

Interactive comment on “Satellite-derived methane hotspot emission estimates using a fast data-driven method” by Michael Buchwitz et al.

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We thank the referee for carefully reading our manuscript and for providing a critical review. Below we are giving our point-by-point answers to each of the referee's comments and concerns. Addressing these comments, concerns and questions helped us to prepare a significantly improved version of our manuscript.

Q1: Referee: Larger concerns: Methodological concerns: The authors need to explicitly state the necessary conditions for their approach to produce robust emissions estimates. What is the size of the region, size of xch4 signal, isolation from other sources, meteorological conditions, and emissions magnitude are necessary for the approach works?

Author's reply: In the revised version of our manuscript we present additional investiga-

tions concerning the performance of our method. These investigations are based on a simulated high-resolution methane data set. Furthermore, we now better highlight already in the abstract limitations of our method. We show that our method typically provides a conservative estimate of the emissions, i.e., our emissions are typically underestimated. We better explain why our method tends to underestimate emissions. The large uncertainty of our method is reflected in quite large uncertainty estimates which are typically on the order of 100% for the source regions discussed in our manuscript. Our fast and simple method has been developed to obtain a reasonable estimate of the annual methane emission of a region which shows elevated methane relative to its surrounding region in maps of annually averaged satellite-derived XCH4. When applying our method to multiple years of satellite data, the results will show, if elevated atmospheric methane is present in all years or not. If the methane is elevated in all years than this is very likely due to an “underlying” methane emission source (assuming that the satellite data do not have a “local bias”). We would not apply our method to situations, where this is not the case (although our method can be applied also to methane fields which are spatially constant/flat but in this case our method will deliver an estimated emission of zero together with a large error bar). There are no limitations w.r.t. the size of the region (as size is (approximately) considered by parameter L) or the size of the XCH4 signal (as explained above) or the magnitude of the emission (which is unknown as the satellite only provides XCH4). As shown in our manuscript we have not identified any conditions, where the method is shown to fail entirely but we recommend to be careful if the targeted source region is known to exhibit “pooling overnight” (more details on this aspect are given below) and/or for regions with complex topography (where, for example, methane can accumulate in valleys; these are however situations where all inversion methods will likely have severe difficulty). Our method assumes that the emission sources are homogeneously distributed in the targeted source region. We show in our manuscript that the estimated emissions are underestimated if this is not the case. Underestimation also results from sources located in the surrounding region. From all this we conclude that our method can be

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applied to all situations but that typically the emission will be underestimated, i.e., our estimates are conservative estimates. If the resulting emission is unexpectedly high, then this is a strong indication that the true emission is in fact higher than expected. In this case we recommend additional investigations, e.g., using a much more advanced (and computationally much more expensive) model than our simple mass balance approach (as explained in our paper).

Q2: Referee: The method the authors employ is essentially a very simple mass balance approach where the elevated methane levels are attributed to a necessary flux assuming a constant wind speed (ventilation time). (note supplemental figure A1 is actually very helpful in explaining the method and should really be in the main text). However, I was quite surprised that the author's determined one single wind speed for use around the globe in this technique. In essence, this states the size of the XCH4 enhancement seen in any hotspot is driven entirely by emissions, as wind speed is taken as globally constant. This would require significant justification, as we know this is not the case, and in particular, we know the manifestation of 'hot-spot' signal is often a consequence of meteorological conditions as well as emissions. For example, the Four Corners region discussed in the manuscript is known to exhibit pooling overnight, and part of what a midday satellite observations sees such an elevated signal is this meteorological dynamic (which is why in analyses such as the Kort et al., 2014 paper the winds are explicitly modeled). A region like North Dakota (discussed later), would have much higher wind speeds, and thus low XCH4 enhancements would actually be linked with higher emissions. There is much more justification needed to justify a single wind speed for all regions, as this would be expected to produce answers that are strongly biased at each individual region.

