Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2016-353-RC2, 2017

Atmospheric Measurement Techniques Discussions

Interactive comment on "Statistical atmospheric inversion of small-scale gas emissions by coupling the tracer release technique and Gaussian plume modeling: a test case with controlled methane emissions" *by* Sébastien Ars et al.

Anonymous Referee #2

Received and published: 9 February 2017

In Ars et al., the authors describe a new method for estimating gas emission rates from industrial facilities, by combining 1) tracer flux measurements, 2) Gaussian dispersion modelling and 3) a statistical inversion algorithm. The new method is evaluated using controlled methane/acetylene releases and compared to results from tracer flux. Four tracer placement scenarios are evaluated to demonstrate the improved accuracy of the method in situations where the tracer and the emission source are not perfectly collocated.

Printer-friendly version

Discussion paper

GENERAL COMMENTS

Overall, I was intrigued by the method presented by the authors, since accurately measuring industrial methane emissions remains a challenge, especially as we attempt to find convergence between bottom-up and top-down measurements. However, I am a little skeptical of value of the method in its current form and would like to see more discussion of the method's wider applicability before this paper is published in AMT.

Some areas of the method that I think warrant more discussion include the effect of different methane emission rates (only one was tested, $\sim$0.4 kg/h) and the role of meteorology – I am particularly concerned that the authors selectively looked at plumes from very specific atmospheric conditions. If this new method is being proposed as an "easy-to-implement" method for operators to employ (as it is described in the Introduction), then I would expect such a method to be robust to different atmospheric stabilities. The quality of the writing is excellent, but the authors would do well to streamline the paper so it is less bogged down in text.

I tend to agree with Reviewer #1 who described the writing as "verbose". This complex writing style makes it more challenging to follow the science. Additionally, I think the authors could limit some of the discussion of the methods, particularly the tracer release and Gaussian methods, as these are well-described in the literature, to make room for a more well-rounded discussion of the results, which seemed rushed. Upon making these major revisions, I expect the publication will be suitable for publication in AMT. Specific comments follow.

SPECIFIC COMMENTS

Section 1 – L105: Is this method easy to implement for operators? How have operators historically monitored their emissions? If the paper is framed as being in support of industry, then this should be discussed; I am not familiar with many facilities actively conducting tracer flux measurements or those with mobile laboratories to measure downwind emissions.

Section 2.1 – L175: I am not sure how you have demonstrated that your method provides satisfying results over those distances and methane emission rates compared to what you have tested with your controlled releases – please elaborate. I am especially interested in how tracer flux and this method differ for large methane emission rates or "superemitters".

Section 2.2 – L180: I suggest that the authors conduct a more thorough literature review, particularly of tracer release measurements conducted in various shale gas basins in the United States. Numerous papers have come out on this subject in the past 3 years.

Section 2.2 – L210-215: Can the authors speak to how this effect scales with methane emission rate? Does its significance shrink if total methane emissions increase? Or does its importance scale linearly?

Section 2.3 L230-235: Can you expand on this more in the text? I find the model justification to be a little lacking.

Section 2.3 L250-253: Is this detail on urban vs. rural configurations really necessary? Especially if you don't mention what configuration was used in this study.

Section 2.5 L305-310: I went looking for an explanation of how the spatial offset was treated in Section 3.2, but this section reference Section 2.5. Please make sure this concept is explained. I would strongly caution against routinely referencing other sections, particularly future sections, and instead focus on a linear narrative for the paper.

Section 3.1 L342-345: If the authors are going to be highly selective of meteorological conditions, then this should be discussed in more detail. What happens on days with low winds?

Section 3.2 L360-364: Here are some more cyclical references – I don't think the spatial offset is ever properly described.

Section 3.5 L430-435: Choosing stability class based on best fit to the measurements

Printer-friendly version

Discussion paper

seems suspect to me. How does this choice compare to the estimated stability class using wind speed and insolation metrics? Furthermore, what were the range of atmospheric stabilities during all your tests? Is this method applicable to all stability classes? This is a main point of concern for me and the authors should better justify their decision regarding the Briggs parameterization.

Section 3.5 L472-479: I could not follow this; can you explain how these uncertainties translate into those methane emission rates?

Section 4.2 L584-590: I am not convinced by the argument that the performance of the new model was the worst compared to the actual emission rates due to the low emission source – it seems to me all the other configurations used comparably low emission rates and this problem was not observed. Please provide a better explanation.

Section 4.2 L584-613: This is repetitive of table 2 and does not to be listed off in the text.

Section 4.3 L628-652: Again, this is repetitive, I would prefer to see more of an analysis vs. repeating of figures in a Table.

Section 4.3 L432: The authors explain the poor performance in configuration 1 does not matter very much due to the fact that the configuration is unrealistic. I am not satisfied with this explanation, if theory dictates that unreasonable or not that configuration 1 should be the ideal case then a good reason should be provided why it was not.

Section 5: Nowhere in the conclusions (or in the results) do I see any statements on the performance of this method vs tracer flux for a range of methane emissions. This was introduced in the introduction and I do not think it was adequately followed through on. If this method is currently limited to low industrial emission rates it should be expressly stated. As it stands, I think the usefulness of the method is overstated and the authors should be realistic about what their experiments have demonstrated.

Table 1: If I understand correctly, on the days where meteorological conditions were

explicitly controlled for, the plume capture rate is roughly 30%. This seems very low to me making me question the robustness of the method. Please comment.

TECHNICAL CORRECTIONS

Section 2.3, L219: Please define acronym "LES" and possibly "CFD", as I am unsure if everyone would know what these are.

Section 3.1 L340: Typographical error "serie"

Section 3.6: Edit section title to be more succinct.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-353, 2016.