

Interactive comment on “Temporal and spatial variations of CO₂, CH₄ and N₂O fluxes at three differently managed grasslands” by D. Imer et al.

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Referee's comments on BGD manuscript "Temporal and spatial variations of CO₂, CH₄ and N₂O fluxes at three differently managed grasslands" by D. Imer, L. Merbold, W. Eugster, and N. Buchmann

General comments

This paper describes the compared temporal and spatial variations of soil greenhouse (GHG) gas (CO₂, N₂O and CH₄) fluxes at three grassland sites across an altitudinal gradient in the Swiss Alps from 400 m to 2000 m amsl. A general objective was to assess how soil GHG fluxes under permanent grassland respond to management and to environmental drivers (especially temperature T and soil water content SWC), with

C726

a gradient in grassland management intensity reflecting the altitudinal gradient. More specific objectives included the assessments of temporal (diurnal, seasonal, annual) variations in GHG fluxes, and of the potential errors in scaled-up fluxes associated with spatial variations among flux replicates at the field scale.

The manuscript is straightforward, well written, with clearly stated objectives and methods and an adequate description of a substantial body of flux measurements and results from the three sites. The intercomparison exercise is valuable as it could also be regarded as a global change experiment, with altered climate, temperature and soil hydrology leading to changes in both management and fluxes. However the focus here is rather methodological, and I am not altogether convinced by some aspects of the discussion of the spatial representativeness of the measured fluxes. The application of complex geostatistical methods to small datasets (N chambers = 16) rather feels like an overkill, which places the emphasis on dry statistics rather than mechanisms and process study. It may have been much more informative, for example, to measure soil moisture at each chamber location (e.g. using a portable TDR probe - if this was done, it does not say so in the text ?-), as well as available N from soil samples, which are known (or at least strongly expected) to be the primary drivers of N₂O emissions. The last figure (effect of slope on fluxes) is an attempt to quantify by proxy the impact of soil moisture, but using actual measured SWC (or % water filled pore space) values would have been much more powerful.

Also, there is too little discussion of the impact of grazing on the spatial variability of fluxes; grazing animals tend to select preferential spots for grazing (grass species composition, forage quality) and for resting (shade, wind shelter), but the manuscript does not make it clear whether the transects were selected to yield representative sub-sections of the field (it does not appear so). Thus there is no telling whether the average of the 16 chambers is a good approximation of the field-scale ecosystem flux; the presence of hot spots (especially for N₂O) makes it clear that the fluxes are log-normally distributed (in space), and thus the average could either be an over-estimate or an under-estimate.

C727

I also have a few reservations regarding the flux calculations and selection criteria. The paper may thus be published subject to the following revisions.

Specific comments

Title: Please make it clear in the text that the paper deals with SOIL fluxes only (assimilation / gross primary productivity and animal emissions are not accounted for by these (opaque) chamber measurements, and the eddy covariance data are not shown or discussed), and thus does not allow a full GHG balance.

Abstract: p2636, l17: it seems rather little counter-intuitive that cattle-grazed grassland should be a net CH₄ sink, which is why it is important to state clearly here in the abstract, as well as in the title, that the fluxes shown are soil fluxes only, and thus not representative of the whole (soil+animals) ecosystem, to avoid confusion.

Abstract: p2636, l18-19: please provide annual fluxes in kg C /m²/s for CO₂ and CH₄, and kg N /m²/s for N₂O

Abstract: p2636, l24: the magnitude of the measured CH₄ uptake does not justify the use of the term "hot-spot", which is usually reserved for strong emissions. Please rephrase.

p2637, l22-25: More orographic precipitation, but also lower evapotranspiration rates due to lower temperatures, both contributing to wetter soils at high elevations.

p2638, l10-12: Grazing (not mentioned here) is possibly the strongest driver of SPATIAL variability in soil GHG fluxes in grazed systems, due to urine and droppings creating emission hotspots. Grazing also drives TEMPORAL variability (l 13-15), though it could be argued that fertilisation (sudden increase in soil available nutrients, energy and N) leads to even more pronounced emission pulses than does grazing, where changes are more gradual.

p2638, l21-22: "to investigate the source/sink behavior of CO₂, CH₄ and N₂O fluxes at three differently managed grasslands...": differences are not just in management,

C728

but equally important is the climatic/altitudinal gradient, whereby the effects are not easily untangled. Obviously it's the climatic gradient which has led over the decades or centuries to the development of different management practices.

p2639, l10-13: was there any grazing at Chamau?

p2640, l10: please indicate how deep the collars were inserted into the soil

p2640, l20-21: "The vegetation inside the chamber collars was manually cut at the times of regular management activities, i.e. cuts and grazing." This sentence seems to imply that the collars at the different sampling locations were fenced off - and therefore not subjected to direct grazing and cattle urine and droppings - from grazing animals? Could you please confirm? Does this then mean that the measured GHG fluxes are not representative of grazed systems, but rather of fertilised/cut grassland, and that the spatial heterogeneity normally associated with urine patches and compaction by animals is not reflected in the chambers? This is an important point for the interpretation and discussion of the spatial variability later on.

