

## ***Interactive comment on “Study of the main processes driving atmospheric CH<sub>4</sub> variability in a rural Spanish region” by Claudia Grossi et al.***

### **Anonymous Referee #2**

Received and published: 2 November 2017

The paper presents atmospheric data of CH<sub>4</sub> mole fractions and Rn222 concentrations observed at a measurement site in central Spain. Surface-atmosphere exchange fluxes of CH<sub>4</sub> are estimated based on the radon tracer method, and compared to values from an emission inventory. The topic fits well in the scope of ACP. In general the paper is well written, and I recommend publication after the following concerns have been addressed.

#### General Comments:

The authors found a strong disagreement of Rn based CH<sub>4</sub> flux estimates with the values in the EDGAR inventory. Potential reasons for this should be discussed in more detail. What is the contribution in the regional EDGAR CH<sub>4</sub> emissions from different source sectors, e.g. enteric fermentation? Which sector seems to be the main cause

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for the disagreement? Discussing such questions would allow for inventory people to better learn from such observationally based estimates.

Footprint calculation: What was used as the height below which particles are assumed to be influenced by surface fluxes? Ln 210 mentions 300 m, but what was assumed in cases with a nocturnal boundary layer height below 300 m? Particles above the top of the nocturnal boundary layer should not be influenced by surface fluxes. If the method assumes all particles below 300 m to be influenced by surface fluxes, the associated uncertainty in the footprint should be described. Note that usually there is strong wind shear near the top of the nocturnal boundary layer, which worsens a potential error in estimated footprint area. Also it is unclear how exactly the weighting function  $w(x,t)$  (Eq. 2) was normalized, and what the exact time limits in the summation in Eq. 2 are. This needs to be clearly described.

Please use an equation to better illustrate the FLEXPART Radon-tracer method derived CH<sub>4</sub> fluxes (FR\_CH<sub>4</sub>).

Rather than showing a somewhat hard to read map in Fig 1, why not show the footprint map and a map of the inventory based emissions? That would be better related to the rest of the manuscript.

#### Specific comments

Ln 90: “flux in this area is of about” I suggest to drop the “of”

Ln 124: “The instrument accuracy for CH<sub>4</sub> is of 0.36 ppb” I suggest to drop the “of”

Ln 143: Is the canopy really below 20 cm? May be this should read “below 20 m”?

Ln 157: Please rephrase the section header, and avoid unreadable terms (i.e. avoid underline characters).

Ln 177: For which time intervals was the correlation between CH<sub>4</sub> and Rn assessed, for a single night? This should be stated

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Ln 231: replace “is” by “of”

Ln 242: drop “of”

Ln 243: “it is of 30 ppb” drop the “of”

Fig. 3 and Fig. 4: it would be useful to show the monthly boxplots also separately for day and night, especially for attributing changes in daily amplitudes; it could well be that low nocturnal PBLH drives the larger amplitude during summer rather than the deeper mixing during daytime as stated in Ln 293.

Figure 7: the legend is unnecessary, I suggest removing

Fig. 8: Why are not the monthly values of the UHU climatology shown? Also, it should be mentioned what “local flux” means; is it the UHU Rn flux value of the local pixel containing the GIC3 station?

Ln 336: “is of” drop the “of”

Ln 336: Looking at the red circles in Fig. 9 it seems that the mean should be much lower, somewhere around 0.1 mg CH<sub>4</sub> m<sup>-2</sup> h<sup>-1</sup>.

Fig. 9: the grey shaded rectangles seem to be at the wrong position. In the figure caption, e.g. week 21-27 June 2014 is mentioned, while the rectangle seems to be at around mid-end of March 2014. Also, the green shaded rectangle (presence of animals) is located at times with low FR\_CH<sub>4</sub>.

Fig. 10: Please use simple numbers as x-axis labels to indicate the months.

Ln 395-397: this is a repetition of Ln 287-289

Ln 404-405: I disagree with the assumption that CH<sub>4</sub> fluxes vary only to a small degree; this has not been shown. In Ln 390 the authors even argue that the hysteresis in Fig. 5 is due to changes in local emissions. I suggest citing literature describing the emissions from animals; what is expected from the process level, e.g. do ruminants

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emit constantly, or more during certain parts of their diurnal feeding cycle?

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