

Interactive comment on “Methane emission measurements in a cattle grazed pasture: a comparison of four methods” by T. Tallec et al.

Anonymous Referee #1

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Review of Tallec et al. (bg-2012-358)

In the present study, basically three different methods (1: EC, 2: breath air sampling+SF6 tracer, 3: plume concentration measurement+modeling) were applied to measure the methane emission of grazing cattle on a pasture. The EC method has been applied continuously over the entire grazing seasons (2 years) with a free moving cattle herd on a 2.81 ha field. In a specially designed comparison experiment (total 8 days), the EC and the SF6 methods were applied simultaneously for 5 cows in a small fenced area (with known localization in the EC flux footprint). In addition, results of a small plume experiment are reported, in which the emission of 5 cows in a small fenced area was derived from downwind concentration transect measurements in combination with a Gaussian plume model. The experimental results are also compared

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with animal emission factors reported in the literature. As declared in the first part of the manuscript, the main objective of the authors was to investigate the applicability and performance of the EC method for CH₄ fluxes in grazed grasslands.

In my view the addressed scientific problem is of high interest and the design of the specific comparison experiment is quite inventive. However, the presentation of the data evaluation and the results is often poor and confusing (see comments listed below). Since the EC method for CH₄ emissions from grazing animals is not yet an established technique, it is crucial that the reader can understand and trace the performed processing steps. This is often not possible here.

Since this is already a re-submitted version of the manuscript that still suffers from some major and many smaller problems, I recommend to reject the manuscript at this point. The authors may consider to split the manuscript into two better focused papers, e.g. one on the seasonal pasture emissions and another one on the specific validation/comparison experiments.

MAJOR CONCERNS

The following four major comments are related to the comparison experiment of EC and SF6 method (Sect. 2.6 and 3.3):

- 1) I already reviewed the manuscript in an earlier version, where obviously no footprint correction had been applied to the EC results in Tab. 1. In comparison to that original values, the D1 results did not change at all in the present version, while the D2 results increased by a factor of about 2-5! I cannot understand how these results were derived from the footprint contributions in Fig. S2 (with the poor description given in Section 2.4).
- 2) The footprint contribution values for the fenced area displayed in Fig. S2 seem to be quite reasonable. However, there are some periods with a near-zero footprint

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contribution (I assume mainly for nighttime periods, although the time on the x-axis is lacking in Fig.S2). Thus I wonder, how a meaningful animal emission has been derived by EC with almost zero footprint contribution by the animals in the fenced area!

3) At the end of Section 2.6, it is said that the cumulative SF6 derived emission of the five heifers was divided by the entire field area (2.81 ha) to obtain the results in Table 1. This makes absolutely no sense to me. Why was the summed emission not related to the actual source area (fenced area of 20m x 20m = 400 m²) of this experiment? The size of the entire field has no connection to neither the footprint size nor the actual source area size in this experiment.

4) Moreover, since the methane emission in the present study is not a property of the pasture field (and therefore the emission-to-area relation is not well defined) but of the grazing animals, it would make much more sense to present the results of the method comparison in Table 1 as average emissions per animal (instead of per ha)!

In order to convince the reader (incl. this reviewer) of the reasonability and correctness of this evaluation, a more clear and comprehensible description (and illustration) of the measurements and the corresponding data processing needs to be presented. For this purpose it would be important e.g. to show the measured EC flux time series for this experiment before and after the footprint correction was applied, together with the footprint contributions (Fig. S2).

MINOR COMMENTS

(note that page numbers are abbreviated, only the last two digits are given)

5) P09 L11: What is meant here with enteric CH₄ production being "variable in space"?

6) P10 L25: What does "to filter for data outside the boundaries of the paddock" mean?

7) P11 L1: What is the meaning of "temporal scale" here?

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8) P14 L8: What is the meaning of "(atmospheric and soil)" here?

9) P16 L13: How big was the "set of emission estimates" (n=?) ?

10) P17 L5-6: I do not understand this sentence. In my view, differences in measured CH₄ emission between daytime and nighttime can be EITHER due to imperfect measurements OR real diurnal emission variations. Additionally the expression "losses of CH₄ emissions" is not clear without further explanations.

11) P17 L16: I do not agree that the spectral differences in the low frequency range indicates an instrument-related effect. This would mean that the CO₂ and H₂O instrument had significant drifts in the time scale of minutes to hours. I rather assume that the difference is due to differences in the specific source structure (temporal and spatial) and background signals for the different trace gases.

12) P17 L17-18: I think "H₂O" should be omitted here. Why should the H₂O spectra be affected by "physical low-pass filtering (i.e. EC closed-path system)" when it was measured by an open-path instrument?

13) P19 L2: I cannot see the "significant decrease" of CH₄ fluxes for $u^* < 0.06$ m/s in Figure S1. I guess that a different diagram e.g. with boxplots would be more suitable.

