Atmos. Chem. Phys. Discuss., 4, S3411–S3413, 2004
www.atmos-chem-phys.org/acpd/4/S3411/
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.

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Received and published: 25 January 2005

Both of the posted anonymous reviews provide in-depth and very useful reviews of this paper. Having read the paper carefully because of my history and interest in this topic, I would like to second those reviews and add my own personal view as to the importance of this paper.

Many of us as IPCC contributors to the TAR's atmospheric chemistry sections have

lived with the IPCC SRES emisisons scenarios as 'the' projections of reactive gases (CH4, CO, NOx, VOC) that drive global tropospheric ozone levels. We worked with the SRES authors since the beginning of the TAR (Dec 1998) to acquire well defined scenarios for the atmospheric chemistry projections. It was amazing to many of us that we were able to use the SRES scenarios to project future levels of CH4 and O3. It was not until after the TAR results came out, however, that the SRES authors, among others, argued that such high emissions of NOx and CO would not be tolerated because of local air quality concerns.

This paper presents two, very important, new projections for emissions of O3 precursors: (1) the Current Legislation scenario (CLE), and (2) a Maximum technically Feasible Reduction (MFR) scenario. Both of these optimistic scenarios provide an alternative to the more aggressive SRES emissions. While the SRES are viewed as pessimistic and unrealistic, CLE/MFR can and should be viewed as optimistic. To avoid the SRES paths in favor of either the CLE or MFR scenarios proposed here requires environmental action and cannot be viewed as any more likely (even the CLE scneario admits that future controls are 'anticipated'). Nevertheless, at least these two emission scenarios present a necessary balance to the SRES.

In terms of new results, the idea that NOx is an indirect greenhouse gas probably originated with Derwent's original work that the 1990 IPCC was based on. Since then, the 1999 IPCC Aviation assessment demonstrated how aviation NOx generated cancelling climate forcing (CH4 decreases paired with O3 increases). More studies of this cancellation between long and short-term climate forcing followed (e.g. Wild et al), and between CH4 and surface air quality (Fiore et al). This paper, however, demonstrates that for realistic scenarios it is vital to control CH4 and NOx together, since the projected NOx controls will only enhance the CH4 growth. This is an important lesson directed at the policy community, and it is a well written scientific paper.

The simulation of the scenarios with the TM3 and STOCHEM models is well done and shows the diversity in atmospheric chemistry modeling today. It appears that - in

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terms of uncertainty in modeling a given scenario - we are no better off than the TAR. I would have liked to seen a bit more synthesis on what are the robust results from the two models (e.g., integrated O3 changes) rather than as many color plots showing the geographic difference (If we are to see a geographic similarity in patterns of O3 in figures 12-15, I did not).

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