

Interactive comment on “Transpacific transport of ozone pollution and the effect of recent Asian emission increases on air quality in North America: an integrated analysis using satellite, aircraft, ozone sonde, and surface observations” by et al.

et al.

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We thank the reviewer for the detailed and helpful comments and suggestions. We have addressed all the comments in our revised manuscript. Please see below for our response to each comment:

Comment: This paper examines transpacific transport of ozone by combining aircraft, satellite, and modeling in a very convincing way. It was a pleasure to read. I recommend publication with a few minor modifications.

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1. page 8150, line 17. The TES profiles are filtered out for poor sensitivity using the diagonal term of the averaging kernel below a certain threshold. One of the papers cited by the authors (Luo et al. 2007) discusses using the degree of freedom (DOF) to examine the influence of the a priori. What is the difference between the two approaches?

Response: We have revised the sentence on page 8150, line 18 to "diagonal term of the averaging kernel matrix at 681 hPa < 0.01 , corresponding to < 0.25 DOFS (Luo et al., 2007a)"

Comment: 2. page 8155. What are the implications of the model's 30% overestimate of OH for the paper? The authors only discuss the impact on CO, as it is most obvious in the comparison to aircraft and ground-based observations. Such a large overestimate would likely impact the top-down estimate of NO_x emissions (underestimate of NO_x lifetime and thus the inferred NO_x emissions could be overestimated). How would the paper conclusions regarding Asian impact on ozone over North America be affected? A brief discussion addressing these issues would be useful to the reader.

Comment: This is really not obvious. First, it is not clear that a 27% difference is significant given the accuracy of the observations. Second, the implications for NO_x emissions would depend on the OH bias in the continental boundary layer, which could be very different. Notice for example in Figure 4 that the OH bias for the C-130 data is absent in the boundary layer. In our view, this issue of reliability in the OH simulation is very much open-ended and we see little point in elaborating.

Comment: 3. page 8156, section 4 lines 17-20. It would be useful to include the percentage increase in PAN for the 2000-2006 rise in Asian anthropogenic emissions. This would allow a comparison to the observed 22% increase in PAN cited in the paper.

Comment: We added the percentage change on page 8156 so that "The 2000-2006 rise in Asian anthropogenic emissions increases the mean simulated PAN concentra-

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tions by 26 pptv (21%)."

Comment: 4. section 7.1. PAN was also measured at MBO during INTEX-B (Wolfe et al., 2007). How do the model calculations compare to observations at the site?

Comment: We now state that "PAN concentrations measured at MBO during INTEX-B have a median of 270 pptv (Wolfe et al., 2007), compared to 190 pptv in GEOS-Chem, with fair agreement in temporal patterns between model and observations ($r = 0.56$)."

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 8143, 2008.

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