Answer to anonymous Referee #2

We thank the Referee for her/his comments and suggestions. We answer point by point in the following with the Referee's comments added in *red/italics*. Text added to the revised version of the manuscript is included here in *blue/italics*.

General comments

The subject of this paper, i.e., identifying and quantifying sources of uncertainty in modeled atmospheric response to 11-year variations in SSI, is compelling and the basic tools (2 sets of CCM simulations, SSI data sets, application of well-established ANOVA statistical methods) are appropriate. The use of a common reference SSI data set for solar minimum and application of relative changes in SSI informed by the different data sets makes sense. There are several areas (identified below) where the present manuscript does not provide enough information to allow the reader to understand this work and its implications. These areas can be summarized as follows: (1) Context – why did the authors undertake this study, and what solid, quantitative conclusions does this study offer that will be of use to other researchers; (2) The advantages and limitations of the ANOVA approach – the limitations in particular need to be clarified; (3) Presentation – some figures and some of the discussion were difficult to understand, some reorganization and revision is warranted to improve the overall readability of the manuscript. These areas should be addressed in a revised manuscript before I can recommend publication.

Major Comments

1. The title of the manuscript is not very descriptive. It should probably be stated somewhere in the title that this is a modeling study. One could read this title and think this is an observational study of variations in climate (e.g., surface temperatures, precipitation patterns, etc.), rather than a very specific examination of how sensitive middle atmospheric processes in CCMs are to imposed variations in SSI related to the 11-year solar cycle. The title also says this is Part I, but I do not understand why this is so. What will part II be about, and why does this need to be a two-part study?

We changed the title. It now explicitly includes a hint to CCMs, to avoid misunderstanding.

Quantifying uncertainties of climate signals in Chemistry Climate Models related to the 11–year solar cycle. Part I: Annual mean response in heating rates, temperature and ozone

The study is entitled "Part I" and deals with annual mean quantities, while "Part II" of the study is analysing the impacts on the dynamics especially in the winter seasons and also includes the impact of auroral forcing. Currently Part II, with the working title "Quantifying uncertainties of climate signals related to the 11 year solar cycle - Part II: Dynamical impacts of irradiance and auroral forcing", is in preparation. We have added the following sentence to Section 1:

In this Part I of our study we concentrate on the annual mean solar response in heating rates, temperature and ozone, while Part II (in preparation) focuses on the dynamical solar and auroral responses in northern winter.

2. The authors have undertaken a very ambitious task requiring a lot of detailed statistical analysis. After reading the introduction, it is still unclear to me why this study is being performed.

There are numerous modelling studies dealing with the 11-year solar cycle response, as mentioned in the introduction. All these studies rely on one specific SSI data set (not necessarily the same one) and employ different CCMs. The motivation for our study is (i) to provide a robust estimate of 11-year solar cycle responses as derived as the ensemble mean after employing two different CCMs and five different SSI datasets and (ii) to provide quantitative estimates of the relevance of these two factors (regarding the uncertainty) in this context. The following paragraph is added to the Introduction.

The SSI data prescribed in the models are the second source of uncertainty when modelling the solar response. Shapiro et al. (2011) investigated the influence of the 27-day variations of four different SSI observations on the chemistry of the upper mesosphere in a 1D radiative-convective chemistry model. The deviant solar cycle behaviour of the SORCE (Solar Radiation and Climate Experiment) measurements has motivated a number of CCM studies (e.g. Haigh et al., 2010; Merkel et al., 2011; Ball et al., 2011, 2016; Swartz et al., 2012) comparing simulations using prescribed SORCE SSI data with reconstructed SSI of the Naval Research Laboratory (NRLSSI) or the Spectral And Total Irradiance REconstructions (SATIRE) model.

The authors state (page 2 line 8) that we have a good understanding of the chemical and dynamical processes, but discrepancies between the observed and modeled responses remain? What is not stated is how big these discrepancies are, and why they are important within the larger context of climate modeling.

We have added the following sentence to the Introduction.

The variability induced by the 11-year solar cycle SSI and TSI variations is part of the natural variability of the climate system. Besides the ability of GCMs and CCMs to model the right climatological state of the atmosphere and their chemical species, it is also an important aspect of climate models to realistically reproduce this natural variability.

