

## ***Interactive comment on “On-farm beef cattle methane emissions measured with tracer-ratio and inverse-dispersion modelling techniques” by Mei Bai et al.***

### **Anonymous Referee #1**

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The Authors submitted a manuscript outlining the measurement of methane emissions from an experimental pen with beef-cattle in Australia. The work presents continuous measurement for 7 days in a row and compare two different methods: trace-ratio and inverse dispersion modelling (IDM). The IDM method, in particular, is presented with two different concentration measurement approaches, a Laser, and an open-path FTIR. The Authors presented an interesting experimental set-up and compared the methods, stating that the IDM is accurate to perform the measurement in the proposed set-up and meteorological conditions. The methodology presented is lacking and needs to be reorganised and completed to improve the understanding of the work. The results do not show sufficient elements to meet the objectives of the paper and the discussion

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part must be strengthened. The paper concluded that IDM is a suitable methodology to perform this type of measurements, grounding this statement by comparing only the cumulative emissions from different measurement techniques. No micrometeorological parameter have been detailed and discussed and no prove of the validity of the application of the IDM method was provided (e.g. homogeneity of the source). On the other hand, the experimental set-up and the dataset of the paper have the potential to contribute to this journal, in a decidedly revised form. For these considerations, I suggest a strong revision of the manuscript before the publication in this journal, which takes into consideration the comments below. I therefore recommend major revisions, with the evaluation of the revised paper.

#### **\*\* Abstract \*\***

1. Should be reconsidered based on the remark of the other sections. I would suggest to add the details of the experiment's management, e.g. duration of the trial, pointing that was an experimental pen. Adding the type of the FTIR and the Laser used can be useful to understand the method used. I would suggest also to add the uncertainty of the cumulative flux.

#### **\*\* Introduction \*\***

2. The introduction is somewhat lacking and should be extended. Moreover, some elements missing to understand the novelty of this work compared to literature. First, I suggest adding some data from the inventories related to the contribution of the livestock to GHG emissions in Australia and the World. This Reviewer suggest a series of questions to be answered with the purpose to improve the introduction section: What is CH<sub>4</sub> and how it affects the climate? Where does CH<sub>4</sub> emissions come from? How agriculture (and livestock) contributes to CH<sub>4</sub>? Which are the most prominent mitigation options?

3. Secondly, the section describing the different available methods and techniques to measure enteric CH<sub>4</sub> is, in my opinion, poor and should be improved. I suggest to

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add examples from literature, which can be used also in the discussion section (this Reviewer add here a non-exhaustive list: Felber et al., 2015; Dengel et al., 2011; Lockyer and Jarvis, 1995; Grainger et al., 2007; Laubach et al., 2008; Todd et al., 2014). The strengths and weaknesses of each measurement method or technique should be stressed in order to defend the type of methodologies used by this study. I suggest also adding some experience about the tracer-ratio technique, since it is defined as “true” in this paper (L 69).

4. Third, the novelty of this study. If the IDM technique has been already applied to perform the quantification of CH<sub>4</sub> from grazing animals, I would encourage adding these information and stressing how the work you are presenting has some novelty (technical, methodological, or environmental conditions) compared to the literature and previous studies (i.e. Bai 2010). Finally, why do you compared two different concentration measurement tools?

\*\* Materials and Methods \*\*

5. This section should be reorganised. I suggest providing a detailed section of the laser and FTIR, along with their working principle and field setup, just below the Experimental Design section, then describe the two methods (tracer-ratio and IDM). The information about the FTIR and Laser is scattered and not well organised, disadvantaging readability and understanding. See also the comments #10 to 12; and #22. I suggest including here the details of the calculation of the losses made with IPPC's guidelines (Table 1); comment #21

\* Experimental design: \*

6. I suggest adding (here or in the Results section, see comment #19) more details about the experimental site, as the meteorological variables measurements from the weather station (e.g. rain, temperatures, wind direction and speed). This will help the reader to understand the validity of the measurements (e.g. wind direction), this particular environment and, of course, the results.

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7. Can the Authors detail here more about the dejections management during the experiment in order to better understand the field set-up and neglecting further sources of methane?

8. L 54-55 I would suggest to add a reference to the part of the tracer description (2.2.1)

9. Figure 1. I warmly suggest re-making the picture with a proper scale. This will help better understating the field setting and the distances of the probes (and the weather station) from the fences.

\* Methodologies \* \* Tracer-ratio technique (N<sub>2</sub>O Tracer) \*

10. I would suggest to explain with more detail and in a few line how is the principle of this method. L75, please explain what is QCH<sub>4</sub> in the text.

