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Atmospheric Measurement Techniques
Discussions

AMTD

Interactive comment

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 #1

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The authors propose a new method to estimate emission rates in industrial sites, combining the tracer release method, a Gaussian dispersion model and a statistical atmospheric inversion approach. The method is evaluated through field experiments and conclusions are drawn based on related results.

GENERAL COMMENTS. The idea of the authors is interesting, since it combines and merges different approaches. In their intention, this should bring benefit to the assessment and estimation of emission rates in complex industrial site. The scientific

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approach is honest, by detailing all possible problems, and rigorous, by trying to address them. Indeed, the method is comprehensive and at the same time complex itself. As the authors discuss thoroughly in several parts of the manuscript, there are many degrees of freedom, many sources of possible uncertainties and errors, many unavoidable approximations, which can affect the reliability of the approach. For these reasons, I have not been fully convinced of the feasibility and applicability of the method. The conclusions are drawn from enough rigorous experimental tests but in a limited number and for a single site. Thus, it is not straightforward to infer whether the method can be effectively generalized, leading to a final novel procedure for estimating even unknown emission rates from industrial sites.

I have some specific concerns on the use of a Gaussian model. Certainly, for such short distances it can be reasonably applicable, as the author discuss, and its simplicity allows better dealing with the complexity of the problem. However, choosing a Gaussian model is questionable, since it cannot capture and describe turbulent motions, thus missing the fine-scale structures that can affect the results and their analysis. It has to be considered that turbulence and stochastic motions may produce uncertainties, for which also the tracer release method can fail, since turbulence acts altering the plume spread and pollutant dispersion. I think these aspects need to be better addressed and discussed in the manuscript, even if already several comments are spread in the text of this version, but too sparsely.

Regarding the manuscript and its form, the text is very dense, with a huge amount of information and comments, and avoidable repetitions. In some parts it gets strenuous to keep trace of the work done until first results are presented and finally discussed. The authors made an effort to provide a good organization of the paper structure, yet I think that it needs improvement, optimizing the description, removing repetitions, avoiding verbosity. The content and the form of the manuscript somehow are paired, in that a long and detailed description of the method and of the work done (almost 11 pages) then flow into confined results and conclusions (about 3.5 pages, plus tables and figures). I think that the manuscript needs further revision before considering it for publication: more detailed comments are provided hereafter.

DETAILED COMMENTS.

* Introduction.

Page 2: why the locations of pollutant sources in industrial sites can be ‘not always precisely known’? Because of possible ‘fugitive emission’ or leakages, or missing information from the industries? It will not be a matter of ‘geo-localization’, nowadays.

Page 2: ‘local atmospheric dispersion models’ might be of various type, from simple parametric methods to Gaussian, Eulerian, Lagrangian models. Since the authors cite ‘models based on mass conservation’ they should better specify that they are here referring to ‘simple mathematical inversion’ methods. In fact, advanced dispersion models are able to account for ‘complex turbulence structures’.

Page 3: the authors state that the skill of statistical inversions approach strongly rely on the transport and source modelling. Then, a Gaussian model is used for the study. Gaussian models have strong limitations, especially when accuracy and turbulence structures are important factors, also given that they are designed for homogenous conditions. This is partly discussed later on, but some justification to support the use of a Gaussian models should be given here already.

* Section 2.2

Page 5: the authors explain their choice to minimize the impact of the differences between the targeted and released tracer plumes due to a not perfect collocation of the sources. Since turbulent motions can enhance the differences between the plumes further downwind the sources, is this the optimal choice for any distance from the emission points?

