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

Anonymous Referee #2

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General comments:

This study uses satellite products for XCH4 to estimate regional methane emissions identifying hotspots. The authors tried to explain the method and results adding details, which I think help the reader understand the material. The topic is important and interesting, but I have a few major concerns about the method and the main result obtained from this proposed approach.

First, the word "hotspot" in the title is somewhat misleading because the main result of this study is a regional or subregional (relatively large area) estimate of CH4 emissions although pixels (but at coarse resolution) with large enhancements relative to surrounding pixels are identified. I strongly suggest that the authors remove the word "hotspot" from the title because this work essentially estimates emissions for source "regions",

for which many studies have already been doing using data from ground tower sites or aircrafts or remote sensing. The more accurate bottom-up inventories the authors cites (e.g., Jeong et al., 2014) can now identify hotspots with a much finer resolution. At the global scale, the source regions in this study may be considered hotspots, but those areas are really regions or subregions as shown in many previous studies.

Second, the authors try to match their satellite-based XCH4 to another assimilated product. This is disappointing because the value of those satellite products for XCH4 is significantly diminished as they are supposed to be used as independent retrievals of XCH4. The authors need a clear justification for this. Please see the related specific comments below.

Third, it looks like that the proposed method ends up with a simple linear scaling of satellite-derived XCH4 to CAMS, in particular with a single parameter of V, which seems to be estimated as one value for the whole globe (as written it sounds like that; if not please clarify it).

Also, given the too large uncertainty for individual annual emission estimates, I wonder what value from this study can be added to the scientific community for regional GHG modeling.

Specific comments:

Page 4, Lines 26 - 28, the sentence needs to be revised because the authors are trying to say two conflicting things in the sentence, making it confusing. Also, I would recommend that the authors be more quantitative instead of saying "agree reasonably well". In terms of data gap, how SCIAMACHY and GOSAT are different, e.g., available data points/pixels at the annual scale?

Page 5, Line 10, I wonder if the authors considered the data scarcity (i.e., small number of data) for the annual averages in terms of uncertainty. For certain pixels, the # of available data would be too small while others have enough for averaging.

Page 5, Line 17, I wouldn't use the term "enhancement" because the surrounding region is not equal to the CH4 "background" region, e.g., the Pacific region for the western US.

Page 6, Lines 4 - 9, Looking at Eq. (1), the authors are trying to estimate emissions (flux) for the source region using ΔXCH4. But ΔXCH4 is not exactly the local enhancement, but only the relative enhancement to the surrounding region, which itself has some local enhancements. This will lead to underestimation of the emissions for the source region.

Page 7, Lines 9 - 10, The authors confirm my point about the underestimation when using Eq. (1). The authors state that "we aim at quantifying the impact of the choice of the surrounding region by varying its size and shape." This makes it very hard to adopt the proposed method in other regions because it involves adjustments of size and shape, likely yielding multiple estimates and subsequently expanding the uncertainty.

Page 7, Lines 20 - 22, There are two important concerns about the method. First, I expected from the title that the satellite products would provide independent observations as in most of the top-down studies. It is not vey satisfactory to try to match estimates from another product, i.e., CAMS. Also, from what is written here, I find that a single value for V needs a serious justification. Also, I am not convinced why CAMS should provide "true" estimates. Can the CAMS estimates be truly representative of any of the study sites/regions? How well are they compared with the estimates from previous studies for those source regions (maybe the word "true" may not be appropriate here; otherwise needs clarification).

With respect to the optimization of V, this parameter optimization would be the key to this study. However, it seems that there is no explanation or consideration of the errors between the relationship between CAMS and XCH4, which can be defined as:

CAMS = f(XCH4, V) + err

where the function f is likely a linear one and err is the irreducible error (e.g., mean 0, normal error). Here for correct estimation of V, we need some independent estimates for err, similar to a linear regression case with errors.

Page 10, Lines 18 - 21, I differ with the authors. The too large uncertainty suggests that the method is not powerful. I would conclude that the only value of the satellite products used in this study is to provide auxiliary information derived from the column-averaged XCH4 which is linearly scaled to match another model product (rather than independent measurements).

Page 11, 33-34, Again, the uncertainty is too large. When we think about hotspots, we expect relatively unambiguous isolation of emissions. The papers cited in this work already estimated emissions for the region with much better uncertainty. What policy makers need is identification of hotspots at the level of km scales and emission estimates for those small regions to mitigate sources from them. However, in this study, even the regional annual total yields very large uncertainty. Is there any way to reduce the uncertainty, even at the annual scale?

Table 3. EDGAR v4.2 happens to estimate the same Mt CH4 for both Four Corners and the Central Valley?

Figure 1. The region needs to be defined more accurately. For example, the region defined as the Central Valley of California in Figure 1 includes Southern California, and is different from that in Table 2.

Figure 8 needs some improvements. First, the data points (circles) should match the years on the X-axis label that are represented. Is the "standard deviation" the standard deviation of 7 annual estimates, e.g., for the 2003 - 2009. If this is the case, standard deviation is not very useful. I would be more interested in knowing the overall mean estimate for the multi-year period and the uncertainty about the mean, e.g., during 2003 - 2009. When individual annual estimates have huge uncertainties associated, I don't see the benefit of using standard deviation.

Also, the 1-sigma uncertainty in estimated emissions for individual years overlap with the EDGAR estimate, making it hard to statistically evaluate EDGAR. Looking at this at face value, I am not sure if there is any statistical power in the proposed method to say about the regional emission, even at the annual scale.

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