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Biogeosciences
Discussions

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

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Review of Manuscript by Tallec et al.

This manuscript describes an effort to quantify methane emissions from heifers using four different methods: SF6 tracer release, eddy covariance flux measurements, Gaussian plume dispersion source estimate, and emission factor based approach. Actually, it is more a comparison of three experimental methods against the IPCC default emission factor estimates, but since there is no true reference estimate, one could also talk of four methods.

The paper presents a relevant piece of work and deserves publication. However, it has serious linguistic and scientific flaws which in my view need to be addressed before

being acceptable for Biogeosciences. I actually mentioned in my technical assessment that *"language is clearly understandable, but there are exceptions (l. 18, l. 25 for examples on first page) which would profit from a final check by a native English speaker"* – this has definitely not been done and hence the authors are probably well prepared to receive a couple of critical remarks that relate to the issue that "clearness of expression reflects clearness of thought".

I am sorry to express some dismay when I see that authors do not try to do their best to finalize their manuscript, but if not even the track changes traces are removed from the Supplementary Material then I believe the authors understand some critical remarks related to sloppy manuscript preparation.

Despite having said this I recommend giving the authors a chance to rectify all issues and have their work published in Biogeosciences. But I strictly recommend **not simply waving through the present version**.

1 Scientific issues to be addressed

14408/18: the numbers given in g CH_4 ha^{-1} but without specify the time duration. Please correct!

14412/07: 583 l min^{-1} seems realistic (we however get more out of this pump!) – but your conversion to 375 l min^{-1} is incorrect; there is **underpressure** (vacuum) in the cell, which **expands** the volume and does not compress it. Please correct. Moreover, I recommend to give numbers in L s^{-1} (s is the base SI unit, and for volumes it is m^3, but here the expression L is adequate)

14412/08: I'm concerned about the reference to Hendriks et al. (2008) here. Did you really do it this way? No improvement over what is shown there? I would not even refer to this, this rises some concern about the quality of you set-up, but since your fluxes are realistic I believe that you got better performance than what Hendriks et al. (2008) found with their very problematic Allan Variance analysis.

14412/10: please specify how data were transferred from the FMA to the system with the sonic anemometer. I assume that "digital data were converted to analog signals which were recorded via the sonic anemometer's analog input channels"

14412: The Aubinet et al. (2000) paper was before CarboEurope and does not include the CarboEurope-IP recommendations. You may want to replace this by Aubinet et al. (2012), the book on eddy covariance, throughout the paper (doi:10.1007/978-94-007-2351-1)

14412/Eq. (1): this equation is wrong! Please check the units and introduce all missing terms. You have μmol m^{-2} s^{-1} on the left side (note that you specified μmol s^{-1} which is probably not what you wanted), but you have ppm m s^{-1} on the right hand side (with ppm being μmol mol^{-1}). This must be rectified. Please double-check that the errors in this equation are not reflecting errors in computation of your EC fluxes!

14413/08–36: this is very confusing text. You corrected latent heat fluxes for density fluctuations. Thats perfect. But you also should have corrected the CH_4 fluxes for density fluctuations. It is incorrect to say "The Webb-correction for density fluctuations was not performed since there was a con- stant temperature and pressure in the sampling cell." The density changes with temperature, pressure **and** water vapor concentration. So you must rephrase this to eliminate the conceptual error in argumentation. Additionally, there are other shortcomings in the argumentation; see Hiller et al. (2012) for details on the issue of density fluctuations by water vapor affecting CH_4 flux measurements performed with an FMA (doi:10.5194/amtd-5-351-2012).

14414/08–10: you write "This gap-filling method was considered to be valid for CH4 (atmospheric and soil) flux at Laqueuille. Nevertheless, on consecutive rainy periods the MDV method was not applicable to methane fluxes and these gaps were not filled." – why? The reader cannot follow this argumentation without additional details! Since you

have a closed-path instrument you also should have been measuring during rainy periods, and why are these data not useful to use the MDV especially under consecutive periods with rain? As a reader I would have expected exactly the opposite argumentation (i.e. a short period of rain with gaps cannot be filled with MDV obtained from earlier and later days with fair weather).

14418/07–09: I don't agree with the statement "Co-spectra signals give no further information since the correction factor is instrument related", but maybe it is again a wording issue. What correction factors? You did not mention any for your cospectral analyses, and yes, indeed, cospectra should directly give you some information on fluxes. Maybe they are not easy to interpret and you did not want to show them, but I find it conceptually wrong to claim this as you write it! Please rephrase.

