

Interactive comment on “Modelling the effects of (short-term) solar variability on stratospheric chemistry” by R. Muncaster et al.

Anonymous Referee #1

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The manuscript describes the chemical response to solar irradiance variability obtained with photochemical model and its parametrization using statistical models. The subject of the study is relevant to ACP scope. The authors applied a relatively simple 1-D photochemical model driven by the spectral solar irradiance data for the solar activity maximum and minimum as well as their combinations. The obtained response of different chemical species is compared with previously published model results and observation data analysis to validate the applied model. Then, the authors introduced the parametrization of the odd oxygen mixing ratio response using linear statistical model with 2 and 3 predictors. The properties of these regression models are presented and their accuracy is estimated using original photochemical model with different definitions of the solar spectral irradiance variability. The applied approach is reasonable for this particular task. The manuscript is reasonably well written and structured, however

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sometimes it is lengthy and hard to read, therefore I think it can be shortened to be more understandable. In addition there are several major problems (see below) which cannot allow me to recommend the publication of the manuscript.

General comments:

1. I still do not see who can be interested in the proposed parametrization. The authors briefly said that the parametrization can be used in chemistry-climate models with simplified chemistry, but only one example was given (Taylor and Bourqui, 2005) and there are no comments on how the suggested parametrization can be applied and what benefits its application will give to the model.
2. The developed parametrization is not properly explained and validated. The description of the on-line validation mode is not sufficiently clear. For example, the author stated that the memory term should be dropped in “on line” mode causing additional errors, but later on they still use 2(3)-predictor schemes. It is not clear why it can be called like this if one of two(three) predictors was dropped. Nothing is clearly said about additional errors. In general, the validation of the proposed parametrization should be done with a potential target model (i.e., CCM with simplified chemistry, but not with the simple photochemical model which is rather far from the real processes in the atmosphere). Such a procedure would clearly show the benefits and issues related the introduction of the proposed method.
3. The applied 1-D model describes only photochemical process. But, is it enough to analyze the response to the spectral solar variability? The model does not take into account temperature changes produced by solar irradiance variability which can contribute to the ozone response. The model does not consider any other processes in the real atmosphere which can contaminate, mask or enhance pure chemical response (e.g., Gruzdev et al., ACP, 2008). These issues and their implications for the presented results should be discussed in the manuscript. Otherwise it is not clear how the obtained results can be compared against observations.

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4. The model set-up for the case with scaled spectral solar irradiance variability is not justified. The authors introduced uniformly distributed scaling factor which does not depend on the wavelength, while Haigh et al.(2010) showed that the difference between the SORCE data and Lean's parametrization is not spectrally homogeneous. Moreover, the postulated by the authors spectral homogeneity of the solar irradiance variability on the daily time scale has not been convincingly validated. It well could be that the variability of the spectral solar irradiance on the daily time scale is far from idealized case studied in the manuscript. This issue should also be carefully discussed.

Minor issues:

1. page 32457, line 7: I think this fact was known long before year 2000.
2. page 32458, line 27: Please, check. I recall the radiative relaxation time could reach 100 days in the lower stratosphere
3. page 32459, line 27: Most of the models participating in CCMVal-2 campaign used monthly mean spectral solar irradiance. So, this statement is not correct.
4. page 32460, lines 2-8: I guess, these statements are not correct either. Most of the CCMs use proper representation of solar irradiance variability.
5. page 32461, lines 9-29: I think this paragraph belongs to conclusions.
6. page 32464: I have noticed that the authors consider the time interval much shorter than 27-day cycle. Any implications for observed responses?
7. page 32463, line 6: Reconstruction of spectral solar irradiance by Lean is based on satellite measurements but not identical to SOLSTICE. Please, refine what exactly was used.
8. page 32467, line 21: I recall the water vapor life time is larger than 5 days. Does it have any implications for the results. I think 5 days should be better justified.
9. page 32468, HOx: Nothing is said about HOx production from H₂O+O(1D) which

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also depends on solar irradiance. Is this reaction chain important?

10. Section 4.2: I think this section (compare to previous ones) is to short and therefore not clear. In particular, I do not understand why there is no sensitivity to NO_x levels while the temperature dependency appears via oxidation by NO_x?
11. page 32482, line 24: Is it allowed to refer to submitted papers?
12. Table 1: CFC11 mixing ratio is for sure altitude dependent.
13. Figure 1: H₂O profile is not instructive. It looks like H₂O does not exist in the stratosphere.
14. Figure 5: I see no lines above stripped area.
15. Figure 6. I see only red lines.
16. Figure 8: The magnitude of the ozone response is only 0.1%. In the text it is mentioned that typical response is around 3%. Please, explain why such an extreme case was chosen.
17. Figure 9: No middle panel. Most of the lines are not visible.

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