14) Section 3.2: The plume experiment is interesting and methodologically sound, but it does not contribute much to the assessment of the EC method for CH₄, because not direct comparison of the two method was made. If an indirect comparison of the animal emission rates is made (as done here), it is obviously very important to consider the time of day of the measurements (see e.g. Fig. 5). This information is lacking in the manuscript.

15) P19 L19-20 (and Fig. 4): It is not clear, which animal emission rate was used for the modeled animal plume.

16) P20 L6: The footprint is "upwind", not "downwind" of the measurement location.

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17) P20 L10-13: The argumentation that higher methane concentrations indicate higher emissions is usually not appropriate. u^* has a much higher influence on the concentration than the source flux. This effect is also observed in the diurnal flux variability in Fig. 5 with lowest emissions in the night, while concentrations are presumably highest during the night (due to low u^*).

18) P21 L4-7: I wonder why the authors find it important to discuss the difference in the footprint contribution between this study and a literature study (Tuzson et al., 2010). It is clear that each study has its own specific geometry and environmental conditions, which are accounted for in the appropriate application of the footprint model. Furthermore the difference in the measurement height is only one possible explanation, the different size and distance of the emitting area might be even more important.

19) P21 L9-24: The authors interpret minor differences (<10%) between EC and SF6 methods in Table 1 as systematic underestimation of either the EC method ("dilution effect") or the SF6 method ("losses of CH4 emissions ... due to climatic conditions and technical problems"). In this context it would be useful to discuss whether the observed differences are (statistically) significant or not. I have to say that I am quite astonished about the generally good agreement between the two methods in Table 1, given that both methods and the footprint model used for correction of EC fluxes usually have higher relative uncertainties.

20) P22 L11-12 "This agrees with daily periodicity in the grazing and behaviour pattern of heifers observed in our own data ...". How were these observations made? (No information or data are given in the manuscript)

21) P22 L15: "...and CH4 emissions decreased in parallel (Fig. 6)." It would be interesting to additionally show the seasonal dynamics of CH4 emission per animal or per LU. It would also be useful to add a smoothed time series (e.g. over 5-10 days) in Fig. 6.

22) P22 L17-18: Dengel et al. (2011) reported about sheep, not heifers!

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23) P22 L25ff.: I think it is not a very good idea to identify systematic errors in the EC method by comparison with general emission factors (such as IPCC). The latter are global or regional averages (or simplified parameterizations) over many different animal breeds, ages, feeding types, and other specific conditions. Thus it has to be expected that emissions of any single herd show some systematic deviation from this average emission factors, independent of any measurement problem!

24) P23 L23-24: I do not understand this sentence. Please explain.

25) P23 L25-26: How was this value for the potential carbon dioxide sink (and the values in Fig. S4) derived? And why is it called "potential"?

26) P23 L28-29: It is not appropriate to call the sum of the carbon dioxide sink and the equiv. effect of CH4 emission a "net carbon dioxide sink"! It is rather a net greenhouse gas sink (with units of g CO2 eq. m⁻² !), yet without accounting for the contribution of N2O.

27) P31 Fig. 1: It is a bit misleading to say that this setup was used for a comparison of the plume and the EC method, because EC fluxes were obviously not measured with this specific setup!?

TECHNICAL AND LANGUAGE CORRECTIONS

P08 L9: "of the eddy covariance technique"

P08 L12: "a Gaussian plume model and parameterized emission factors"

P09 L19: "There is a number of"

P10 L3: better write "cannot be appropriately accounted for by the SF6 technique"

P10 L4: "tunable diode laser"

P10 L8: replace "offers" by "uses"

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P10 L9f.: replace "over a larger measure area" by "representative for a large source area"

P10 L28: "of the EC technique"

P13 L16: omit comma

P14 L21: "which was meant to investigate the reliability of the EC method"

P15 L5f.: Rephrase this sentence. The "artificial CH₄ source strength" does not have to be "in line with measurements" of cow emissions.

P15 L18: "During the experiment in late summer..."

P15 L21: "multiple Gaussian plume model"

P16 L25: The expression "was dosed" is probably not appropriate here. Please use a better formulation.

P18 L2: Better change title to: "3.1 EC measurement performance"

P18 L15: The formulation is confusing. Better write: "showed a clear decline in the low frequency range"

P19 L22: "on average"

P23 L5-6: The sentence is incomplete. Better: "... is negligible as its CH₄ exchange was orders of magnitude smaller..."

P23 L22: "...were concentrated within a small area."

P24 L3-5: This sentence is confusing and needs to be reformulated.

Figure S2: Add the time scale on the x-axis (similar to Figure S3).

Figures S3: is erroneously labeled as S4.

Interactive comment on Biogeosciences Discuss., 9, 14407, 2012.

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