

## ***Interactive comment on “Spatially resolving methane emissions in California: constraints from the CalNex aircraft campaign and from present (GOSAT, TES) and future (TROPOMI, geostationary) satellite observations” by K. J. Wecht et al.***

**Anonymous Referee #2**

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This paper presents an Eulerian methane inversion study for the state of California at a high spatial resolution for the period May 1–June 22 2010, based largely upon measurements from the CalNex aircraft campaign. The resultant methane fluxes are found to be significantly higher than those predicted by either EDGAR4.2 or CARB emission inventories, a finding consistent with multiple previous studies. Flux estimates are also carried out based on satellite data (GOSAT and TES) from the same period, which

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are found to be unable to significantly constrain the fluxes. An OSSE is carried out to assess the applicability of the planned satellite sensor TROPOMI and the proposed geostationary mission GEO-CAPE, both of which were found to be able to constrain the methane fluxes as well or better than a dedicated aircraft campaign. Overall the paper is very well written, and the arguments are clear and well laid out. Despite this, I have two significant reservations about the paper in its current form.

The first has to do with its heavy reliance upon and reference to not-yet-published results. Without being able to refer to the more detailed methodology of Santoni et al. (2014), which has been submitted to JGR, it is difficult to assess the results. The data upon which the entire study depends, namely the measurements of the CalNex aircraft campaign, are introduced only briefly, and never really shown. (Figure 2 doesn't really give an idea of the density or timeline of the measurements - it would be nice to see a plot of the flight paths.) Furthermore, the description of the modelling system refers heavily to Wecht et al., 2013/2014 (<- this should be changed consistently to 2014), which apparently has been submitted somewhere, but certainly cannot be found at this point for further information. This may well resolve itself over the course of the editorial process, but at the moment it is troublesome.

The second problem relates to possible errors with the transport in the model. Although the model is being run at a fairly high resolution (~50–60 km), this is not necessarily sufficient to resolve many mesoscale transport effects. My first thought upon comparing the distributions in Fig. 1 and Fig. 3 was that the main inland red area most likely corresponded to a topographical feature. Not being overly familiar with the geography of California, I consulted an elevation map and found it to be a near-perfect match with the Central Valley. In mesoscale modelling it's common to see "lakes" of tracers pooling in valleys, and persisting for quite some time under some conditions. This can be difficult to reproduce with a coarser model, and may partially explain the high RSD values near the surface in this region. But more telling than having a higher standard deviation between model and measurement, an inability to represent the transport over such

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complex terrain would likely result in a systematic offset, which would be interpreted as a mismatch in the fluxes. If the model were unable to simulate (for example) the pooling of tracers in the Central Valley, the inversion would respond by increasing the posterior fluxes in this region, which is exactly what we see in Figure 3. There is no assessment presented to convince the reader that the simulation of the transport over such complex terrain is actually sufficient to allow for flux inversion: perhaps here some comparison of simulated and measured meteorological parameters would be warranted. Surely CalNex measured more than just methane?

Related to this (and transport errors in general), the error in the simulation of the planetary boundary layer is discussed in some detail, and the use of weighting of data points to ensure that the region from 0-2 km is evenly represented seems valid. I presume this even sampling is pressure-weighted rather than altitude-weighted? The explanation at the end of section 3.1 does not make this entirely clear - some explanation of the methodology is lacking. In general, it would be nice to have some (graphical) idea of the distribution of the flight data. What does it mean that "most" of the observations were under 1 km - is that 55%? 80%? Again, I wanted to see some sort of plot of the measurement locations, but this was lacking. I have access to the EDGAR emission inventories, but found it helpful to see Figure 1 to help understand the results. I do not have access to the CalNex flight paths, but I find this information similarly necessary in order to interpret the results.

Regarding the robustness of the results: the posterior total flux was surprisingly sensitive to the prior flux uncertainty. The fact that the total posterior fluxes increased even further when allowed that latitude implies that the optimized fluxes still have a systematic (low) offset. It might be instructive to see how the model-measurement mismatch looks, before and after optimization (based on a forward run of the optimized fluxes). What about repeating the experiment with the gridded version of the CARB dataset as the prior? If the spatial distribution and/or category breakdown of the posterior result remained consistent, it would certainly lend credence to the conclusions.

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Once these points are addressed, the manuscript would be suitable for publication in ACP. The subject matter is certainly fitting to the journal, and the study addresses important challenges related to the verification of emissions by atmospheric measurements.

Minor comments:

p4121 (18-19): Should be rephrased, of course there aren't really observations from future satellite instruments, but rather simulations using pseudo-data representing the expected measurement characteristics of future spaceborne sensors.

p4124 (21): inconsequent -> inconsequential

p4125 (5-6): How important is the timing of the rice growing season to your results? The flight campaign straddles the onset of the growing season. Can this onset be seen clearly in the measurements? If you're solving for the total flux over the whole time period it may sort of cancel out, but the step function is unlikely to represent reality.

Figure 1: Please put total flux units on the maps themselves, not just in the caption. Also, the colour scale is in rather a strange unit: why in molecules instead of mass ( $\text{mg m}^{-2} \text{day}^{-1}$  is often used for methane...)?

p4126 (10-14): An example of where I need to read Santoni et al. (2014) to understand the data selection and free troposphere correction. How big was this correction? How noisy? Perhaps it is presented there, but it is not clear.

p4126 (24): underestimate -> underestimation

p4127: see PBL discussion above.

p4129 (2): nstate -> n state

p4131 (16): Are there spaces between number and unit (km)? (Here and elsewhere - hard to tell, but I think not.)

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p4132 (1): This is the first time that the specific dates of the campaign are mentioned - this information should appear much earlier in the paper.

p4132(2-3): Awkward sentence, rephrase.

p4133: OSSE is overly optimistic in several ways, not all of which are pointed out. The random removal of clouds (rather than correlated, bunched, persistent patterns) is almost a best-case scenario for cloud screening. (Why not use MODIS or similar?) Dividing measurement errors by the square root of the number of measurements assumes that the measurement errors are uncorrelated, which is unlikely to be the case. The assumption that there would be no significant (and hard to detect) bias between a TIR sensor used to correct the free troposphere and the SWIR sensor is also rather optimistic. Nonetheless, this optimism is somehow the nature of OSSEs, and not the primary focus of this study. Still, some further discussion should be added.

p4135 (1): I think this should be Santoni et al. (2014)?

p4136 (9): underestimate -> underestimation

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