Atmos. Chem. Phys. Discuss., 4, S3978–S3981, 2004
www.atmos-chem-phys.org/acpd/4/S3978/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of air pollutant and methane emission controls on tropospheric ozone and radiative forcing: CTM calculations for the period 1990–2030" *by* F. Dentener et al.

F. Dentener et al.

Received and published: 11 May 2005

In addition to the general comments made previously, we reply here to reviewer #2. The manuscript has been somewhat re-organized to accommodate the remarks of the reviewers, as well as informal comments of readers. Most notably: we have re-organized the emission section so that parts of the discussion have been moved to section 2; and -for the interested reader - to the appendices. We have moved the discussion on radiative forcing from the discussion section to section 6. We have reduced the amount of surface ozone plots. We refer in the text to the original ACPD paper for the NH sum-

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mer plots. We have performed some additional studies to convince ourselves and the reviewers of the sensitivity of certain processes (ships and NMVOCs).

Reply to the detailed comments

1. S3167 l.21: Emission controls are now mentioned in the abstract, also in the context of IPCC SRES that rather made BAU assumption regarding emission controls. It is mentioned again in the conclusions.

2. S3168 l. 8. We added to the conclusions: “We show here the importance of attaining at least the CLE scenario, although we realize that there may a significant lag between legislation and implementation of emission controls in developing countries”. In the introduction we added some references: Cifuentes et al. (2001), He et al. (2002), and Murray et al. (2001).

3. S3168 l.15 Indeed we are also somewhat surprised by the large differences in the model responses regarding biomass burning in the 1990s (new Figure 10); although on the other hand the delta ozone in Africa and Latin America (new Figure 11a) do not show strong differences. Looking at the monthly biomass burning response, it shows that TM3 shows rather weak biomass burning peaks of 45-50 ppbv over equatorial Africa in February, and 55 ppbv in August over southern Africa. The corresponding values for STOCHEM are over 80 ppbv for equatorial Africa, and like TM3, 55-60 ppbv over southern Africa. We have checked from budget analysis that both models have implemented the same biomass burning emission totals. One difference between STOCHEM and TM3 is the geographical and temporal distribution applied to the biomass burning emissions (an updated Hao inventory for TM3, and Cooke and Wilson (1996) for STOCHEM), which is what we meant with ‘the implementation of biomass burning emissions in our models’. At this stage we can only say that the TM3 model sensitivity to biomass burning emissions seems rather low (but consistent with the also low O3 results presented in the study of Marufu et al, JGR, 2001), and that the STOCHEM results, especially in equatorial Africa show quite a high sensitivity. Further

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analysis would require dedicated inter comparison studies of biomass burning, which is clearly not the scope of this study. We still think the difference in mixing characteristics of both models may play a role in these differences on which we remark the following. Both models have been successfully tested and partially tuned with Radon-222 measurements. However, as the reviewer is probably aware of, there are no Radon measurements in the tropics. This leaves our models, as well as other models, rather unconstrained with regard to the effects of boundary layer mixing and deep convection.

4. S3168 l.26 Unfortunately the IIASA group did not have the NMVOC inventory and scenarios available when this work was initiated. Indeed globally the anthropogenic (non biomass burning) source of NMVOC and CO are quite different. According to EDGAR3.2, 95 % of the CO sources are related to combustion processes; whereas 60 % of the NMVOC emissions are from combustion; the remainder coming from solvent use, industrial processes, oil and gas production. In our simple scaling approach we did not include any explicit knowledge on emission reduction policies for NMVOC emissions from non-combustion sources; NMVOC emissions in recent years seem to be relatively constant, in accordance with the assumptions made in the CLE scenario. Likewise we think it is possible that further emission control technologies lead to NMVOC emission reductions along the lines of MFR. It is further noteworthy that the IPCC SRES increases of NMVOC relative to those of CO are quite similar; although it is not possible to track the underlying assumptions. Regarding the impacts, we expected mainly some feedbacks on OH; when NMVOC emission pathways would be different. They will however be relatively small compared to the impacts of the assumptions made for CO and CH4. Further to this, we tested the sensitivity of the ozone calculations (without CH4 feedback) to the NMVOC assumptions. To do so, we repeated the simulation of the year 2030; but now with NMVOC emissions of the year 2000. The average surface ozone levels decreased by 0.16 ppbv, with peak values of 1.0 to 1.5 over China and India. These values clearly present an upper limit of the effect of the NMVOC scenario assumptions on the larger scales. However, as we also discuss in section 7, somewhat larger effects may be expected using higher resolution

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models.

5. S3169 l. 3 Changed into latitude bands

6. S3168 l. 5 To estimate which part of the ozone increase is due to ships emissions, and which part is due to increasing CH4-O3 background chemistry, we performed another sensitivity study for the year 2030, but using ship emissions of 2000. The difference in model results is on average -0.23 ppbv; with peak values of -1.0 to -1.5 over the North Atlantic and Indian Ocean. An increase of ozone was seen over the Baltic Sea. We added a sentence about this in section 5.2.

7. S3168 l. 10 changed at end of section 7.1

8. S3168 l. 16 changed in Table 1.

9. S3169 l. 17 We meant that in the Northern Hemisphere high latitudes biomass burning was probably rather high. We removed the remark altogether, since it is not very relevant for this paper.

10. S3169 b-5; Figure 7 (new Figure 4) has been updated; and runs to 2003. MFR results are no longer included. Coordinates added.

11. S2170 l. 1 we tried a 4x2 figure option; but the ACP page format would make the figures very small. We decided not to reduce this figure.

All further technical corrections have been implemented.

We thank reviewer #2 for the detailed comments which helped to improve the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 8471, 2004.