Interactive comment on "Global atmospheric carbon monoxide budget 2000–2017 inferred from multi-species atmospheric inversions" *by* Bo Zheng et al.

Anonymous Referee #2

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This study present the atmospheric CO budget constrained by 4 different global inversions, including the discussion on the trends of emissions by sources and sectors. The paper is well written and provide a documented and interesting discussion of the results and should be published after revisions.

However, the authors stress results that are already known, which is the decline of the emissions in Europe and in the USA that has been going on for several decades, and from China since the 2010's, for which the trend is not well represented in bottom-up inventories. The inversion system has been designed for multi-species inversions (e.g. Pison et al. 2009), so it looks like that aside from updating MOPITT retrievals and the

model, the addition of GOSAT is the main update in the system. It would be great to have more discussion about the impact of GOSAT and the overall CH4 to CH2O to CO yield within that context. In particular, the direct comparison to CO observations does even suggest a slight deterioration when using GOSAT (inversion #3, Fig. S4 and S8). Then it is desirable to see the impact of CH2O and CH4 (inversion #2 and #3) on the spatial and temporal distribution of CO sources and sinks (showing Fig. 1 and Fig. 4 with inversion #2 and #3). It is surprising that the CO dry deposition is not presented, when the focus of the paper is about the CO budget, please take into account the following major and minor comments before resubmission.

Major comments: 1. Why is the dry deposition of CO not included in the Eq. 1? The dry deposition constitutes around 10 % (up to 15 %) of the total sink and it has been showed that an alternative representation of the CO deposition could improve the atmospheric CO budget (see Stein et al. 2014 and reference therein). If you did not take the dry deposition into account, it means that the global CO budget has an uncertainty of 10 to 15 % error bar, which likely introduce a bias such as underestimation of emissions or an overestimation of the chemical sink. This precludes strong conclusions about the temporal variations of the CO budget, in particular with regard to the chemical sink. Please correct the equation 1 and the following: P6L1; "The picture of atmospheric CO budget derived from our inversions includes surface emissions from different source sectors, CO chemical production, and CO chemical sink." P2L22: "Interpreting atmospheric CO trends requires accurate quantification of the global CO budget (Duncan et al., 2007), including surface sources, atmospheric sources (oxidation of hydrocarbons, known as CO chemical production), and atmospheric sinks."

2. The publication must discuss and includes reference to the limitation of using a simpler and linearized chemical scheme. This is one of the main points of the already cited paper; Lelieveld et al. (2016) showed that OH is well buffered because of the secondary OH production (OH recycling), which is not represented in your system. It has two implications, one is the absence of atmospheric chemical feedbacks given

their distribution of chemicals and the second is the impact of the ignored species. For instance, NOx emissions have a strong impact on OH (e.g. Miyazaki et al. 2017). Several studied have pointed out that because of the coupling between CH4, CO and OH, their errors are potentially corelated and thus reducing one bias could lead to an overall benefit for the whole system (e.g., Strode et al. 2015, Gaubert et al. 2016). In particular one recent study showed that the CH4 observations could thus be used to constraint OH feedback (Zhang et al. 2018). The use of a simplified chemistry is totally acceptable knowing the computational costs for studying long-term trends, as done in this study. It is however important to point out those potential limitation when points are made on the constraints on the CO (+OH) sinks. One easy thing to do would be to divide the CH4 loss by the CH4 concentrations used (i.e. the CH4 lifetime), since there is a, to check whether the growing CH4 sink is due to increasing CH4 alone or if there is a change due to a change in OH.

Minor comments:

P4L12: "To solve the inverse problem, forward and adjoint codes are iteratively run until sufficient convergence of the cost function (Eq. (2)), and the last iteration with optimized model states gives us the best estimate that matches all available information within their uncertainties." Please move this sentence to the next section, where Eq. 2 is actually defined.

P5L23: "The WDCGG measures surface hourly CO concentrations". It looks like there are flasks measurements, please rephrase.

P5L13: Add the references for GOSAT (Kuze et al. 2009) and GOSAT CH4 retrievals (Parker et al. 2011). [Kuze, A., Suto, H., Nakajima, M., and Hamazaki, T.: Thermal and near infrared sensor for carbon observation Fourier transform spectrometer on the Greenhouse Gases Observing Satellite for greenhouse gases monitoring, Appl. Opt., 48, 6716, https://doi.org/10.1364/ao.48.006716, 2009.]

P6L3: "which calculates the CO yield from the oxidation of CH4 and of NMVOCs and

the CO oxidation sink in each model grid box at each time step of the model simulation." Here you can recall that the yield is assume to be linear.

P8L2: "Tropospheric CO columns measured by MOPITT have declined at a relative rate of $-0.32\pm0.05\%$ yr-1 (p<0.01) during 2000–2017, highly consistent with the relative trend in the estimated CO sink ($-0.35\pm0.23\%$ yr-1, p<0.01). This suggests that decreasing CO concentrations are the primary driver of the declining CO sink, while the combination of OH and reaction rate has negligible influence." While I agree that based on other evidence the CO emissions are reduced, which leads to a reduced CO levels, and thus the CO sink is reduced. MOPITT also see a reduction in CO concentrations. But this does not mean that OH does not have an influence, understanding OH feedbacks are far more complicated, since it requires the understanding of the OH budget itself. One can only say that the CO + OH flux has slowed down because of a reduction of CO. This also means that the OH sink has slowed down, but it does not fully explain the OH budget. This is important because even a tiny change in OH would have a strong impact on CO, so that it is hard to identify those small changes precisely.

P8L7: "These two inversions make a small difference (<10%) in the global CO budget estimates compared to Inversion #1 (Table S5)" Why is there only one significant figure? In particular for differences within 10 %, it makes it impossible to compare the simulations. This is also true for table S6.

P10L13: "The global CO sink is symmetrically distributed around the equator (Fig. 3a, 4c)." Is this due to the (TRANSCOM) prior?

P11L15: "As Inversion #1 tends to underestimate/overestimate anthropogenic emissions decrease/increase (Sect. 3.2)," Is there something missing? You mean in corresponding regions? it underestimates the increase in regions where CO is decreasing and vice versa? Please rephrase.

P11L17: "the CEDS inventory probably has large biases in emission trends estimates over CHN and SAS, which is the main reason why it estimates growing anthropogenic

emissions globally (Table S6) and cannot match the observed declining CO when used in the input of our LMDz-SACS model." You can note that this is consistent with previous studies, in particular Strode et al. 2016, for a different inventory.

Figure S4/S5/S6. In panel a, "Opimized"

P12L3: "The larger biomass burning emissions derived from inversions are most evident in late fire seasons when burned area declines after the peak fire month (Fig. S2)." It is also evident for the peak itself for SAF and BRA.

P14L18: "The global burned area is observed to have declined since 2000 (Fig. 9a) with the largest declines in the grassland and savanna ecosystems over EQAF (Fig. 9b)." I guess "decline" should not have an s

P16L18: "The other three inversions"; you can mention that those are also multi-species inversions, since it was mentioned for the previous paragraph.

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