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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interannual variability of tropospheric composition: the influence of changes in emissions, meteorology and clouds" *by* A. Voulgarakis et al.

A. Voulgarakis et al.

avoulgarakis@giss.nasa.gov

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We would like to thank Referee 2 for reviewing our manuscript. Below we reply to the issues raised:

1) We agree that it is very important to validate the model against observations as much as possible. In the current manuscript, we have compared with measurements from GOME which show that our model can simulate general features of pollution levels and variability well. There are inevitably some discrepancies, as might be expected with any modeland these are highlighted in the paper.

We note that Figure 1 is correctly labeled. As we discuss in the text, in the model the

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Interactive Discussion

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interannual variability of biomass burning emissions over Indonesia is overestimated. So, the high tropical emissions are not missing from our simulation. For Europe (one of the two regions on which we are specifically focusing), where continuous ozone records are available for the period of interest, we validated against Zugspitze data and found a remarkably good comparison. Following the comments, we compared at one more site in Europe (Donon), representative of lower altitudes, and found a quite good agreement again. This plot has now been included in the manuscript along with the plot for Zugspitze. A very detailed validation of modeled ozone against observations is beyond the scope of this study.

We now note explicitly that model performance has been evaluated against global CO and ozone observations in Voulgarakis et al. (2009). Following these evaluation steps, our purpose in this study has been to explore a variety of issues around our model's sensitivity.

2) The suggestion that GOME standard deviation is too low may have been based on an assumption that Figure 1 is incorrectly labeled which is not in fact the case. In the panel showing GOME standard deviation, it is clear that the standard deviation is not low over China. We also note that the most dramatic increases in NO_2 over China occurred after 2000 (e.g. Zhang et al., 2007).

3) As noted earlier, we have added an additional site for Europe (selected to be at a lower altitude). The comparison demonstrates that the model performs well for both sites examined.

4) Europe and Indonesia are chosen as examples to highlight specific aspects of inter-annual variablity. We have not attempted to catalogue all variations everywhere across the globe. Figure 1 (c,d) shows that these two regions are among the most interesting globally.

5) Isoprene emissions do not vary from year to year in the model and we have highlighted this caveat in the manuscript. We now also discuss possible influences of inter-

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annually varying isoprene emissions. Please, also see our reply to comment S6 of the 1st reviewer.

From the regions that we focus on, Indonesia has the highest lightning NO_x emissions. For this region, we now show a plot of the variability of lightning NO_x (Figure 4b) and discuss it in the text.

6) Our paper entitled “Clouds, photolysis and regional tropospheric ozone budgets” has a different focus: it describes a process-based study on clouds, while the manuscript reviewed here describes an impact–based study focusing on all processes (not just cloud effects) aimed at explaining observed variability. We do not feel that there is any overlap here - the papers just use the same model. The cloud focus of the first paper turns out to be important for explaining the interannual variability discussed here.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 14023, 2009.