Response to reviewers (egusphere-2024-3577)

Dear authors,

The reviewers found the manuscript to be much improved from the previous version, but they have a few remaining points that they insist to be further looked at.

One point is the albedo correction proposed by Foote et al. (2020), which you argued is less relevant for a higher spectral resolution instrument like MethaneAIR, but the reviewer argues that it is nevertheless necessary. I invite you to have a closer look at this point.

The other reviewer would like to see, for completeness and transparency, also the results from the 29 October release experiment, even if conditions were less favourable on that day.

The final critical point concerns the implications of the lower detection limit offered by the matched filter algorithms on the total point source emissions.

For more details, please have a look at the reviewer's comments.

Best regards
Dominik Brunner

Dear Editor,

Please, find enclosed our point-by-point responses to the reviewer’s comments. The comments from the reviewers are in black, and our responses are in blue.

Based on the reviewers' comments, the main changes to the manuscript after this revision are:

- Figure 8 has been extended by 2 more panels in order to accommodate the results from the 29 October controlled release campaign, as requested by Reviewer 1.
- A discussion of the impact of plume detection limits on total point source quantifications has been added, as suggested by Reviewer 1.
- An analysis of the effect the albedo correction suggested by Reviewer 2 has been carried out. Some results are shown in this response letter, and a discussion of the convenience of this correction for the processing of MethaneAIR data has been added to the manuscript.

We hope that the current version of the manuscript satisfies the expectations of the Editor and the reviewers.

Sincerely,

Luis Guanter, on behalf of the authors

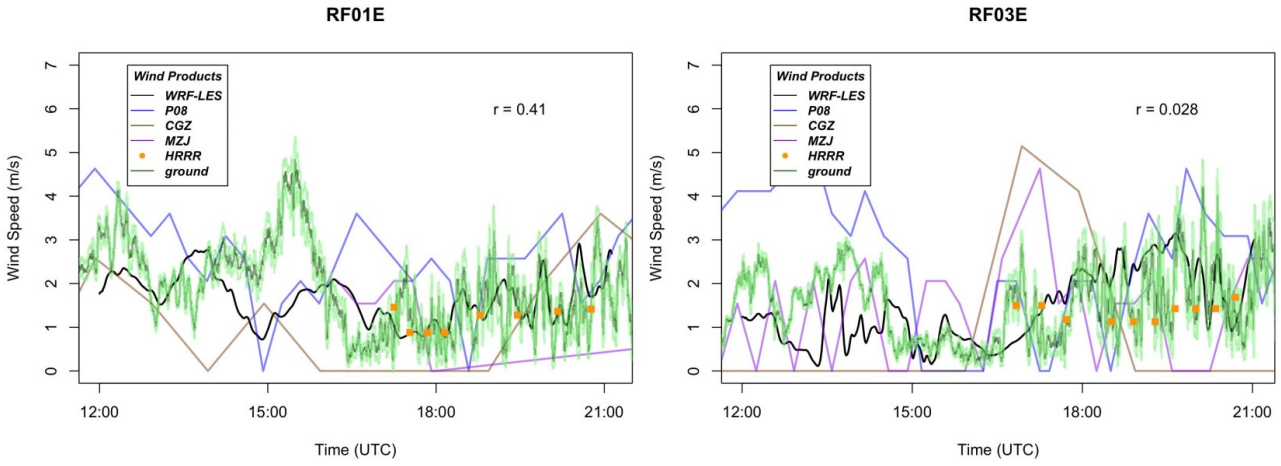
Report 1

I appreciate the authors' responses to my comments and feel the manuscript has greatly improved. I also appreciate the inclusion of the data with the SI. I still have a few lingering comments before I can recommend for publication.

1. Responding to my original comment #3 - in an effort to be transparent, it is still important to show the controlled release results from 29 October. What do you mean by more stable winds? How are you sure that the conditions in the Permian were of sufficient stability to quantify emissions? It's useful to see controlled release results in a less ideal environment because it gives a general sense of how accuracy depends on environmental conditions out of one's control.

