Atmos. Chem. Phys. Discuss., 14, C10599–C10603, 2014
www.atmos-chem-phys-discuss.net/14/C10599/2014/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Seasonal changes in the tropospheric carbon monoxide profile over the remote Southern Hemisphere evaluated using multi-model simulations and aircraft observations" *by* J. A. Fisher et al.

D. Parrish (Referee)

david.d.parrish@noaa.gov

Received and published: 30 December 2014

Summary:

This paper presents an interesting discussion of measurement-model comparisons of CO in the southern hemisphere, and presents insightful analysis that is quite valuable. It is worthy of publication as it now stands. However, below are some specific suggestions for additional discussion to include in the paper that would further increase its value. Also, some relatively minor issues discussed below should be addressed before

publication.

Suggestions for additional discussion:

This paper, in common with many such papers, presents a very useful comparison between results from different models and with measurements. Here the comparison was organized and presented in a manner to test important aspects of the models. Conclusions are reached regarding the importance of secondary CO production from biogenic hydrocarbons; these have important implications for the use of model inversion studies to correct emission estimates.

However, in the end this and other such model-measurement comparison studies do not have as much impact on model development as would be desirable. I think that this lack of impact arises from two problems. First, completing the model calculations, conducting the comparisons and publishing the results requires a significant period of time, likely years; over that time models evolve, and so it is not clear that the published comparison results are relevant for present-day models. Second, model-measurement differences point to shortcomings in the models, but exactly how to remove those shortcomings and improve the models is not identified. The following two suggestions for additional discussion may help to increase the impact of the work presented in this paper.

1) Future comparisons of model results with observations following the approaches presented in this paper will involve results from different (hopefully improved) models, but the observational data set will not change. The comparisons developed in this paper using these observations can lead to quantitative metrics for comparison with the new model results that can be conducted immediately after completion of the model simulations. If these metrics are explicitly tabulated in this paper, then their use in future model-measurement comparisons will be greatly facilitated, and thus more likely to effectively guide model evolution. Suggested below are two observationally derived metrics that could be tabulated in this paper.

– Seasonal cycles of median monthly CO observed in three altitude ranges as shown in Figure 3. Rather than tabulate the 12 monthly medians (with corresponding median deviations), it likely will be more effective to tabulate the few harmonic terms that define the seasonal cycle (Francey et al., 1999 effectively did this in their Figure 5). The derived quantitative metrics would then include the annual mean plus amplitude and phase (with confidence limits) of all statistically significant harmonic terms in the seasonal cycle in each altitude range. I expect that no more than two or three harmonic terms would be significant in each altitude range. Extracting these harmonics is equivalent to performing a Fourier analysis of the monthly median data. A similar analysis of any present or future model results followed by comparison of the measurement and model derived parameters would allow a prompt evaluation of the model calculated seasonal cycle.

– Median CO vertical profiles for each season as shown in Figure 4, much as was partially included in the top line of Table 2. Here the absolute concentration, which was normalized out in Figure 4, could be included along with the slope and other statistically significant terms (quadratic and perhaps even cubic, all with confidence limits) to fully describe the vertical profiles. These are only poorly described by the slopes (i.e., average vertical gradients) included in Table 2. Extracting these polynomial terms is equivalent to performing a power series expansion of the vertical profile data. A similar analysis of any present or future model results would allow a prompt evaluation of the model calculated vertical profiles. Here particular attention should be paid to properly describing the profile through the marine boundary layer, which as expected, appears uniform below 2 km in Figure 4.

2) The authors have carefully investigated the factors affecting CO in the southern hemisphere. They have developed insightful analyses supporting their concept of these factors and tested the fidelity with which the four models replicate these factors. This work has put them in the position of having the best insight into model modifications required to improve that fidelity. At present the final section is more of a summary of

the analysis than a true conclusion section. I suggest that the Conclusions Section be rewritten not as a summary of the analysis, but rather with the goal of clearly presenting the authors' recommendation of how these four models in particular, and all similar atmospheric models in general, need to be improved to accurately simulate the vertical profiles and seasonal cycles of CO in the southern hemisphere.

Relatively minor issues to address:

1) The bottom panel of Figure 1 needs more discussion. It seems to indicate that there are primary emissions of CO from biogenic sources, but I assume these must be the secondary source of CO produced from the biogenic emissions of isoprene (and monoterpenes?). Please clarify.

2) A particularly interesting feature that is only briefly touched upon in the paper deserves further discussion. Figure 3 shows that the peak of the seasonal cycle in the marine boundary layer is delayed by about a month from that observed in the free troposphere. What is the cause of this delay? Is it simply very slow transport between the free troposphere and the boundary layer? None of the models appear to reproduce this delay. Does this indicate that the models fail to strongly isolate the boundary layer from the free troposphere? Is this also indicated in Figure 5 where the observed uniform vertical profile below 2 km is not well reproduced by the models, at least in some seasons?

3) Section 4.1 - The authors conclude "The differences in $CO_{2.5}$ between models are much smaller than differences in total CO, especially in summer-autumn, suggesting inter-model differences in meteorology and transport are small relative to other drivers of variability." This conclusion requires some further discussion. In summer (DJF) and autumn (MAMJ) all models exhibit very small vertical gradients, which could indicate that all models have vertical mixing that is rapid with respect to the 25 day $CO_{2.5}$ lifetime. In that case, large inter-model differences in meteorology and transport would not be apparent, but coupled with other altitude dependent inter-model differences,

perhaps could still be significant drivers of variability. Later discussion in the paper (e.g. Figure 9) does indeed identify important differences in transport between models that drive inter-model variability. The discussion of this issue should be clarified.

4) On pg. 27540, the authors write "A major component of the mean difference between models is the difference in the simulated mean OH background, shown in Table 1." This statement does not really appear to be correct. The two models with the largest difference in mean CO mixing ratios (Figures 2 and 3) are TM5 and CAM-chem; yet these two models have similar global mean tropospheric OH concentrations (Table 1). The two other models that have intermediate mean CO have similar, but significantly larger, mean OH. This discussion should be clarified.

5) The observational data sets were collected over the period of 1991 to 2011 and the modeling is done for 2004-2008. There should be an expanded discussion of any long-term changes in the ambient CO concentrations over this two-decade period, with a focus on justifying the neglect of any such changes in the analysis presented.

6) The HIPPO investigators are acknowledged in the paper, but are not included as coauthors. Were they invited to join as coauthors? If not, such an invitation should be considered.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 27531, 2014.