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Atmospheric Chemistry and Physics Discussions

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6, S6078–S6083, 2007

Interactive Comment

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

Anonymous Referee #1

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General comments

The manuscript illustrates the response of the tropical ozone and temperature to the variability of the solar irradiance during solar rotation and 11-year solar activity cycles simulated with CCM AMTRAC. The analysis of this aspect of the Sun-Earth relationship is relevant to the scope of ACP. The model and results presented in the manuscript are original. The description of the conducted numerical experiments is mostly clear and other scientists can reproduce them. The manuscript consists of the description of the model set-up and analysis of the solar signal in tropical ozone and temperature extracted from the different model runs. The obtained solar signal is compared with

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the solar signal extracted from several observation data sets with similar statistical techniques. The manuscript is well written and contains several interesting findings. In particular, it is shown that the solar signals in the tropical ozone from transient and time-slice model experiments substantially differ in the middle stratosphere and the results of transient simulation are closer to the observations. I think that the manuscript can be published in ACP. However there are still several issues (see below), which should be clarified or explained in more details before the manuscript is ready for publication.

1. The manuscript lacks some brief explanations of the physical-chemical-dynamical processes involved in the formation of the atmospheric response to the solar irradiance variability. It makes the manuscript difficult for unprepared readers.

2. I would recommend to show zonal mean cross-sections of the decadal scale solar signal to illustrate the general pattern of the model response. Maybe, it will help to identify the causes of the difference between transient and time-slice experiments. These results can be also compared with the analysis published by Soukharev and Hood (2006). In particular, it would be interesting to see the latitudinal structure of the ozone signal and how successfully the model reproduces the dipole structure in the observed solar signal in the upper stratosphere.

3. I think the authors are overoptimistic talking about good agreement of the simulated and observed decadal scale solar signal. It is clearly seen in the Figure 7, that the differences are still significant. Above ˜20 hPa the vertical structure of the simulated ozone response is in opposite phase with the observations (especially in Figure 7, upper panel). In the upper stratosphere/lower mesosphere the model substantially underestimates the magnitude of the observed ozone response. The simulated local minimum appears at lower altitude than in the observations. Moreover, the authors do not explain the causes of the local minimum, therefore we cannot reject the hypothesis that this minimum is rather the result of applied statistical technique than the manifestation the solar irradiance variability effects. The temperature response is very difficult to validate because there is still no consensus between different analyses.

ACPD

6, S6078–S6083, 2007

Interactive Comment

4. I suggest to shorten the description of the solar rotation cycle effects, because the experimental set-up does not include direct effects of the solar irradiance variability on the heating rates, which dominates above ~10 hPa.

Specific comments

1.Page 12123, first paragraph "only about two cycles are available"

I would say about three.

2.Page 12123, second paragraph "these differences persist"

As was pointed out by Rozanov et al., 2005 (Rozanov, E., M. Schraner, T. Egorova, A. Ohmura, M. Wild, W. Schmutz, and Th. Peter, Solar signal in atmospheric ozone, temperature and dynamics simulated with CCM SOCOL in transient mode, Mem. S.A.It. Vol. 76, 876-879, 2005) the agreement is better if the solar signal is extracted using multiple regression analysis from the transient CCM run driven by realistic forcing. However, they did not explain what is the reason of such improvement.

3. Page 12124, first paragraph "Hood and Soukharev, 2006"

Due to rather short time series I would not completely trust the solar signal extracted from the HALOE data. It is somewhat confirmed by Figure 7, where the solar signal above 10 hPa from HALOE data is much smaller than from the other two satellite instruments.

4. Page 12124, second paragraph "SST/SI"

What is the source of SST/SI data?

5. Page 12124, second paragraph "solar forcing is specified from observations"

There were almost no observations before the satellite era.

6. Page 12125, first paragraph "solar maximum and minimum conditions"

How the solar maximum and minimum were defined?

ACPD

6, S6078–S6083, 2007

7. Page 12126, second paragraph, SL2000 and SL2000B runs set-up

The description of the solar forcing needs clarification. What is it mid-cycle solar forcing? How the solar irradiance for heating rate calculations was prescribed?

8. Page 12127, Equation 4

How the t (time) was defined? Month, year, day? Is this term responsible for trend approximation? Why the halogen loading was not included and how this could affect the results? I guess, the linear trend is not a good approximation for the ozone, because the halogen loading for 1960-2004 is far from linear.

9. Page 12127, first paragraph, "solar flux at 205 nm"

As far as I understood daily solar irradiance was prescribed using artificial sin-wave. What data (artificial or observed) were used for solar flux at 205 nm?

10. Page 12129, second paragraph, "Lag correlation"

The better agreement with Williams et al., 2001 Run A (with different set-up) is confusing and should be somehow explained or commented. If there were no explanations I would recommend skipping this comparison.

11. Page 12129, third paragraph, "The results"

The results should not be directly compared with observations, because the lack of direct radiative perturbation in the model.

12. Page 12133, first paragraph, "poor simulations"

I think it is not appropriate term. The time-slice experiments are necessary to understand the response of the atmosphere to the solar variability.

13. Section 6

The response of H2O should also depend on the temperature changes in the tropical UTLS. In case of warming (Figure 8) the stratospheric H2O could increase. The H2O

ACPD

6, S6078–S6083, 2007

Interactive Comment

response in the mesosphere for the time slice experiment is not clear. I would expect smaller H2O in the mesosphere for the solar maximum case due to an enhanced H2O photo-dissociation. It would be helpful to comment on this.

14. Page 12135, second paragraph, "has not previously been published"

See item 2

15. Page 12135, third paragraph, "The temperature response . . . in large part"

This sentence is not correct. There were several publications, which showed that the direct radiative heating dominates in the upper stratosphere.

16. Page 12136, last paragraph, "indicative of enhanced upward motions"

Which mechanism could be responsible for the enhanced upward motion? From the theoretical consideration (e.g., Kodera and Kuroda, 2002) we should expect stronger polar vortex and slower Brewer-Dobson circulation, i.e. slower upward motions in the tropics. Some comments on this are necessary.

17. Figure 7,

The authors used the time intervals, which does not coincide with the time interval used for the satellite data processing. It would be interesting (at least for HALOE case) to estimate the effects of the selected time interval on the ozone response. Could it be the reason of the difference between the solar signal extracted from HALOE and SAGE data?

18. Figure 10

It would be helpful to plot also the results of the transient runs in Figure 10.

Technical corrections

1. Page 12129, "Williams et al."

2001 is missing

ACPD

6, S6078–S6083, 2007

Interactive Comment

2. Page 12130, 0.06 percent

Check the units, probably it should be 0.06 K

3. Figure 4, "regressio"

"n" is missing

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

ACPD

6, S6078–S6083, 2007

Interactive Comment