Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-811-AC1, 2017

Atmospheric Chemistry and Physics
Discussions
Open Access

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

Zhe Jiang et al.

zhejiang@ucar.edu

Received and published: 17 January 2017

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.

Thank you for your comments. Modifications have been made to improve this manuscript.

Q1: l. 80: The authors anticipate on their results, which is not really appropriate in an introduction (it breaks the logic flow).

Changed.

Q2: l. 97: measurement and model systematic errors can be damped but not suppressed.

Changed.

Q3: l. 98: “systematic biases” -> “systematic errors”.

Changed.

Q4: l. 143: the previous example of the SCIAMACHY bias is time-dependent. The authors should explain why they think that the MOPITT bias does not vary much with time (mostly with the season).

The limited measurements provided by the HIPPO aircraft will result in uncertainties in the correction factors, which is more significant in the seasonal average than annual average. On the other hand, we are focusing on the interannual variation of CO emissions. The seasonal variation of CO emissions is not very important in this work. Consequently, we decided to use the annual mean correction factor. More description has been added.

Q5: 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.

A very good question! We have compared the discrepancies associated with two types

Printer-friendly version

Discussion paper

of error covariance matrix in the preparation stage of this work: 1) diagonal matrix (this work); 2) full error covariance matrix including vertical correlation, based on MOPITT error covariance. Our results show that the difference in the scaling factors is small, perhaps due to the large amount of satellite measurements in our global scale inversion. Because we are focusing on the mitigation of effects of systematic errors, we used the diagonal matrix to keep consistency with our previous studies. However, as the reviewer indicated, a better description for the error covariance matrix is important. We will improve our methodology in our future study.

Q6: 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.

As the reviewer indicated, we cannot completely separate the a posteriori emission estimates from different sources. However, the various spatial and temporal distribution of emissions sources (e.g. anthropogenic vs. biomass burning) provides valuable information to distinguish the contribution from each category. In order to further isolate the influences of biomass burning, the months dominated by biomass burning (biomass burning CO > 50% of total CO emission in an individual grid) are excluded in the trend analysis for anthropogenic and VOC sources (Figure 5). More description has been added.

Q7: 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.

Thank you for your suggestion! The statement has been changed.

Q8: l. 219: “ model” -> “ models” .

Changed.

Q9: 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).

I am sorry for the confusion. In the two-step approach:

Step 1: We directly modify CO concentrations using sequential Kalman filter assimilation. Both MOPITT data over land and ocean are used. Step 2: We constrain CO emissions over land with MOPITT data over land only. The boundary condition is from step 1.

The objective of Step 1 is to provide the best global CO fields, based on MOPITT. We need to assimilate MOPITT data over land in the first step to keep the consistency between boundary conditions and emissions.

Q10: l. 254: Montzka et al. (2011) is recalled, but these authors wrote “Despite the much lower atmospheric CH3CCl3 mixing ratios in recent years (’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 0.7ôĂĂĂ1.1% 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.

The website (NOAA) shows: “NOAA flask data obtained by the GCMS for some compounds analyzed during the 2008.5-2009.5 period are subject to some small biases owing to instrumental issues during that period. Data obtained for CH3CCl3 during that time period, for example, should not be used for deriving hydroxyl radical concentrations"

According to Figure 4, we believe the influence of the instrumental problems (2008.5-2009.5) on our analysis (2001-2015) is small.

Q11: l. 276: "demonstrate" is too strong.

Changed.

Q12: l. 296: there is also an initial increase in the measurements that should be commented.

The initial increase at 2001-2002 could be caused by uncertainties in the data. We are trying to avoid to make a conclusion about trend based on short (2 years) period data. A sentence has been added for this issue.

Q13: l. 313: this is only true for the profile results.

As shown in Table 1, an increase of Chinese emissions from 2001 to 2004 is shown by all three analyses.

Q14: l. 335: large PBL height errors happen everywhere over the globe. Why should they just affect India and SE Asia?

Thank you for pointing out this issue. We have removed "PBL height" in the discussion.

Q15: 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.

The discussion has been changed. We didn't cite Yin's work here, because we hope to demonstrate the consistency between our inversion results with studies using different approach (not an inverse modelling).

Q16: l. 376: extra comma.

Changed.

Q17: l. 396: the above-mentioned issue in the observation error statistics is also a likely explanation.

The lower tropospheric profile data includes the lowest three levels (1000hPa, 900hPa and 800 hPa). The influence of correlation of these three levels should be small.

Q18: 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.

Changed.

Q19: References should be ordered.

Changed.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-811, 2016.