Author's reply: We are not assuming a constant wind speed. Wind speed is a parameter of our inversion model. However, we show that the consideration of (regionally varying) annual mean wind speed (as obtained from meteorological data) does not help to reduce systematic errors of our annual emissions as obtained from annually

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averaged XCH4 (which is the goal of our method). We therefore use a constant wind speed but this is not because we assume this but because this results from our analysis, which shows that the use of spatially resolved annual mean wind speed (from meteorological data) does not help to improve our method. In the revised paper we present more details on this.

We removed Appendix A and present figure A1 now directly in our methods section.

“Pooling overnight” is in fact a concern for our method as this could result in a significant overestimation of the estimated emissions, which is what we aim to avoid as this would result in “false alarm” in comparison to emission inventories. For Four Corners we have no indication for overestimation of the Four Corners emissions as estimated with our method. In the revised version of our manuscript we investigated this using high-resolution methane simulations and found underestimation in line with the general characteristics of our method, which tends to underestimate emissions. We also present new results for several other regions (incl. California) and never found significant overestimation.

Q3: Referee: The comparison with the global model at 6x4 does not really provide a satisfactory answer as to why one wind speed would be appropriate – this analysis would suggest that integrating globally using one wind speed does not produce a biased estimated, but for individual regions (the whole point of the analysis) there can and will be large bias errors. Furthermore, calibrating with a model that is at 6x4 degrees would then restrict the conclusion to analyses that are of the same resolution, as wind speeds in this type of box model setup will be rather different at a 6x4 degree region compared to a 1 or 1/2 degree region.

Author's reply: In the revised version of our manuscript we address this aspect by presenting additional results based on high resolution (< 1 deg) methane simulations.

Q4: Referee: How can you justify applying the analysis on such different spatial scales – small in CA and Four Corners and large areas in Turkmenistan and Azerbaijan? All

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of which are different scales than the 4x6 degree model used for calibration?

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Author's reply: In the revised version of our manuscript we address this aspect by presenting additional results based on high resolution (< 1 deg) methane simulations applied to small regions such as Four Corners and large (country-scale) regions such as large parts of California.

Q5: Referee: Why are these four regions chosen only? There should be some discussion of what selection bias may be present and the reasoning behind the choice.

Author's reply: We selected these four regions because they show up as regions of elevated methane in the satellite data products (e.g., our Fig. 1) and because they are extensively discussed in the peer-reviewed literature (Central Valley, CA, and Four Corners) or other data sets exist which can be used for comparison (e.g., EDGAR for Turkmenistan and Azerbaijan). Initially our main motivation to develop our method was to at least roughly estimate Turkmenistan's emissions as this country prominently shows up as a region of elevated methane in the satellite data. We also studied Azerbaijan because it is located close to Turkmenistan primarily to see how the estimated emissions of these two countries compare (as typically relative accuracy is better than absolute accuracy).

Q6: Referee: Why have the author's ignored two other regions in the US which they have published on previously (Schneising et al., 2014 for North Dakota and Texas)? It is true the recent publication by Peischl et al., 2016 JGR collected aircraft data in North Dakota and showed the Schneising 2014 paper was physically inconsistent with the atmospheric observations and emissions estimates (and that it is implausible that emissions all of a sudden declined in the face of increasing production between the Schneising and Peischl studies) but the authors here do not acknowledge that in citing the Schneising paper. One would suspect the discrepancy is because the Schneising paper relied on data from SCIAMACHY post-2009, which the author's have deemed not robust in this analysis. Given that this paper is discussing methane hotspots and

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an approach for quantifying a region and cites the Schneising paper, the North Dakota and Texas regions analyzed and published on previously by this group need to be addressed.

Author's reply: As explained in our manuscript our method has been developed to obtain emission estimates for regions where satellite XCH4 is clearly elevated compared to their surrounding areas. This condition is not met for the areas studied in Schneising et al., 2014 (see their Fig. 3). Furthermore, Schneising et al., 2014, used a method to minimize the potential impact of systematic errors of the used satellite product in later years by analysing differences of the satellite product between two 3-year time periods including years we are not analysing in our manuscript for reasons explained in our manuscript. We have therefore not ignored the two areas studied in Schneising et al., 2014, but we do not study them here because our method is not optimized to deal with them, in contrast to the method of Schneising et al., 2014.