Some more quantitative discussion of these discrepancies in the introduction are needed. For example, page 2 line 25 states there is "large model spread" – how large is this and why is it important? Is this spread larger than uncertainties in observational-based estimates of 11-year variations related to solar forcing?

A quantitative statement about the range in simulated solar responses is now included.

The simulated solar response in annual mean tropical (25°S–25°N) temperature (1960–2004) near the stratopause ranges from 0.45 to 1.4 K, whereas the SSU satellite data (1979–2005) show 0.85 K for a comparable height region, and ERA-40 reanalyses (1979–2001) show 1.4 K. The annual mean solar response in ozone mixing ratio for the same region and time frame shows less model spread with an ozone increase of 2% in the upper stratosphere, which is in good agreement with observations. Towards lower altitudes the model spread increases and discrepancies to the observations get larger (SPARC CCMVal, 2010).

3. The discussion of the "top down" vs. "bottom up" mechanisms (page 2 lines 13-25) should be condensed and revised to clearly state that this study is focusing entirely on the "top down" effect. The "bottom up" effect relies not only on changes in TSI but also on a very complex interaction between ocean and atmosphere, and it should be stated that the present study cannot address this mechanism with the model simulations presented here.

The nature of an introduction section should be to introduce the reader into the broad scope of the topic (in this case the response of CCMs to the 11-year solar variability and its uncertainty). To include other pathways of the solar influence is therefore justified. The "bottom up" mechanism is only briefly introduced to distinguish from the more important "top down" effect for completeness, and no link is made here to the goals of our study.

4. The ANOVA method finds the largest uncertainties in the upper mesosphere, but I don't think any of the observational studies cited in the Introduction deal specifically with the upper mesosphere. From what's presented in the manuscript, it's unclear *how quantifying these upper mesospheric uncertainties are directly relevant to improving our understanding of climate signals.*

Also taking into account your comment "3." in the section "Additional comments, revisions, suggestions" we have added a paragraph to the introduction, dealing with studies of the 11-year solar response in the mesosphere.

Model intercomparions as CCMVal-2, CCMI, and CMIP5 focus on the solar response in the troposphere and stratosphere. Higher up in the mesosphere where shorter wavelength are not absorbed yet the irradiance variations over the 11-year solar cycle are even larger and have a large effect on atmospheric trace gases like H_2O and CO_2, producing large solar responses in HO_x, CO and also effecting O_3 by subsequent catalytic cycles. These effects in the mesosphere are analysed by a number of modelling studies (e.g. Marsh et al., 2007; Merkel et al., 2011; Beig et al., 2012).

Looking at Figure 2, it appears that in the stratosphere (where most of the ozone resides and where this "top-down" mechanism dominates), the details of the SSI input don't really matter – you get essentially the same modeled response (excluding SATIRE-T), i.e., any differences are smaller than the internal model variability. If this is the case, this should be clearly stated in the abstract and in Section 7.

This is correct and it is now explicitly stated in the abstract and the summary/conclusions section.

Added to abstract:
However, in the region of the largest ozone mixing ratio, in the stratosphere from 50 to 10 hPa, the SSI data sets do not contribute much to the variability of the solar response when the Spectral And Total Irradiance REconstructions-T (SATIRE-T) SSI data set is omitted.

Added to summary/conclusions section:
In the region of largest ozone mixing ratio, in the stratosphere from 50 to 10 hPa, the SSI data sets do not contribute much to the variability of the solar response when SATIRE-T is omitted.

5. Appendix A describes the ANOVA method. I am not an expert in this field, so my comments here are for clarification rather than criticism.
a. My understanding is that part of the ANOVA approach is to construct a model describing the sources of variance, making certain assumptions about what these sources are, and then testing this model to see how much of the variance is explained. The model should be described and listed in equation form – is it a two way model with interaction?

The statistic model describing the solar responses is now given in equation form in the appendix (new equations A1 and A2). The interaction term, describing the variances that are emerging by the interaction of the two treatments (CCM and SSI data set), is included in equation A3.

