Response to referee

The manuscript "Towards accurate methane point-source quantification from high-resolution 2D plume imagery" by S. Jongaramrungruang et al. introduces a procedure to quantify the methane flux of a point source from a high resolution 2D imagery of the plume. Large Eddy Simulations are used to deduce the method. The flux inversion is described in detail and an error estimate for the method is given. The procedure is then applied to one case of a controlled release experiment, where it could reproduce the flux rate within the assumed error estimate.

The method seems useful, especially as it does not need the wind speed as an addi-tional input variable. All required values are only extracted from the 2D scene of the(vertically integrated) plume. This makes this method useful for optical measurements,and also for future satellite missions aiming at high resolution methane retrievals.

The method is novel and clearly outlined, the paper fits well into the scope of AMT. The manuscript is well structured, however, sometimes (long) sentence structures made it hard for me to follow. It would be nice, if the authors invest some time for rephrasing,giving the reader a more fluent reading experience.

The authors should use SI units in the preferred inverse notation throughout the manuscript (as stated in AMT manuscript guidelines), several times ppm-m is used (in the text and figure labels), which may be ppm m in SI units (?).

For better understanding, the authors should avoid synonyms (e.g. synthetic measure-ments vs. pseudo measurement vs. synthetic observation).

Generally, many of the figure legends, axis labels, and other labels might be to small for a good reproduction in the final publication.

Please, do not forget to add the necessary sections: "Data and code availability","Author contributions", "Conflict of interest".

I recommend publication in AMT, subject to some improvements.

We thank the reviewer for the constructive comments and appreciate the thoughtful review. Please find a point by point response below:

Specific comments

p. 3, ll. 6ff.:The inclusion of retrieval noise in the simulation of synthetic measure-ments is mentioned, but I have not found anything about this topic in Sec. 4.1 or Sec.5, which describe more details. Is additional noise used (and if, which) in preparing the synthetic measurements? Is this affecting the estimated errors (and how)? Maybe more details can be included in Section 4.2.

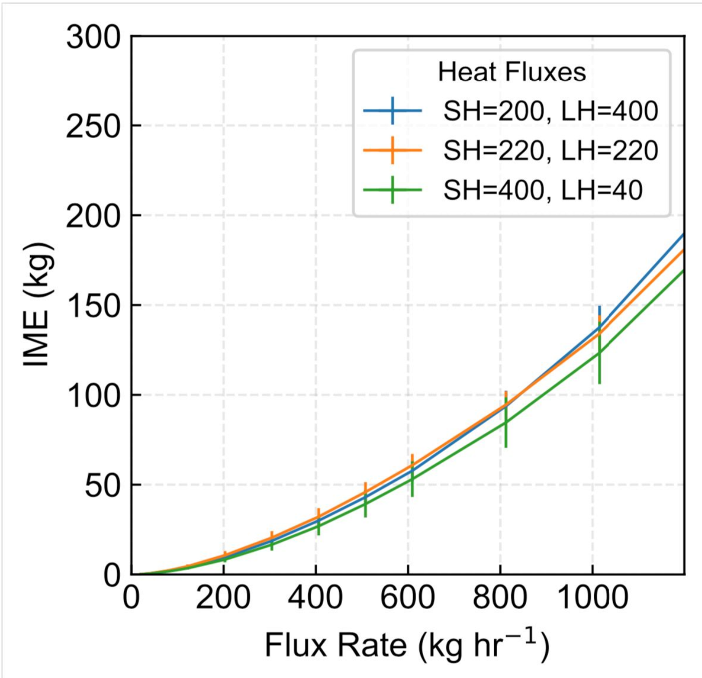
We performed experiments to add noise to the scenes using random Gaussian noise and found that the IME only changes by 7%, well within the 1-SD error from snapshot average. So we did not include the retrieval noise in our figures (but will consider this in the future when we will focus more on real data for which proper treatment of noise and plume segmentation will become increasingly important).

p. 3, l. 26: A 15 minute revisit time is mentioned, sub-figures of Fig. 1 show instrument overpasses in time intervals of 7 to 9 minutes (as also stated in the figure caption).
The revisit time is indeed 7-9 minutes apart. We have corrected the details in the figure caption to match with this.

p. 6: I recommend to restructure Section 4, as Section 4 itself is empty. It could rather be: "4 Large Eddy Simulation" and "5 Synthetic measurement", following Sections change accordingly. The section on synthetic measurement could be extended by some details on the additional noise added. Maybe the applied detection thresholds could also be included here, instead of in Section 2.
Based on the reviewer's suggestion, we adjusted the structure of the paper accordingly, and changed the number in the texts that mention to these sections.

p.6, l. 14: In this study latent and sensible surface heat fluxes are kept constant. Was the method tested with other settings (except of the controlled release exper-iment)? What would happen if these fluxes are varied? How would the plume be affected? Would this impact the method? If the method is still applicable with the derived correlations, would the error estimates change?Some of these questions are answered in "Discussion and conclusion" and answers may not fit in "LES setup". The authors should consider a reference to the discussion section and a strengthening of the corresponding paragraph there, or an additional section on limitations.

