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Interactive Comment

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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We thank the reviewer for their detailed suggestions of ways to clarify and improve the paper. We found the comments quite useful, and have revised the paper taking into account both the general and specific comments.

We have addressed the general comments about the length of the paper by shortening it considerably, removing the discussion of the alternate A1B scenario from the climate runs portion and removing much of the discussion of sulfate aerosols, CO and NOx deposition (all of which have been published elsewhere). We hope that the removal of these sections and their accompanying 4 figures has made the paper more manageable for readers. We felt it was best not to split the paper, and that a pure 'model

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documentation' paper would not be terribly interesting to many readers.

To clarify the various scenario runs we've revised Table 3 on the experimental setup of the various runs to show clearly which are the runs performed to evaluate the model against observations, and which are the climate experiments. With the removal of the two A1B runs that had different setups from their A2 analogues, the setup of the various runs is now much simpler to follow, we feel. In the text, we combined all the specific discussion of experimental setup into a new section (2.4) at the end of Section 2 (Model description).

What's new about this latest model version is the inclusion of stratospheric chemistry, as the reviewer notes. However, there are also changes to the tropospheric chemistry (e.g. heterogeneous reactions on mineral dust), and more importantly, the previous tropospheric chemistry package is now operating in the new GISS GCM, which leads to substantial changes as described in the discussion of the effects of the new liquid tracer budget, for example. We believe this is more clearly conveyed in the revised paper.

The removal of the A1B scenarios has addressed the reviewer's point about the setup of the future runs being confusing. We agreed, and now use only the simpler A2 runs with emissions and climate and A2 climate-only. This scenario was chosen to allow comparison with other models, as in the STE discussion.

In response to the reviewer's suggestion that more justification be given that the model can reasonably simulate STE, we now explicitly diagnose the portion of STE occurring at the high latitudes where the model simulation is less realistic, and show clearly that this is typically quite small. We point out specifically that in the case of PI-to-PD Antarctic ozone depletion, the high latitudes have a stronger effect on STE changes with time, so that the model results are less reliable in this instance.

As suggested, we've added the vertical layering to Figure 3 for the stratosphere.

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The GCM documentation paper (Schmidt et al., 2006) showed model temperatures and zonal winds in the stratosphere and compared with observations. The model was able to capture the basic structure of both fields in the stratosphere, though a polar cold bias during summer was present (as in many GCMs).

Specific comments:

1) The concept of effective equivalent stratospheric chlorine is widely used to describe total halogen loading. Our use of one species for chlorine is similar to this, as we in fact prescribe the amount of our ‘chlorine source gas’ to follow total anthropogenic chlorine, using the properties of CFC-11 merely to calculate its decomposition rate.

2) As noted above, we now include the stratospheric vertical layering in Figure 3. Yes, it’s fairly coarse.

3) We’ve added to the new section 2.4 on the model setup that the aerosol indirect effect, though present in the GCM, was not included in these runs (as aerosol changes were not included at all in the climate runs). This addresses the reviewer’s question, though in response to the suggestions of reviewers 1 and 4 that the aerosol portion of the text be reduced as it wasn’t that relevant, we’ve overall shortened the description of aerosols in the model.

4) The Bewer-Dobson circulation doesn’t slow actually, but increases. All values in Table 7 (residual vertical velocity) are positive (unlike Table 6, which shows ozone fluxes), indicating greater upwelling in the tropics and hence downwelling in the extratropics to balance this flux change. This is consistent with analysis of many GCMs, as in Butchart et al., 2006. However, the ozone changes were actually due to the increased flux of air containing ozone-depleting radicals rather than ozone-poor air itself. The text has now been corrected.

5) Section 4.3 is now about half its former length, and hopefully much easier to follow. Some of the larger paragraphs were broken into two, and a new subsection was added

to better group the analyses. A large part of section 4.4 on the differences between the setups of the A1B and A2 climate-only and climate plus emissions/concentration was also cut, making section 4 more readable as a whole and bringing the conclusions closer to the results sections.

6) We've added station latitudes to figure 7, and altered the y-axis limits on the bottom row as suggested. A typo in our analysis script caused the 500 hPa plot at Resolute to show the 300 hPa again for the full-chemistry run. We thank the reviewer for spotting this, which we've now corrected.

7) The changes shown in Figure 20 (now Figure 19) are the total changes diagnosed between the start and end of particular portions of the model code. Thus by definition the total is a linear sum of these terms. The changes include all feedback processes, but it is not possible to clearly diagnose these from the total changes saved by the model for a single simulation (feedbacks could be identified only via multiple model runs). We now note that the total change is the linear sum of these terms in the figure caption, which also elaborates on the meaning of 'change by dynamics' (in response to the suggestion of another reviewer).

8) As noted previously, the temperature and winds in the model were recently documented. In response to this suggestion, we now note in the text that indeed the tropospheric vertical EP-flux, indicative of wave generation, increases in the warmer climate, in accord with our hypothesis that this drives the increased Brewer-Dobson circulation (as evidenced by the fluxes presented in Table 7).

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