The anomalies x_{ijk} are the individual solar responses, and their fluctuations around the averaged solar response $\overline{x}$ can be described as

$$\begin{aligned} x_{ijk} &= \overline{x} + (\overline{x_i} - \overline{x}) + (\overline{x_j} - \overline{x}) + (\overline{x_{ij}} - \overline{x_i} - \overline{x_j} + \overline{x}) + \epsilon_{ijk} \\ &= \overline{x} + \alpha_i + \beta_j + \gamma_{ij} + \epsilon_{ijk} \end{aligned}$$

with α_i the deviations of the averages of the two CCMs from the overall average $\overline{x}$; β_j the deviations of the averages of simulations applying the five SSI data sets from $\overline{x}$; γ_{ij} the deviations of the averages of the individual simulations $\overline{x_{ij}}$, $\overline{x_i}$, and $\overline{x_j}$ from $\overline{x}$; and ϵ_{ijk} the random fluctuations that cannot be explained by α_i, β_j, or γ_{ij}. The null hypotheses (H_0) are that the averaged *solar response does not depend on the CCM ($H_{0,1} : \alpha_i = 0$), the applied SSI data set ($H_{0,2} : \beta_j = 0$), or the interaction of CCM with applied SSI data set ($H_{0,3} : \gamma_{ij} = 0$). We apply the two-way ANOVA to test the validity of these hypotheses.*

Is there a specific term for variance from random error?

The term describing the variance from random error is also included in equation A4 as SS_w.

$SS_t = SS_{bA} + SS_{bB} + SS_{bAB} + SS_w$.

b. With regard to figure 1, center and right columns, it's clear that not all the variance is being explained by the SSbB (center) and SSbA (right) terms. This is alluded to on page 10 line 1-2, where the authors state that the random contribution is largest.

c. For a complete description of the problem, would it be better to limit figure 1 to the annual mean responses, and construct a new figure 2 listing all terms of the ANOVA model so we can see all relevant terms (e.g. the treatment A term treatment B term and the interaction term)?

As recommended by the Referee, we have divided Figure 1 into a part showing only the averaged solar responses (new Figure 3) and a part showing the results of the ANOVA, including also the part of the variance that can be explained by the interaction of the two treatments (now Figure 4).

Compared to $R_{a,A}$ and $R_{a,B}$ the fraction of explained variance by the interaction of SSI data set and CCM ($R_{a,AB}$) is only small. Some significant differences of the solar responses in SW heating rates and ozone are explained by the interaction in the upper mesosphere, and near the stratopause for temperature.

It would be most helpful if the description of current Figure 1 middle and right columns referred directly to the terms and equations in the Appendix so we know for sure what is being plotted, i.e. middle column is SSbB term equation A3, etc. Also the hatched areas in the middle column for SSI/temperature and SSI/ozone are extremely hard to see, making it difficult to understand what is and isn't significant.

We revised the discussion of the ANOVA results. The terminology of the Appendix is now explicitly used.

d. Please explain in more detail how degrees of freedom were determined. The CMIP6 data set is an average of NRLSSI2 and SATIRE data sets, so it's not an independent member of the K=B group. Shouldn't this affect the degrees of freedom that ultimately impact the significance tests with the F statistic?

The degrees of freedom are calculated as required for a two-way ANOVA. This is now described in the appendix in more detail. We agree with the Referee on the problem that the CMIP6 SSI data set is not an independent SSI data set, as it is build by averaging the NRLSSI2 and SATIRE-S SSI data sets. When decreasing the degrees of freedom for the group K=B by 1 ($df_{nB} = 3, df_{nAB} = 3$) the statistical significance is affected. We now have set $df_{nB} = 3, df_{nAB} = 3$ for the revised version, but the differences to the previous calculations are only marginal.

The degrees of freedom of the model are calculated as

$$\begin{aligned} df_d &= n_t - N_A N_B \\ df_{nK} &= N_K - 1 \\ df_{nAB} &= (N_A - 1)(N_B - 1). \end{aligned}$$

These are the degrees of freedom within the groups (df_d) which is the degrees of freedom of the denominators when calculating the F-statistics (Equation A15), df_{nK} the degrees of freedom between the groups, and df_{nAB} the degrees of freedom of the interaction between the treatments which are the degrees of freedom of the nominators in Equation A15. Note that the CMIP6 SSI data set depends on the SATIRE-S and NRLSSI2 data sets and therefore cannot be counted as an independent SSI data set. For the calculations of the degrees of freedom we use $N_B = 4$.

e. Outside of the upper mesosphere, it seems like the SSI changes and CCM differences together don't explain the majority of the variance in the total sum of squares. What does this mean? Is this analysis meaningful? Should we conclude that differences in SSI reconstructions or differences in details of model photochemistry or spectral resolution in the SW heating aren't that important relative to the random model variability?

