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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 #3

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General Comments

In general this is a good paper with interesting information. It consists of a model description portion with evaluation of the model against observations, and a series of 'time slice' experiments for different conditions, including pre-industrial, present day and simulations of the future.

This paper should be publishable in Atmospheric Chemistry and Physics subject to major revisions. I think the authors should seriously consider shortening or splitting the paper. In the first part (model description) more justification for using a 23 level model up to the stratopause needs to be given. The discussion of the various scenario runs

needs to be clearer about WHY the various runs were done, and the conclusions could do a better job of highlighting the key results.

The paper seems very long, and would benefit greatly by being split into two pieces, one for model description, and one for the description of simulations.

I am not sure what is new about his chemistry formulation: it appears to be the assembly of a stratospheric chemical package. I think this could be a paper in it's own right, or it could be drastically shortened and the model description moved to a report or something. The underlying GCM has already been described, as has the water vapor distribution, so it appears that just the stratospheric chemical package is new.

The second piece (description piece) describes a series of sensitivity experiments. I was very confused throughout by these runs, because it was not obvious why the runs were done, or the intended differences. Why are A1B and A2 scenarios used? (it appears to be because A2 has higher emissions and concentrations than A1B). Why is the set up between the climate and no climate cases different between the A1B and A2 cases and why? This is not clear from the text. The result is that it is more difficult to understand the conclusions.

A good chunk of the discussion is about STE, which is almost a 3rd topic of the paper. I think this is better presented than some of the other piceces, but I worry that this model needs more justification to effectively resolve STE.

In addition, I think the authors need to show the vertical level structure. Much of this paper concerns the stratosphere and STE, yet the model has 23 levels? What is the vertical resolution? This could be done by plotting the pressure (or log pressure) coordinates above 100hPa on figure 3 for each model level, or ideally making figure 3 a series of boxes representing the thickness of a model level.

I am tempted to conclude that a 23 level model is worthless for diagnosing STE unless convinced otherwise. Has the climate (temperature structure and winds) been vali-

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dated or evaluated up to .1 hPa and ˜60km (the statopause).? If not, then some more information on the model dynamics needs to be presented. There are temperature difference plots, but not relative to observations. This makes the interpretation of results difficult.

Specific Comments.

Section 2.2: Is there precedent (reference) for treating halocarbons as a single species? How realistic is it?

Section 2.3: How many vertical levels are there above 100hPa? What is the vertical resolution? It must be pretty coarse if there are only 23 levels.

Section 3.6: Please supply a few more details about clouds and aerosols. How do clouds and aerosols interact? Does the model have aerosol indirect effects? If you change aerosol chemistry, are you changing cloud properties? Are the cloud properties the same in these runs? Does that affect your RF calculations, or are they clear sky?

Section 4.3: you make the statement that "The ozone decreases in the LS result from an increase in relatively ozone-poor tropospheric air upward across the tropical tropopause". Yet the brewer-dobson circulation slows? What is going on.

Also, the discussion in section 4.3 is very verbose. The paragraphs run a page or more. This was tedious to read. Also, the run descriptions are not enlightening and confusing as written. What have you done differently between the A1B climate v. no climate change and the A2 climate v. no climate change? It might be better to discuss all these runs together and be more concise. It would also help if the conclusions better reflected the time spent on the various sections (The conclusions about these simulations are very brief).

Figure 7: please put latitudes on each station title. Also, what is happening in the Resolute 500hPa figure? Why is the triangle line off scale? (The lowest row here could have a different and finer scale too).

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Section: 4.3.4 How have you made the determination of causation in determining relative impacts in figure 20? More discussion is needed. Are the changes linear combinations or are there 'feedbacks'?

Section 5. Conclusions: you make the statement that in the NH "wave generation increases as climate warms and the flow increases" You have not shown any wind diagrams. Does the flow increase? Does wave generation increase? For that matter, as noted above, is the model temperature and wind structure in the middle atmosphere reasonable? If this has been shown before fine, if not it needs to be part of the model description.

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