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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying uncertainties of climate signals related to the 11–year solar cycle. Part I: Annual mean response in heating rates,temperature and ozone" *by* Markus Kunze et al.

Anonymous Referee #2

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Review of "Quantifying uncertainties of climate signals related to the 11 year solar cycle. Part I: Annual mean response in Heating Rates, Temperature and Ozone" by Kunze et al.

This manuscript presents results from a series of CCM simulations using two different models (WACCM and EMAC) and five different SSI data sets (NRLSSI1, NRLSSI2, SATIRE-T, SATIRE-S and CMIP6). These simulations are used to investigate how the annual mean response of solar heating rates (due to UV absorption by ozone and O2), temperature, and ozone in the stratosphere and mesosphere in each model depends

on the input variations in SSI from these 5 data sets relative to common solar minimum SSI specified by the ATLAS-3 reference spectrum. The relative responses of the modeled variables due to differences in SSI (i.e, external forcing) versus differences between the two models (i.e., internal variability) are calculated using two-way analysis of variance (ANOVA). According to the abstract (page 1 of manuscript, lines 9-12), the authors report that differences among the SSI data sets have strongest influence on the modeled shortwave heating rates, ozone, and temperature in the upper stratosphere and mesosphere, while the largest differences attributed to the treatment of photochemistry, etc. in the CCMS are identified in the upper mesosphere (page 1 lines 13-14). The authors also indicate an apparent model-dependent solar cycle response in the lower stratosphere.

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?

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. 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. 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?

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.

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. 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.

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? Is there a specific term for variance from random error?

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)? 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.

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?

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?

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.

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.

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

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.

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.

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

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. 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).

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.

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? 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.

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

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?

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? 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?

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