We are convinced that it is still meaningful to do the two-way ANOVA even if the majority of the variance is caused by randomness. There are large regions outside the upper mesosphere where a significant part of the variances can be explained by the usage of different SSI data sets of the different CCMs, even if the majority of the variance is due to randomness.

6. Section 7 needs revision as noted below:

a. First page 19 lines 12-25 repeat what has already been said in the introduction and could be removed or condensed significantly.

Due to the nature of a summary section, some recurrences can not be avoided.

b. Page 19 line 17: it is stated that SSI data sets provide largest fraction of solar cycle variance in the upper stratosphere/lower mesosphere (30% for heating rate, 30% for ozone, 10 % for temperature) but that is not strictly true. The majority of the variance is unexplained, wrapped up in an interaction term or some other manifestation of random model variability. I think it would suffice to say that the SSI differences explain up to 30% in variance in heating and ozone, and only 10% in temperature.

We agree with the Referee and have added the following paragraph in Section 7.

However, in the upper stratosphere/lower mesosphere the largest fraction of solar cycle response variance is random and not related to differences in SSI data sets or the applied CCM.

c. Page 21 lines 5-13: The discussion of the total ozone effects is confusing, and does not seem to produce any specific conclusions. The sentence "Distinct differences in TCO anomalies between the CCMs are also reflected by the relatively large fraction of the anomaly variability that can be explained by differences between the CCMs" seems circular and it's not clear to me what the authors are trying to say.

The ANOVA of TCO shows the largest contributions of the CCM treatment to the explained variability of the anomaly in regions with systematic differences between the CCM's TCO anomalies (i.e. in high southern latitudes and in northern mid-latitudes).
The word "reflected" is now replaced by "expressed".

The finding that WACCM and EMAC models have lower percentage of TCO response from p > 16 hPa compared to one observational study (Hood 1997) does not seem to be directly relevant to the state purpose of this study, especially since by design these CCM simulations do not have realistic decadal variations in lower atmosphere forcing (i.e., fixed repeating monthly *mean SST's, etc).*

We agree with the reviewer that we miss a part of the variability without transient forcings. But even though the simulations do not capture the full scope of natural variability, we are convinced to receive ANOVA results of the TCO and partial column ozone that are relevant within the scope of this study. Our main purpose, to quantify the part of the variability of the solar response that can be explained by SSI data set or CCM, is also achievable with the time slice setup of our study.

It's clear from Figure 8 that the ANOVA method cannot disentangle variance related to SSI and model transport in the lower stratosphere.

That the SSI data sets have nearly no effect on the lower stratospheric ozone column is also a result worth showing.

Based on what's presented in this paper, it would make sense to keep Figure 6, omit Figures 7 and 8 and related discussion, and summarize your findings (i.e., that statistically significant attribution of TOC variance related to SSI or CCM differences as you've defined them is not possible due to large internal model variability).

As justified in our responses to the two preceding remarks, we want to keep the related material in the manuscript.

d. Page 21 lines 14-19: Based on the discussion here, it's not clear why a two-way ANOVA approach is warranted compared to a 1-way (SSI changes) approach. Basically, you are saying you don't think you have fully sampled the "CCM spread" as it is referred to here. So why is 2-way justified? This might be a good place to note the importance of experimental design when using ANOVA that could help guide future investigations.

We believe that it is useful to apply a two-way ANOVA with only two CCMs, as we do not demand to draw a general conclusion with respect to the influence of CCMs on the solar response. The null hypotheses of the ANOVA are that there are no significant influences by the usage of five different SSI data sets in the CCM simulations (i.e. by treatment $K = A$) or by the two CCMs (i.e. by treatment $K = B$) or the interaction of both treatments. This study was initiated within a national research project where two project partners could apply either EMAC (FUB) or WACCM (GEOMAR), thus we were forced to limit our analysis to two CCMs. Although, ideally it would be desirable to apply a wide range of CCMs, this option is quite unrealistic within a national research project.

