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Interactive comment on "A fifteen year record of CO emissions constrained by MOPITT CO observations" *by* Zhe Jiang et al.

Anonymous Referee #1

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The authors make an interesting contribution to the quantification of CO surface emissions and of their trend over the past 15 years. I recommend its publication provided the following issues are addressed. Most of them are minor, but a couple of them deserve much more attention.

- l. 80: The authors anticipate on their results, which is not really appropriate in an introduction (it breaks the logic flow).
- l. 97: measurement and model systematic errors can be damped but not suppressed.
- l. 98: "systematic biases" -> "systematic errors".
- l. 143: the previous example of the SCIAMACHY bias is time-dependent. The

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authors should explain why they think that the MOPITT bias does not vary much with time (mostly with the season).

- l. 185: the authors seem to neglect the error statistics provided by the retrieval product. We can understand that they prefer raising them at 20% to be conservative, given likely systematic errors, but ignoring the vertical correlations is really surprising. **This point is important because it bears most of the credibility of the following profile/lower profile inversion results vs. column inversion results**. In addition, the ad-hoc uncorrelated observation budget used here is not internally consistent: when summing the profile level (error) covariances, one does not get the column (error) variance. **This inconsistency basically suppresses the possibility to compare the two types of results meaningfully**. Last, model errors are very likely correlated in the vertical and even uncertain large or medium vertical correlations (let us say 0.5 for instance) for this term of the observation error budget are better than the null correlations assumed here.

- l. 186: the authors seem to combine combustion and VOC sources of CO together but later in Section 4.2 they show result by source type. They should explain how they split the information on the source type with simple column or profile retrievals of CO. In particular, I cannot see how VOC sources and their trends can be separated from the rest.

- l. 215-216: This sentence ("... indicated that regional inversions have more advantages than global inversions ... better controlled") is unnecessarily polemical and may actually be wrong depending on how we understand "better controlled". There are pros and cons and the statement cannot leave the impression that the case has been closed.

- l. 219: " model" -> " models" .

- l. 228: the authors need to be clear that they do not use the same land data in the first and in the second step. Otherwise they would correlate boundary condition errors and observation errors in the second step and possibly induce weird side effects on their results (because those correlations are not accounted for).

- l. 254: Montzka et al. (2011) is recalled, but these authors wrote “Despite the much lower atmospheric CH3CCl3 mixing ratios in recent years ($\simeq$13 ppt in 2007), they remained precisely measured through 2007. Precision for the analysis of CH3CCl3 (0.5 to 0.75% as repeatability) has remained comparable to the nearly constant (on a relative basis) standard deviation of paired flask means collected within a month at remote stations of 0.7$-$1.1% through 2007. Data after the end of 2007 are not included in this report owing to instrumental problems that developed in 2008.” The present authors should give the same level of detail and clarify the fact that the instrumental problem does not affect their results.
- l. 276: “demonstrate” is too strong.
- l. 296: there is also an initial increase in the measurements that should be commented.
- l. 313: this is only true for the profile results.
- l. 335: large PBL height errors happen everywhere over the globe. Why should they just affect India and SE Asia ?
- l. 374: these 2014 and 2015 studies are not “more recent” than Field et al. (2016). Actually, the authors could discuss the “more recent” study of Yin et al. (2016) that seems to well overlap with their approach.
- l. 376: extra comma.
- l. 396: the above-mentioned issue in the observation error statistics is also a likely explanation.

- l. 464: to be fair and consistent with the second part of the sentence, the authors should also speak of an update about this question, since it has been (imperfectly) addressed before.
- References should be ordered.

Reference

Yin, Y., et al. (2016), Variability of fire carbon emissions in equatorial Asia and its nonlinear sensitivity to El Niño, Geophys. Res. Lett., 43, 10,472–10,479, doi:10.1002/2016GL070971.

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