We may misunderstand you but it appears that your comment suggests that it is "true" that the Schneising et al., 2014, results are "physically inconsistent" with other published observations. We do not agree with this as these other observations have not been made during the time period analysed by Schneising et al., 2014, but later and because such a statement needs to consider the uncertainty estimates as reported in Schneising et al., 2014. The uncertainty estimates as reported in Schneising et al., 2014, are large (nearly 70% 1-sigma) and statements w.r.t. consistency or inconsistency need to consider this. If one would do that one would find out that there is no inconsistency at a 5% (or even much higher) significance level. Concerning "that it is implausible that emissions all of a sudden declined" please see Schwietzke et al., Nature, 2016, showing that leakage rates of fugitive emissions decline with time.

Reference: Schwietzke et al., Upward revision of global fossil fuel methane emissions based on isotope database, Nature, Vol. 539, 88-91, doi:10.1038/nature19797, 2016.

Q7: Referee: What emission model underlies the model runs used for simulation? Is

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that also EDGAR?

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Author's reply: Several emission data bases are used as input as explained in Bergamaschi et al., 2009, but the anthropogenic a priori emissions are based on EDGAR. These emissions are however not used directly for the data set we used as this data set is based on forward modelling using optimize (a posteriori) emissions.

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Q8: Referee: Representation problems: The abstract reads as if the paper provides a satellite estimate for emission in different regions that are statistically significantly different from best-estimate inventories for different regions (for example lines 26-27 about the central valley in CA). This is actually quite misleading. This oversells the utility and robustness of the conclusions compared to the rather heavily caveat-ed discussion in the main text.

Author's reply: For the revised version of our manuscript we have modified the abstract to also highlight the limitations of our method. We tried to eliminated all potential misunderstandings and clearly do not want to oversell our method and results.

Q9: Referee: Firstly, the authors imply through much of the text the uncertainty in their approach is often 100% or greater, and this is neglected in the abstract. Secondly, the central valley CA result is much larger than EDGAR, but is rather close to the best estimates made in the literature from both other top-down studies, but also from other bottom-up inventories specifically made for California! The authors cite and acknowledge this in the main text, but the abstract sensationalizes a 6-9x discrepancy with EDGAR, which is known to fail at these spatial scales and really does not mean reported or inventoried emissions are too low. In general, comparisons with EDGAR are fine to do, but should not be overemphasized as being thought of as an accurate representation of emissions on small spatial scales (or representative of government reported inventories on this scale).

Author's reply: For the revised version of our manuscript we have modified the abstract to also highlight the limitations of our method, e.g., by explicitly stating that uncertainty

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is often around 100%.

Q10: Referee: Also, what is the overall utility of this method?

Author's reply: The overall utility of our method lies in the fact that it provides at least rough estimates of emissions of source regions from large amounts of satellite data. As explained in our paper, we recommend further studies using more complex (and therefore computationally much more expensive) methods in case our method indicates significantly higher emissions compared to emission inventories. We write in our "Summary and conclusions" section: "More detailed assessments likely require the use of much more complex approaches compared to the simple method used in this study. Nevertheless, simple and fast approaches also have a role to play as they permit to perform quick assessments on possible discrepancies with respect to emission inventories or other data sets and can also be used for plausibility checks for more complex approaches".

Q11: Referee: Where around the world can it be used?

Author's reply: We have not identified any region where it cannot be used but please see also our detailed response to your first concern Q1.

Q12: Referee: Which regions satisfy the criterion for usage (and what is the criteria)?

Author's reply: We have not identified any region where our method cannot be used but please see also our response to your earlier questions but in particular our detailed response to your first concern Q1.

Q13: Referee: What percentage of emissions can be tracked or observed this way?
Need to see these numbers to understand the utility and impact of the approach.

