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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Stratospheric variability and trends in IPCC model simulations" *by* E. C. Cordero and P. M. de F. Forster

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Review of "Stratospheric Variability and trends in IPCC model simulations"

by Cordero and Forster.

This paper needs a major rewrite. It repeats a lot of what is already known, feels like a review paper with many citations, and is very descriptive. I recommend that the paper be rewritten to focus on what results are actually new and to focus completely (as the title suggests) on the stratosphere. Given this the authors should only focus on those models that include important stratospheric forcings i.e. CO2/WMG, Ozone and Volcanic Aerosol (not implemented as a redn in the solar constant).

The paper could also make the point that lack of standard experimental design makes the multi-model ensemble less useful. I suspect better quality control on the meta-data i.e. accurate description of forcings would help users of the model results.

Minor points.

1) Simulations are not IPCC model simulations (which implies they are owned by the IPCC). Suggest title change.

2) 7660/3 What is full reference for "(2001)"

3) Section 2. Some discussion of observational uncertainties is needed. In particular the authors need to establish how good the data is for their purposes.

4) Section 3. This section should focus its effort on the question of stratospheric biases. I think it poses an interesting question. "How important is it to have a high lid". My take on the answer is that it matters for 10 hPa but that is all. So if you don't think that biases at 10 hPa are a particular problem then you don't need a high lid! So this section should focus on this. I think some exploration of the mechanisms that lead to biases would be useful or at least some speculation as to reasons. I suspect that models without ozone decline will be biased warm in the stratosphere compared to models with. Consequently I strongly recommend that only models with key stratospheric forcings are used.

The authors use NCEP reanalysis to estimate biases. What evidence to the authors have that the NCEP climatology is adequate for this purpose?

5) Section 4.

I found this section very descriptive and a repeat of lots of earlier work. The authors should focus on those models that use important stratospheric forcings. This would result in a smaller and more focused section. In short we already know that key forcings are needed to get the right stratospheric response. The authors could improve knowledge by seeing how sensitive the results are to model formulation. The tropospheric results are unnecessary in this paper and the recent papers by Santer et al have covered this topic well anyhow.

6) 76666/20-30 – this text feels like a figure caption. Please put it in the caption and tell us what the figure shows.

7) 7667/19. It is possible that these models implemented changes in volcanic aerosol through changing the solar constant rather than adding stratospheric aerosol. I think worth verifying that with the groups concerned. If so I would recommend not using those models for this study. (or it might be poor quality control)

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 7657, 2006.