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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Evaluation of model-simulated source contributions to tropospheric ozone with aircraft observations in the factor-projected space" *by* C. Shim et al.

**C. Shim et al.**

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Reply to Anonymous Referee 3

I thank the reviewer for the comments to improve this manuscript and my answers as the author are to be shown by italic fonts hereafter.

This paper uses a statistical analysis technique (Positive Matrix Factorization) and observations from aircraft to evaluate the GEOS-CHEM model simulation of elements that contribute to ozone variability. The authors are to be commended on working to find appropriate ways to evaluate global models with in-situ data. This paper is moving in the correct direction, I believe.

A very general comment is that this paper can seem like a litany of cited statistics, making it somewhat tedious to read. While that does not weaken the science behind the paper, it does make it difficult for the reader to identify and retain the key points that the authors want to make. The authors may want to be less rigorous about citing every EV percentage and mixing ratio (particularly those that are already listed in the tables) and limit citations of statistics to those most relevant to their key points.

*Answer: We modified the text as suggested.*

I found Figures 7 and 8 to be the most useful and a nice summary of differences in factors influencing O3. The authors may want to think about if similar summary plots for some of the other factors would be a good addition to (or in place of?) Figures 2-6. It is a bit difficult to compare contributions to variations of a particular species from factor to factor in the current plots.

*Answer: Our goal in this work is to understand the factors contributing to tropospheric O3. Figures 7 and 8 summarize our findings. We cannot do it for the other species. For example, comparing factor contributions to CH3Cl would show that in most places it comes from only 1 factor.*

A concern I have is the very few number of points used in the analysis (between 65 and 200 for the various regions discussed). While the authors are clear that this is an exercise to evaluate the model simulation using aircraft observations rather than to evaluate the dominating factors that influence O3 in the environment, it is still necessary to do this type of evaluation in a factor space that is regionally representative in order for the comparison to be robust. Can the authors comment on the representativeness of their subset of points? Would it be worth looking at the larger GEOS-CHEM data set to evaluate if the subset of model points you have culled it down to are representative of the larger model region? That does not necessarily lend more confidence to the representativeness of aircraft observations within the real atmosphere, but it is one step that could be taken. In particular, I have difficulty trusting the analysis of a seasonal



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trend based on so few data points.

*Answer: Ozone concentrations in the 7Be-filtered subsets show similar probability distributions (binned by 10 ppbv) as those with all measured O3 included. The seasonal O3 trend of the subset for TOPSE is in agreement with that of all O3 measurements above 5 km. Thus the analyzed data sets appear to be representative to all the O3 measurements in the middle and upper troposphere. We clarified in the text.*

Some specific comments: Section 2.1: What is a "liquefied gas tracer?"

*Answer: It is a liquefied petroleum gas tracer. The text is corrected.*

Figure 3 seems to be included to show the similarity of results when O3 concentrations larger than 100 ppb are included, but then as far as I can tell, the rest of the analysis still does not include these data. What is the purpose of Figure 3?

*Answer: The figure is deleted to avoid confusion.*

Discussion of Figure 4 directs the reader to an orange bar, which I cannot see.

*Corrected.*

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 15495, 2007.



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Interactive Comment