

Interactive comment on “Methane emissions from a Californian landfill, determined from airborne remote sensing and in-situ measurements” by Sven Krautwurst et al.

Anonymous Referee #1

Received and published: 6 February 2017

Summary: Krautwurst et al. report on a measurement campaign conducted to quantify CO₂ and CH₄ emissions (and concentration patterns) within the LA Basin. Here, in-situ observations and remote sensing data from the MAMAP instrument and their ability to detect CH₄ plumes released from several landfills (and their associated emissions) is discussed. Repeat mass-balance experiments found emissions estimates ranging from 13ktCH₄-18.2 ktCH₄ with a reported uncertainty range of 17%-46%. While a comparison with another remote sensing instrument revealed qualitative similarities for the observed plume shape.

General comments: The study nicely illustrates a use-case for the MAMAP instrument and how the combined use of airborne remote-sensing and in-situ instruments can help

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to assess emissions from a landfill site. Unfortunately, the structure of the paper is confusing and should be revised to help the reader to focus on the key results (see general comments below). For example, the methodology of how uncertainties are calculated (e.g. for CH₄ emission rates) is given in the results section (6), while other results are given in the emission comparison section (7). I would suggest to include the comparison with the inventory in the results section. Furthermore, the uncertainty calculation should either be a specific section or logically added to section (5), where the MAMAP retrieval and calculation of emission rates are described. In the last paragraph of the conclusion, the authors claim that this study shows that this type of air-borne remote sensing observations are “well-suited” to estimate CH₄ emissions from a “large landfill”. Yet, the study showed that for 3 out of 4 landfills investigated, the instrument did NOT detect a significant plume. Here the authors need to critically discuss: Why were the other sites neglected in the analysis and conclusions? What were the EPA emission estimates for the other sites? Maybe there is a detection limit for this methodology or are there other limiting external factors (e.g. meteorology, topography of the site)? These would be crucial information to be added. All that can really be claimed seems to be that, in this instance, a landfill with emissions of above ca. 11ktCH₄/a can be monitored using this technique. Concerning the size of the landfill (“large landfill”) - I doubt that the size of the landfill is critical here, but rather its CH₄ emissions, or maybe its CH₄ emission density.

Overall, the study contains important data and very interesting results that could help the readers to understand GHG emissions at the scale of an industrial site and even better plan future campaigns by e.g. quantifying uncertainty contributions of individual parameters. It has also the potential to gauge the relative value/usefulness of in-situ versus remote-sensing observations in future studies. If the authors can appropriately address the major (and minor) comments this manuscript should be considered for AMT.

Specific comments: P2 Line 7: consider citing a peer-reviewed publication instead of a

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webpage here. <http://www.earth-syst-sci-data.net/8/697/2016/essd-8-697-2016.pdf>

P2 L30f: Please give an order of magnitude for the size. As a significant share of publications in AMT focus on regional to global scale studies, a landfill might not qualify as having a *large* surface area.

P4 L6f: A more diligent reason for why 3 out of 4 landfills are ignored in the manuscript from here on out needs to be given. Why do those other landfills not produce pronounced plumes? What are their (EPA) estimated emissions? Are they lower, equal or higher than the expected emissions at Olinda Alpha? Consider adding a table with the key indicators for all 4 sites.

P4 L9: Why are: “Campaign and target description”, “Aircraft instrumentation and collected data sets” and “Flight strategy” three different sections? Please consider combining them as subsections into one “methods” section.

P5 L12: Please correct CDRS to CRDS

P5 L14: What is the uncertainty of total column concentrations determined with the MAMAP instrument? There is a discussion of this in the “results” sections. Please consider moving this discussion into the method section of the paper.

P5 L25: What was the typical uncertainty (repeatability and reproducibility) of the in-situ measurements

P7 L10: Please consider adding the flight track of the second instrument hosting the AVIRIS-NG on Figure 2 if possible.

P9 L2f: The resulting uncertainty of choosing 0.31 as mean albedo could be discussed here. (Similar point for other assumptions/simplifications made throughout section 5).

P10 L3: The authors correctly note that the estimated emissions are only valid during the overflight, yet the units indicate an annual estimate. Suggestion: report the emissions as tCH₄ per hour and also calculate the mean hourly emission according to EPA.

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This avoids the erroneous implication that annual emission rates can be calculated from this data set.

P14f: After nicely describing all calculations in section 5, why are the uncertainties associated with these calculations now included in the “results” section and not within the previous section? Are the calculated uncertainties considered a key result of this study? If so, this needs to be stated more clearly.

P14/15/16: Here CH₄ emissions are reported without any uncertainties and the reader will have to “wait” for the next subsection to judge if emission of 13.0ktCH₄/a (27.8.2014) are significantly different from the reported 13.7 ktCH₄/a a day later – why?. Please consider restructuring to improve the readability of the manuscript

P18 L6: It seems that only a bias in the determined wind-direction has been accounted for here. What would be the impact of changing wind conditions between two legs of the down-wind legs?

P20 L19: Is subsection 6.1.2 really an independent “result” that needs a subsection or rather additional information about the data exploitation/error calculation?

Table 2: Why is the uncertainty of the albedo of the landfill not considered here?

P23 L25: Why/How was the pseudo-surface concentration enhancement range determined to be 50%-150% of the lowest flight track?

P24 L27: Why is the "comparison of emissions" not considered a result (sect. 6) or included in the conclusions (sect. 8), but discussed in an independent section?

P24 following: Section 7 is called "comparison of emissions" – yet subsection 7.2 compares CH₄ concentration results only.

P25 L16: Is the reported average absolute difference statistically significant?

P27/P28: Please consider adding a discussion on the implications of the ability of the suggested observational techniques given that only 1 out of 4 landfills could be studied

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within COMEX.

Especially - P28 L28f: Please expand on the claim that the observations are well suited to estimate CH₄ emissions from larger landfills (see general comments). The uncertainties of the in-situ estimates you report are smaller than the uncertainties for the remote-sensing estimates and you can only detect stable plumes for 1 out of 4 landfills.

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