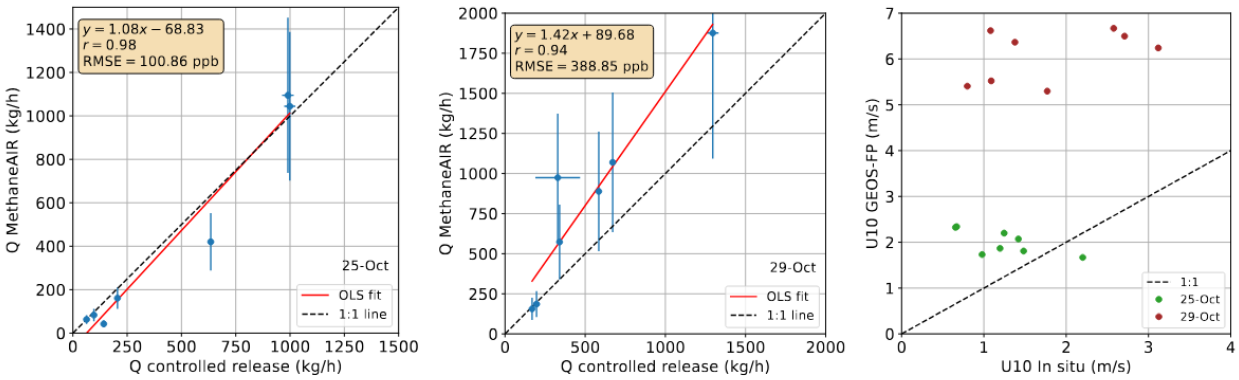
We thank the reviewer for emphasizing the importance of transparency regarding the 29 October controlled release (RF03E).

By "more stable winds," we refer to the fact that wind conditions on 25 October (RF01E) were more consistent and better aligned between different observational sources and model output. To support this, we compared WRF-LES simulations (driven by HRRR), ASOS observations at nearby airports (P08, CGZ, MZJ), HRRR inputs to the inversion, and ground-based measurements at the release site. We analyzed the period from 16:30 to 21:00 UTC and found that the WRF-LES winds and ground-based winds were significantly more correlated on 25 October (r = 0.41) than on 29 October (r = 0.028). Visual inspection confirmed that 29 October featured more variable and less coherent wind fields. This situation is summarized in these plots:



In any case, in order to satisfy the reviewer's concern, we have now included the results from the 29-October campaign, which has implied to download and processing of a substantial volume of L1B data. The results are shown in the new Fig. 8 (reproduced below), which adds the scatter plot between estimated and in situ flux rates for 29 October, and a comparison of the GEOS-FP and in situ winds for the two dates. We find a bias of about 40% for the 29-Oct data set, which can be largely explain by the substantial overestimation of wind speed by GEOS-FP with respect to the in situ measurements.

This is discussed in the text as "*However, we find an important overestimation of about 40\% in the MethaneAIR flux rate estimates from 29 October, which we attribute to the large overestimation of wind speed that we find in GEOS-FP with respect to the metered wind speeds for that date (Fig.\,\ ref{fig:cr_scatter}). This bad performance of GEOS-FP winds for 29 October is consistent with the poor performance of other wind sources and WRF-LES simulations to reproduce in situ winds for that date.*"



2. Regarding my original comment #5 on point vs area sources. Appreciate the response, and I suppose it could be argued that it is out of scope to talk area fluxes in this manuscript, though I generally disagree. That said, putting area fluxes aside, I do believe the newly proposed text is misleading. A central focus of this manuscript is that by application of a matched filter algorithm, the effective detection limit of MethaneAir has reduced. However, in the authors' response, they filter out all the new plumes they detected with this new algorithm for their comparison with the CO2_proxy algorithm - i.e., after filtering out plumes > 200 kg/h, then summing their plume dataset, they get a similar emission total. This greatly undermines what makes this study so compelling!

What I would like to see is one additional sentence or two where the authors describe that when summing all plumes detected from the CO2 proxy approach, they get 26.7 t/h, but when they sum all plumes detected with the matched filter approach, they get 35.9 t/h (I'm just summing the provided RF06 list of detections). This means that there are 9.2 t/h more emissions attributed to point sources that is purely the result of an algorithm update.

This is a fundamentally important point as observing systems are not just the result of instrument specifications but are a result of the algorithms applied to them, and for point source detection, the plume detection protocols as well. And depending on algorithm, you may come to slightly different conclusions about an area/process/etc. The authors should do more highlight this point, in my opinion.

We appreciate this point by the reviewer. The following discussion has been added to the manuscript: “*On the other hand, when using all the detected plumes in our quantification of total emissions (i.e. without filtering out plumes $<$200\,kg/h), we obtain an increase of the total emission estimate of about 9 t/h (RF06) and 6 t/h (MX025) with respect to the Level-4 product. This result confirms the sensitivity of total emission estimates from single plumes to the detection limits offered by the instrument and the processing chain, and suggests that smaller plumes contribute substantially to the totals despite the typical heavy-tailed distribution of point sources \citep[e.g.][] {Cusworth_pnas_2022}*”.