* Section 2.3

Page 5: I do not understand, and do not agree with the statement: “While LES and CFD models allow for turbulent patterns over such spatial scales to be generated and for changes in the terrain topography and for buildings to be accounted for (Letzel et al., 2008; Britter and Hanna, 2003), they can hardly be set-up or controlled to perfectly match the turbulent patterns at a given time and location downwind of a source.” What do the authors mean? Why these models can be hardly set-up and controlled? Due to their complexity? No model can ‘perfectly match’ the turbulent patterns, but advanced models are in principle the best option, in particular when LES approach is used. Placed this way, this statement sounds just like a weak justification to use a simple Gaussian model, which on its side has instead severe limitations, since stationary solutions for homogeneous conditions are indeed a strong approximation of real atmospheric processes. Surely, advanced models (maybe available even in the Polyphemus system?) need more established modelling expertise and large computational resources for their application. It would be worth to include some discussion about the expected limitation when applying the Gaussian model in this specific site, where obstacles and buildings affect the flow and dispersion.

Page 6: Please, better explain in the text why “Instead of being deposited, the emission plume rebounds when it reaches the ground” is a ‘decent’ approximation for the studied gases.

* Section 2.5

Page 7: the new method intend to overcome the issues associated with the individual usage of the different methods. Could the author foresee possible ‘new’ uncertainties and issues linked or due to the merging of three approaches? I mean, a sort of propagation of uncertainties, of error propagation? Given the discussion about the limitations of the single method, I wonder whether it is proper to consider ‘a priori’ that the information on the atmospheric transport from the tracer release method and the Gaussian model simulations can be defined as ‘very accurate information’. The final part of this subsection (“The statistics of the misfits...”) is rather verbose, a bit compromising its clarity. The multiple references to topics treated in next sections indicate possible repetitions. I suggest revising this part, optimizing the links with next ones, being more precise and less descriptive.

* Section 3.1

In which hours of the day were the measurements performed?

* Section 3.3

Page 8: please check the formalism accepted from the Journal for the units, if to use “l” for litre instead of “L”, seconds “s” according to the SI instead of minutes “min” and hours “h”.

* Section 3.5

Page 9: regarding the general applicability of the method, ‘specific wind conditions of each cross-sections’ to estimate the H observation operator are not commonly/routinely available, when not provided from experiments: could the authors comment on this aspect?

Page 10: some comments and interpretation about the best-fit obtained with stability class B (moderately unstable conditions) would be of interest: what were the atmospheric conditions during the experiment? Was B class effectively representative of them or the best agreement resulted ‘by chance’? Here the authors ‘admit’ the limitation of the Gaussian model in reasonably reproducing the observed motion when turbulence, low wind etc may occur. So the question comes: why using a type of model that may not fit the purpose of its use? Also: how the threshold of 70% relative error was chosen to remove the data? Saying that the empirical choice has been defined based on the dataset is not enough, what was the reasoning behind?

Again, the cross-references to previous section 2.5 and 2.4 suggest that this part of the text should be optimized and harmonized with the previous one to avoid repetitions. Also, the description of the choices for the variances set-up might be improved, making it less descriptive and clearer.

* Section 3.6

Minor: the title is rather long and descriptive, surely it is possible to shorten it. The authors may consider to combine this section with next section 4.1, since a few items are repeated and 3.6 is in fact functional to the results presented in 4.1.

* Section 4.1

See previous comment on section 3.6.

* Section 4.2

Page 13: it is redundant to repeat in the text the numbers already reported in the cited Table 2. Please revise this part to avoid such redundancy and repetition.

* Section 4.3

Same as for Section 4.2 about redundancy with Table 2. It would be worth to discuss more in depth why the tracer release method is better than the combined approach for the configuration 1. Because of fewer sources of uncertainties?

* Section 5.

In the results analysis and conclusions there is not a definitive and decisive proof that the combined method provides better and more reliable results than the tracer release method, given the limited number of cases and conditions considered, and the connected uncertainties. In addition, the potential ability of the method for multiple sources could not be fully addressed. The authors honestly recognize this and it becomes clear that these are 'preliminary' results and that more experiments are needed. Thus, the paper is mostly the presentation of a method but not a final test of it, supporting its adoption.

Figure 2: different colours (or line type) for the curves for different stabilities may better

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highlight the results.

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