p2641, l6-8: "A deployment time <40 min is considered short enough to neglect saturation effects inside the head space..." : this may have been your experience during this study, but I disagree that this can be presented as a general rule. A detectable curvature of the temporal evolution of concentrations can occur over much shorter time frames than 40 minutes, it very much depends on the source strength of the underlying soil, but also on the collar depth. Theoretically the concentration in the chamber will level off at the concentration occurring in soil pore space at the depth of the base of the collar.

p2641, l19-20: "... calculated by the slope of the linear regression between gas concentration and time. Fluxes were always small enough that no curvature in measured concentration data could be detected which would be indicative for saturation effects inside the chamber": How did you quantify the curvature? Using the linear R²? I would argue that no curvature was detected presumably also because only 4 samples

C729

per chamber flux measurements were taken, such that the noise to signal ratio over the 40 minutes was comparably high. Chamber measurements made using fast response continuous gas analyzers (e.g. LiCor or other IRGA for CO₂) have demonstrated that at least a slight curvature can be almost always detected, and is indeed theoretically expected, resulting in systematic flux underestimation (typically 10-30%), even though the linear R² is consistently and comfortably well above 0.98. A very useful reference is Petersen et al. (2010), A comprehensive approach to soil-atmosphere trace-gas flux estimation with static chambers, *European Journal of Soil Science*, 61, 888–902. An alternative re-computation of your fluxes using the Petersen et al. algorithm in R would be very informative, providing an uncertainty estimate for individual flux measurements.

p2642, I1-2: "...fluxes were only computed if the linear regression yielded a r² >0.8." Do you mean by this, that GHG fluxes (for the three gases CO₂, N₂O and CH₄) were only computed when the R² for CO₂ was above 0.8, with CO₂ being taken as a quality criterion for the whole chamber operation (based on the assumption that there must always be CO₂ mineralisation and thus evolution from the soil, and that any noisy CO₂ temporal profile indicates a dysfunction of the sampling system, for example a large leak)? Or do you actually mean that for each gas taken separately you applied a selection based on the R² of the gas in question?

In the case of the latter, consider the hypothesis that the N₂O concentration is almost exactly 320 ppb (+/- the uncertainty in the GC concentration measurement) at sampling times t₀, t_{10min}, t_{20min}, t_{30min}. The R² is very close to 0, and yet the flat concentration profile tells us that net emission or uptake takes place. Discarding all such flux events would inevitably bias the temporal or spatial average upwards (overestimation of the annual-scale and field-scale emissions). Please comment.

p2642, I2-4: "CO₂, CH₄ and N₂O fluxes per chamber which were then filtered for obvious out of range values (±10 SD) for each sampling campaign": on what statistical population is the SD computed, on the 16 chambers measured on each day at each field? Does that mean that you discard and exclude hot spot fluxes from the spatial av-

C730

erage, thereby artificially reducing both the natural variability in fluxes and the arithmetic mean emission or uptake? I don't understand the rationale. As long as each chamber flux has successfully passed the individual flux selection criteria (based on R² as discussed above), I don't see any reason for rejection, especially for gases like N₂O and CH₄, which are known to vary spatially/temporally over several orders of magnitude.

p2642, I17-22: What do you mean by each "site"? Do you mean at each "field site" (CHA, FRU, AWS), with one soil measurement station for T and SWC for the whole field? Or did you measure T and SWC at each of the 16 "flux chamber/collar sites", not necessarily continuously but at the time of each flux sampling campaign, using portable soil probes?

p2643, I4-12: only linear regressions are mentioned in the statistical models fitted. Yet the temperature responses of at least CO₂ fluxes (soil/ecosystem respiration), and possibly N₂O in wet soil conditions, are expected to be exponential, not linear (cf Lloyd and Taylor, *Funct. Ecol.*, 8, 315–323, 1994, functions for CO₂, on which countless gap-filling exercises in the EC flux literature have been based). Similarly, for N₂O, the SWC response is expected to be bell-shaped, with optimum conditions for nitrification and denitrification occurring at mid-range and upper mid-range (around field capacity), while at saturation (100% water-filled pore space) denitrification proceeds all the way to N₂, and thus the N₂O efflux decreases; thus the SWC response can't be linear. It seems to me that the fraction of the (temporal) variance in GHG fluxes you are trying to explain using multiple regression approaches can only be underestimated by using strictly linear functions, and that a more process-based selection of models would benefit the analysis. (see e.g. Meda et al., *Biogeosciences*, 9, 1493–1508, 2012, in which we also studied spatial and temporal variations in GHG fluxes, in that case of the grass outdoor run of free-ranging chicken; or again Luo et al., *Biogeosciences*, 9, 1741–1763, 2012, Decadal variability of soil CO₂, NO, N₂O, and CH₄ fluxes at the Hoeglwald Forest, Germany).

