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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global ozone–CO correlations from OMI and AIRS: constraints on tropospheric ozone sources" *by* P. S. Kim et al.

Anonymous Referee #2

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Summary:

This paper presents a useful analysis of extensive ozone and CO data sets retrieved from satellite measurements. The correlations between these two measured species is compared to correlations predicted by the GEOS-Chem chemical transport model. The strength of this paper is that it compares model with measurements through relationships that are specifically sensitive to particular atmospheric processes, and diagnoses their representation in the model. The comparisons undertaken here are relatively modest, but this paper is certainly a welcome addition to the literature.

As noted by Anonymous Referee #1, there are large uncertainties, systematic as well as random, in the satellite data sets. I strongly support his/her request that the authors

present whatever support they can for the accuracy of the satellite derived ozone/CO relationships.

I have some additional, relatively minor concerns, most importantly related to lack of quantitative analysis in some places. When these concerns are addressed I judge that the paper will be suitable for publication.

Concerns:

1) Pg. 8909, Lines 10-16: The authors are evidently using unweighted RMA regressions. This should be stated. It would be preferred to weight each ozone and each CO measurement by the inverse of the square of its uncertainty. A useful reference might be Cantrell, C.A., 2008. Also, I do not believe that the following statement is necessarily correct: "The magnitude of the RMA slope is independent of the magnitude of the correlation coefficient and comparisons to previous studies using ordinary least squares (OLS) regression slopes can be made by dividing the RMA slopes reported here by the absolute value of the correlation coefficient." This statement may be correct for an example of perfectly correlated data to which perfectly random noise is added. However, if the nose is highly skewed, it may be a poor approximation. This is a small point (providing the uncertainty of the derived CO and ozone mixing ratios are approximately equal), and I do not think that the analysis needs to be redone. However, the authors should give a more complete description of their procedures and ensure that their statements are in fact correct.

2) Pgs 8910-8912: Beginning with the 3rd paragraph of Section 3, the authors present several paragraphs of qualitative comparisons between the measurements and the model, and between the present results and those presented in previous publications. Much of this discussion is not clear (at least to me). I suggest that the qualitative discussion be greatly shortened, and any specific comparisons that the authors believe important to be discussed, be put on a clear, quantitative basis.

3) The last two paragraphs of Section 3 are also highly qualitative. Any specific com-

parisons between the two transport simulations that the authors believe important to be discussed should be put on a clear, quantitative basis. Further, to my eye Figure 3 shows that GEOS-4 is superior to GEOS-5, yet GEOS-5 is used for the calculations that are the primary basis of this paper. Hence, a quantitative comparison is particularly important here.

4) This is a small point, but the slopes in the two panels of Figure 4 actually differ by more than the sum of the two confidence limits, when those are added in quadrature, as they should be. Thus, the statement on lines 15-16 of pg. 8915 is technically not correct.

5) It would be useful to mention the season (JJA) in the first sentence of Section 4.2.1.

Reference

Cantrell, C.A., Technical Note: Review of methods for linear least-squares fitting of data and application to atmospheric chemistry problems, Atmos. Chem. Phys., 8, 5477–5487

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 8901, 2013.