

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 “CH₄ 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.

Anonymous Referee #2

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GENERAL COMMENTS

The authors propose a study to infer methane emissions from sub-areas of a landfill site by using a computational fluid dynamics model, and they present a short field campaign as a dataset for the model validation.

The paper is well written, the introduction addresses the background issue satisfactorily, but the paper objectives (not the project ones) could be better clarified; similarly, the novel character of the work presented does not get through within the paper.

My main concern with this paper is that the observations presented are very few: the data are generally well described, but I think they are not sufficient for a full characterisation and validation of the proposed model: for example, there is no data for describing meteorology-driven variations, and so on. However, this may not necessarily be the main focus of this paper, and besides, the presented work is of clear interest to the scientific community.

We agree that longer term measurements would need to be carried out to cover a wider range in meteorological conditions and to build up a larger data base for a full validation of the model. In the context of this field campaign, it was not possible to extend the measurement period. Therefore the focus was rather a feasibility study for the proposed method.

Measurements over three sampling days in the year can be considered a spot-measure, useful to verify rather than characterise an emission source: chemical reactions in the substrate and subsequent emissions can be driven by changing atmospheric pressure and temperature, for example, not only by the daily development of turbulence.

The study presented here rather has a focus on the method combining CFD with in situ measurements to derive fluxes, than to assess the whole landfill emissions from that site. The referee is right, that parameters like pressure and temperature can have an effect on landfill emissions. For that the measurements would need to be run longer or at different times of the year for short periods. But it was shown that this would potentially be possible with this approach.

The summary was extended to include discussion of the measurement period.

I see the presented work as mainly a modelling work: I think more emphasis should be put on the main advantages of the CFD models compared for example to backward Lagrangian models. I think this issue is touched upon in the abstract, but not in the conclusion, where it could be expanded.

To address Referee #2's concerns, paragraphs were added in the introduction and section 2.4 to discuss the differences/advantages of using CFD models against other dispersion models. A note was also added to the conclusions.

Also, the usage of LIDAR data and people surveying the site could be expanded in the method section (or in the conclusion).

The description of the LIDAR data collection deserved to be more detailed, section 2.4.1 was added therefore in the manuscript and people acknowledged for carrying out the surveying work.

From what emerges from the results, the model seems fit for representing emissions in conditions of well-developed turbulence regime: however common this could be, it is a big limitation, and should be addressed in the conclusions, perhaps including criteria for good functioning of the model vs bad.

That's correct, the model ideal conditions needs to be emphasized in the manuscript. The model best performs for wind speeds greater than 2 m s^{-1} and stable wind conditions. On the contrary, unsteady wind and low wind speeds are the worst conditions.

The authors decided to add a new section (2.4.3) to describe the model limitations.

Overall, I think this work is well done and useful, but I recommend major revisions to be made.

SPECIFIC COMMENTS:

The description of the experimental site and of the FTIR and CFD methods, including the setup, is clear and well detailed; I think adding information on the dump age (the different sections of it) would be a benefit. The section with the background measurements would benefit from a better explanation on how the measurements were used, or explain it better in the results section.

P6 L6: wind is not a fluid, air is.

Changed to air.

P7 L10-15. I agree with the authors that the emissions from the hot spot areas are not representative of the landfill site, however I believe omitting those measurements does introduce a bias as well, in that they will be taken into account as much lower emission areas. For a model validation they may not be suitable, but under an observational point of view they should not be ignored. Perhaps you could expand on this point.

P7 L15 – P8 L3: has been rephrased to point out that these hotspot emissions contribute to the total emissions of the landfill and are taken into account with the secondary source area (section 3.4), but that the measurements in close proximity to them were not suitable for a separate flux estimation approach of the active site with the CFD model.

P8 L10-13. I think that here it is not clear why you need an enhancement factor rather than an emission ratio: what are the advantages of the technique you're using? Adding explanations would help the reader in understanding the value of your work.

The enhancement factor should be equal to the emission ratio as long as there are no additional sources or dilution of the plume during transport to the FTIR. The term enhancement factor is used to emphasise that we did not measure directly at the source.

P8 L10-15. It is not clear here when you did use the background measurements and when not: is it only for CO₂? Is it only for some calculations? Explain better.

