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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Can a global model reproduce observed trends in summertime surface ozone levels?" *by* S. Koumoutsaris and I. Bey

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1. I suggest to include in the discussion (possibly in the conclusions) also regional modeling results

We agree with the reviewer and we discuss in the text that the model resolution is relatively low and this is in part the reason why the model fails to capture the large variability in O3 trend even between nearby sites. Nevertheless, we think that a global chemistry-transport model better suits the objectives of this study, notably to be able to realistically simulate the long-range (inter-continental) transport of pollutants. As suggested by the reviewer we are going to include in the discussion regional modeling

results.

2. p. 2027, line 5: I suggest to try to find more recent references for ozone trends at urban sites and at sites downwind of urban centers than those referenced by Vingarzan, 2004;

We will include additional references on ozone trends at urban sites (e.g. Jenkin et al 2008, Wang et al. 2009, Hogrefe et al. 2011, and others)

3. Fig. 3 and 4: I suggest to try to improve: It is difficult to read the very small symbols for the not significant trends and the color code is not suitable to discern trend magnitude;

We propose to add a table with the observed and simulated trends at the 5th and 95th percentiles.

4. Fig. 5 and 6: Did you check the "robustness" of the frequency distribution concerning individual sites ? Or: would the frequency distributions look much different when few sites would be excluded ? Or in other words: are the frequency distributions of the individual sites much different from the average ?

We think that the large spatial variability in European ozone trends in clearly shown in Figure 3. We have examined the influence of using subsets of sites on a logical basis e.g. based on geographical criteria such as using only mountain sites, and we found not significant different results. In the case of US, the trends are far more robust as shown in Figure 4.

5. p. 2033. Last paragr., ff: I find it very useful to derive VOC sensitivity from the numerical simulation, but I don't believe that VOC sensitivity is the same as titration (fast reaction between NO and O3);

We agree with the Reviewer that VOC sensitivity and titration are two very different things and that our sentence was confusing. We have changed the sentence to make it more clear. Jenkin (2008) found that these UK rural sites are affected by local pollution

ACPD

12, C2530–C2534, 2012

episodes from upwind sources. In coincidence with our results, he also found upward trends in the low percentiles which he attributes to reduced titration by NO, as a result of control of NOx emissions.

We also found that these sites may not be as rural as originally thought since they were showing VOC-sensitive chemical regime during several days in summer in the early 1990s. During such episodes, O3 tends to decrease with increasing NOx and the low O3 values in these sites in the beginning of the record may be in part also responsible for the observed upward O3 trends. In the later period of our study (2000-2005), such heavily polluted episodes do not occur as a result of pollution control measures. The chemical regime is normally NOx-sensitive (typical for rural sites) which is associated with O3 production during high NOx episodes.

6. p. 2035, line 22: " :::. Indicating performance issues when O3 levels are strongly influenced by background concentrations": In your discussion of the significant and sometimes large discrepancies between numerical simulations and measurements at low concentrations you stress the role of "background ozone" changes. I agree with this argument at high mountain sites but I am less convinced that this this is the only important reason for deviations at typical PBL sites: Here I think that titration could be important as well: At a "background" site NO emissions e.g. from a close road might occasionally influence ozone distributions: Even if the average is possibly well representative of the location the series might be affected by close vehicle emission;

We agree with the Reviewer and we already mention in section 4.1.1 that even though stations may be designated as rural, they can be affected by anthropogenic local pollution (see Vingarazan et al 2004). We have made this argument clearer in the revised text.

7. p. 2036, last paragr.: You might consider to site and shortly discuss the paper of P.T. Martien, and R.A. Harley, adjoint sensitivity analysis for a three dimensional photochemical model: Application to Southern California, Env. Sci. Technol., 40, (2006);

ACPD

12, C2530–C2534, 2012

We agree with this suggestion

8. p. 2040, line 4: The model overestimates the decrease in high concentrations of O3 and you list as possible reason a too strong decrease in O3 precursor emissions: Do you have independent information confirming an overestimate of the decrease in O3 precursors in the used emission inventory ? Are available primary pollutant monitoring data confirming the emission inventory changes ?

As mentioned in section 5.1, it has been suggested that the REAS emission inventory used in our study underestimate the magnitude and the trends in Asian NOx emissions (Kurokawa et al. 2009). In addition, NO2 retrievals from satellite measurements suggest an overestimate of the western U.S urban emissions by models (Kim et al. 2009).

References

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ACPD

12, C2530–C2534, 2012

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