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Interactive Comment

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

E. C. Cordero and P. M. de F. Forster

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RE: Manuscript Number: 00.0351

Authors response to review of "Stratospheric variability and trends in IPCC model simulations" By E. C. Cordero and P. M. de F. Forster

Dear Editor:

The authors would like to thank both referees for their comments and constructive suggestions. The comments, which are primarily minor in terms of the scientific content, have been addressed in the revised manuscript and outlined below. Referee 1 (Simon Tett) had general concerns regarding the tone and focus of the paper, and these have also been addressed below.

Best regards,

Eugene Cordero Department of Meteorology San Jose State University

Response to Referee 1. (S. Tett's) comment on SRef-ID: 1680-7375/acpd/2006-6-7657 "Stratospheric variability and trends in IPCC model simulations" by Cordero and Forster

Referee 1 has two primary comments (labeled major points) and a number of minor points. The major points are focused on the direction our paper takes and whether it overlaps with other work, rather than point out any scientific shortcomings or flaws. We will address each major and minor comment as outlined below.

Major points:

The first point Referee 1 makes asks us to constrain our analysis to stratospheric results and only include models that contain important stratospheric forcings. While we could understand such an experiment design, it was not the purpose of this study to restrict our analysis to a subset of the models, nor to limit our analysis to an arbitrary line in the atmosphere. Employing models with different forcings gives insights into attribution of stratospheric temperature change, as we have shown. Referee 1 has employed this methodology on several occasions himself, and we feel it is important to assess as best as possible the impact of different mechanisms on stratospheric temperatures. An important result from our work suggests that a key component to understanding temperature variability in the upper troposphere/lower stratosphere may come from the stratosphere and variability in the stratosphere. It is the overarching theme of the paper, and one of the primary goals for investigation.

The second point Referee 1 makes is to suggest that the paper repeats a lot of what is already know, and that it should be refocused on new results. He further states that Santer et al. (2005) has already done an excellent job looking at tropospheric trends using these models, and that our work repeats those results. While we agree that the

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Santer et al. paper provide good insights into tropospheric trends, the goals and aims of our paper is distinct and complementary to that paper. Nevertheless, even analyzing the same models, our paper gives different results and insights compared to the Santer et. al. work. In particular, our paper also points out the discrepancies between model and observed trends in the tropical tropopause region, which the Santer papers did not. The principal aim of our paper is to investigate how models primarily developed to study tropospheric climate represent the stratosphere. It focuses on stratospheric variations, and to our knowledge, such an intercomparison using the latest generation of AOGCMs has not been completed. The diagnostics used for stratospheric comparisons in sections 3 (20th century climate: model intercomparison), 4 (20th century trends) and 5 (21st century climate predictions) have not been examined elsewhere, and our analysis of the impact of number of stratospheric levels also provides novel understanding. We also note that in Section 3, and parts of Section 4 and 5, the various diagnostics that include all models provide valuable feedback to not only the participating models groups, but also to researchers using climate model data.

In recognition of these concerns, we have made changes in the abstract, introduction and conclusions to a) more clearly explain the purpose of our analysis, b) more clearly distinguish our results from other work, and c) provide more focus on our new results. We have also taken the Reviewers suggestion about the lack of standard experimental design and metadata and included this point into our conclusions.

Minor Points: The remaining minor points are addressed below using the numbers presented and include the reviewers comment first.

1) Simulations are not IPCC model simulations (which implies they are owned by the IPCC). Suggest title change. We have revised the title to “Stratospheric variability and trends in models used for the IPCC AR4”

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

3) Section 2. Some discussion of observational uncertainties is needed. In particu-

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lar the authors need to establish how good the data is for their purposes. Additional comments have been made regarding observational uncertainties. In particular, additional references to Randel and Wu (2006) and Seidel et al. (2004) have been used to provide further background on uncertainties.

4) Section 3-a. 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 referee asks us to explore or at least speculate as to reasons for stratospheric biases. This has been initiated with the low and hid lid analysis, and further explored in our trend analysis, where we explore how the ozone forcing affects trends. We have now added additional statements about the relationship between ozone forcing and absolute temperature in the stratosphere, where we don't find any relationship based on model inclusion of ozone depletion. We also include in the conclusions a statement suggesting that other mechanisms (i.e. gravity wave drag) should also be investigated.

