bg-2015-31

Author response to comments of referee B. Heinesch

(referee comments are printed in *italic*, author responses are printed in blue)

1. I would appreciate the addition of one or two sentences in a visible part of the paper stating clearly that authors are lacking information on cows' activity in their study. The consequences being that they are not able to discuss meaningfully the diel CH_4 cow emission cycle and to investigate how much of the 30-min variability is coming from the variation in cow's activity. In addition, their use of the term "grazing" for all moments when cows were in the paddocks is misleading, cows spending part of their time ruminating/idling, especially during the night.

Actually, we did record the cow activity by the mean of pressure noseband sensors (RumiWatch, Itin+Hoch GmbH, Switzerland). Originally we considered the corresponding information not conclusive enough and thus did not use it in the original manuscript version. But motivated by the referee comment, we analyzed the data again more thoroughly. The new analysis showed that there is indeed a daily pattern of cow activity, which to a certain degree correlates to the emissions. Therefore we will complement Fig. 11 by adding the cow activity data (see below) in the revised version and enhance the discussion accordingly.

In addition, we agree with the referee that the term 'grazing' can lead to confusion if used to generally indicate the periods when cows were present on the pasture. We will change the wording where necessary.

2. I'm wondering about the best strategy for computation of the 30 min FP weight (Eq3). You chose to combine your positions with the footprint at a time step of 5s in order to optimize the use of your position information. In doing so, you compute a FP over an interval of only 5s. What is the physical meaning of this 5s footprint and how is it linked to a cow position taken simultaneously? For example, with a wind blowing from the South at a wind speed of 2 m s-1 and a cow located 100m away South of the mast, methane emitted by the cow will take 50s to reach the mast. If there is a wind direction shift during these 50s, your 5s FP weight will be totally wrong. On the other hand, making a 30 min average of a cow positions and combining it with the 30 min average footprint will also raise this type of questions. So pragmatically, my question is: was the computed flux per cow sensitive to the computation strategy? Might be useful for other teams.

This is probably some kind of misunderstanding. As stated in the text, we always used the footprint (FP) weight function determined for 30 min intervals; and even if this is combined with a set of 5s-position data, the result is still an (average) property of the 30 min interval (i.e. the temporal sequence of the 5 s data is not taken into account).

The issue here is, whether it would be meaningful to first average the position data to 30 min values before combining it with the FP function. Since the footprint function is very non-linear, a position averaging is generally problematic, e.g. one cow on the right side of the main FP and another on the left side could average to two cows at the center of the FP. So the calculation with 5 s position data is just a simple way to avoid unwanted effects of position averaging. This method is fully equivalent to the determination of a 30-min. two-dimensional cow density distribution, which is then combined with the 30-min. FP weight function.

Through modification of Eq. 3 (acc. to comment *P3432Eq3* below) and the surrounding sentences, the FP averaging procedure was clarified in the revised version and should prevent misunderstandings.

3. The immediate vicinity of the studied pasture should be better described, especially in the wind sectors that were not filtered out. Was it crops or pastures? If it was pastures, do cows were present in these nearby pastures during the experiment? The measured signal could have been significantly polluted by the presence of cows on these possible nearby pastures, amplified by herd behavior at times when cows were also present on the principal pasture.

The neighboring fields of our experimental pasture and their use are indicated in Fig. 1. The meadow in the East was under mowing, the pastures in the South and South-West were managed very similarly to the experimental field (most of the time they were alternatingly grazed by the same cow herd due to the lower than expected growth on the study field). Thus the influence of cows on the neighboring fields was mostly limited to non-grazing conditions, and the calculated FP contributions of these neighboring areas where typically below 5%. Therefore cases with cows in the far footprint have been removed for the determination of the background CH4 flux from the soil.

We will modify the text to better explain this point.

4. Removal of outliers is always questionable for data showing a huge variability. In this case significant and natural flux variability can arise for important cow footprint weight events due to the cow movement or changing wind direction. The authors should precise how they define exactly an outlier (P3429L23: How is this R boxplot function working?) and how they can distinguish between an outlier and natural variability?

The standard boxplot, as implemented in R, defines the extend of the whiskers as the most extreme data point which is no more than 1.5 times the length of the quartile box away from the box. The data points outside the range of the whiskers are considered as outlier. We will clarify this outlier definition in the text.

We agree that the removal of outliers for data with a huge variability is critical. However emissions per cow are expected to be much less variable than the measured flux. We therefore determined outliers not in the flux data but in the calculated cow emission. The limits for the outlier determination correspond to the upper and lower limit of values shown in Fig. 13. This range is much larger than the range of ('natural') systematic diurnal and seasonal variability of E_{cow} shown in Figs. 11 and 12b (see discussion in Sect. 4.4).

5. "In 92% of the cases when cow fluxes could be measured more than 70% of all GPS devices delivered usable data" (P3430L28). Meaning that among these 92%, you often have several cows (0 to 6 over 20) that are not localized. What did you do with these "missing" cows? Did you simply ignore them in the cow footprint weight calculation or did you position them at the mean of measured cows positions for this 5s data? In all cases, rather than simply concluding that "it was considered as sufficient for the quantification of cow FP contribution", you should recognize that this is also a significant source of (random) uncertainty in the CH4 emissions/cow estimation.