14418/16–21: I do not agree that "the low range was instrument-related" and I do not agree that "confirming that no physical low-pass filtering (i.e. EC closed-path system) had aÃřected the H2O and CH4 spectra", and it is not correct to deduce that "Accordingly, our EC-setup delivered reliable measurements of CH4 emissions from ruminants". I will explain what's wrong here and hope this helps you revise this subsection.

First, with spectra you can only assess whether one instrument worked, but not whether flux measurements (emissions) worked (this would require cospectra). Second, your spectra shown in Fig. 3 clearly show damping **for all compoents** – which puzzles me. I double-checked with your initial submission, and there the y-axis label was "f.sectral density Hz" with the same lines. So, it means that in this display the inertial subrange is proportional to $f^{-2/3}$ and the damped spectrum is proportional to $f^{-8/3}$ (see Eugster and Senn 1995, BLM). In the figure in the attachment I plotted both lines. I have now clue why your temperature signal is damped, but I can say that either the graph is erroneous, or its interpretation, or both.

In the revision please make sure you get control over this – it raises the alarm flag with a reviewer who sees this!

You should also clearly mention what you plotted. Why did the information get lost that the spectral densities were multiplied by the frequency? Why is the information that these are normalized spectra only found in the caption but not in the y-axis label? Why do you use the wrong unit (Hz) on the y-axis label (this is wrong in both cases, but the correct version depends on whether you multiplied with frequency and whether you normalized fluxes, which definitely is the case).

The deviation from expected spectra in all cases around 0.5 Hz is interesting – is there no interpretation for this? Is it an artefact of the procedure you used to compute the spectra, or is it real and deserves attention?

As a bottomline you can only deduce from Fig. 3 that your instruments worked and that all had damping (why?). So I would expect you correct for this damping at some point

14418: my interpretation for differences in the low frequeny range in Fig. 3 is that the low frequencies are (a) not necessarily in equilibrium with the local surface, and (b) it is quite obvious that the surface area influencing the spectra is different for CH_4 than for the other three. Since you had heifers in the near footprint, it appears clear to me that you have higher CH_4 flux from where the heifers stand than in the far part of the footprint where no heifers stand, whereas their CO_2 emissions, their latent and sensible heat released appears small to what the surround landscape does, and hence the differences in the low frequencies.

14419/02: sorry, but I cannot see this significant decrease a $u_* < 0.06$ m s^{-1}. I simply see a lot of scatter, but no "significant" trend. Please reword and make sure that you only use the word "significant" if it relates to statistical testing.

14419/02–06: I cannot confirm these findings. As said above, I only see lots of scatter, but no thresholds whatsoever.

14423/03–06: for this you really would need an uncertainty analysis

14424/09: sorry, but without a reference you cannot talk of "accuracy" in any aspects of this paper! You probably wanted to write "We found that the reliability of the EC flux estimates varied, depending on. . . "

14424/13–15: "In contrast, the IPCC method Tier 2 clearly overestimated the CH4 fluxes at our site" – this is too broad a statement; you should clearly specify that this applies to heifers. See e.g. Zeitz et al. 2012, 10.1080/1943815X.2012.709253 for a more differentiated view on succler beefs and fattening bulls. I think the bottom line is that one cannot boldly state that the IPCC values are too high (or too low) for a site, since it really depends on the type of cattle you look at – and you only investigated heifers (if I correctly understood).

2 Language issues to be addressed

14408/13: how does one "park" a heifer?

14408/21: → The IPCC Tier 2 method . . .

14409/17: wording: the units used in the specification of a flux do relate to the spatial and temporal scale, but they do **not** imply a specific scale. If you disagree, then at least I do not think of the field scale to be a the m^2 scale, rather at the ha size. Suggestion: remove the confusing parenthetical note.

14409/26: wording: CH_4 emissions do not only occur during grazing, but also during ruminating. Please rephrase correctly.

14410/10–11: wording: you cannot claim that these studies reported "accurate" use of the EC technique. Namely, I put a **big** question mark to the Hendriks et al. 2008 study: if the Allan Variance plot shown in her Fig. 2 is correct, then this paper actually shows that **their measurement did not work** or at least they cannot claim they were accurate given such an Allan Variance plot. Please rephrase – I even recommend not to relate to the Hendriks et al. (2008) paper in this way, you don't want the reader to believe that your study is flawed in the same way as the Hendriks et al. (2008) study is.

14410/13–14: "ruminating animals" is not identical with "restored wetlands" – please correct this error by rephrasing.

14410/25: liked → you probably wanted to write "wanted"

14411/01–02: "by assessing the temporal scale and CH_4 budget over an annual grazing period" – I do not understand; what do you want do say? I don't see any explicit assessments of temporal scales in the manuscript. Please rephrase.

