General comments

The publication "Towards accurate methane point-source quantification from high-resolution 2D plume imagery" by Jongaramrungrang et al. deals with the important aspect of inverting atmospheric concentration gradients to fluxes or emission rates of not well-constraint CH4 point sources. In order to invert these observations, wind information in the measured area is needed and equally important as the concentration measurements themselves. However, as outlined in the introduction of the manuscript, acquiring wind information, preferably, simultaneously and at an adequate spatial and temporal resolution can be a challenging task. Therefore, the authors propose a new method, which solely relies on spatially resolved imaging data from airborne remote sensing instruments and high resolution large eddy simulations (LES) to estimate the prevailing wind speed during the time of the overflight. The wind speed is then derived from the shape of the observed plume and used during the inversion process to compute a flux of the investigated sources.
The descripted method is a novel and promising approach to quantify CH4 point source emissions from aircraft but also potentially from space without having to rely on real wind measurements. The manuscript fits well in the scope of AMT and I recommend publication after some modifications along the line of the comments below.

In general, the manuscript is well written. The method is described in a comprehensible way, however, some additional information concerning the figures would improve their readability (see also specific comments). Additionally, the authors should add some more information regarding the stated errors and their propagation to the predicted fluxes (see also specific comments).
In my opinion, the manuscript could be strengthened by adding more extensive comparisons and applications to real data. So far, most parts of the approach were developed based on (theoretical) model (LES) studies, whereas only few real observations were used to support the novel approach. Therefore, I would recommend to expand on the already given examples in the manuscript: (1) controlled release experiment and (2) analyses of overflights shown in Figure 1 (see also specific comments).

Specific comments

P2, L12: Consider to also add publications regarding the HyTES instrument (e.g., Hulley et al., 2016) or other imaging instruments, which also have CH4 point sources successfully detected, e.g., MAKO (Tratt et al., 2014).

P2, L24f: I would suggest to either only focus on remote sensing studies (by satellite and aircraft) and remove Conley et al., 2016, or to better distinguish between remote sensing and in-situ studies. If in-situ studies are included I would also recommend to add, e.g., Cambaliza et al., 2015, Gordon et al., 2015, and Lavoie et al., 2015., which have also performed extensive analyses regarding airborne in-situ observations and resulting fluxes.

P3, L6: "… under various background wind speeds and **surface heat fluxes**.": Later in the manuscript (P6, L14), it is stated that only one value for the heat fluxes was used ("The surface sensible and latent heat fluxes are 400 and 40 W/m².").

P3, L11: "… and presence of ancillary information on the **actual wind speed (Section 5.3)**.": Based on this statement, I would expect to see a comparison between the derived wind speed based on the new method and actual wind observations or reanalysis data in Section 5.3., however, I was not able to find a paragraph referring to actual wind speed observations or reanalysis data.

P4, L1: Why not also showing a real example observation of HyTES. Maybe, there is one available for the same scene as shown in Figure 1. If this is the wrong place, because in Figure 1, the authors intend to show the high variability of the plume structure during multiple overflights, I would recommend, if available, to add a comparison of an AVIRIS-NG and HyTES observation after Figure 5, where simulated plumes observed by the two instruments are compared.

P4, L1ff: This would also be a good place to add some more information regarding the applied retrieval algorithms. As Frankenberg et al., 2016, are already cited multiple times and the plume shown in Figure 1 (bottom right) appears to be similar to the one in Figure S1 in Frankenberg et al., 2016, I assume that either the matched filter technique or the IMAP-DOAS method, described in that publication, has been used to retrieve the columns shown in Figure 1 or the ones from the controlled release experiment at the end of Section 5.3 used for a flux inversion.
Similar for the HyTES algorithm if real observations are added.

P4, L26: What is the basis of the chosen threshold of 500 ppm-m? As stated in (P3, L30), it is connected to the measurement precision of the AVIRIS-NG instrument and is thus a property of the instrument (Why does it then differ from the value of 200 ppm-m applied in Frankenberg et al., 2016, Section 'IME' in 'Materials and Methods'?).
Furthermore, what is the reasoning in using the same threshold also for HyTES simulations, e.g., as shown in Figure 5?

P5, L5: Please add some explanation for the 'sum' and the parameter 'm' in Equation 2 to the text.

P6, L10-15: What is a reasonable range of the proposed values (initial inversion height, potential temperature, specific humidity, surface sensible and latent heat flux) for initializing the LES model? Are these educated guesses or are they based on actual field or reanalysis data?
Given these ranges, the authors could also verify their assumption "… our method … should not be significantly impacted …" in the conclusions (P12, L6) by performing various LES runs using different initial values. I would agree with the authors that the column-integrated enhancements are not significantly influenced by the surface heat fluxes if the threshold were 0 ppm-m. However, I am curious if and how Figure 6 and 7 might change under various initial conditions, or whether they are well within the error bars.

P8, L27: Do the authors only mean Frankenberg et al., 2016, by "… has been ignored in previous studies." or are there further ones?

P9, L33: Could the authors add the fitted polynomial also to Figure 10? Why do the authors use a fifth-degree polynomial? Is there a physical relationship, which relates wind speed and plume angular width by a fifth-degree polynomial or is it just the 'best' fit?
Could the relationship in Figure 10 also be explained by an exponential curve? Assumption: The relation in Figure 10 is determined by averaging over an ensemble of LES realizations. If this is done, I expect the resulting plume to be approximately Gaussian as seen in Figure 4, e-f.

