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Atmospheric Chemistry and Physics Discussions

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.

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General Comments (GC):

This manuscript investigates on estimating the methane emissions in California by applying inverse modeling technique and utilizing atmospheric CH4 observations from the CalNex aircraft campaign. The results are compared with other studies which use

different inversion methodologies, and are summarized. Additionally the study uses the satellite (GOSAT) observations to assess its ability to constrain methane emissions in California; particularly in the Los Angeles Basin. The study is further extended to assess the efficacy of future satellites, using observation system simulation experiment (OSSE) results. The model described in the paper is GEOS-Chem with 1/2 degree x 2/3 degree horizontal resolution and uses EDGAR v4.2 emission inventory. The manuscript is organized well and is written concisely and clearly; hence easy to follow in most of the cases. The topic of the study is certainly within the scope of ACP.

However, I do have certain comments. The main concern is the robustness of the inversion results that is sensitive to the choice of a priori and its uncertainty. Unfortunately, there is also no real discussion on potential reasons for seeing such a large discrepancy in emission estimates (between a priori and optimized fluxes + between the inventories). Changing the confidence in a priory by 25 % alone has resulted about 7.5 to 10 % change in estimated California emissions of 2.8 Tg yr-1 (see Section 3.2, 2nd paragraph). Transport related errors constitute another issue anyway (see the Specific Comment). What happens if using CARB as a priori in the same set-up? In that case, I don't have the reason to believe that the estimated emission will be as high as reported here. I am curious to see the improvements (i.e., reduction in model-observation discrepancy) when using optimized fluxes in the GEOS-Chem forward model. I assume that these simulations are already performed (see Section 5). An independent evaluation (other aircraft or satellite data) will be of great help here to support the robustness of the results. Another concern is regarding two citations which are not yet in the stage of "accepted" or "published" – Wecht et al., 2014 (the year is also wrongly cited in the text) and Santoni et al., 2014 – the issue here is that these citations are largely used in the present manuscript to compare the results and the methodologies. I recommend the paper to be published after considering the above and following comments.

Specific Comments (SC):

p. 4121: "In Wecht et al. (2013), we present . . ." + "Santoni et al. (2014) previously.."

ACPD
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Please see the comment above (GC) and update the citation + text accordingly. Since these citations are used many times in the manuscript, it is worth to check the entire text and modify accordingly.

p.4124: “CARB only reports statewide totals. A gridded version of the CARB ..” Do this study use gridded version? If so, please specify the resolution.

p.4126: “The middle panel of Fig. 2 shows . . . discrepancies in patterns that point to errors in the EDGAR emissions.” I am less convinced here. By looking at the patterns in Fig 1 (top left panel) and Fig 2 (left + middle panel), I have the strong feeling that (model) transport related errors are more pronounced here rather than flux errors, provided that the prevailing wind could be from west. This could be the reason on seeing better model to observation match in some pixels in the South (Fig. 2). I highly recommend authors to comment on this.

p.4130: “The MLR best fit has an R2 of ..” I am a bit confused here. Are you talking about R2 averaged over all grid squares? Please clarify.

p.4130: “..we examine their combined value for constraining..” As far as I understood, this study does not use GOSAT+TES combined observations, as observation vector (y), for the inversion. TES data are used only for the tropospheric background correction. Please clarify.

p.4132: “Figure 5 (right) shows the optimized correction . . . GOSAT observations" It is much helpful if you also include the “GEOS-Chem a priori” (forward) simulations on interpreting these inversion results. The middle panel (TES observations) can be omitted if it is not as a part of the observation vector, y and only used for subtracting the mean bias.

p.4132-4133: Section 5. Please include figures to support your results, particularly the spatial plot of synthetic observations representing true atmosphere.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 4119, 2014.