013 and 2014 were actually >1 – please correct.

This is correct for the years 2012, 2013 and 2014 and the values seen are very similar to values reportend by Felber et al. (2015). *Given the magnitude of the other GHG fluxes, methane remains a minor contribution to both the C as well as the GWP budgets (less than half the contribution of N2O for the years 2013 and 2014 which are dominated by the CO2 signal).*

Line 538: Correct the format of the reference

Done

Line 579-580: Are you referring to the measured CO2 exchange to be _50 g C m-2 y-1, or the uncertainty? This sentence is very unclear as no uncertainty has been presented, so please clarify.

This refers to the statement made by Baldocchi et al. 2003, whom stated that annual numbers presented from EC measurements can vary by as much as by 50 g C per year. Thus, we want to encourage that this is an uncertainty anyone should keep in mind when evaluating annual budgets derived by the EC technique.

Table 1: I find the “max data availability” columns repetitive – perhaps just a single column of this data?

Good point, thank you! We will remove the repetitive statement of numbers in the revised manuscript.

Table 4: I suspect the labelling of Parcels A and B for both fertilizer and harvest are not correct. As written, fertilizer was only applied to Parcel A, and Harvest to Parcel B. Please correct is appropriate.

This is actually only an incorrect labelling and should refer to harvest for Parcel A and B as well as fertilizer for Parcel A and B. This will be corrected in the revised manuscript.

References
Ammann, C., Neftel, A., Jocher, M., Fuhrer, J., Leifeld, J., 2020. Effect of management and weather variations on the greenhouse gas budget of two grasslands during a 10- year experiment. Agric. Ecosyst. Environ. 292.
Felber, R., Bretscher, D., Münger, A., Neftel, A., Ammann, C., 2016. Determination of the carbon budget of a pasture: effect of system boundaries and flux uncertainties. Biogeosciences 13, 2959-2969.
Rutledge, S., Wall, A.M., Mudge, P.L., Troughton, B., Campbell, D.I., Pronger, J., Joshi, C., Schipper, L.A., 2017. The carbon balance of temperate grasslands part II: The impact of pasture renewal via direct drilling. Agric. Ecosyst. Environ. 239, 132-142.

We thank the reviewer for pointing us towards these references and we refer to these in the revised version of the manuscript.