Atmos. Chem. Phys. Discuss., 6, S6536–S6540, 2007
www.atmos-chem-phys-discuss.net/6/S6536/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Solar cycle variations of stratospheric ozone and temperature in simulations of a coupled chemistry-climate model" *by* J. Austin et al.

J. Austin et al.

Received and published: 26 January 2007

Response to Reviewer 1's comments

We would like to thank the Reviewer for the comments which have significantly improved the paper.

1. The opening paragraph of the Introduction has been expanded with the addition of four more references.

2. The revised paper includes an expanded subsection 4.2 showing results on the latitudinal variation of the ozone and temperature solar response, together with comparisons with observations. Two additional figures (6 and 7) have been added and

subsequent figures have been renumbered accordingly.

3. The model agrees within the error bars for the HALOE data, but as the Reviewer points out agreement with other datasets is poorer. One cannot expect agreement with all datasets when those datasets disagree in detail. The optimism pointed out by the Reviewer comes not from a point-by-point agreement with observations — such agreement would be unlikely in a climate model with its own variability. Instead the so-called optimism stems from the successful simulation of the tropical minimum. At no point in the paper do we say that agreement with observations cannot be improved. Figure 11 (old Figure 9) demonstrates that the minimum is not the result of the statistical technique since the model results included there are merely differences between opposite solar phases. This is now emphasised more. The section also now points out that the observed diurnal variation may not be accurate in the upper stratosphere and mesosphere where the diurnal variation is large. This is because the observed signal is usually a day measurement and daytime ozone is not representative of the zonal average. In contrast, the model result is a strict zonal average and includes night points. We agree that the temperature response is difficult to validate. Some observations are included, but we do not make a strong case for any improvement in understanding there.

4. We believe the 27-day results are worthy of including in their entirety. Firstly, they do not take up a large amount of journal space. Secondly, while generally agreeing with observations and previous studies, they also reveal differences between cycles, a point also made recently by Rozanov et al. (2006).

Specific Comments

1. The paper includes 24 years' data or 2.2 cycles, not 3.

2. The Rozanov et al. 2005 paper is rather short and doesn't demonstrate clear improvements compared, for example with the summary of Shindell et al. (1999) or the summary of model results in Soukharev and Hood (2006). In particular, they do not

ACPD
6, S6536–S6540, 2007

simulate the tropical ozone minimum, which is a key aspect of our paper and has been a centre of attention for some years. The results of Figure 9 of our paper demonstrate that in our model it doesn't make much qualitative difference whether linear regression is used or not. Either method yields the tropical minimum.

3. The error bars for all the instruments are large. The short HALOE period is not necessarily a problem in that regard. Another possibility is that with different data periods, chemical changes are different. This is beyond the scope of the current work and in any case has been discussed in Soukharev and Hood (2006).

4. The source of the SST dataset has now been specified. The sources of various other datasets used are indicated in the climate and ozone papers referenced earlier.

5. See response to 4 and the top of P12125

6. The lookup table has to be constructed with specific numbers of fluxes as a function of wavelength. This is clarified in the revised paper.

7. The model description includes these details on page 12126. As explained mid-cycle is equivalent to the flux present in Jan 2002. Further details on the radiation scheme may be found in the description of the climate model (Anderson et al., 2004) and other works cited. especially Austin and Wilson (2006) and Austin et al. (2006). The latter is not yet published, but a copy is available on request. Clearly, only so much duplication of the literature is reasonable. The current paper cannot include a full description of the whole climate model or the chemistry scheme or it would become unwieldy.

8. The time term in the regression has been explained in the revised paper. The halogen loading is included in the trend term. This is a reasonable approximation over the multi-decadal time scale used here, and in any case was chosen to coincide with the method analysed for the observations (data from Hood and Soukharev, 2006).

9. This has been explained in the revised paper in Section 2, in the paragraph describ-

ACPD
6, S6536–S6540, 2007

EGU

ing the calculation of the photolysis rates.

10. Sometimes no explanations are possible when different climate models are used. The fact that different results are obtained does not invalidate the results. Rather, it is important to convey differences between models where these exist, and this is what we have done.

11. Again, it is perfectly reasonable to compare our limited results with observations. For example, the results should agree better with observations in the lower and middle stratosphere where the direct UV effect on temperature is small. According to similar logic, one would not compare 2-D models with observations, as the atmosphere is three dimensional. Or, one would not compare fixed phase solar results with observations, as the observed solar phase varies.

12. The time slice experiments we have seen disagree with observations in a fundamental way and therefore provide misleading information about solar effects. The epithet 'poor' is appropriate.

13. The model chemistry was not designed for the mesosphere, so the upper levels of the results plotted may be in doubt. A comment to this effect has been added. Further interpretation of the water vapour results have been added.

14. The Rozanov et al. study did not show a minimum in ozone response in the tropical lower to middle stratosphere.

15. The comment referred to the results in the lower and middle stratosphere. In the revised paper this has been clarified to address the upper stratosphere situation.

16. Additional comments have been added on the possible tropical upwelling.

17. The sensitivity of the observed solar cycle to the period analysed has been explored by examining the results for the period 1992 to 2003, excluding data during the period of high volcanic aerosol. Unfortunately, the measurement error bars are too large for definitive statements to be made. A comment has been added to the paper in

ACPD
6, S6536–S6540, 2007

EGU

the discussion of the Figure 9 (old figure 7) to this effect.

18. The transient model results have been plotted on Figure 12 (old Figure 10), the text has been revised to highlight the comparison.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 12121, 2006.