Section 3-b. The authors use NCEP reanalysis to estimate biases. What evidence do the authors have that the NCEP climatology is adequate for this purpose? An extensive study of middle atmospheric climatologies was conducted by Randel et al. (2004), and no systematic bias was found that would suggest using another climatology such as ERA-40 would alter our results. Our results also show no large scale difference between these reanalyses. We have noted both of these in the revised manuscript.

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.

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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. Under Major Points, we addressed the Referee's concerns regarding the use of only models with important stratospheric forcings, and limiting our analysis to the stratosphere. Although we agree that determining the sensitivity of results to model formulation (i.e. type of gravity wave parameterization etc.) is important, we feel this is beyond the scope of this paper and plan to pursue this in future work. This has been noted in the conclusions of the revised manuscript.

6) 7666/20-30 - this text feels like a figure caption. Please put tell us what the figure shows. Done.

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) This point was addressed under Major Points.

References:

Randel, W. et. al., 2004: The SPARC intercomparison of middle-atmosphere climatologies. J. Climate, 17, 986-1003. Randel, W. J. and F. Wu, 2006: Biases in stratospheric and troposheric temperature trends derived from historical radiosonde data. J. Climate, 19, 2094-2104. Santer, B. D. et. al., 2005: Amplification of surface temperature trends and variability in the tropical atmosphere. Science, 309, 1551-1556, doi:10.1126/science.1114867. Seidel, D. J., J. K. Angell, J. Christy, M. Free, S. A. Klein, J. R. Lanzante, C. Mears, D. E. Parker, M. Schabel, R. Spencer, A. Sterin, P. W. Thorne, and F. J. Wentz, 2004: Uncertainty in signals of large-scale climate variations in radiosonde and satellite upper-air temperature datasets. J. Climate, 17, 2225-2240.

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Response to Referee2's Comments on SRef-ID: 1680-7375/acpd/2006-6-7657 "Stratospheric variability and trends in IPCC model simulations" by Cordero and Forster

The comments from Anonymous Referee #2 were very positive and suggested publication with only minor revisions. Each of the suggestions from Referee 2 has been addressed below with our items corresponding to the page/line number presented by the referee. Comments on Pg 7659, 7663(ln16-18), 7664(4), 7665, 7667(ln10), 7673 are related to typos and wording and have been corrected in the revised manuscript. The remaining minor points are addressed below, where we include the reviewers comment first:

Pg 7661, ln 2: Do the authors really only compare nineteen simulations, or do they compare nineteen ensembles of simulations? If they only use a single simulation from each model, why not use the whole ensemble? We use a single simulation from each model (run 1) and have made this point explicit in the revised manuscript. Although we could have computed an ensemble mean, based on our analysis of differences between ensemble means and individual runs for a subset of the models, we don't feel this would affect our conclusions

Pg 7662, ln 18-20: I am surprised by the claim that there were no significant differences between the NCEP reanalysis and ERA-40 trends for the regions considered in this study. My impression was that the NCEP reanalysis trends in the stratosphere are unreliable, even for the satellite period. We doubled checked this but did not find any significant differences in the trends, even during the satellite period. An extensive study of middle atmospheric climatologies was conducted by Randel et al. (2004), and no systematic bias was found that would suggest using another climatology such as ERA-40 would alter our results. We have noted this in the revised manuscript.

Pg 7663, ln 16-18: The right panel of figure two shows a larger standard deviation in the stratosphere, not in the troposphere as the authors claim. This has been corrected in the revised manuscript.

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Pg 7663, ln 20-21: The models also underestimate the temperature in the troposphere, in fact the difference is most apparent here. Good point. This has been noted in the revised manuscript.

Pg 7667, ln 6-12: It is perhaps not surprising that many of the models underestimate the observed cooling, given that many of them lack ozone forcing. The models which do have trends close to that observed all have ozone depletion. This should be commented on. This is discussed later in the paper, but a reference to this has been made here in the revised manuscript.

Pg 7669, ln 8: How was the 2-sigma uncertainty in the trends calculated? Was autocorrelation taken into account? The trend uncertainties were computed using the standard error, while each measurement was assumed to be independent with autocorrelation not accounted for. This has been noted in the revised manuscript.

Pg 7673, ln 24: I think the super-recovery in global ozone is a model-dependent result. Insert 'in some models' at the end of the sentence. We agree, and this has been noted in the revised manuscript.

Pg 7677, ln 22-23: But the authors haven't shown any evidence that variability at lower levels is affected (they could easily check this). Our initial examination of lower level variability did not show significant changes, but we are suggesting that more subtle variations may be present but would require additional analysis. This has been made clear in the revised manuscript.

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

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