Atmos. Chem. Phys. Discuss., 12, C792–C795, 2012
www.atmos-chem-phys-discuss.net/12/C792/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulation of solar-cycle response in tropical total column ozone using SORCE irradiance" *by* K.-F. Li et al.

Anonymous Referee #1

Received and published: 21 March 2012

General comments

The manuscript describes the response of the total column ozone to the variability of the solar irradiance simulated with CCM WACCM v.3.5 driven by different sets of spectral solar irradiance (SSI). The authors applied two SSI data sets: NRL reconstruction and the extrapolation of the recent data obtained by SORCE. The subject of the manuscript is relevant to the ACP scope and potentially interesting for the community. However, the manuscript has substantial flaws and I cannot recommend it for publication in the present form.

Specific comments

1. The applied SSI data sets are not properly described in the manuscript. It is not so

crucial for well known NRL reconstructions, but absolutely necessary for the extrapolated SORCE SSI because Wang et al., (2012) paper is not available (in the reference list the status of this paper is "under review" and it is not clear what to do if this paper is not accepted for publication). Therefore, it is absolutely not clear what are the properties of the applied SORCE based SSI data set. Moreover, the extrapolated data set should be carefully justified, because there is no consensus in the community how to merge SORCE data with UARS measurements which are very close to NRL reconstructions. The authors emphasized that the total ozone response to the solar irradiance variability simulated with the extrapolated SORCE SSI is close to the TOMS/SBUV data. The authors also mentioned that TOMS/SBUV data are better than ground based, therefore the overall conclusion is that the extrapolated SORCE SSI is closer to reality than UARS data. I do not think that this conclusion is solid and well supported by the presented results.

2. The choice of observation data for the comparison with model results is strange. In the text and in Figure 2 caption the authors said that they use TOMS/SBUV and ground-based data extracted from Randel and Wu, 2007 (I guess, the authors used Figure 12 and not Figure 6 as stated in the text). The paper by Randel and Wu (2007) is mostly devoted to the analysis of SAGE data complemented by ozone profiles measured by ozone-sondes. The response of the total ozone to solar variability depicted in their Figure 12 shows the SAGE and TOMS/SBUV data obtained by Randel and Wu (2007) in comparison with ground-based and SBUV data obtained from other sources (WMO, 2003). The authors used only TOMS/SBUV and ground-based data omitting SAGE data. Probably they did it because SAGE data are in better agreement with ground-based data and do not support the author's conclusions. Moreover, the total ozone response to the solar irradiance variability was analyzed in WMO (2003, section 4.2.6.1) and the disagreement between the ground-based and merged satellite data was partially explained (see also Appendix 4A) by some problems with TOMS data. I think all these issues should be properly discussed to avoid any misinterpreting of the results.

3. The performed model runs are useful and help to understand the model sensitivity to the external forcing; however the lack of volcanic aerosol, QBO as well as constant chlorine loading (and possibly constant greenhouse gases) makes the comparison of the model results with observation data very doubtful. To justify the absence of volcanic aerosol the authors stated that "Aerosol effects are considered to be negligible (Randel and Wu, 2007)". I think this statement is completely wrong. Randel and Wu (2007) excluded volcanic term from their regression analysis due to the problem with SAGE data. They stated "Note that we do not include a volcanic aerosol proxy term in our statistical analysis (as in work by Stolarski et al. [2006]), because there are no SAGE data available for postvolcanic periods (the eruption of El Chichon (April 1982) occurred during the SAGE I and II data gap, and SAGE II data are unavailable after the eruption of Mt. Pinatubo in June 1991, as discussed above). The importance of volcanic aerosol for the total ozone and especially for the proper detection of the solar signal (due to possible aliasing) has been widely discussed in the literature during last 30 years. The role of QBO is also very important and discussed in the literature. The absence of these important drivers is crucial for the proper validation of the simulated solar response against observation data.

4. The last paragraph of the section 5 is really mysterious. The authors discuss warming in the lower stratosphere without any explanation/illustration. The reader cannot even guess where this effect is coming from.

5. The reasons for the total ozone enhancement for the solar maximum conditions are not properly discussed. In most cases the authors say that "...an enhanced production of stratospheric O3 at wavelength below 240 nm..." is responsible. However, this photo-chemical process cannot explain the secondary maximum of ozone and temperature responses to the solar irradiance variability observed in the lower tropical stratosphere. The chain of processes responsible for this feature has been widely discussed in the literature and should be mentioned in the text and illustrated using the model results. I think, it is necessary to show not only the total ozone response but also the

vertical structure of the ozone and temperature responses to understand which layers and mechanisms are responsible for the total ozone changes.

Minor comments and technical corrections

I think that the manuscript should be completely rewritten; therefore I do not describe many minor errors and unclear statements in the text.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 1867, 2012.