Atmos. Chem. Phys. Discuss., 6, S1798–S1802, 2006
www.atmos-chem-phys-discuss.net/6/S1798/2006/

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.

Anonymous Referee #4

Received and published: 13 July 2006

General comments

The manuscript describes a new version of the GISS CCM (composition and climate model), which includes tropospheric as well as stratospheric chemistry, and results of the past, present and future climate and atmospheric chemical state simulations with this model. In general, the manuscript is well written and contains many interesting findings concerning the evolution of the climate and chemical composition of the atmosphere and stratosphere-troposphere exchange (STE). However, the quality of the stratospheric part of the model is rather low, therefore the obtained results looks doubtful. Anyway, I think that the manuscript can be published. However there are

still several issues, which should be clarified or explained in more details before the manuscript is ready for publication.

Specific comments

1. Introduction

The introduction contains no reference to the previous papers describing CCM development. I am not aware of any CCM with full description of stratospheric and tropospheric chemistry, but there were many useful papers describing the progress in the development of CCM with tropospheric or stratospheric chemistry.

2. Model description

I would add to the section 2.1 a brief description of the applied chemical solver and treatment of gas transport. It would be interesting to learn how the stratospheric and tropospheric chemical routines (I presume they differ by species and reactions) are coupled. From the model description it is not clear whether the oxygen photolysis was calculated with Fast-J2 or some special parameterization was used. Did you include the photolysis in Lyman-alpha? Without it the water vapor in the mesosphere will be most likely overestimated. In the Table 2 some important chemical reactions are missing (or wrongly defined). I would recommend to check and to correct Table 2. Parameterization of the PSC is oversimplified (temperature thresholds for PSC-1, 2). There is no explanation at all how the sedimentation was parameterized. Given that some important part of the past and future climate changes (i.e. the intensity of the southern polar vortex) depends on the ozone depletion due to heterogeneous chemistry over the polar areas, it would be useful to discuss how it affects the reality of the ozone depletion. Could this simple approach be responsible for the absence of the ozone hole (total column ozone is always above 200 D.U.) shown in Figure 4? Table 3 is confusing for the reader. The authors defined in the Table 3 CH4 mixing ratio, but in the text the authors mentioned that these values have not been used in the model. Then, it is better to eliminate them from the table and to show better what source of

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bromines was applied. Obviously, bromine group is presented in the Table 1, but the source was not described. The authors also stated in the Table 3 that the ocean is responsive, however in the text (page 4801, 4803) the authors said that climatological SST has been used. In the present version the information about the model set-up is scattered over the paper and it is hard to clearly understand what experiments were performed. I would suggest to introduce a paragraph describing in details all performed model runs, their length, initialization and experimental set-up.

3. Model evaluation

Schmidt et al. (2006) validated many quantities simulated with GISS GCM using climatological ozone. Because the evaluated model version has different ozone distribution it is necessary to show and analyze at least the temperature and zonal wind fields obtained from the model version under consideration. The residual vertical velocity is shown in Figure 2 only for 40oN-40oS. Would it be possible to extend the figure to cover middle and high latitudes? It would help to understand the problem with total ozone fields described later on. Probably it makes sense even to show the structure of the BD circulation (stream function or residual meridional velocities, for example) to support the author's statement about the strength of the BD circulation and the causes of the disagreement between simulated and observed distributions of the long-lived species. It is important to understand why the model failed to simulate main climatological features of the total column ozone (winter-spring ozone build up and spring time maximum in the Southern and Northern hemispheres). To clarify the issue I suggest adding total ozone maps for March (Northern hemisphere) and October (Southern hemisphere) and comparing the simulated geographical distribution with observational data. It would be very interesting if the authors could explain why the vertical transport in the model is too slow. This point is also important because one of the STE (central point of the paper) depends on the vertical transport, and substantial problems with this quantity diminish the value of the obtained results. It should be mentioned that the observations presented in the Figure 6 are not pure SAGE data. Because SAGE is

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not measuring ozone over high latitudes, the data from ozone-sondes were also used. The authors should also comment on the potential importance of the solar cycle and volcanic eruptions, which are presented in the data, but not included in the simulation. What was the length of the 1979 run? If the model was ran for 20-40 years, then the chlorine loading in the stratosphere could reach ˜1985 level. Could the authors comment on the potential implications of this time shift for the magnitude of the simulated ozone trends? Additional space for the proposed figures and analyses can be compensated by the elimination or reduction of several chapters (3.5, 3.6 for example), which are less important and already published elsewhere.

4. Climate change simulations

As I proposed above the experimental set-up should be described earlier. It should be also stated what was exactly the length of the different runs. The temperature changes in the upper stratosphere for A2 simulations are puzzling. I think that substantial ozone increase (up to 60%) in the upper stratosphere for the A2 climate/composition run should partially compensate the cooling due to CO2 increase, therefore the cooling for A2 climate only case should be more pronounced. However, the model response is opposite (-8K for A2 climate and 12K for A2 climate/composition). I do not think that the author's explanation is satisfactory, because the influence of the other gases (CH4 for example) should be rather small in the upper stratosphere, where its mixing ratio is much smaller than near surface. Could the authors provide more discussion on this? Could it be related to different stratospheric water vapor in these simulations? The first paragraph of the section 4.3.3 is not clear. If the there is more than full "recovery", why the ozone layer will not return to its earlier state? Please, consider rephrasing.

5. Conclusions

The authors stated, "The observed total ozone columns are well reproduced throughout the tropics and mid-latitudes". Due to absence of the total ozone maximum in the Southern hemisphere this statement looks too optimistic.

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Technical corrections

1. Page 4822, dot is missing before "Such a result implies" 2. Page 4840, Sate is a typo (should be Sato, I guess) 3. Figure 7, 8, 10 are hardly visible

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