Biogeosciences Discuss., 12, C6074–C6078, 2015
www.biogeosciences-discuss.net/12/C6074/2015/
© Author(s) 2015. This work is distributed under
the Creative Commons Attribute 3.0 License.

Biogeosciences Discussions

Open Access

BGD

12, C6074–C6078, 2015

Interactive Comment

Interactive comment on "Combining two complementary micrometeorological methods to measure CH_4 and N_2O fluxes over pasture" *by* J. Laubach et al.

Anonymous Referee #1

Received and published: 2 October 2015

Aim of pape This is a very detailed examination of the use of two micrometeorological techniques for long-term measurements of emissions of methane and nitrous oxide. The MS describes how they were used to measure fluxes of CH4 and N2O from a pasture by using CO2 as a tracer gas. In one application, fluxes of CH4 and/or N2O were calculated from the ratio of the mole fractions of the target gas and CO2 determined simultaneously in the boundary layer of the pasture by FTIR analysis. The difference in the CO2 mole fraction between two heights was coupled with the flux of CO2 from the pasture, determined by eddy covariance, to form a gas transfer coefficient. The coefficient was then multiplied by the mole fractions of the target gases to yield their fluxes into the atmosphere. It was assumed that the coefficient was determined by at-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

mospheric turbulence and atmospheric stability and was the same for all three gases.

The second application was confined to the nocturnal boundary layer. It was assumed that in calm, conditions at night with stable surface-layer stratification, gases emitted at the surface accumulate in the surface-layer where increases in the mole-fractions of gases are highly correlated. As in the first application, CO2 was used as a tracer, its flux was determined by eddy covariance while its mole fraction and that of the target gas were measured simultaneously by an FTIR spectrometer. The correlation between the two mole fractions was then used to infer the unknown flux from the known flux of CO2.

Both measurement techniques were employed in each of two neighbouring pastures with different management schemes: one essentially an unfertilised dry-land pasture, receiving no irrigation and the other fertilised and irrigated at regular intervals to keep the volumetric soil moisture content above 0.2. The dry-land pasture was grazed only once during the measurement period while the irrigated pasture was fertilised with urea 9 times annually.

General Comments

As the authors point out in their title the two approaches are complementary: the first is appropriate for the well–developed atmospheric turbulence often experienced in the daytime while the second is applicable on calm nights. However, this could introduce some uncertainty when there is a diurnal cycle in gas emissions as has been observed in other field studies of N2O production. Nonetheless, the paper should prove useful to other researchers of the target gas emissions. The authors point out that their study was initiated because of the high cost of fast sensors appropriate for eddy covariance measurements of CH4 and N2O, most notably for N2O.

I have some areas of concern. One is the length of the MS. It is well written, but I feel that the great detail in it makes it longer and more discursive than need be. Some examples follow:

BGD

12, C6074–C6078, 2015

p.6: The sampling procedure for the FTIR spectrometer is described. It was found that changes were needed to attain the desired sensitivity and a new system was used. Since the first system was unsatisfactory it should be enough to cut to a short description of the second , without wading through an unnecessary page of detail.

p.10: I am not expert in gap-filling procedures, but I find it surprising that gap-filling was apparently used so freely. Perhaps the authors could quantify just how much.

p.10, Eq.5: Is it acceptable to use whole-night averages rather than shorter term determinations in this equation and how are “sufficiently calm” nights identified?

pp. 11 & 12: To me, the description of the Soil and Vegetation conditions and CO_2 fluxes contains more detail than is needed and could be shortened.

p.13: Define non-resolvable gradients. My guess is that they exhibit a change from negative to positive (or vice versa) within the gradient

pp. 15-17: These pages, which discuss various aspects of the GGR method, are good examples of the highly detailed patches in the MS that I think could be shortened.

pp. 18-19 (section 3.3.3 on footprints and nocturnal fluxes): long discussion

I have some concern about the use of the term turbulent diffusivity, as in Eq. 1. My understanding is that it should be used in a partial derivative equation rather than a finite difference one like Eq. 1 so that

$F_\chi = -K \, \partial\chi / \partial z$,

as, for instance, in Thom (1975, Momentum, Mass and Heat Exchange of Plant Communities in “Vegetation and the Atmosphere” Vol.1, Ed. J.L Monteith, pp.57-109, Academic Press, London). This allows for a non-linear form for χ (z) and a non-steady state. I prefer to describe K as used by the authors in Eq.1 as a transport or transfer coefficient. I haven’t gone through the ramifications that might arise from using Eq.1 in the present context. It may be that in the end, both Eq.1 and the partial differential equation above give the same answer.

pp.23-25: These discussion pages make good points, particularly the opening paragraph on p.25 which recommends combining the GGR and NSR techniques to give long term means since the GGR method yields more data during the day than at night and the NSR method is nocturnal only. The authors point out that their combination optimises data usage.

I noticed some typos in the manuscript:

p.7, line 4: thermostate for thermostsat

p.7, line26: instationary for non-stationary

p.28, line 17: contents for content

Recommendation

This is a valuable paper and I have no doubt that it should be published in the journal. However, i encourage the authors to consider reducing the detail and length of the present version.

1. Does the paper address relevant scientific questions within the scope of BG? Yes

2. Does the paper present novel concepts, ideas, tools, or data? Yes

3. Are substantial conclusions reached? Yes

4. Are the scientific methods and assumptions valid and clearly outlined? There is a large amount of detail

5. Are the results sufficient to support the interpretations and conclusions? Yes

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes, but detailed

7. Do the authors give proper credit to related work and clearly indicate their own

BGD

12, C6074–C6078, 2015

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

new/original contribution? Yes

8. Does the title clearly reflect the contents of the paper? Yes

9. Does the abstract provide a concise and complete summary? Yes

10. Is the overall presentation well structured and clear? Very detailed

11. Is the language fluent and precise? Yes

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? See comment on use of term turbulent diffusivity

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? I have pointed out that some of the detail is unnecessary

14. Are the number and quality of references appropriate? Yes

15. Is the amount and quality of supplementary material appropriate? Yes

---

Interactive comment on Biogeosciences Discuss., 12, 15245, 2015.