Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-755-RC1, 2016

Atmospheric Chemistry and Physics Discussions
Open Access
EGU

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

Anonymous Referee #1

Received and published: 26 September 2016

Overview: Buchwitz et al. present a manuscript describing a simple, fast method for estimating methane emissions from intensely emitting regions using annually averaged satellite observations of XCH4. They apply the method considering both SCIAMACHY and GOSAT methane data, and focus on four distinct regions, two in the US (Four Corners and CA central valley), as well as Turkmenistan and Azerbaijan. They compare results with the EDGAR inventory and other independent estimates, and discuss the possible utility of this 'hot-spot' tool with future space-based observations. The manuscript is appropriately place in ACP, writing is generally clear, the topic is interesting and the work is worthwhile. I do have some larger concerns with the work however, both in the appropriateness/robustness of the method (in particular the usage of one representative ventilation speed globally) and the representation of the findings (outlined below). Once the authors have satisfactorily addressed these concerns I would

Printer-friendly version

Discussion paper

reconsider the manuscript for publication in ACP.

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?

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.

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 $\frac{1}{2}$ degree region.

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

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.

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 be 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)âĂŤbut 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 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.

What emission model underlies the model runs used for simulation? Is that also EDGAR?

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.

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).

Also, what is the overall utility of this method?

Where around the world can it be used?

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

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.

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

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

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

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

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.

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.

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).

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.

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, sub-national scales.

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

Page 14 Line 28: typo, "toinvestigate"

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

Printer-friendly version

Discussion paper

possible magnitudes of bias errors?

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