We agree that this sentence (together with the previous one) was not very conclusive and rephrased them.

6. Authors do not make use of the traditional u filtering for exclusion of low turbulence events where turbulent fluxes do not represent the true exchange anymore. Probably some of their data filtering steps overlap with the u* filtering but they should make it more clear to which extend.*

As mentioned by the reviewer, the applied data filtering overlaps to a large degree with a u_* filtering. Additionally applying the two above described u_* thresholds did not significantly change the results

(u_* thresh = 0.07 m s^{-1}: 426 and u_* thresh = 0.1 m s^{-1}: 429 gCH_4 $head^{-1}$ d^{-1} compared to 423 gCH_4 $head^{-1}$ d^{-1} without specific u_* threshold filter). The application of a u_* filter has an effect of less than 2% on cow emissions. The effect on soil fluxes was around +5%.
We added a comment about the negligible effect of additional u_* filtering in the manuscript (at the end of Sect. 2.2.3).

Specific comments:
P3422L21-29: Objectives formulation involves non-trivial concepts like "compensation for heterogeneous distribution" and "footprint weight" not yet introduced and therefore difficult to understand for non-experts. Please make it more explicit.
We have rephrased the objectives in a more explicit way without the mentioned terms.

P3426L5-7: Meaning that the calibration was strongly affected the first half of August, when cows were back on the pasture. What did you do with the data?
First we have to say that the formulation was not fully appropriate here: 'accuracy' has to be replaced by 'instrument sensitivity'. We used the individual calibration (sensitivity) for this phase.

P3425L22: What do you mean by "the noise level was determined as minimum of the SD of the 10Hz data". On which time window?
For each 30 min interval the SD of the 10 Hz time series was calculated. These SD were plotted against time. The lower bound of all SD (over few weeks) was then used to estimate the noise level and its temporal development. We will be more precise on this statement in the text.

P3427L4: Why did you use linear detrending and not block averaging? Did it have significant influence on the computed fluxes?
Both linear detrending (for concentration only) and block averaging are commonly used methods for EC flux calculation. The question, which method is more appropriate is a basic question of EC flux measurement theory and cannot be assessed to a satisfying degree within the specific scope of this paper. The application of a linear detrending for the concentration time series did have a general systematic influence on the computed fluxes but could avoid non-stationarity effects for some cases in the transition phases of the diurnal cycle.

P3427L26-29: I agree that CO_2 can usefully be used for analyses of high-frequency losses. However, elsewhere you mention that many CH_4 fluxes were well above the detection limit. So why these CH_4 data were not used for spectral analyses? If this is because spectra have some specificity for cows related fluxes, please comment.
For the fully empirical ogive method applied here, a preferably large number of ogive ratios (trace gas vs. temperature) for each wind speed and stability class is needed. It was found that for CH_4 the cow respiration signals led to generally higher (random-like) scatter in the cospectra and ogives, which complicated the quantitative determination (regression) of the damping factor as a function of wind speed and stability. Therefore, the CO_2 flux ogives determined with the same setup and analyzer were mainly used for this purpose.
We modified the text to clarify this explanation.

P3428: Why "around"? You can give the precise value.
We do not understand this comment, the precise value is given at the end of the sentence (L17).

P3427L21: Why "statistically" more representative?

The sentence was clarified to "… is more representative on statistical average, because it is not biased by the choice of the maximum".

P3428L14-21: Please add a reference for this definition of the flux detection limit.

We do not know about a literature reference for this detection limit determination. However, it is based on the flux error estimation from the variation of the 'baseline' of the covariance function. We will add a reference about this error estimation method.

P3428L26: How was this range of acceptable tilt angles defined?

We rephrased this sentence to: "small vertical vector rotation angle (tilt angle) within ±6° to exclude cases with distorted wind field". The original non-symmetric range may be misleading. It was indicated, because there were practically no cases between -6 and -2°.

P3430L4: Precise on which part of the cow and with what kind of fixations the GPS were installed and if the selected position was efficient to avoid damages on the GPS. It may be a useful info for other teams.

The GPS was attached to the nylon web halter on the cow's neck mainly to optimize satellite signal reception. This information will be added to the text.

P3430L13: Our own experience with the dilution of precision information given by the GPS systems is that the time evolution of this variable shows from time to time abrupt changes not correlated to the error in the localization. It therefore makes it difficult to simply define a threshold on DOP above which data should be discarded. Did you observe the same behavior?

We did not evaluate the PDOP behavior of the GPS in this respect.

P3430L14: Which criteria were used for identifying visually a "bad data"?

This were obviously implausible data showing erratic changes in position (e.g. faster than a cow is able to run…).

P3430L26: How do you know the spatial accuracy of your EC footprint model? Please add at least a pertinent reference.

We will change this sentence to: "We assessed an accuracy of 4.5m as sufficient for the present experiment because it is much smaller than the typical flux FP extension and also smaller than the typical cow movement range within a 30 min interval."

