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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.

D. T. Shindell et al.

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The reviewer raises concerns that the model is too premature for publication. We believe that like any model, ours shows some areas where the simulation performs well and others where there are substantial biases. In the present case, we believe that much of the bias in high latitude ozone stems from problems in transport in the underlying GCM rather than in the photochemistry. As such, we believe that the new photochemistry for the stratosphere that is being presented here is doing a reasonable job. Additionally, we believe that the tropospheric portion of the GCM is performing well and that hence a description of the performance of the chemistry package in that region is warranted as this is rather different than prior versions (liquid tracer budget added, heterogeneous chemistry on dust, etc). We stress here, and in the text, that the

evaluation is designed to inform future experiments (and future model development). The results indicate that unfortunately, as the reviewer states, the stratospheric portion has substantial flaws and hence we wrote in the paper that the model was premature for use in studying high latitude ozone depletion, for example. However, in the tropics and parts of the mid-latitudes, where photochemistry typically dominates over transport, the model appears to do a fairly good job of simulating stratospheric constituents, and hence the model could be used to study those regions. We show in the revision that STE is dominantly an extrapolar phenomenon, and hence we feel that the model's results for STE are also useful. Additionally, the simulation in the troposphere compares well with most data, so that the full-chemistry simulation produces plausible results in many areas and hence can be used for many applications. We feel that the paper is appropriate to document the status of the model, including the flaws present in the current configuration, and that the presentation makes clear both the strengths and weaknesses of the model.

The coarse vertical resolution is of course a limitation. However, we are trying to perform simulations incorporating a large range of physical phenomenon, including a responsive ocean and coupled chemistry throughout the troposphere and stratosphere. Computational resource limitations require a trade-off between physics and resolution, so that we have more physical phenomenon than are often included in CCMs but less resolution. However, we will certainly use higher resolution as our computer power increases, and are currently testing a 40-layer version of the GCM. We note that while the horizontal mixing is certainly too rapid, reducing the seasonality of high latitude ozone, large-scale features such as the Brewer-Dobson circulation are simulated by the model, and the comparison with sondes (Figure 7) shows that there is in fact some seasonality, though it is too small in amplitude.

We have reorganized the description of the model setup (new section 2.4) and corrected a minor (though admittedly important) pair of typos in the reaction list (Table 2). As the reviewer points out, the results are often worse than with the tropospheric

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chemistry only version. However, that is to be expected as we are attempting a more difficult problem now. Using boundary conditions prescribed to observations usually guarantees a better simulation than calculating them, but precludes interactions with the prescribed regions. When we prescribe the stratosphere to exactly match observations, as in the tropospheric chemistry only version, then of course areas strongly influenced by the stratosphere look good. A stratospheric chemistry model can only make this worse. However, as discussed in the paper, while observations can give us a 'perfect' climatology, they don't give us the future (or the preindustrial). Hence the more complete model is a useful tool even if it doesn't do as good a job.

Specific comments:

1) We stated specifically that the gases were well-simulated in the extrapolar tropopause region, while high-latitudes showed substantial biases.

2) References to other CCMs were added.

3) Reference to Shindell et al 2001 added. The reactions 47-49 in the list erroneously all had the same reactants due to a faulty cut-and-paste. In fact 48 should have been N+NO2 and 49 N+NO, though the products were correct. This has now been corrected, and the error was only in the listing for the paper, while the model has the correct reactions. PSCs will be improved in later model versions, and we agree that these could be important for climate. Scavenging by clouds in the troposphere has been described extensively in the referenced Koch et al papers.

4) The text on scavenging was moved as suggested. We also note now, in the new section 2.4 on experimental setup, that nudging was not applied in these simulations.

5) Section 2.4 also now states how long the model was spun-up for and how long it was then integrated and that the runs were time-slice. The 'responsive' mixed-layer ocean is also described more clearly.

6) The circulation at higher latitudes was not shown as the UKMO data we had did

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not go to higher latitudes. We had followed Butchart et al., 2006, but in response to this comment (and that of another reviewer) have revised Figure 2 to extend the plot to the whole globe. We agree that the stratospheric dynamics is not state-of-the-art, and clearly state this in this section (3.1) and also attempt to show the model biases quite clearly. We hope the future improvements to the stratospheric dynamics will greatly improve these aspects of the simulation.

7) We have already discussed the seasonality issue. The reviewer is incorrect in suggesting we were trying to hide the shallow ozone hole by including 2002. We showed our ozone field and its biases and stated quite clearly that the high latitude distributions were not well reproduced. We used data extending to recent years to best match the late 1990s chlorine loading used in our model. The single low year 2002 has only a marginal influence on the total column. The reviewer points out some of the areas of disagreement with observations that we also pointed out in the text for high latitude ozone. We agree that near the tropopause there is not always good agreement, and hence we had written explicitly that the agreement was reasonably good only in the extrapolar regions. Though we never claimed there was good agreement at Resolute, the reviewer is correct in noting the problem at 500 hPa there. A scripting error caused the wrong level to be plotted here, which has now been fixed. 8) The bias in tropospheric HNO3 has in fact been greatly reduced in many areas. It does persist in some regions, and we now discuss this further (especially for the SH during biomass burning season). This is a common problem in tropospheric chemistry models.

9) We've added a description of the chemical solver. The hydrocarbon scheme is discussed at length in Shindell et al., 2003.

10) We've clarified that equilibrium refers to both climate and the chemical composition. Solar minimum was used for computational savings as we planned to perform solar maximum runs as well (and this is a computationally expensive model), and we now note this rationale and that the solar effect itself is small enough that it has little impact on the climatology.

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11) Agreed, we now note the importance of the O+O3 reaction in the text.

12) We now show that most of the water vapor change aloft is from methane. However, there is an increase of ˜1 ppmv even near the tropical tropopause, reflecting an increased temperature at the stratospheric entry in our model.

13) CFC-11 was used primarily as a proxy to set the total anthropogenic chlorine loading. It only matters in the sense that the decomposition rate follows the properties of CFC-11. This seems to yield a reasonable total chlorine distribution.

14) In fact, the ozone bias compared with sondes is 4% at 125 hPa, and 0% at 200 hPa, and the biases are the worst at high-latitudes. Since the most crucial area for radiative forcing is the extrapolar tropopause region, we feel that it is justified to say that the model simulates ozone reasonably well in this area.

Technical comments: We've added a sign definition to the Figure 14 (now 13) caption. We've remove the scale factors in Figures 16 and 18 (now 15 and 17). We've fixed the odd labeling in Figure 20 (now 19).

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