11. Can the Authors add the details of the producer of the FTDIR, the measurement range (to justify also lines L112-113 and L142-145), the uncertainty and sensitivity (to justify lines L162, L178), and all the technical parameters that can help to characterise this measurement. Could the Authors detail where the measurements were recorded ?

12. Can the Authors add some information about the close-path FTIR used in laboratory in this section, or in the section of the concentration measurements (see comment #5)?

\* Inverse Dispersion Modelling technique \*

13. This section should represents one of the main methodological part of the paper and, I retain, it can be improved. I suggest adding the principle behind the backward application of the short-range Lagrangian dispersion model used in the study (equation, number of trajectories used and principles of the MOST). This will improve the understanding of the scientific ground, the application of this technique in the case study presented here and better understand the Equation 1. Any reference to other

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study using IDM in the short range is recommended.

14. Please, detail how the roughness length was calculated (reference or equation, and the results). Furthermore, can the Authors detail if they're using a constant or a variable  $z_0$ .

15. L139-142. Can you address why these thresholds were imposed for this case study and why these are different from Flesch et al 2005? Please, refer here to the methodological part requested in the comment #13. Furthermore, how many "15-mins" data were excluded from the dataset with these thresholds and in which part of the day?

16. L 143. Please, explain what "spec.max" stands for.

\*Calculating Average Emissions\*

17. I would suggest to rewrite this part more clearly, giving some reference to other studies which use the same calculation. This will greatly help the reader. A gap-filling procedure has been used? Please add these details.

18. Were the periods when the animals were not in the pen excluded from the measurement dataset? This point should be better described.

\*\* Results \*\*

\*Climate condition\*

19. This part should be improved and extended. I would suggest adding a figure with the dynamics of air temperature, wind speed and rain, at least. Moreover, I warmly recommend to add a figure with the trends of  $u^*$  and the turbulence parameter  $z/L$ .

\*Tracer-ratio measurements\*

20. To better understand the measurement performed, given that two different methods are compared in this study (Laser and FTIR), it might be interesting to evaluate the concentrations observed over time by the two systems and by the tracer, before

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evaluating the final daily cumulative emissions. I suggest to provide these results.

21. Table 1. I suggest putting the measurement uncertainty for each of the measurements. I would also suggest removing the reference (Charmley) from the table and keep it exclusively in discussions section along with the other sources cited to defend your findings. Furthermore, I would better explain the calculation with the IPCC's guidelines in materials and methods (see comment #5).

22. L161-163. What about the sensitivity of the laser source?

\* The inverse-dispersion modelling (IDM) emissions \*

23. L175. I would suggest to detail better what "low wind speed" means for the Authors. Or, if these percentages are comprehensive of the periods not considered because of the MOST conditions failure (L139-142)?

24. I cannot see any comparison about the "sensitivity" of the two sensors. I suggest to address this part on the Materials and Methods section (see comment #5) and in the results (comment #19).

25. The lowest emission value is at 9 am, the time when the animals left the pen. How did this event affect the dataset? Are these gaps filled and how ?.

26. I would warmly suggest to insert a further figure about the trend of 15-mins emissions over the 7 days of measurement. This will give the real picture of the dataset, without the period of failures (technical), filtered because of the MOST failure.

27. Figure 2. IDM-FTIR does not have the measurement at 11 pm.

\*\* Discussion and conclusion \*\*

28. The discussions should be better set up and expanded with other literature studies to defend the validity of the measurement, i.e. defending that the conditions of the experiment were always suitable for the application of the IDM. It seems that the reliability of the IDM method is related only to the final cumulative emissions (Table 1). In order to

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define that the source was homogeneous, and therefore the monitoring of the animals is not needed, as stated, further results from this study - or results from other studies - should be provided.

29. Referring only to the method of the IPCC guidelines is, in my opinion, limited. I would suggest broadening the discussions with other case studies, reporting their characteristics and results to make the measurement more robust (e.g. references cited online 226).

30. The conclusions, with respect to the use of IDMs, should be much more cautious given that this is an experiment of only 7 days, performed in micrometeorological conditions not detailed in the paper, without a real defence of the validity of the application of the method itself (homogeneity of the source).

\*\* References cited in this document \*\*

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Dengel, S., P.E. Levy, J. Grace, S.K. Jones, and U.M. Skiba. 2011. Methane emissions from sheep pasture, measured with an open-path eddy covariance system. *Glob. Change Biol.* 17:3524–3533. doi:10.1111/j.1365-2486.2011.02466.x

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