These values are based on typical field campaign data in the Four Corners area. To show how the result from our method applies to the field of different conditions, we have added additional LES runs with different combination of sensible and latent heat fluxes (SH and LH) in two more cases: SH = LH, LH > SH to compared with the typical condition we use in this paper (SH > LH)



We found that the relationship between observed IME, flux rate and wind speed under new conditions (orange and blue lines) lie within 1-SD error from our original condition (green line). This shows that the uncertainties associated with the change in these conditions will not significantly impacted our method and are captured well with the range of errors we have analysed.

We added a small section to show the relatively small impact in the discussion.

p. 8, ll. 4ff.:If I understand it right, the instantaneous value for U-10 is written out from the LES simulation every minute. When the plume structure is – as stated in line 7 – influenced by the wind during this minute, the LES model (integration)time-step should be smaller. The model time-step should be somewhere around few seconds, taking into account the high resolution of 5 m. However, I found no value for the model time-step, maybe you could include it in the Section about the LESsetup. Maybe a rephrasing of the sentence in ll. 4f. could help to understand that instantaneous wind values are written out from the model simulation. Also, using the term "timestep" for one instance of the model output may be a bit misleading, as output does not coincide with every model time-step, maybe you could use "snapshot".

You are right, the model time-step is much shorter, namely one second. We added “The model computational time-step is one second.” to the LES Set Up section. We paraphrased and adjusted the world “time-step” to “snapshots” accordingly.

p. 9, ll. 17ff.:For the method to work, has the plume origin to be known for the angular mass binning? For LES simulation and field campaigns this should be no problem (when measuring unknown flux rates from a known point source). Also, for flux inversions of known sources from future high resolution satellite imagery, e.g, foremission monitoring, the method will be useful. In other situations, it might be more challenging. Limitations of the method should be addressed in the discussion section,or an additional section on limitations. (e.g., more than one point source or parts of a second plume included in a scene ...)

For large enough plumes, the plume origin can be easily found from the imagery itself. In fact, we will work on image classification schemes to automate this procedure in the future. However, in the current study we don't consider the plume origin to be a major factor. For HyTES data it might be as the origin cannot be readily inferred but with total column data in the short-wave infrared, the error in the origin is rather small.

p. 10, ll. 16f.:The paragraph about error estimates could be strengthened. Please do not just give the value of$\chi 2$ without interpreting this result. Maybe additional description how the "average percentage error" is calculated, and how the error propagates through all steps of the method could be added.

We clarified that the average percentage error is the average of the percentage differences (in absolute terms) between the predicted value and the actual value for single point source predictions. It is approximately 30%. The reduced chi-2 value is 3.84 which suggests that the error variance may tend to be slightly underestimated for this individual point source prediction. However, when calculating the aggregate prediction, the predicted value is different to the actual value within only about less than 10%. We added additional information on this section in the paper. We have added additional explanations for how the errors propagation is calculated from Figure 6, 10 and 11.

p.10, ll. 26ff.:Additional information about the controlled release experiment would be nice. Maybe, include a figure of the scene, or a figure of the angular distribution (comparable to Fig. 8)...

We are adding the scene from the controlled release experiment and the angular distribution accordingly.

p. 12, ll. 3ff.:This paragraph addresses the limitations arising from using constant sensible and latent surface heat fluxes. The authors should consider to present the limitations in a separate section. The last sentence of the paragraph should be moved closer to the discussion of the constant surface fluxes. All limitations should be named and discussed.

We added more analysis for the runs with two additional cases. One being the sensible heat flux (SH) and latent heat flux (LH) are equal to 220 W/m2. Another case is when SH is 200 and LH is 400 W/m2. This is two different cases in contrast to the typical condition of SH being higher than LH (400 W/m2 and 40 W/ms respectively in our method.) Our result shows that the relationship between IME, flux rate and wind speed is not significantly impacted by the large change in heat fluxes, as described in the response to p.6, l. 14 above.

p. 20, Fig. 8:The figure contains two black lines, which are not explained, I as-sume they denote something like the main axis of the plume.If the plume is already rotated, why is the maximum of the normalized angular IME distribution not at 0?

The two black lines denote an angular bin of 0.5 degree that sweeps through the 2D plume to construct the angular distribution. It is just for illustrative purposes. We have added the meaning of the two black lines to the caption of Figure 8. The plume is rotated based on an axis that is defined by an angle that separates plumes into 50/50 ratio, which can be slightly different from the angle of maximum density.

Technical corrections

We have adjusted our text according to the suggested technical corrections.