p2644, I7-8: "CH₄ fluxes did not follow any seasonal trend": there does seem to be

C731

systematic seasonality in CH₄ fluxes, with Fig. 2 showing consistent uptake in spring-summer-autumn (Jul-Nov and then Mar-Jul), and consistent emission in winter (Nov-Feb) at both CHA and FRU. Incidentally, the figure caption in Fig.2 should say "...mean SOIL flux of the respective greenhouse gas...", not "ecosystem" flux, for reasons explained above. The rest of the document should be checked for similar occurrences.

p2644, I19: "At AWS, an average efflux of 0.23 nmol m⁻² s⁻¹ was observed": were there any measurements during or just at the end of the melting of the snowpack, and during freeze-thaw events in late winter and early spring, which could both release substantial quantities of N₂O?

p2644, I25: what kind of fertilizer response of CH₄ fluxes would you have expected?

p2645, I13: please change "small-scale GHG flux variability" to "temporal GHG flux variability", to make it clear that this paragraph is not about the spatial variations. My comment here again would be that the PCA based on linear models might miss important non-linear features such as the effect of SWC on N₂O (e.g. p2646, I12-13 "SWC had much less influence on the N₂O efflux, with a RI of 16.1 %"; the linear approach may completely miss the increase of N₂O emissions at field capacity due to the reduced emissions at saturation?). Linear models explain 19-42% of temporal N₂O variations, and it would be interesting to see whether non-linear models can show higher explanatory power. Also, I wonder why management was not included as additional factors in the analysis to explain the temporal variations, as surely the introduction of grazing animals and fertilisation should be strong drivers of seasonal changes in fluxes.

p2648, I7-25: please specify what metric is used to quantify "spatial heterogeneity" to compare between sites and gases. Presumably the coefficient of variation defined by CV=SD/average ?

p2648, I8-9: "Spatial heterogeneity in N₂O fluxes was largest at AWS...probably due to large variations in SWC": but also possibly due to a larger impact of grazing, since the

C732

other 2 sites were also cut and thus the grazing fraction of time was reduced compared with AWS?

p2649, I9, or p2643, Statistics section: please define "spatial auto-correlation" mathematically, as well as "semivariograms", for the lay reader.

p2653, I18-19: you argue that significant diurnal changes in N₂O fluxes were observed at FRU because the site had recently been fertilised (contrary to CHA). I do not find the argument entirely convincing; if the diurnal process is temperature driven, there is no reason why the relative effect, as quantified by the % change (day-night)/day, should be different. Unless what happens is that, in background situations (long after any fertilisation has taken place), the measured N₂O fluxes are not significantly different from zero for both day and night (i.e. within the uncertainty of the flux measurement system, because of random errors in concentrations). If fluxes could be measured (with a more precise flux system) then one might also see diurnal flux differences.

p2654, I2-4: "Working with soil chambers requires information on the spatial distribution of GHG fluxes at ecosystem scale to design appropriate experiments and to be able to correct mean ecosystem fluxes for potential biases". Does this imply that the flux variability, and thus the fluxes themselves, should be known a priori (before the measurements start)? Obviously this is not possible, but one recommendation may be that the field be divided into several sub-sections assumed to be homogeneous, based on objective criteria: terrain slope (as rightly noted p2655, I5-10); soil moisture mapping; preferential grazing and resting areas for animals; vegetation species composition; soil hydromorphy; etc.. Each section should ideally be sampled with a number of chambers proportional to the expected emission rate, because hotspots dominate the field scale emission and thus the measurement effort should be commensurate. However, in practice, few studies can afford such intensive measurement efforts.

p2654, I12-13: "Chambers placed in terrain with greater inclination systematically exhibited lower SWC values." Was this a visual observation?

C733

p2654, l17-18: "Omitting permanent hot spots may lead to a systematic bias in GHG flux budgets." This is true; however, an over-representation of hotspots will conversely lead to an over-estimation of GHG emissions; and so the question of what fraction of chambers should be hotspots remains entirely open. Thus the spatially averaged GHG fluxes presented in this paper, as in most others, may be over- or under-estimates of the true field-scale integral.

p2655, l5-10: the recommendations for sloping terrain are useful, but should be more general and include other considerations about local-scale drivers of spatial variability, such as mentioned above. And what is "big enough"?

I would argue that a workable compromise should be based on 1- routine measurements using a few ($N = 5 - 10$) automatic or manual chambers with adequate temporal cover, and 2- a few campaigns in each season with high spatial resolution measurements using a "fast-box" system (Hensen et al., *Agriculture, Ecosystems and Environment* 112, 146–152, 2006) to map out the spatial variability and thus extrapolate fluxes to the field scale.

Technical corrections

p2641, l24: concentrations are actually mixing ratios and their units are $\mu\text{mol/mol}$ (ppm) or nmol/mol (ppb)

p2651, l27: "environment"

p2652, l9: "..taken IN generally drier soils..."

p2654, l9: change "merely" to "only"

p2654, l11, and Fig.9: suggest change "inclination" to "slope"

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