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Interactive comment on "The influence of tillage on N_2O fluxes from an intensively managed grazed grassland in Scotland" *by* N.J. Cowan et al.

E. Diaz Pines (Referee)

eugenio.diaz-pines@kit.edu

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I have read the manuscript from Cowan and colleagues on the effects of tillage on the N2O fluxes of a managed grassland in Scotland. The work applies a combination of EC and dynamic and static chamber measurements over a growing season on a tilled and an untilled field. The tilled plot showed higher N2O emissions after ploughing, but the tilled plot was less responsive to fertilization than the untilled one (0.4 vs 0.75 kg N ha-1), partially offsetting the overall differences between tilled and untilled plots. The topic of the work is highly suitable for the intended SI (GHG hotspots): tillage of grasslands and its effect on N2O fluxes is not well addressed in the literature (presumably because ploughing in grasslands is not done yearly and relies on the managers′ opinions, as the authors point out), despite the fact that it can constitute a significant source of N2O. The manuscript is of high quality, well elaborated and structured, although there is still

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some room for improvement prior to final publication.

The editor specifically asked my opinion about 1) the potential of the work for scaling up the measurements and 2) the suitability of the temporal coverage. Regarding 1): I do not recommend extrapolating results of a single field without solid arguments to do it, but this work can surely contribute to the growing body of information on the issue; from this point of view, a paper is solid even if no extrapolation can be implemented (as it is in this case study). Regarding 2) (temporal coverage): the length of the measurement campaign is enough to obtain sound conclusions on the effects of tillage and fertilization events (all applications throughout the year were monitored) so that both plots can be consistently compared; but authors should also try to reflect how they could come up with annual fluxes and what would be the uncertainty there (e.g. are there soil freeze-thaw events?); in other words, authors should mention what are we (likely) missing in the 190 days which were not measured. Spatial and temporal variability is somehow addressed in the discussion (L350-357), but could be more elaborated, and some information on spatial variability (as assessed by chamber measurements) should be included.

I recommend making a stronger point in the discussion addressing the implications of tillage for accounting of N2O fluxes in the context of grassland management, including the fundamental fact that tillage (responsible for an additional release of 1.08 kg N ha-1) "substitutes" one fertilization event (responsible for 0.72 kg N ha-1, L317). For instance, confidence intervals of N2O emissions from tilled and untilled plots overlap by the end of the season (Figure 7), so I presume differences in cumulative fluxes were not statistically significant; that should be also addressed clearly in the work.

My second major point is oriented to methods. I missed some necessary explanations on 1) whether footprint analyses (or any other method) were applied to discriminate between fluxes from one field or the other, or did you just take time windows with no variation in wind direction? You refer to "mean wind direction" (L152-153), but this doesn't necessarily imply the wind came solely from one of the plots. Indeed, there is

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an increase in the N2O fluxes in the tilled plot at the same time as the partner, untilled plot was fertilized (first half of June, roughly), so further explanations are needed to exclude potential artifacts wrt that matter. 2) I also miss information on the spatial and temporal coverage using the three techniques. What was the distribution, how many measurements did you do with each method? Probably a summarizing table is the best option, along with more comprehensive information in the Figure captions (e.g. in Fig 4, see below for specific details). Furthermore, do you have simultaneous measurements? Did you appreciate any offset between techniques (it seems so in Fig 5) suggesting you may have a bias depending on which technique you used? Finally, 3) you may need to discuss your gap filling method a bit more: for example, in Figure 6 I see two big mismatches after fertilization, I don't know whether this is due to the subjective adjustment of the degree of smoothing (L206-207) or to some other reason. Did you take observed values or GAM in Fig 7? it is not that clear to me.

Additional minor details:

L137. Remove "See"

L157: Write "N2O emissions " or "N2O emission rates" and not only "N2O"

L185. Maybe I made a mistake when converting units, but I think your detection limits are 0.02 and 0.2 nmol m-2 s-1. 1 nmol N2O m-2 s-1 equals roughly 100 ug N2O-N m-2 h-1.

L212: What is the length of the time steps (30 minutes)?

L218: Strictly speaking, you need to mention Figure 2b as well.

L247. It is stated that the tilled field was not fertilized the previous year. Was the untilled? If there are differences, you should report them in line 95.

L323-334: Don't you have any record on differences of soil water content, bulk density, etc, between tilled and untilled plots? Including these data could feed and improve this section.

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L336. Write “Gap filling of N2O flux measurements is challenging” or similar. Gap filling itself is easy (a linear interpolation may be sufficient), but doing it in a sound manner, this is the problem.

L347: This data gap due to instrument failure/unavailability is not to be seen in Figure 4, at least not for two weeks. In L246 you mention a one-week long data drop. Please clarify.

L348: Which type of uncertainty do you refer to? This period of time is showing the smallest uncertainty according to the Figure 7, what makes your statement inconsistent. See also my comment below to Figure 7 on how to show uncertainty

Table 1: Grazing info should be included here, as it is part of the field management.

Fig 2: soil temperature at which depth? In the tilled or the untilled plot? Any soil water content data available?

Is Figure 5 merely a zoom-in into Figure 4? It looks like, but after a closer sight, many static chamber symbols are missing in Fig 5 compared to Fig4. Please clarify and eventually merge the Figures in a single one. Further, please specify what are the values showed in Figs 4 and 5 (averages of # chambers, single chamber values?).

Figure 6. Describe the content of the figure (“mean N2O fluxes as determined by Eddy cov, static and dynamic chambers and estimated N2O flux by using the general additive model from both . . .” or similar).Are the chamber results the average of # chambers? Please specify this info. Make clearer the difference between the tillage and fertilizer dates by using lines of different color/shape. Again, I have the impression the data shown is “basically” the same as on Fig 4, but with some differences (e.g. there are several dynamic chamber fluxes of 4-8 nmol m-2 s-1 right after tillage in Fig4, or static chamber fluxes after the second fertilization application in 4a, which are gone I Fig 6). By the way, replace “October” by “September” (also in Fig 7).

Figure 7. I guess uncertainty from a time step should propagate to all future dates (in

other words, when you estimate cumulative fluxes, you need to sum up the uncertainty of the precedent values).

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