

Interactive  
Comment

## ***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 2

I thank the reviewer for the comments to improve this manuscript and my answers are to be shown by italic font after each referee’s comment.

Shim et al present an interesting analysis of observed and model tropospheric ozone using a statistical technique (positive matrix factorization, PMF) that identifies individual factors that determine ozone variability, e.g., surface emissions, transcontinental transport, stratosphere-troposphere exchange. The PMF methodology has been published once before but is now applied to model and observations to test model source contributions to tropospheric ozone. Here, the authors apply the PMF technique to two

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contrasting aircraft datasets: TOPSE and TRACE-P.

The one broad criticism of the paper is that it is unclear from reading the manuscript what I learnt from the PMF method applied to TOPSE and TRACE-P data that could not have been understood from applying more traditional model analysis methods. The advantages of PMF should be stated more clearly throughout the paper.

*Answer: We added in the introduction "However, direct attribution of O3 sources based on atmospheric measurements is difficult since tropospheric O3 is a secondary product from primary emissions of trace gases" and "The factor attributions based on atmospheric measurements provide additional constraints on model simulated tropospheric O3 sources beyond those provided by direct comparisons of simulated and observed trace gas concentrations"*

*Answer: We modified the conclusion section by including "The factor attributions based on the projections in the factor space allow a direct evaluation of model performance in simulating source contributions to tropospheric O3 variability and its springtime increase"*

Specific comments In section 2, there is little discussion of previous GEOS-Chem work on stratospheric-tropospheric exchange (STE) of ozone. Does the model simulate reasonable STE estimates?

*Answer: We added in the text (section 2) that "The cross-tropopause O3 transport from the stratosphere is simulated in the model using a passive ozone-like tracer (Synoz) (Mclinden et al., 2000). The annual net flux is 475 Tg of O3."*

In section 2, is there is little discussion about potential temperature. Do the authors know how good GEOS potential temperature is?

*Answer: GEOS-3 meteorological fields are assimilated with atmospheric observations. For the purpose of this study, we find that most potential temperature gradient is found along latitude, which is consistent with current understanding and aircraft observations.*

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*As long as the latitudinal change is simulated by GEOS (which we show it is), potential temperature can be used as a dynamical tracer for latitudinal transport.*

In section 2, there appears to be little quantitative information about budget terms for ozone precursors, etc.

*Answer: In this paper, we cannot evaluate the global budget of ozone precursors. Therefore, the global inventory of fossil fuel NO<sub>x</sub> emissions, for example, are not discussed in the paper. Readers interested in the emission inventories used in GEOS-Chem can consult the paper by Bey et al. (2001) and other references given in the paper.*

Section 3.1, first paragraph. Why is ozone data > 100 ppb included then not included in subsequent steps? The reason needs to be clearer for the reader.

*Answer: Per suggestion from the other reviewer, we deleted Figure 3. We clarify in the text that the same result (i.e., little change by including O<sub>3</sub> data > 100 ppb) is found in other regions.*

Equation 4. Why is there no model error in this equation?

*Answer: This is essentially the same equation as equation (3) except we use the measurements in the denominator. Error term is included only in the denominator because we compute the fractional contribution of a factor to the total concentration of a species, which includes both resolved part (GF) and error (E).*

Reminder the reader what constitutes the units of 7Be.

*Answer: Added.*

Section 3.1.2. Can the authors definitively state that the overestimate in Liu et al is too large given that their model (meteorology) will be different to that used by Liu et al?

*Answer: In the paper by Liu et al., 7Be source was reduced artificially to account for excessive model transport. There is no physical basis to reduce that source by so*

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*much. We are seeing the same results for two different years, so it is unlikely that meteorological variation is large enough to account for the change of  $^7\text{Be}$  source.*

Typos in lines 24 onwards in section 4. ...last(ed) longer....compar(ed)...Despite (of) reasonable...

*Corrected.*

Tables 1-5 are very confusing. Is r the same as R? I question whether R should be in the Table at all. Surely "Factors" should be where R is?

*Corrected.*

Figure 1 looks strange to this reader. The associated caption should tell the reader that the authors have plotted only data filtered by  $^7\text{Be}$ .

*We modified the figure captions as suggested.*

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

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