We would like to thank the referee for the valuable comments and their time to review the manuscript. While the referee comments were kept in black, the author comments are in blue. The italic font indicates where changes are made.

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 CH4 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 CH4 emissions based on downwind concentration measurements from a landfill site. Since the paper focuses strongly on the landfill site aspect, similar methods from other types of CH4 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 Δdifferent source (waste water treatment) but not in combination with modeling. And then there are methods strongly used for estimating NH3 sources using downwind concentration measurements in a similar way, but maybe not specifically for CH4 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.

The introduction was extended to give a broader overview over similar approaches for emission estimates using dispersion models and also covering other gases of interest.

Especially the use of dispersion modelling with respect to other target compounds and other sources (e.g. Bell et al. 2016) and the deployment of an FTIR in another set up (Yver-Kwok et al. 2015) has been mentioned. Additionally, another mobile sensor platform (UAS) has been included (Allen et al. 2014, 2016). It is emphasized that still a great effort is put into establishing the most appropriate sampling approach and measurement techniques for site-wide flux measurements of landfills.

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.

One of the main advantages of OpenFOAM is the open source availability, which makes it attractive to use. One of the downside is that it requires specific computational skill (linux based platform coded in C++).

Unfortunately, the authors were not in a position to release the code used in this study. However, we agree that the OpenFOAM description section was too limited to allow other users to reproduce this work.

Therefore, a detailed section on model parameterisation was added in the manuscript (see section 2.4.2 Numerical settings).

3. Table 2: I do not really understand the percentage (with one decimal!) of the uncertainty: if a flux is 0.99 ± 0.39 and ± 0.39 denotes the standard deviation, then the 95% confidence interval is 1.96×0.39 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.

We agree that the origin of the uncertainty in Table 2 was lacking explanation. The calculation of the uncertainty is based on the standard deviation of the averaged methane mole fraction of each bin (as shown in Fig. 6), not on the standard deviation of the calculated fluxes.

The paragraph in section 3.3 (p13, ll13-16) was revised and the way that the uncertainty was calculated described more clearly. The caption of Table 2 was changed.

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 km2 (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.

Chamber measurements (results not shown) on the side area between the active site and the ridge detected additional, irregular methane emissions. This area was initially not taken into account, because we were focussing on the active site, but was considered as the secondary source area.

The turbulent parameterisation of OpenFOAM made use of a standard turbulent dispersion (Sct = 0.7, see section 2.4.2). This parameter can be changed in order to control the amount of turbulent mixing (lower Sct leads to greater turbulent mixing and vice-versa). Lowering the Sct would increase the width of the Gaussian shape in Fig. 5b, however the strength in concentrations would decrease as well. The authors were therefore keen to keep a standard dispersion setting as the CFD model was initially evaluated using very similar parameterisation (see Jeanjean et al. 2015; Jeanjean et al. 2017).

The boundary conditions were setup using the terrain roughness, hence the wind entering the computational domain is already accounting for roughness turbulent mixing. Another reason for the discrepancy between model and measurements would likely be the time averaged assumption used (here 3 minutes aggregated wind and concentrations data). During the aggregated time period, wind speeds and directions will fluctuate, emissions from the landfill are likely to oscillate as well, which could explain the difference found. This is addressed by using at least 5 3-min data points per bin for calculation of the fluxes.

A paragraph was added at the end of section 3.4 to address this point.

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).

The referee is right pointing out, that having only one sampling point is a drawback in terms of sampling plume coverage. This is a limitation of this method as sampling is dependent on the right wind direction. A mobile application would allow for a broader sampling of the plume like it is done in tracer release experiments. A disadvantage there is the requirement of moving the sensor around, which does not allow for longer term/continuous sampling. For future applications one could make use of the multiple inlets of the FTIR by spreading sampling lines along the cross section of the plume or even at different heights and sampling in turns from there. Data from the CFD could be extracted to match the different sampling spots.

The CFD modelling is not a suitable tool to describe the concentrations on a high temporal resolution. For a good representation of the distribution of emissions it needs a few minutes of integration time.

Some text on plume coverage has been included in the summary and conclusions. P 23, lines 1-3 have been removed and the position of the sensor is mentioned further up in this section in the context of the definition of the source area.

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
Done.
p2/l20: use USA for country specification
Changed.
p3/l5: then ! than “wider area than”
Corrected.
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”
Changed.
Table 1: “slope of the correlation”: a correlation has no slope, you mean “slope of the regression”
Improved.
Fig. 4: “CH4 distribution” is misleading, you show ΔCH4 – please adjust the wording.
Changed in the figure caption and title of section.
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)
Removed.