Author's reply: This question is difficult to answer but in general (as shown in more detail in the revised version of our manuscript) the local or regional emission sources must be quite strong, on the order of several 100 ktCH₄/yr. As also shown in the revised version of our manuscript there are many of these source regions in the USA

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and therefore likely also in many other parts of the world. However, we cannot give a reliable number in terms of percent of total methane emissions at this stage as this answer also depends on the spatial distribution of the sources (as our method requires relatively well isolated sources).

Q14: Referee: Page 1 line 26-30: these concluding sentences in the abstract are misleading and overstated as discussed above.

Author's reply: We have modified the abstract to highlight also limitations of our method and we explicitly mention that our uncertainty is on the order of 100%.

Q15: Referee: Page 2 line 24-26: This is where the question of selection bias and why these regions comes into play.

Author's reply: Please see our answers as given above on these aspects.

Q16: Referee: Page 7: This would be where defining the location requirements (ie XCH₄ signal, size of area, wind speeds, emissions rate) would be valuable

Author's reply: Please see our answers as given above on these aspects.

Q17: Referee: Page 7 line 22: This is where the single wind speed is defined – see above for the concerns related to this approach.

Author's reply: Please see our answers as given above on these aspects.

Q18: Referee: Page 8 Line 8-9: This claim is really not robust. My assessment of these tests suggest that integrating globally the single, constant wind speed does not lead to a (large) bias, but for individual regions it will be strongly biased and this must be addressed and fixed. Author's reply: Please see our answers as given above on these aspects and please note that in the revised version of the manuscript we present additional investigations using high-resolution methane simulations and we apply our method to these simulations to obtain a better understanding of the performance of our method.

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Q19: Referee: Page 10 Line 17-18: Agreement here does not indicate the approach is sound and robust and therefore can safely be applied. There could easily be errors that cancel and lead to a coincidental agreement, or it could be that this one region is particularly good for this method.

Author's reply: Yes, it is true that this could be a coincidental agreement. Therefore, we added for the revised version of the manuscript additional investigations using high-resolution methane simulations and we also added Four Corners to this extended assessment.

Q20: Referee: Page 10 Line 23-26: This type of comparison is misleading – the under representation of EDGAR on this small regions is well known and defined previously, and emphasizing this gives an inaccurate impression that these high emissions are not accounted for properly in inventories (on this spatial scale EDGAR does not agree or match even the US inventory).

Author's reply: EDGAR is an important, frequently used and carefully constructed data base (which does not mean that EDGAR is perfect) and we have not found statements in the peer-reviewed literature that EDGAR is inaccurate and therefore should not be used for applications like this.

Q21: Referee: Page 12 line 29: This is a prime example of why the assumed constant velocity globally is of concern. Four Corners experiences even more pooling of emissions than the Central Valley, yet that isn't discussed. This problem or wind speed representation gives great concern to this approach.

Author's reply: Please see our response as given above.

Q22: Referee: Page 12: Far to much discussion and emphasis on the comparison to EDGAR for the central valley. The emissions being higher there than in EDGAR is well understood and documented from top-down and bottom-up emissions estimates in citations referenced, and is more an illustration of the failure of EDGAR on small,

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sub-national scales.

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Author's reply: Please see our response w.r.t. EDGAR as given above.

Q23: Referee: Page 13 Line 21-22: If this is not a well-defined emission hotspot, why focus this study on this region?

Author's reply: Please see our response w.r.t. Turkmenistan as given above.

Q24: Referee: Page 14 Line 28: typo, "toinvestigate" Author's reply: Many thanks. This has been corrected.

Q25: Referee: Page 15 Line 7-9: This type of statement about concern about errors/problems in the approach needs to be addressed more explicitly in the abstract, and also should be addressed more quantitatively in sections such as this in the manuscript – what are the possible magnitudes of bias errors?

Author's reply: For the revised version this comment has been considered by modifying the abstract and by providing additional investigations using high-resolution methane simulations and more detailed discussion at several places.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, doi:10.5194/acp-2016-755, 2016.

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