P10, L5-13: Basically, in this paragraph the authors summarize their developed method. I have some questions/comments to the used example.
a) The error bars (shaded area) shown in Figure 11: Are they related to the errors shown in Figure 6 or to Figure 10?
b) Have the flux rates, shown in Figure 11, been corrected for the missing IME (as indicated in Figure 7)?
c) How is the error (1-SD of the plume angular width) shown in Figure 10 related/translated to the wind speed error, which then linearly propagates to the estimated flux?

P10, L14-17: Could the authors be more precise in terms of the given "average percentage error" of 30%? I assume it is the mean value of the vertical error bars shown in Figure 12. However, as in reality not only entire fields/regions of CH_4 sources (as then investigated in P10, L18-25) are investigated but also single plumes, which are typically observed only once, an interesting measure would also be the average of the absolute differences (also in percent) of the predicted flux rates and the corresponding prescribed flux rates. This would give an idea of the magnitude of the bias one can expect from the method. I assume that the observed bias in the flux for the controlled release experiment of ~32% lies within this computed theoretical value.

P10, L25: What do the authors exactly mean by "mean percentage of error"? Is that the average of all differences between actual and predicted flux OR is it a similar error as computed for Figure 12 in P10, L14-17? If the authors refer to the latter one, I would suggest to also compute the average of all differences between actual and predicted flux (not the average of the absolute differences as in the previous comment). For example, a positive or negative value would then quantify an over- or underestimation caused by the method on average. The same exercise can be done for Figure 12 because it appears (as for Figure 13) that more predicted fluxes lie above the 1-to-1 line then below especially for larger fluxes.

P10, L26ff: The possibility to compare the novel approach, which is mostly based on 'theoretical' models, to real data is a huge strength of the publication. Therefore, I would recommend to expand this part of the publication. Some starting points are already given.
First of all, the authors could add some more information regarding the already analyzed **controlled release experiment** allowing for a better judgement by the reader. Useful information would be **(a)** a figure showing the overflight and the retrieved plume and CH_4 column enhancements (similar to Figure 1), **(b)** the fitted wind speed, which is then used to invert the IME to a flux, and **(c)** as the observation is based on a controlled release experiment, do the authors have access to real wind observations on-site or at least to meteorological reanalysis data, which can then be compared to the fitted wind to test its plausibility?
Additionally, the authors nicely show multiple overflights of one source within ~25 minutes by the AVIRIS-NG instrument in **Figure 1**. It would be an interesting opportunity to apply the developed method to the four overflights shown in that figure and discuss the resulting fitted winds and inverted fluxes.

P10, L29: I assume the given estimated emission of 118 kg/hr is already corrected by the potentially missed IME as indicated in Figure 7, right? Additionally, could the authors elaborate on what error sources are included in the error estimate (of 30 kg/hr) of the predict flux.

P15, Figure 2: Please clarify whether altitude on z-axis is given in meters above sea level or above ground level. Additionally, please harmonize the minimum altitudes of the computed averaging kernels, either to 0 m or to a specific surface elevation. Consider also adding the aircraft altitude(s) which the examples CAKs are valid / have been computed fo.

P16, Figure 3: Consider adding the true IME (idealized threshold of 0 ppm-m) to the caption. Additionally, consider adding labels for the stability classes to the caption so that the reader sees immediately their meaning without looking up the relevant information in the cited publications, e.g., A = very unstable; B = moderately unstable; ...

P17, Figure 4: Consider adding the true IME (idealized threshold of 0 ppm-m) to the caption (as suggested for Figure 3), and the variance of IME (of the 60 individual snapshots) to each plot in the right column for the three cases of wind speed so that the reader can assess the statement from (P7, L10f).

P18, Figure 5: Consider adding the true IME (idealized threshold of 0 ppm-m) to the caption (as suggested for Figure 3)

Figure 3-5: For clarification: The wind shown in Figure 3 (Gaussian plume model) is not directly comparable to the wind shown in Figure 4 and 5 because the latter one is the geostrophic wind, whereas the former one is the wind at plume level(s), correct?

P19, Figure 6: Which threshold was used for Figure 6, 500 ppm-m? Consider adding this information to the caption.

P20, Figure 9: Please add meaning of vertical bars to caption.

P21, Figure 10: Please add meaning of shaded area also to caption.

P21, Figure 11: Please add meaning of shaded area also to caption.

P22, Figure 13: Consider using a density plot for better visualization of the data cloud.

Technical corrections

P1, L18: "… Large Eddy Simulation …" → "… Large Eddy Simulations"

P1, L29: "… large geographical area …" → "… large geographical areas …"

P2, L13: "… at a resolution of 3-m …" → "… at a resolution of $3x3m^2$ …" or "… at a resolution of 3 m …"

P2, L20: "… the retrievals measure the fine …" → "… the instrument observes the fine …"

P3, L26: "… approximately 15 minutes revisit time." → "… approximately 10 minutes revisit time." (compare to Figure 1)

P5, L27: "… the plumes structure." → "… the plume's structure."

P6, L3: LES is already defined in (P3, L5)

P10, L11: "… 1-SD error bars in the plot." → "… 1-SD error bars are shown in the plot."

P10, L22: "… large enough represent …" → "… large enough to represent …"

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