Additional comments, revisions, suggestions

1. Abstract: It should be noted somewhere in abstract that you are using time slice integrations based on 1989-1994 differences in SSI.

The experimental set up is now described in more detail in the abstract:

The solar response is derived from climatological differences of time slice simulations prescribing SSI for the solar maximum in 1989 and near the solar minimum in 1994. The SSI values for the solar maximum of each SSI data set are created by adding the SSI differences between November 1994 and November 1989 to a common SSI reference spectrum for near solar minimum conditions based on ATLAS-3 (Atmospheric Laboratory of Applications and Science-3).

2. Page 1 line 16: can you define middle atmosphere?

We have rephrased this sentence slightly and skipped the term 'middle atmosphere'.

Solar ultraviolet (UV) radiation is largely absorbed in the stratosphere and mesosphere, thereby heating these regions and forming the ozone layer, filtering the most harmful part out of the solar spectrum and protecting life on Earth.

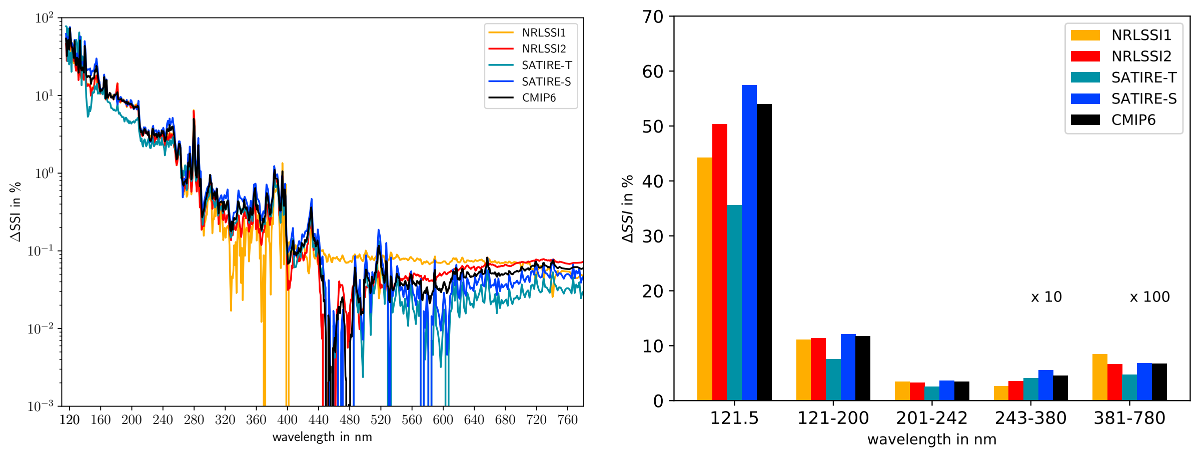
3. Page 2 line 3: the authors cite one reference here (McCormack and Hood), but there are a lot of subsequent studies on observed solar cycle variations that should also be referenced.

Additional references are now included: Soukharev and Hood, 2006; Randel and Wu, 2007; Maycock et al., 2016; Ball et al., 2019.

As mentioned above, observational studies for the mesosphere in particular would be good, since this is where you end up seeing the biggest impact of SSI differences. For example, Beig at al JGR 2012 (https://doi.org/10.1029/2011JD015697).

The study of Beig et al. (2012) is now cited in the new paragraph included in the introduction, discussing modelling studies of the solar responses in the mesosphere. We have added:

Model intercomparions as CCMVal-2, CCMI, and CMIP5 focus on the solar response in the troposphere and stratosphere. Higher up in the mesosphere where shorter wavelength are not absorbed yet the irradiance variations over the 11-year solar cycle are even larger and have a large effect on atmospheric trace gases like H_2O *and* CO_2*, producing large solar responses in* HO_x, CO *and also effecting* O_3 *by subsequent catalytic cycles (e.g. Marsh et al., 2007; Merkel et al., 2011; Beig et al., 2012).*



(a) Solar Cycle 22 (descent-2)

(b) Broad bands Solar Cycle 22 (descent-2)

Figure 1. (a) Solar cycle SSI variations from Nov. 1989 to Nov. 1994 relative to Nov. 1994 (ΔSSI) in % for wavelengths ranging from FUV to the VIS bands; (b) as in (a) for the Lyman-α (121.5 nm), Far-UV (121–200 nm), Herzberg continuum/Hartley bands (201–242 nm), Hartley-/Huggings-bands (243–380 nm) (multiplied by a factor of 10) and visible (381–780 nm) (multiplied by a factor of 100).

