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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulations of preindustrial, present-day, and 2100 conditions in the NASA GISS composition and climate model G-PUCCINI" *by* D. T. Shindell et al.

Anonymous Referee #5

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General Comments

The paper on simulations with a chemistry climate model (CCM) would fit into the focus of ACP. However, the results presented are not sound enough to support the conclusions given in the abstract. The model for the stratospheric part appears to be too premature (that is even stated in model description!). It needs substantial improvement to be acceptable for ACP. This holds also for the way the paper is presented.

Specific Comments

If there is no seasonal cycle in midlatitude and high latitude total ozone in the northern hemisphere and major features of stratospheric chemistry (e.g. PSC chemistry) and dynamics are missing (e.g. the Brewer Dobson circulation and the descent in polar vortices) it is invalid to estimate stratosphere troposphere exchange and its changes due to climate feedback. That the results for annual average are not too far off from other papers (e.g. Stevenson et al, J. Geophys. Res. 2006) is just fortuitious or due to canceling errors. Also, it should be known from the literature that the very low vertical resolution is not appropriate for stratospheric dynamics. The simulations should be repeated with a better vertical resolution.

In the text a lot of less important details are mentioned while the major problems are often not mentioned or hidden in formulations like 'reasonably well'. A clear definition how the simulations are carried out is also missing. The list of reactions in Table 2 should normally go into an Appendix, however here it reveals that reactions important for budgets of stratospheric ozone and nitrogen are missing while negligible or missleading ones are included. The figures are often of poor quality or missleading. They contain a lot of comparisons with observations, the model results are however often so far off from reality that any interpretation is questionable (e.g. ozone above the high latitude tropopause in Figure 5 or HNO_3 in Figure 10). Sometimes also the caption does not fit to the figure. Generally the results are worse than with the tropospheric model only presented in ACP 3 years earlier.

Details

Abstract: It is in contradiction to the presented figures that it is claimed that stratospheric gases are 'well-simulated'. The scenarios need a reference.

Introduction: References to other CCMs are missing.

Section 2.1: A reference to Shindell et al, 2001, J. Geophys. Res. (hidden in Shindell et al 2003) is missing for understanding the set of chemical equations in Table 2. The reaction N+NO, which is an important sink for NOx in the upper stratosphere, is missing. PSCs are important for climate feedbacks even if chlorine is wrong due to

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missing descent in the polar vortices. Some details on scavenging by clouds should be mentioned in this section also since there appears to be problem with HNO_3 (Figure 10). The part on mineral dust is interesting but of minor importance here.

Section 2.3: The text on scavenging should be moved to section 2.1. A more detailed analysis would be useful. Is 'nudging' applied in one of the presented simulations?

Section 3 or earlier: It should be clearly said for how many years the model is integrated and if it is in the 'time-slice' mode or transient. How is the ocean coupled, what means 'responsive' here?

Section 3.1: Stratospheric dynamics is not state of the art. Polar vortices are absent in Figure 3. Figure 2 gives the impression that the circulation at higher latitudes is not shown because it is wrong. Also the caption is too short. To capture the Brewer-Dobson-circulation and its driving forces a better vertical resolution is essential even if you don't like to resolve the Quasi-Biennial Oscillation.

Section 3.2: The most serious flaw is the missing seasonal cycle of total ozone. It is also not fair to try to hide the obvious problem with the shallow ozone hole by including the very exceptional year 2002 with the southern hemispheric major warming in the short timeseries of observations in Figure 4. Ozone in Figure 5 points to severe problems with the tropopause in high northern latitudes and again to the missing descent. This is also consistent with the worse results in Figure 7 when the stratosphere is included. Near the tropopause there is not a 'good agreement' as stated in the text. If 'percentage difference' would be included in Figure 5 it would be obvious that the model often strongly disagrees with observations. Is a factor of 4 or more (out of range) difference at 500hPa in Resolute (Figure 7) good agreement?

Section 3.3: There is still a large difference in HNO_3 in the troposphere that requires more explanation. For the stratosphere $NO+NO_2+2N_2O_5$ should be compared with HALOE sunset $NO+NO_2$.

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Section 3.5: More details on the hydrocarbon scheme (or solver) would be useful.

Section 4.1: Equilibrium for ocean and/or for longlived chemical species? It is odd to assume solar minimum instead of average conditions.

Section 4.2: The cooling has the largest effect on chemistry via $O+O_3$.

Section 4.3.1: A discussion on water vapor changes should be included. Not all models predict an increase in stratospheric water vapor because climate change can cause a more efficient cold trap at the tropical tropopause. Observations indicate that this might be dominant.

Table 1: CFC-11 is not the most abundant CFC, CFC-12 would be a better approach.

Table 4: Here the paper is rather honest, however, if ozone is off by almost 50% in the region most critical for radiative forcing, I doubt that it is a useful tool for assessments.

Technical Comments

Figure 14: Clearer definition of sign needed ((PD-PI)/PD).

Figure 16, 18, 22: The use of scaling factors in confusing, please change. Reduce number of contours (e.g. use logarithmic contour steps and say that in caption).

Figure 20: Please use log(pressure) axis, replace at least the odd labeling.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4795, 2006.

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