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Atmospheric Measurement Techniques Discussions

Interactive comment on "Comparison of flux gradient and chamber techniques to measure soil N_2O emissions" *by* Mei Bai et al.

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Reviewer's comments on AMTD-2018-90 manuscript "Comparison of flux gradient and chamber techniques to measure soil N2O emissions" by Mei Bai et al.

General comments

The paper describes a useful comparison of soil N2O flux measurements by static chambers versus a newly-developed slant open-path flux gradient method. There have been numerous studies on this topic, and like many previous studies, the present manuscript discusses the discrepancies between the two techniques in terms of spatial variability and representativeness of the very different footprints of the two methods.

The results are rather inconclusive, possibly for two reasons which are not (but should

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Discussion paper

be) explored: i) there is no quantitative footprint analysis for the flux gradient (FG) measurements, and thus it is hard to judge the extent to which the FG flux results are influenced by neighbouring fields; and ii) chamber fluxes are presumably calculated by linear regression of headspace concentration versus time elapsed since chamber closure (although no details of the calculation are given); however, a non-linear behaviour is in principle expected, which, if accounted for, would make the static chamber fluxes larger, and would thus reduce the discrepancy versus the FG results.

These two aspects, as well as the following comments, should be adressed in the revised version.

Specific Comments

Title: should be more precise. I would suggest the following: "Comparison of slant open-path flux gradient and static closed chamber techniques to measure soil N2O emissions"

p5, l109: please provide details of how the rate of concentration change dC/dt is calculated : presumably a simple linear regression? Later on, p10, l235-240, you state that a linear regression can underestimate emissions. In that case you should apply existing alternative non-linear algorithms (eg Levy et al, 2011; or Pedersen et al, 2010, EJSS 61, 888–902, doi: 10.1111/j.1365-2389.2010.01291.x) to your data set and calculate the effective bias caused by the linear regression. It is now widely recognized that a linear regression is not theoretically valid nor acceptable for static chamber measurements.

p6, l140: What is "high precision" ? Please provide precision estimates for the different gases.

p7, l170-171 and fig 2: the background N2O fluxes of around 1-5 mg N2O-N m-2 hr-1 are equivalent to around 240-1200 gN ha-1 d-1, or 88-438 kgN ha-1 yr-1, which makes no agronomic or physical sense. There must be a unit issue somewhere. Perhaps

what you write as mg N is in fact μg N ? If so all results and figures must be corrected.

p8, l192-193 "...static chambers (once-a-day snapshots) were in general agreement with the FG measurements in terms of the long-term pattern...". Change to "...long-term background exchange patterns...", since the N2O peak was not captured/not sampled by the manual chambers (no chamber data available during the peak).

p10, l237: "...can lead to an underestimate of emissions...": I agree, and therefore the chambers fluxes should be recalculated in this study using a non-linear algorithm, which has become the norm for such measurements.

p10, l247-248: "...This is considered to be sufficient in most conditions..." This statement is a little vague and demands more scrutiny. You should demonstrate that the fetch is sufficient in daytime neutral conditions by running at least one footprint model or using bLS (eg Windtrax) modelling. Also, the site description indicates prevailing wind direction of SE and NW, but Fig.1 does not provide any indication of where North lies. Please provide a proper site map with the windrose.

p11, l249-250: "...In this case the FG measurements would have been contaminated by emissions occurring upwind of our sites..." This argument is not relevant in the context of comparing FG to chamber fluxes, since manual chambers were operated only during the daytime (as indicated p5, l103). Further, the fact that the fetch encompasses nearby fields does not necessarily mean that fluxes are over-estimated; if emissions beyond the field boundaries are smaller or near-zero, then fluxes for the field of interest would be under-estimated.

p11, Conclusion, l263-265: "...supports an interpretation that our FG emission calculations were accurate and in this instance the chamber measurements were biased too low..." I am not sure I agree with this interpretation, based on the available evidence. I believe you should stop at the statement a few lines above that says "...It is difficult to conclude that one technique or the other is biased based on this experiment alone...". Unless perhaps new evidence appears from the additional re-calculation of

static chamber fluxes using non-linear algorithms, and from the footprint analysis.

Technical Corrections

p1, abstract, opening sentence: delete "the" between "Improving" and "direct"; also, delete "from the large agriculture farm", which is poor English (large farms? large sources? large fields??), and replace with "from cropped agricultural fields" or something similar.

p1, l12: the common name of the technique is the aerodynamic flux-gradient method. Suggest keep FG as acronym, but at least introduce the aerodynamic flux gradient terminology at the start of the paper

p2, l42: Change to "Moreover, manually operated static chambers..."

p2, l44: Change "These weaknesses..." to "The temporal variation issue..."

p3, l59-60: change to "...an estimate of the turbulent diffusivity...". Gas diffusivity suggests molecular diffusion.

p4, l86: "The average min and max..." ? I presume it's daily min/max?

p4, l86 "...were 6 and 33, respectively..."

p5, Eq.1: remove the K "density factor", and replace with the appropriate conversion formula using the ideal gas law. This K factor is not explicit and can be confused with K (Kelvin) and with the eddy diffusivity Kappa of Eq.2-3.

p5, l109: what kinf of "concentration" is C ? Is this a dry mole fraction, or a bulk mole fraction, provided by the GC analysis? What are the units?

p5, l119-120: Equations 2-3 look a little unorthodox and fairly different from the usual typical aerodynamic FG forms (F $\sim$ -Kdiff * dC/dz). Please add a short prior sentence to say that Flesch et al (2016) showed that the conventional FG equation can be transformed into Eqs 2-3.

p6, l126: it must be a "length integral", not a "height integral" if it's dx from x1 to x2 in Eq.3?

p7, l153-154 "...at a frequency..."

p7, l155, "...were calculated from the ultrasonic anemometer data...'

p7, l168: avoid acronyms in titles, change to "Flux-gradient fluxes"

p9, l200: change title to "Comparison of the two measurement techniques"

p10, l235: change to "...have reported that chamber flux calculation procedures introduced large uncertainty..."

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