4. Section 2: It would be most helpful to have a plot comparing the different SSI data sets somehow. Is it possible to plot the SSI differences relative to the ATLAS solar min values over a range of UV wavelengths. This would illustrate for the reader how differences among the different data sets compare to the overall 11-year max-min differences. Since the change in SSI from solar max – min is strongly wavelength dependent, this might be more informative than Table 1 that averages over very large intervals.

Figure 1 is now included in the manuscript, showing the SSI changes from November 1989 to November 1994 relative to Nov. 1994 for wavelengths ranging from 120–780 nm (new Figure 1a) and Figure 1b showing ΔSSI in % as in Table 1. There is not much new information added by this Figure, as the ΔSSI of the integrals over broad bands in Table 1 summarize the main message, but we agree with the Referee that a visualisation can highlight the differences among the SSI data sets.

5. Section 3.1: Why are the QBO treatments different? What observed winds are used for the relaxation in EMAC and for what period of time?

We have to admit that the different treatment of the QBO in the two models is the result of a lack in coordination. However, there is a (weak) reason why the QBO-treatment is different for the two models for the specific type of experiments as analysed in our study. As a reminder, we performed timeslice-experiments for year 2000 forcings. For EMAC this was done by starting the simulations in model year 1995 and running them towards model year 2045. For the relaxation of the zonal wind in the equatorial, lower stratosphere in EMAC the QBO data set of the FU-Berlin is used that is based on radiosonde observations at stations near the Equator (Canton Island, Gan, Singapore). The observed QBO time series is used from January 1995 to November 2011 of the 50 years EMAC time slices. The time period from December 2011 to December 2044 is covered by subsequently adding compatible segments of the observed QBO. For WACCM, the standard setup for this type of timeslice experiment starts simulations in model year 1. This was done for our study, too. We integrated the model over model years 1-48. Regarding the QBO-nudging it would have been possible to use a representation of the observed QBO. WACCM offers the possibility to do so for any simulation period via spectral coefficients, essentially repeating the observed QBO-timeseries forward and backward in time. Given the generally idealized nature of our timeslice simulations, the modeling group responsible for the WACCM-experiments refrained from using this approach (basically extrapolating a QBO-timeseries almost 2000 years back in time) and made use of the WACCM default procedure for timeslice simulations that is using an idealized QBO with a fixed period of 28 months as described in the cited study of Matthes et al., 2010. We agree that it would have been better to properly coordinate this element of our experiments, avoiding this difference between EMAC- and WACCM-simulations. However, most importantly, the identical QBO-nudging has been used for the respective solarmin- and solarmax-experiments of each of the two models. Hence, the differences between solarmin and solarmax which are further analysed in our study are purely a result of differing solar activity.

In doing ANOVA, experimental design is very important. Were these CCM simulations designed and performed especially for this study, or is this study using simulations that were generated previously. This could be helpful to note in the paper. If these were simulations already generated, this paper is more of a proof of concept on how to apply ANOVA and how perhaps future multi-model CCM experiments should be designed in order to best use the ANOVA method.

All EMAC and WACCM simulations in this paper have been made explicitly for this study. The intention was to apply a setup as close as possible for both the EMAC and WACCM simulations. As explained above, with respect to the QBO-nudging the setups are slightly different, as explained above. We have added a short statement to the introduction to Section 3.1.

All EMAC and WACCM simulations have been made explicitly for this study, to ensure that the differences in the solar responses are exclusively related to the SSI data set prescribed or the CCM applied, and not due to differences in the scenario.

6. Figure 4 and related discussion in the text could be removed. In its present form, it doesn't add much information, especially since I'm not sure how much data ERA5 uses in the upper stratosphere/mesosphere, meaning the ERA5 fields could be very model-dependent themselves, and not the best standard to compare with.