14411/11: splits → applications (or amendments)

14412/04: wording: delete "is located in"

14412/05: wording: technically it is a "scroll pump" and since it is not for liquids it is a "dry scroll pump" (not a scroll dry pump nor a pump scroll dry)

14412/08: sucked → pulled

14414/21: liked to → aimed at investigating

14416/22: what is meant with "main instantaneous" – this seems to be an internal contradiction, probably you wanted to say "mean average" (which is the opposite to instantaneous)?

14419/06: you do not have any reference to use the word "accurately". Replace "could not be accurately measured with the turbulent flux term solely" by "had to be rejected"

14421/07: "higher" → larger (?)

14423/19–20: what is meant with "seem to offset the CH4 emissions "losses" (i.e. bias) observed during the short-term comparison experiment"? Please rephrase

14424/01: what is "an improved net greenhouse gas sink"? Do you mean an improved **quantitative estimate** of the net greenhouse gas sink? Please rephrase

14424/15: "We like to underline that" → "We believe that"

3 Other minor issues to be addressed

14408/09: the EC method was used for many other species, so refering to CO2 only in this context is arbitrary. Simply delete "already used for CO2".

14410/04–05: with such examples it is always a question whether they should be inclusive or random. In this case, however, you mix two things, one is the light source (e.g. tunable diode lasers) with the measurement principle (e.g. cavity ringdown specroscopy). There are other light sources (e.g. quantum cascade lasers or full spectrum lamps for FTIR) and other measurement principles than cavity ringdown that are fast. Please clarify this by rephrasing.

14410/06–07: this connects with the previous point: especially for CO_2 the vast majority of scientists actually uses a different measurement principle (NDIR) as e.g. used by the Licor sensors for CO_2. I find such statements rather confusing than elucidating. It is completely unclear to the reader why a reference to CO_2 is made although this is a very good example of other techniques and light sources than what typical CH_4 measurements use. And again – the limitation of the mentioning to CO_2 is misleading, it has been used for many species and applications besides CO_2.

14410/08–10: "The eddy covariance technique precise nonintrusive concentration measurements at a high sampling rate (10 to 20 Hz) over a larger measure area" – this is again confusing and misleading: EC is a point measurement at a high sampling rate. It does **not** measure over a larger area, but using a footprint model (conceptually or numerically) one can relate this point measurement to the fluxes from a certain area. Please correct by rephrasing. 14411/22: size of fenced area? size of paddock?

14411/23: confusing: an R3 sonic can output 100 Hz and has an even higher measuring rate internally. Originally I thought you were recording at 20 Hz, but it turns out that not even this is correct (see later in your manuscript). So please rephrase and give the correct information. Note that there is a difference between the measurement rate and the reporting or logging frequency which must be correctly reflected in the wording you chose.

14412/04: in Fig. 1 you call it an "FMA" (Fast Methane Analyzer) which is the typical wording I suggest instead of DLT-100.

14415/06: why is the "y" in boldface in analyzer?

14415/26: in your equation there is a formatting error: the subscript to σ are written as separate variables ($\sigma\, y$, for example, instead of σ_y); three occurrences in all exponents

14416/05: at which height above the paddock?

throughout the manuscript: you seem to have worked with heifers, not with cows, so please replace any erroneous reference to cows by heifers. A cow is a "fully grown female animal of a domesticated breed of ox, used as a source of milk or beef", and in farming in particular it is "a female domestic bovine animal that has borne more than one ", whereas a heifer is a "a young female cow that has not borne a calf." So cow and heifer are mutually exclusive terms.

14416/08: specify what A, B, C and D are: "A, B, C and D are scaling parameters that depend on. . . "

14416/13: you cannot use the symbol D for two (actually even three) different meanings in one manuscript. Reorganize your symbols; if you use Pasquill's stability class D then you must find another symbol for the parameters in your equation for σ_y (e.g. lowercase characters). Moreover, you use D for the paddocks in Fig. 2. Why not use P for paddocks instead of D?

14416/13: specify the unit for roughness length (it should be m)

14416/21: your definition of D1 and D2 (besides the problem of using the same symbol again) is that it disagrees with Fig. 2. You **must** be consistent and rectify either in the text or in the Figures.

14417/03–05: "as during night time low turbulences can lead to stratification of the atmosphere which can make impossible to measure CH4 by the EC-method": please rephrase; stratification is also a function of the heat and radiation fluxes, not only due to low turbulence, and the main problem is not the measurement, but how this point measurement can be related to the upwind surface (if at all).

