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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.

Anonymous Referee #2

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General Comments

This is an excellent paper that explores the impacts of future emissions of air pollutants and methane on tropospheric ozone and radiative forcing using two new sets of emission scenarios for the period 1990-2030 and two global chemical transport models. It describes the development and implementation of new emissions data reflecting greater pollutant controls, and investigates the evolution of ozone, CO and methane, with particular emphasis on changes in surface ozone and on radiative forcing. The pa-

per concludes that control of methane would provide an effective way of reducing both surface ozone and radiative forcing, supporting and better quantifying earlier work by other authors. It also provides a reliable revision of earlier estimates of radiative forcing from the IPCC Third Assessment Report, and suggests that a reduction in radiative forcing and in surface ozone would be possible by 2030 given optimal application of current emission control technologies.

This paper makes significant contributions to our understanding of the possible evolution of tropospheric composition and its impacts, and the degree to which current policy and control technologies can affect it. Particularly valuable are (1) the introduction of two new emission scenarios that show less-severe increases in pollutant emissions than the commonly-used SRES scenarios generated for the IPCC due to stricter emission controls, (2) the use of two unrelated models using different dynamical and chemical approaches to investigate future composition changes, and (3) comparison of these results with those derived by earlier studies. The paper is very well written and is clearly laid out. It should be suitable for publication in ACP once the minor comments below have been suitably addressed.

The results are of sufficient general and political interest that ACP and the involved institutions might consider preparing a press release to disseminate these valuable results to a wider audience.

Specific Comments

It is refreshing to see model studies of future ozone and climate using less pessimistic emission scenarios that those provided by SRES. However, the emphasis on emissions controls could be stressed more clearly in a number of places; it is not mentioned in the first sentence of the abstract, for example. This will help to remind readers that the CLE scenario is also an "optimistic" control scenario, and that "Current Legislation" reflects not the way things currently are ("Business as usual" as the casual reader might assume), but the active implementation of policies that in many cases have not

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been previously applied. The emphasis on controls should also be highlighted in the conclusions, where the reader should be reminded that the increases in ozone and radiative forcing with the CLE scenario occur despite adherence to current, stricter legislation than was in place prior to 2000. Comparison with results using the IPCC SRES scenarios again in the conclusions would be useful to highlight the benefits and importance of attempting to follow at least the CLE path.

The uncertainty in the attainability of the CLE scenario is addressed in the discussion section (page 8496, line 7). It would be helpful to remind readers here that emission controls are rarely as well implemented in developing countries (where emissions are rising fastest), and that it is highly likely that there would be a significant lag between increased emissions and effective control. Public concern about air quality in developing countries is high (references here would strengthen this point), but remains secondary to concern about economic development. While there may be reasonable compliance by 2030, as suggested, emissions in 2010 and 2020 may be somewhat larger, and this would have a significant impact on the predicted evolution of ozone and methane.

The differences in model simulations of surface ozone are not explored well. Figure 7 shows reasonable model agreement at most of the selected locations, but Figure 12 shows some striking differences at the surface, notably over biomass burning regions and over the ocean. This is acknowledged in the text (page 8490, lines 26-28) but is not adequately explained. Is the surface distribution of ozone representative of the lower troposphere, or is it a surface artifact of the PBL treatment? I suspect that the Louis scheme in TM3 is more confining of surface emissions, leading to trapping of shipping emissions over the ocean but isolation of the surface over biomass burning regions where emissions may be injected at a higher altitude. What are the implications of this for assessment of surface ozone changes - are these PBL treatments really equally suitable?

Page 8481, line 4: the assumption that NMVOC emissions closely track CO may be reasonable for transport or combustion sources, but is questionable for other sources

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(solvent usage, oil/gas production, etc). How sensitive are the results likely to be to this assumption?

Page 8484, line 7: section 4 would be easier to understand if the "regions" used in the comparisons were described as "latitude bands". See also the captions to Figs 8-10.

Page 8491, line 11: increases in ozone of 4-6 ppbv over the oceans are somewhat large to attribute solely to ship emissions, which have increased by less than 60% over the period; it seems likely that background increases at Northern mid-latitudes also make a significant contribution, and are likely to be more visible here than over polluted continental regions.

Page 8497, line 10: "...CH4 emissions increase as much as the anthropogenic emissions reductions among scenarios" This is unclear and needs to be rephrased; I assume that the intended meaning is "...the increase in natural CH4 emissions more than compensates for the reduction in anthropogenic emissions".

The first of the 2 columns under the 'Simulation model' column label in Table 1 is unnecessary. The MFR-CH4 and MFR-pol labels should be moved to the 'Emissions scenario' column.

The doubts about CO biomass burning emissions expressed in the caption to Figure 5 are not in accordance with the explanation of the source given in section 2.2, which suggests that the source strength, while high, is reasonable. Some additional explanation is required to reconcile these views.

Figure 7 is cramped and difficult to see clearly. It would be helpful if the individual panels were enlarged and the space between them reduced, and if the Y-axis limits were unified at 0-80 ppbv. It would be clearer if only the period 1990-2003 was shown, though I appreciate that the comparison of CLE and MFR up to 2010 is not shown elsewhere. It would also be clearer to present station locations as xx N, xx E, xxx m rather than lat/lon/alt. Ryori is at 141 E, and Mauna Loa is at -155 E.

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Figure 8 would be easier to interpret if the two parts of the figure were combined. I would recommend a 4x2 arrangement of plots, with the two rows showing 400 and 800 hPa respectively, and the 4 columns spanning the latitude bands pole to pole. With all plots on a 0-80 ppbv scale, a direct comparison of the latitudinal and altitudinal variations in ozone would then be possible.

Technical corrections

Page 8486, line 26: remove "realistic"

Page 8490, line 26: remove "present"

Page 8498, line 25: insert "from" before 1750 ppbv.

Page 8499, line 5: "Table 4" should read "Table 3"

Page 8516, caption: typo "emissions"

Figs 12 and 13: "decadal average" should be sufficient in the captions; "decadal and annual average" is unnecessary.

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