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Atmospheric Measurement Techniques Discussions

AMTD

Interactive comment on "CH_4 emission estimates from an active landfill site inferred from a combined approach of CFD modelling and in situ FTIR measurements" *by* Hannah Sonderfeld et al.

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The authors propose a method to model CH_4 emissions based on downwind concentration measurements from a landfill site. The main focus is on the application on a short field campaign of a few days.

Major points

1. The authors propose a method to model CH_4 emissions based on downwind concentration measurements from a landfill site. Since the paper focuses strongly on the

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Discussion paper

landfill site aspect, similar methods from other types of CH_4 sources are not included in the comparison. In principle each existing method could be considered a new method if applied to a different source than before, and all adaptations necessary to do so suggest it is a new method. However, for the reader it would be beneficial to get a better hierarchical overview over the general type of a method (independent of instruments used and specific tracers employed) and what is new/different/improved over existing methods. For example, there is a paper by Yver-Kwok et al. (2015, doi:10.5194/amt-8-2853-2015) that uses similar instrumentation but a different source (waste water treatment) but not in combination with modeling. And then there are methods strongly used for estimating NH_3 sources using downwind concentration measurements in a similar way, but maybe not specifically for CH_4 and using different instrumentation (e.g., Bell et al. 2016, doi:10.5194/amt-2016-350). It would thus really be desirable to get a broader overview over these methods and how the new proposed method differs from existing methods.

2. The use of the open source OpenFOAM software platform (I did not know this but it seems to be a good open source alternative to Comsomol) is interesting and thus making model code associated with this paper available to others would be a real benefit. This would in fact be the best option to increase reproducibility of the study. With the brief information about the model setup I would not be able to set up OpenFOAM in a way that corresponds to what the authors did.

3. Table 2: I do not really understand the percentage (with one decimal!) of the uncertainty: if a flux is 0.99 $\pm$ 0.39 and $\pm$ 0.39 denotes the standard deviation, then the 95% confidence interval is 1.96$\times$0.39 or 0.76, thus the uncertainty of the flux is 0.76/0.99 or 77% (not 44.4–44.9%). If I correctly understood your precentages are assuming a 40% uncertainty of the model and thus you somehow put 4.4–4.9% on top, but I cannot follow here.

4. The inclusion of a secondary source area without additional measurements rises the question whether the difference between CFD model and measurements is not simply an artefact of the turbulence parametrisation in OpenFOAM. According to Fig. 5a the domain of the model is only 1.2×0.7 km^2 (approx.) and thus turbulent mixing (at least the large eddy mixing) is most likely pure parametrisation, not a model result. At least turbulence cannot equilibrate with the roughness of the topography in such a small domain. I think alternative explanations besides the hypothesized existance of a second source should be mentioned in the manuscript. It appears that Section 3.4 is rather speculative, and the comparison between model and measurements shown in Fig. 9 do not suggest that this secondary source solved the discrepancy between model and measurements.

5. Unfortunately the comparison between model and measurements is limited by the narrow wind direction sector available for the comparison. This strongly suggests that measuring concentration with a mobile setup to fully cover the plume (as e.g. in Herndon et al. 2005, doi:10.1039/b500411j) would have substantial benefits even in this application. (basically, I do not fully agree with your take-home message on page 23, lines 1–3).

In general the study is nicely carried out and the language of the text is of high quality, thus my critique really addresses more the aspect of novelty of the method (for a methods-centered journal, to be clear) in comparison to similar approaches that may not have been used explicitly for land fill sites yet. The empirical part quantifying the fluxes looks OK, although I was not quite clear whether I understood your approach to uncertainty estimations.

My recommendation: major revisions

Details

p2/l20: use minus sign in –0.00154
p2/l20: use USA for country specification

p3/l5: then $\rightarrow$ than “wider area than”
Fig. 3 (and elsewhere): use scientific/ISO8601 date and time notation (21:00 not 9 pm; 06:00 not 6 am); rather use the term “panels” for the two components of the “graph”
Table 1: “slope of the correlation”: a correlation has no slope, you mean “slope of the regression”
Fig. 4: “CH_4 distribution” is misleading, you show ΔCH_4 – please adjust the wording.
Eq. (1): I find the multiplier (10^6 ppm) confusing. I think it is correct to leave that away and know that such a ratio is easier to report in percent, permil, ppm or whatever (this is not a unit conversion it is only a way how to express ratios)

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