Atmos. Chem. Phys. Discuss., 13, C4826–C4831, 2013
www.atmos-chem-phys-discuss.net/13/C4826/2013/


Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Stratospheric $O_3$ changes during 2001–2010: the small role of solar flux variations in a CTM" *by* S. S. Dhomse et al.

**Anonymous Referee #1**

Received and published: 16 July 2013

## 1 General Comments

This paper is well structured and generally well written. It adresses an important question: is stratospheric ozone sensitive enough to (multi-annual) solar flux variations to allow a discrimination between significantly different solar flux datasets (i.e. SORCE versus NRL or SATIRE-S)?

This question is approached through a sensitivity study using the 3D CTM SLIMCAT, which is driven by realistic wind fields from ECMWF. This is an excellent idea, but the paper will be useful only if sufficient details are given about the SLIMCAT model to help understand the differences with previous modelling studies on this topic. Hence great



care must be given to the description of the model and its set-up, which are serious shortcomings here – especially regarding the importance of the solar emission line at Lyman-$\alpha$. Due to the low lid of SLIMCAT (0.1 hPa), I also doubt the validity of the results presented in the Upper Stratosphere-Low Mesosphere (USLM): they are probably too influenced by upper boundary conditions which are not described at all.

This review assumes that even though their variations with the 11-year solar cycle were overlooked, SLIMCAT accounts for two major processes in ozone photochemistry above $\approx$ 10 hPa: the absorption of solar radiation by $O_2$ above the model lid, and the solar Lyman-$\alpha$ line. If that is not the case, we recommend to reject the paper as SLIMCAT would not be able to separate properly the dynamical and photochemical influences on ozone variations. Otherwise, the excellent agreement between the SLIMCAT runs and MLS or SABER observations certainly deserves publication and I recommend to publish this paper after major modifications, as described in the comments below.

## 2 Major Comments

### 2.1 Solar Lyman-$\alpha$ line

The solar emission line at Lyman-$\alpha$ (121.59 nm) matches a deep minimum in the $O_2$ absorption cross-section, allowing this line to penetrate the middle atmosphere much lower than the neighbour wavelengths. In view of the large and well-documented variations of the solar flux at Lyman-$\alpha$, this is a key process that must be included in any modelling study of the impact of solar flux variations on ozone. The existence of Lyman-$\alpha$ is not mentioned in this paper, so it is impossible to know if (and how) it was taken into account by the model. Section 3 should describe the intensity of solar Lyman-$\alpha$ used in the different runs, as well as the parameterization used to account for the variation of the $O_2$ cross-section over the emission line.

If the solar flux at Lyman-$\alpha$ is kept to one constant value in all model runs (e.g. a value typical of solar minimum), then an additional experiment should be run with this flux set to a a very different value (e.g. a value typical of solar maximum). The results of this experiment should then be added in the figures and discussion. Note that this could well change the conclusions of the paper.

If, on the other hand, the 11-year variations of Lyman-$\alpha$ are already taken into account, these variations should be clearly described for each model run and the consequence stated in the discussion – i.e. the impossibility to discriminate between two solar flux datasets if they do not cover wavelengths shorter than the Schumann-Runge Bands (SRB).

### 2.2 Upper lid and boundary conditions

The variations of ozone in the USLM are discussed a lot in this study, while the model lid is the same as ERA-Interim i.e. at 0.1 hPa, right in the middle of the lower mesosphere. Hence I expect that in the USLM the model results are very sensitive to the Upper Boundary Conditions (UBC) used for ozone itself, even if they are specified as a vertical flux. This is indeed a major weakness of all ERA-driven CTM's to study the USLM.

In view of the excellent comparison with observations, I recommend the following approach to get around this difficulty: describe precisely, in section 3, the UBC used for ozone in each model run; perform a sensitivity test with a very different UBC to show the vertical range where the results are significantly influenced by this choice of UBC; exclude this vertical range from the other figures and from the discussion.



### 2.3 Description of the radiative transfer model

The paper will be useful only if sufficient details are given about the SLIMCAT model to help understand the differences with previous modelling studies on this topic. Besides the realistic winds (and lower model lid), a key component to explain these results is the radiative transfer model used by SLIMCAT to compute the photodissociation and solar heating rates. There is not any description of this module in the paper.

This should be corrected with the following information (and appropriate references):

- What radiative transfer model is used by SLIMCAT? What is its wavelength range and resolution? Does it use the same vertical grid as SLIMCAT itself?
- What parameterization is used for the absorption by the SRB of $O_2$ (175âĂŤ205 nm)? Fig.1 shows that the different solar flux variations seen by SORCE cover precisely this special wavelength region.
- How is the absorption of solar radiation above 0.1 hPa taken into account?
- Is the same radiative transfer model used for solar heating rates and for photodissociation rates? If not, what are the differences?

## 3 Minor comments

- Section 4 states that the SORCE dataset used here is “the available SORCE data that was used in Haigh et al (2010)” but no reference describes directly this dataset, nor the instrument itself. If no peer-reviewed paper is available on this topic, please cite a technical report or at least a web page describing this dataset. This should be part of a new paragraph in section 2, which currently does not describe SORCE.

- On Fig. 2, SABER shows a diurnal variation of ozone in the 30-45 km range (≈3.5 pmv) while SLIMCAT finds none. This variation (0.2 ppmv/6 ppmv) may be negligible but is worth mentioning. What does MLS show on this topic?
- Fig. 3: how are computed these ozone anomalies? In other words: they are departing from what monthly mean climatology?
- Section 4 (p. 9, line 8, while discussing Fig. 4): "middle stratospheric enhancements can be simulated with NRL (or SATIRE), *fixed* and SORCE solar fluxes". I do not understand the "fixed" part, as run C_FIX is not used on Fig. 4. That is a pity since otherwise this sentence would be an important conclusion. If possible, please include results from run C on fig. 4
- P. 9, line 28: Figure 3h does not exist
- P. 10, line 6 mentions "a fixed dynamics simulation with SORCE fluxes" but no such simulation is reported in Section 3 nor in Table 1
- Fig. 6 does not show any model results using SORCE. Please do it or explain why it is not done.
- P.11, line 1 (while discussing Fig, 6): "our simulations and SABER show $O_3$ differences of nearly 4%". I see a rather different result, with SABER observing maximum differences of ≈ 2% much like run C_FIX but twice lower than runs A_NRL or B_SATIRE.
- Conclusions: the last paragraph is quite unclear – does it refer to Fig, 7a or 7b? Fig. 7a does not cover 2001-2010 while Fig. 7b does not include SBUV nor SAGE, and it is not possible to compare both since 7a uses SMAX-SMIN while 7b uses a regression model.
- The key sentence from the abstract ("Ozone changes in the lower mesosphere cannot be used to discriminate...") should be part of the conclusions.



Interactive comment on Atmos. Chem. Phys. Discuss., 13, 12263, 2013.