14418: please homogenize the spelling of EC set-up

14418/06: use normal parentheses around sonic temperature

14419/01: define R and how it was measured

14419/08–12: I cannot follow this argumentation.

14419/16–18: It is not the CH_4 concentration that had a maximum of 200 to 270 ppb but its **difference** respective to the ambient concentration. Please reword.

14419/23: don't break the unit apart; specify $280(\pm 18)$ g day^{-1} animal^{-1}

14420/16–17: friction velocity must be in m s^{-1} please double-check the values and correct their units.

14421/10: 6.5% difference does not appear to be statistically significant. I generally missed a serious error estimate in all these comparisons. If you assume that EC fluxes are $\pm$20% anyway, then you could almost speak of "perfect agreement".

14422/03: space missing in CH_4 emission

14422/04: not data sets but data records (in one data set)

14422/10: use scientific time format (00:00 to 23:59)

Table 1: round to full figures, giving one decimal appears to imply an unrealistic preci-

sion.

Fig. 2: why not use P1 and P2 instead of D1 and D2 here?

Fig. 3: see critique above; in the caption: use FMA instead of CRDS (and homogenize the use of such names throughout manuscript, figures and tables). Reword the caption, it is not “distribution in frequency of”

Fig. 4: replace cows by heifers

Fig. 5: what is a “mobile mean”? Probably a running average – and if so, they do not match the data. There is an unexplained right shift of the curves. Please retify this! Moreover, since mean daily cycle is a cyclic phenomenon you must consider this in the running mean computation and extend the curves to 00:00 and 24:00 hours. In all cases the curve must fit the data, as is this figure is not acceptable (sorry that I did not see this in the initial submission already).

Fig. 6: don’t mix units written with a slash with units written with negative exponents! In the text you always use the latter version, so you must use this concept also in the figures. This also concerns Figs. S1 and both S4.

4 Supplementary Material

Fig. S1 modify x-axis and y-axis labels. Homogenize the writing of u_* in caption and use the correct notation for units in all cases.

Fig. S2: you never use P1 and P2 for periods anywhere, not even in the figure! Remove this information and simply write period 1 and 2 as in the figure.

Fig. S4, the first: this should probably be Fig. S3. Correct the units for u_* (which is of course m s^{-1}). Use fixed number of decimals in each of the y-axes. Simplify the x-axis: only give the hours 00, 06, 12, 18 and write the date only once below the range

of hours of the respective day.

Fig. S4, the second: correct y-axis label. Remove track changes annotations in the caption. Replace Net CO2 sink by net GHG sink (GWP).

5 References

Eugster, W. & Senn, W. (1995) A cospectral correction model for measurement of turbulent NO_2 flux. Boundary-Layer Meteorol., 74, 321–340.

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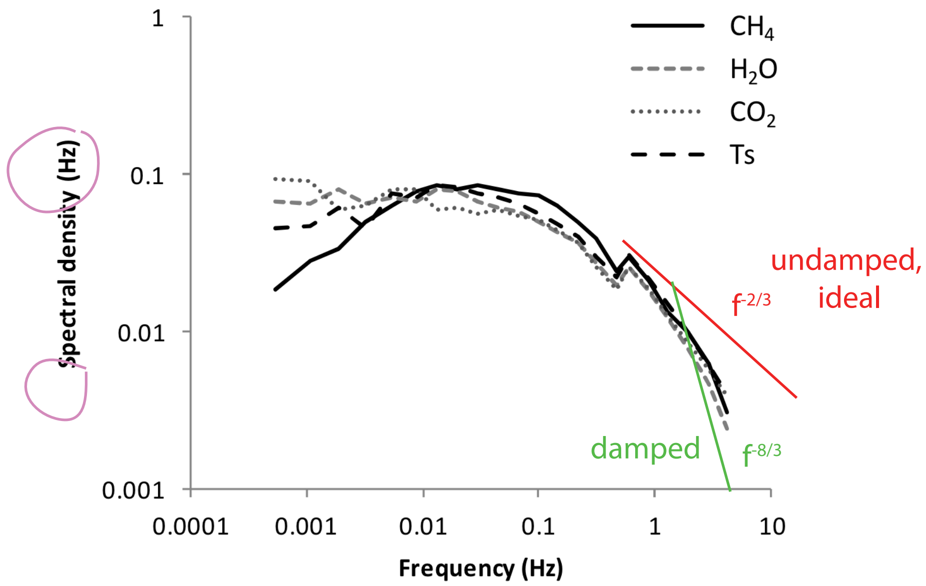

Fig. 1. Interpretation of damping in spectra shown in Fig. 3 in the Tallec et al. manuscript