Background values were not available for CO₂ for the whole period. That's why we chose to determine the enhancement factor from the slope of the regression of CH₄ to CO₂. So, no background measurements were used for determination of the enhancement factor.

P8, L10-16 were rephrased.

P8 L20. "wind field" instead of "wind". *Changed.*

P8 L28-30. Can you really conclude this from your data? Perhaps change the wording highlighting this is a possible interpretation.

"These ratios are still representative of waste degradation under aerobic conditions, but show a higher CH₄ content compared to the EF observed at the portakabin."

Changed to

"Compared to the EF observed at the portakabin they show a higher CH₄ content, but can still be interpreted as being representative of waste degradation under mainly aerobic conditions."

P10 Table 1. The slope is an outcome of a regression, not a correlation.

Corrected.

P11 L8. Substitute "emissions" with "emitted gases". *Changed.*

The section on "methane distribution" is not very conclusive: what is the message here?

The section is supposed to give an overview of the methane enhancement (after subtracting the background) observed at the portakabin depending on the wind direction. The reader gets familiarised with this kind of representation of the data, which is further on used when the fluxes are calculated. Additionally, it shows that the observed enhanced methane comes only from the direction of the landfill site and that highest concentrations correspond to low wind speeds.

This section has been slightly reorganised to make these points more clear.

P12 L9. Molar mass density, not mass concentration.

In this context, either mass concentration or density can be used (<https://doi.org/10.1351/goldbook.M03713>). As it refers to the methane concentration, which is then converted to the mole fraction, we decided to change the symbol back to C. This is also used in the newly added Eq. (4), section 2.4.2.

P13 L17. Delete "are" after fluxes. *Done.*

P13 L20. Any suggestions on what these extra sources could be?

Hotspots along the side area between the slope and the active site have been observed, but were initially not taken into account, because we focussed on emissions from the active site. The sentence was rephrased, "unknown source" was not the appropriate expression here.

P13 L27. Why do you think night fluxes should be higher? How is the production (emission of methane) connected to day/night pattern?

During daytime new waste is deposited on the active site and vehicles drive there and shift waste around. This mixes fresh air into the top layer and could lead to increased oxidation, while at night methane production could be favoured.

Decrease in temperatures over night could also result in higher methane emissions, when methane oxidising bacteria are less active. A small inverse temperature relationship was found by Riddick et al. 2016 for this landfill site.

Same CFD run used for day and night, which was optimised for daytime conditions. Change in atmospheric stability and turbulence could lead to artefacts in the results. From Antoine's experience the model results don't change much for night, unless the boundary layer is very low, e.g. in winter.

More data would be needed to investigate the effects on the nighttime emissions.

A paragraph was added for discussion in section 3.3.

P13 L31-32. It is good to show all data for completeness, but it would be very useful to have possible explanations for peculiar data, or just further discussion.

A paragraph was added to the manuscript in section 3.3.

P18 Figure8. Specify that concentration refer to the portakabin location as well.

Done.

P18 L14-18 This really would be sorted with a longer period of measurements. . .

Yes. A comment was added at the end of the paragraph.

P20 L5-10. This is not fully clear: are you suggesting that a larger area is wrong for there are e.g. roads in between, or non-emitting areas that are considered emitting?

What would the suggestion be, if this is the case, and spell it out.

Estimating the actual emitting area at the landfill is quite difficult as the terrain is very heterogeneous. Our focus was on the open active site as the main emission area, while Riddick et al. 2016 took a more generous approach by including the surrounding area. As is described in the manuscript, the surrounding area also contributes to the overall emission, but has a lower emission strength.

An explanation was added to section 3.5.

P20 L17-19. Specify the meteorological measurements are easy and can be maintained over long periods.

Done.

P20 L20. "stable" has a definite meaning when talking about atmospheric processes (I refer to stability parameters), and I am not sure you mean this here.

Changed to: "Consistent fluxes from the active site were found for three different days with southerly winds transporting air from the source area towards the portakabin"

P21 L1-3. This last sentence seems to be there without having any evidence to support it.

These lines have been removed. The position of the instrument is now discussed a bit higher up in the summary.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-382, 2017.