Response to Interactive comment on "Systematic detection of local CH_4 emissions anomalies combining satellite measurements and high-resolution forecasts" from Anonymous Referee #1

This manuscript introduces a method to sytematically detect local CH_4 anomalies by combining total column CH_4 satellite observations from TROPOMI with high resolution CH_4 forecasts produced by CAMS. The manuscript is well written but it is at the edge of the scope of ACP, because it rather demonstrates the theoretical potential of the introduced technical method than having general implications for atmospheric science as it is not analysing how well the method can be applied globally (see general comments). However, as the editor has suggested to stay in ACP (instead of moving to AMT), I recommend publication after the following comments have been addressed.

General Comments

My main criticism of this manuscript (in the sense of a publication in ACP) is its demonstration character with a lack of general implications for atmospheric science. The advantage is that the method can in principle be applied globally. However, it is not clear if the majority of the detected global anomaly candidates are due to actual unreported or over-reported sources or due to local systematic retrieval biases (e.g. as a consequence of small-scale albedo variations). The presented analysis of the method is limited to a few local case studies (e.g. confirmation of known underreported sources) and does not include an evaluation of the global capabilities to distinguish between missed sources and retrieval biases. Although this would be sufficient for an AMT publication, the global detection statistics and the impact of retrieval biases should be investigated further if possible to better fit the scope of ACP (see also specific comments).

We firstly thank the reviewer with the review that led to significantly improve the paper. We agree with the reviewer and the editor that the article is at the edge of the ACP scope. The goal of the article is to present the capability we newly developed using part the ECMWF IFS data assimilation system to perform this automatic detection so we were not sure if this would fit the AMT journal scope as well. We are happy to have the article moved to AMT if the editor and the reviewer think that this is more appropriate.
We showcase few cases studies (now 5) to show the potential of such methods but also highlight the short comings. Going into details on more case studies to provide a global analysis would significantly lengthen and change the paper. We have however added one more case study to strengthen the point on local persistent biases in response to one specific comment below (see section 4.3 and figure 14).
Significant restructuration of the paper would be needed with providing a global evaluation of the capabilities and this would require diagnosing the system for an extended time period (probably several years) to provide a robust analysis. This will be done in further steps as we plan to have this system running routinely soon. But this cannot be done within the time frame of this present review. We do however have changed figures to include the global detection results for few months (now in figure 9) with numbers to show how the system is performing globally. We also now mention this point in the conclusion motivating for a further global analysis once the data set will be available.

Specific Comments

Page 2, Lines 56-59: There are also other relevant studies, e.g. Zhang et al. (2020) or Schneising et al. (2020).

We have added the references to the text.

Page 4, Lines 115-117: Is it advisable for the presented method to use a model in which satellite data have already been assimilated (IASI and TANSO)? The assimilated satellite data may already correct for under- or overestimations in the emission data bases to some extent and thus complicate the interpretation.

Our current data assimilation technique does not correct the emissions but only the concentrations. We clarify the text to make this point clearer.

Page 5, Line 142: Why is Tours visible in Figure 5? What is the origin?

We have added a brief explanation in the text.

Page 6, Lines 161-163: Please give a reference for the TROPOMI averaging kernel function as a function of pressure.

We have now added the reference in the text.

Equation 6: What does the lp stand for?

We subscript lp here was wrong as it should stand for high pass. We changed the subscript to hp and clarified the text accordingly.

Page 8, Line 235: Should "positive" be "negative"?

This has been corrected.

Page 9, Lines 252-254: You have identified two candidates, which have not been investigated or documented yet: southern Nevada and northern Baja California. You conclude that the latter may be due to local albedo properties as you were not able to identify a responsible facility. What about the former (Nevada)? Is there are a source or local albedo variations?

This is likely the case as well for the enhancement seen at the Nevada-Arizona border. We clarified the text accordingly.

Page 10, Line 284-287: "Consistent shapes over months" sounds longer than it actually is. Figure 12 only spans a time period of 6 weeks. The red Turkmenistan features in Figure 10 look similarly consistent over comparable periods. This exemplarily illustrates the difficulty of distinguishing between underreported sources and retrieval biases. Concerning persistent shapes over time, please also discuss the potential impact of temporally and spatially variable small-scale albedo features (e.g. due to snow).

We now have improved the Turkmenistan figure to a more zoomed version to show that the shape of the feature is not consistent over months like in the Siberian case. Regarding the Siberian case we can only show it over a reduced span (figure 13 spans over more than 10 weeks as the window length is 30 days i.e. around 4 weeks) as the measurements are only available for a short summer period over these latitudes. To make our point stronger and clearer we have identified an additional case over Australia illustrated with figure 14 were four 30-day windows are displayed over 4 months. We also address the potential impact of temporally and spatially variable small-scale albedo features (e.g. due to snow). We clarified the text accordingly.

Page 10, Lines 293-295: What is praised as an advantage here (does not only allow for the detection of anomalies but also has the potential of detecting local retrieval errors) is also the main problem: It is not clear if it is possible to reliably distinguish between the two cases in general and there is no global analysis performed to try to approach the answer quantitatively.

The response to the comment above partially answers this comment. We do agree with the reviewer of the current limitation and that this issue deserves further works as already mentioned at the end of section 4.3. We now have clarified the text accordingly.

Figure 7: Please specify the time period. Is it 2019-07?

This has been clarified.

Figure 8: Please highlight the plot associated to the final choice of parameters (30 days, 2_) and describe in the caption.

This has been clarified.

Technical Corrections

Please replace CH4 by CH_4 in all instances.

Fixed

Page 7, Line 193: Please delete "and": "... where d_m is the average departure ..."

Fixed

Page 10, Line 315: Please delete "to": "... emission events could occur ..."

Fixed

Caption of Figure 4: Is "next fluxes" correct or should it be "net fluxes"?

Fixed