Report 2

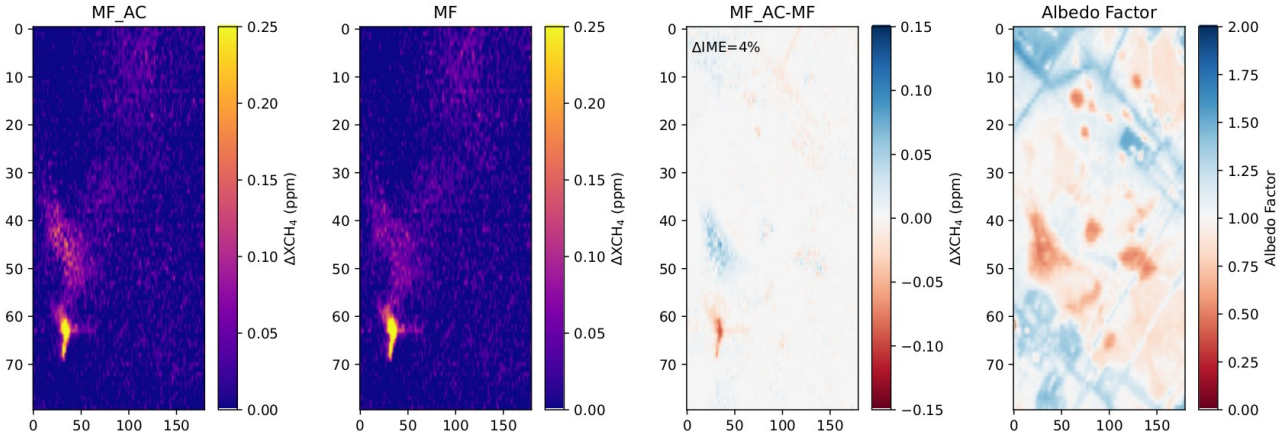
The authors have addressed most of the reviewers' comments. However, I am not fully convinced by the answers regarding the albedo correction and therefore suggest a minor revision addressing this.

The correction should be necessary because the target spectrum t (=mu k) is calculated from the unit absorption spectrum (k) and the background spectrum (L0), i.e. the radiance spectrum without CH4 enhancement (alpha = 0), which is only approximated by the column-wise mean spectrum (mu). Since the true background spectrum is higher/lower for higher/lower true albedo, CH4 enhancements can be under- or overestimated. Therefore, it is necessary to apply the albedo correction.

Since the correction term will be largest for low/high methane enhancements, i.e. XCH4 > 1000 ppb as seen in the plumes, I would not expect to see the effect in Figure 3, which shows the variability of the background. Even if the effect is small, due to the high spectral resolution of MethaneAir or due to the low variability of the albedo in the flight lines, I think it is absolutely necessary to apply the albedo correction to show its impact on the CH4 enhancements in the plumes and the resulting impact on emission quantification.

Thanks to the reviewer for this point. After a careful evaluation and some tests with real data, we now agree with the reviewer that the albedo correction proposed by Foote et al. would indeed be beneficial for the processing of MethaneAIR data as well. We are including one example below these lines, showing a change of ~4% in the largest plume of Fig. 1. On a plume by plume basis, this number will depend on the similarity between the surface under a given plume and the average spectral albedo of the column, so this effect would manifest itself as an increase in the variance of the Q estimates, rather than as a general offset of the distribution. For this reason, we have opted for including this correction in our processing chain, but not to reprocess the whole MethaneAIR data set presented in this manuscript. This would require an enormous amount of time without a significant impact on the conclusions of the study.

This issue is now discussed in the text as *“We acknowledge that this per-column $\vec{\mu}$ formulation neglects the impact of difference between each pixel’s spectral albedo and $\vec{\mu}$. This issue may be alleviated by the albedo correction proposed by \citet{foote}, which adds an ``albedo factor'' to Eq.\,\ref{eq:mf_retr} in order to quantify the difference between $\vec{\mu}$ and $\vec{x}$ for each pixel. The magnitude of this correction will depend on the spatial heterogeneity of the scene. Preliminary tests show that this correction can modify the single \dx\ retrievals by up to 10\% in the Permian (results not shown). However, the sign of the correction can be either positive or negative depending on the albedo of the surface (or surfaces) crossed by a particular plume. For this reason, we can expect that an uncorrected albedo effect may lead to an increase in the scatter of the estimated flux rates within a distribution, but not to a change of the total or the average flux rate of the distribution. In any case, we will implement this correction in future version of the retrieval.”*


MF_AC
MF
MF_AC-MF
ΔIME=4%
Albedo Factor
ΔXCH4 (ppm)
Albedo Factor