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Interactive Comment

Interactive comment on "Decadal trends in global CO emissions as seen by MOPITT" *by* Y. Yin et al.

Anonymous Referee #1

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This paper uses as data-assimilation framework to infer CO emissions from satellite observations. The CO inversion system is coupled to CH2O, CH4, and MCF, which provide constraints on the sources and sinks of CO. The target period of the study is a full decade (2002-2011). The study has the aim to provide a consistent analysis of the drivers of the observed decline in CO total columns from MOPITT (figure 3). Although the paper is relative convincing in some aspects, some inconsistencies are also apparent, which need to be better explained or analyzed. Below, these are listed under major issues.

Major issues

1. Is the system well balanced?

The focus of this paper is on CO, but also CH4 and MCF measurements are assimilated. One of the burning questions around today is the role of OH, and a possible trend in OH, in the observed CH4 growth rate changes, exactly in the analyzed period. I was therefore a bit disappointed to find only one sentence: "Similarly for CH4 and MCF, the inversion fits the assimilated data fairly well, but these results are not shown, as they are not the main focus in this study". This casts doubts on the added value of the CH4 and MCF assimilation. I presume that the cost function consist of a term related to model-data mismatch of MOPITT observations, a background term (emissions of CO, MCF, and CH4 that deviate significant from the prior), and terms related to CH4 and MCF misfits at the stations. If such a system is not well balanced, it might be that little of no information is drawn from the MCF and CH4 misfits. Since the authors claim that they infer no trend in OH they have to verify if their system is adequately set up to detect a possible OH trend. Stating that the inversion fits the MCF observations fairly well is certainly not enough. Moreover, it would be interesting to present some analysis of the cost function, showing how the optimization changes the cost function, and how CH4 and MCF observations are used to inform the CO budget in terms of sources and sinks (e.g. by neglecting couplings).

2. Are the results realistic?

A large fraction of the atmospheric CO comes from the oxidation of NMHCs. Yet, figure 6 shows that large seasonal biases exist with independent satellite observations of CH2O. This implies that the atmospheric CO sources are also seasonally biased, and these biases will be projected on CO emissions. Even more worrying are the large regional emission increments that are presented in figure 9. For instance, in the region SHSA the emissions are calculated to increase from roughly 50 Tg/yr in 2002 to almost 200 Tg/yr. In later years calculated increments are smaller but the recently described biomass burning year 2007 (Bloom et al., GRL, 2015) visible in the prior seems to disappear in the posterior. Over Australia and Africa also some large increments are calculated. Likely, the two issues are related since CH2O from isoprene is a major source of CO over SHSA (Stravakou et al., ACPD, 2015). Finally, only the results for

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the TRANSCOM-OH are shown. These fields have a NH/SH OH ratio closer to 1 and this will surely influence the NH/SH CO emissions (Patra et al., Science, 2014). On the global scale the CO budget might not be influenced too much by the OH field, but given the importance of OH as CO sink, some analysis and discussion about this issue is also needed.

Minor issues:

14507, 23: TES is probably not referring to the “Technology Experiment Satellite”, but to the Tropospheric Emission Spectrometer.

14508, 1: “The interpretation . . .”: sentence reads awkward, rewrite.

14508, 8: “Understanding this model-data misfit is all the more so challenging that surface emissions and chemical production each account for about a half of the total CO sources” → “Understanding this model-data misfit is challenging because surface emissions and chemical production each account for about half of the total CO source”.

14508, 11: can contribute → could have contributed

14508, 20: information piece → piece of information

14508, 27: to infer the origin of the observed CO concentration decrease in the past decade → to infer the most likely origin of the observed CO concentration decrease over the past decade

14509, 2: remove “at once”

14509, 4: “chemically connected to hydrocarbons”? unclear

14509, 13: “The algorithm has undergone continuous improvements and several re-processings of the archive have been made (Streets et al., 2013).” The algorithm has undergone continuous improvements and the archive has been reprocessed several times (Streets et al., 2013)” By the way: is the Streets et al reference correct? It is not in the list, like the Cressot (2014) reference. Please check all references!

14511, 6: this requires some explanation. If you use a "fixed OH field", it is important in know how this has been obtained, and possibly what was the role of NOx in obtaining these fields.

14511, 14: pressure weighted concentration? Is that not simply a mixing ratio? 14511, 17: CH2O has also direct source from biomass burning (Stravakou et al., ACPD, 2015). How do you account for surface emissions in this procedure?

14511, 27: as described below

14512, 18: the Cressot reference is not in the list.

14513, 12: Leeuwen → van Leeuwen

14513, 13/16: m2 → m-2

14513, 23: month? The period is 8 days, right?

14515, 25: what about observational errors? Probably smaller than model errors, bur still good to mention.

14515, 26: how is the yearly mean of the synoptical variability defined? DO you apply the same filter as for the 3-sigma filtering?

14517, 8: whatever → irrespective

14518, 8: I suggest to add something like: "when for instance the vertical mixing in the model is too conservative, this could lead to a positive bias at the surface, because the sources are adjusted to fit the satellite data."

14518, 16: Negative → A negative

14519, 1: A logical discussion here would be: what are the trends in the direct prior CO emissions from anthropogenic activities and from biomass burning? I see this discussion later...so please point forward to that discussion.

14519, 2: To compare the trend in columns to trends at the surface, please convert the

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column in a mean mixing ratio.

14520, 7: I find this not very convincing. To my eye, for at least some stations, it seems the prior simulation reproduces observed trends better than the posterior simulation. So, why not provide the information in a table? (e.g. average improvement of trend).

14520, 17: fairly agree → agree fairly well

14520, 20: trend → a trend

14521, 7: “INCA-OH has higher than TransCom OH concentrations in the NH during summer OH maximum, but lower than TransCOm OH concentrations in the SH Tropic” → INCA has higher OH concentrations than Transcom in the NH during summer, but lower OH concentrations in the SH Tropics”

14521, 19: A trend of roughly half a percent per year should have an influence on the CO trend (which are typically 0-2.5 %/year). The sink term read -k.CO.OH and trends in CO and OH should be equally important. In figure 8 the posterior trend in the "sink" (k.OH.CO) is also steeper than the posterior trend in the "source", which indicates some role of OH trends (but indeed rather small). 14521, 20: considered of minor effect on the CO trends → to be of minor importance for the CO trends

14522, 5: Please check all units in the paper. For instance, emission maps now have the label “Tg/year”, which misses a unit area. In figure 8, the unit should be Tg CO/ months, and the trend should also have a unit.

14522, 7: SD? You have only two realizations.

14523, 1: more negative → steeper negative

14523, 9: no significant trend in the OH concentrations IS found by the inversion. . . ..but: when the burden of CO decreases, one would expect OH to go up, because one of the most important sinks goes down. In that respect, the absence of an OH-trend is surprising, and I think your results point to a small positive OH trend.

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14523, 14: I think that this argument does not make sense. MCF lives 5 years, so a trend in OH anywhere on the globe would be reflected in the MCF mixing ratios also on remote stations.

14523, 21: This is also incorrect. The Montzka (2011) study only addresses variability, and not trends, since all data were de-trended.

14523, 26: unclear why the positive dots appear over oceans. Legend does not explain this.

14523, 28: "estimated by the prior"?? Do you mean: "in the prior emissions"?

14524, 28: changing rate → growth rate?

14526, 11: "Such decreasing. . .observations". This is not a conclusion of this paper.

14527, 15: OH: like above, invalid argument.

Figure 2: Units Tg per year per unit area (gridbox?)

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14505, 2015.