We disagree with the referee on this point, as a comparison of modelling results with a common base can always be helpful to identify potential problems. Comparing to ERA5, the cold bias of EMAC in the tropical UTLS that affects water vapour in the stratosphere and mesosphere can be identified.

It might be more illuminating to directly compare differences in zonal mean T, zonal wind, and ozone between WACCM and EMAC.

For a direct comparison of zonal mean T and ozone please see Figure 5b and 5c. The comparison of the zonal mean zonal wind is omitted, as this quantity is not in the scope of this paper.

7. Page 9 line 33 – I really can't see the grey hatching in Figure 1 very well. Is it possible to plot it another way? Maybe only plot significant values?

The grey hatching is now changed to cross hatches to strengthen the presentation of the non-significant regions.

8. Page 11, line 9: it is stated that the t test is and resulting error bars come from the complete ensemble but Figure 2 caption states the error bars are for the WACCMX/EMAC CMIP6 simulations. Which is it?

We thank the referee to point to this inconsistency. The information in the Figure caption was outdated. The description in the main text is correct. The Figure caption is now updated.

The 95% uncertainty error bar is given for the averaged solar response over the complete ensemble, calculated with a Student's t test.

9. Page 12: I'm not sure it's worth reviewing Chapman cycle photochemistry here. If the authors wish to describe specific reactions in detail, I would suggest using equation form rather than in the text, and perhaps put some of the more complex reaction in an appendix?

Chapman cycle photochemistry is discussed here, as the differences in the SSI amplitudes of the adequate UV spectral regions are directly related to the photochemical reactions involved.

References

Ball, W. T., Unruh, Y. C., Krivova, N., Solanki, S. K., and Harder, J. W.: Solar irradiance variability : a six-year comparison between SORCE observations and the SATIRE model, Astronomy & Astrophysics, 530, A71, doi:10.1051/0004-6361/201016189, 2011.

Ball, W. T., Haigh, J. D., Rozanov, E. V., Kuchar, A., Sukhodolov, T., Tummon, F., Shapiro, A. V., and Schmutz, W.: High solar cycle spectral variations inconsistent with stratospheric ozone observations, Nature Geoscience, 9, 206–209, doi:10.1038/ngeo2640, 2016.

Beig, G., Fadnavis, S., Schmidt, H., and Brasseur, G. P.: Inter-comparison of 11-year solar cycle response in mesospheric ozone and temperature obtained by HALOE satellite data and HAMMONIA model, J. Geophys. Res.: Atmos., 117, doi:10.1029/2011JD015697, 2012.

Haigh, J. D., Winning, A. R., Toumi, R., and Harder, J. W.: An influence of solar spectral variations on radiative forcing of climate, Nature, 467, 696–699, doi:10.1038/nature09426, 2010.

Marsh, D. R., Garcia, R. R., Kinnison, D. E., Boville, B. A., Sassi, F., Solomon, S. C., and Matthes, K.: Modeling the whole atmosphere response to solar cycle changes in radiative and geomagnetic forcing, Journal of Geophysical Research, 112, D23 306, doi:10.1029/2006JD008306, 2007.

Merkel, A., Harder, J. W., Marsh, D. R., Smith, A. K., Fontenla, J. M., and Woods, T. N.: The impact of solar spectral irradiance variability on middle atmospheric ozone, Geophysical Research Letters, 38, 1–6, doi:10.1029/2011GL047561, 2011.

Shapiro, A. V., Rozanov, E., Egorova, T., Shapiro, A. I., Peter, T., and Schmutz, W.: Sensitivity of the Earth's middle atmosphere to short-term solar variability and its dependence on the choice of solar irradiance data set, Journal of Atmospheric and Solar-Terrestrial Physics, 73, 348–355, doi:10.1016/j.jastp.2010.02.011, 2011.

Swartz, W. H., Stolarski, R. S., Oman, L. D., Fleming, E. L., and Jackman, C. H.: Middle atmosphere response to different descriptions of the 11-yr solar cycle in spectral irradiance in a chemistry-climate model, Atmos. Chem. Phys., 12, 5937–5948, doi:10.5194/acp-12-5937-2012, 2012.