

Interactive comment on “Memory effects on greenhouse gas emissions (CO₂, N₂O and CH₄) following grassland restoration?” by Lutz Merbold et al.

Anonymous Referee #2

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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.

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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.

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

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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.

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)?

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.

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).

Minor/Technical Concerns

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

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

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.

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.

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?

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

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

Line 137: “respectively” is not needed – please delete.

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

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.

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

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

Line 314: Remove the word “Up”

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

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 restora-

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tion is to improve growth, and therefore modification of CO₂ exchange should also be expected.

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

Line 538: Correct the format of the reference

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.

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

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.

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., Munger, 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.

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