Atmos. Chem. Phys. Discuss., 6, S3110–S3113, 2006
www.atmos-chem-phys-discuss.net/6/S3110/2006/

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.

Received and published: 13 September 2006

We thank the reviewer for their many specific suggestions on improving our paper. We believe the paper is clearer and more informative following the revision along the suggested lines.

1) We've added references to prior stratospheric and tropospheric chemical modeling in GCMs, using the Austin et al paper that included many stratospheric models and the Stevenson et al paper that included many tropospheric ones, and the recent Dameris et al paper that had chemistry in both the troposphere and lower stratosphere.

2) We've revised the description following these suggestions. We now add a description of the chemical solver. Chemistry in the stratosphere and troposphere are calcu-

lated in the same way to allow maximum flexibility in simulating perturbations to the atmosphere. Oxygen photolysis is calculated with Fast-J2, which is used for all reactions except water and NO, as written. Lyman-alpha is not yet included, and this is perhaps a reason for some of the biases in mesospheric water. This will be included in the next model version, as we now note in the text. Table 2 has been corrected (reactions 48 and 49). We agree that the treatment of PSCs is oversimplified, but this is not as relevant here due to the substantial biases in the high-latitude ozone simulation. In the future we will run with higher resolution and hope to improve the gravity-wave parameterization, and once the high latitude ozone is fixed we will then implement a more realistic PSC model. Table 3 has been clarified so that the evaluation runs are clearly separated from the climate runs. In the latter, methane is prescribed, while for the evaluation against observations, methane is of course calculated. The revised Table also clarifies which runs had responsive oceans, and which fixed SSTs. The model setup information, which indeed was scattered, has now been grouped into a new section 2.4 'experimental setup for model evaluation' which compliments the revised Table 3 and the existing section 4.1 on the setup for the climate runs. The bromine source is also now specified.

3) The reviewer points out that the climatologies of wind and temperature, for example, in the stratosphere evaluated in the Schmidt et al. GCM paper will be somewhat different in the model with interactive chemistry. This is true, however the differences are small as the ozone biases are largest in the polar winter, when they do not affect SW radiation. As the differences relative to the model runs with satellite ozone climatology (as in Schmidt et al) are generally small, we did not feel it was worthwhile to include these in an already long paper. Again with length in mind, we have not added in additional plots of streamfunction or meridional winds. We have, however, extended our plot of residual vertical velocity to higher latitudes, as suggested. We do not believe that the model's circulation problems reflect a vertical transport that is too slow, but rather horizontal mixing that is too fast. This seems clear in the age-of-air diagnostics, and the comparisons between simulated and observed long-lived species. We believe

ACPD
6, S3110–S3113, 2006

EGU

this stems from the gravity-wave parameterization and the coarse vertical resolution, rather than flaws in the planetary waves that might be seen in latitude-longitude plots of ozone. Work is in progress to test and improve these aspects of the model. We have revised the reference to the Randel and Wu trends to note they're SAGE supplemented with ozonesondes. We do not believe that the variations in ozone induced by solar cycle variations should have much effect on the long-term ozone trend, as they are small and they average out over the cycle. Volcanic forcing has substantial short-term effects, but again the long-term impact seems minimal. Both issues have been addressed in the literature. We thank the reviewer for noting the discrepancy between the 1979 conditions and 1979 CFC loading. Our run was an equilibrium simulation, and we had accounted for the lag between the surface and the stratosphere by using a CFC loading from ~1973, but erroneously listed the 1979 value in Table 3. This has now been corrected. While we did not want to lengthen the paper by adding figures, we did follow the reviewer's suggestion to shorten 3.5 on CO and 3.6 on sulfate (we cut this one entirely), and also reduced 3.3 on NOx deposition. All these had been discussed elsewhere.

4) The new section 2.4 covers the setup for the evaluation runs, while 4.1 covers the climate runs. Table 3 list both groups, but separately, making the setups all clear. Run length (spin-up and analysis) is now given where it was absent. The greater upper stratospheric cooling in the A2 emissions plus climate run in comparison with the A2 climate-only run is indeed due to water, as the reviewer suggests. We stated that methane was responsible, but were not clear that we meant this was because it produces water high up, and not that it was the radiative effect of methane. The increase in water is only ~1 ppmv in the climate-only run, while it is more than 4 ppmv in the climate+emissions run. Since methane increase by ~2 ppmv, and yields 2 water per methane, an increase of 3-4 ppmv water due to methane oxidation makes sense. This water causes substantial local cooling, accounting for the difference in temperature responses. This is now discussed explicitly in section 4.3.2. We've rewritten the portion of the text discussing the 'super-recovery' of ozone (no longer in 4.3.3, which has been

ACPD

6, S3110–S3113, 2006

cut, but now in 4.3.1) to clarify the writing.

5) We've altered the statement about the quality of the ozone column simulation to reflect the model's limitations more clearly.

The technical corrections to the text were implemented. For the figures, we do not believe it is possible to fit larger versions into the text, but we note that the online versions can be enlarged by the reader.

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