P3431Eq2: This equation has two unknowns: z0 and d. What about the displacement height d? You probably equated it to a fraction of the vegetation height z. But did you have a dynamic evolution of z based on field measurements? I guess this is extremely difficult due to non-uniform grazing within your different paddocks.

Indeed we considered the displacement height d as a fraction (2/3) of the canopy height. Since the latter varied between about 5 and 25 cm, the aerodynamic measurement height (z-d) varied between 184 and 197 cm corresponding to only 7%. This is much lower than the relative variability of z_0 (see Fig. 8) and therefore the temporal variation of d was found to be negligible.

Besides, we measured the canopy height of the main paddocks each week by means of low-weight plates (see Ammann et al., 2009) as well as by a medium-weight rising-plate herbometer at multiple spatially distributed points. Because of the intensive rotational grazing, we found distinct time series for the vegetation height (not difficult to measure but just a lot of work …). The corresponding results will be presented in another paper on CO2 exchange.

P3432Eq3: I fully understand this average FP weight quantity but I think you should rather use the total footprint weight of the herd when you plot a dependent variable like the flux or z0 in function of the footprint weight. This remark holds at least for fig 7, 8, 9. Of course, in your case, the number of cows stays constant during the whole experiment so it will not change the figures but fundamentally, your dependent variable depends on the total footprint weight and not on the mean footprint weight. If you agree with this comment, introduce the total footprint weight in LSU m-2 (the denominator of Eq4) and change the text and the figures accordingly.

We agree with the referee that the total footprint weight of the herd is a more useful quantity in the mentioned Figures, also for comparison with other studies. We therefore introduced the quantity $\bar{\varphi}_{\mathrm{herd}} = n_{\mathrm{cow}} \cdot \bar{\varphi}_{\mathrm{cow}}$ in Equation (3) and used it in the corresponding Figures and also adjusted the thresholds and the text accordingly. As mentioned by the referee, this mainly changed the scale of the x-axis by a factor of 20 (= the usual number of cows).

However we did not change the units/normalization from 'head' (animal) to LSU as suggested. In our opinion this could make sense for beef cattle but not so much for dairy cows. The CH4 emission is strongly correlated with the amount of feed intake (energy demand) of the animal and for dairy cows this mainly depends on the milk yield (and only to a minor extent on the body weight). Therefore a normalization to LSU using the animal live weight would not lead to meaningfully comparable values for dairy cows.

P3432L15-17: What is the quantitative impact of this “blurring procedure” on the final flux per head estimation? Is it really a useful step?

The blurring procedure had no significant effect on the (statistical) mean results presented here. However, we also used the same data for other more detailed evaluations (not presented in this manuscript), for which the uncertainty of the cow positions (and FP function) for individual 30-min-intervals was more important. Therefore we used the 'blurring' as a general procedure for our data. It represents and illustrates the uncertainty of individual GPS position data.

P3439L10: Baldocchi et al. 2012 do not define how their detection limit was computed, making the comparison difficult.

The calculation of the uncertainty can be found in Detto et al. (2011). The reference was changed accordingly.

P3440L15-19: There you discuss random uncertainties. Please separate more clearly the discussion about random and systematic uncertainties.

We agree that the statement about random uncertainty here was not adequately separated from the systematic uncertainties mainly treated in this Section. Therefore we will remove the random-error related part from this Paragraph.

P3445L24-26: I found this sentence confusing, please re-formulate.

We agree that the sentence is not fitting into the text flow here. Since it is not crucial, we will remove it from the Conclusions section.

Fig4: It took me some time to understand the meaning of the blue line. It's indeed useful to show that successive positions are correlated but it should be commented in the main text or the legend. However, given the high number of figures in your paper, I would suggest to remove this one, information in the text being self-explaining.
We follow the suggestion and will remove this Figure from the manuscript.

Fig7: This important plot is a bit confusing. Probably due to the expression ("most of the diagram") used in the legend when describing the zero FP weight case. Use rather something like "left panel containing most of the points". Or you could group all "soil" cases and label this group "FP weight <= 10-7"? Also I do not understand why you have so many gray points in the same y-axis range as colored points. According to your legend, gray points that are within panels containing also colored points should be outliers. And an outlier should be by definition "out of the range".
We agree that the plot is somewhat complicated in the present version. Therefore we follow the referee's suggestion and will group all cases with zero FP contribution into one boxplot. This will also illustrate much better that the large majority of the data is within a very small range between 0-10 nmol m-2 s-1. The number of gray points in the original Figure denoted as 'undefined' also included cases without cow GPS information. We will remove this cases in the updated Figure.

Technical corrections:
P3420L20-21: I prefer: "Methane is after carbon dioxide the second most important human induced greenhouse gas, contributing about 17% ..." making it more clear that it is not two separate information.
Will be changed as suggested.

P3421L2: "to assess their effect on global scale".
Will be corrected accordingly

P3422L27: Comparable to what?
Will be rephrased to "... comparable results to detailed cow GPS positions"

P3431L20: typo: replace z by z0.
This error was introduced during typesetting and overseen during proof-read, we will check that in the final version.

References

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