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Interactive comment

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 #3

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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 M. Kunze et al. Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-1010-RC1, 2020

This study is focused on quantifying the uncertainties in two different CCM model output (heating rates, temperature, ozone, HOx, etc.) due to the 11-year solar cycle and, in addition, to quantify what portion of the uncertainty is due to differences in SSI forcing dataset and what portion is due to differences in the radiation and photolysis schemes/parameterizations of the different CCMs. The study used a two-way ANOVA

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for results. I am not an expert in ANOVA so I greatly relied on Appendix A to interpret the manuscript. My understanding is that the two-way ANOVA consisted of binning the simulations into two groups and evaluating for how each group, and the interaction of the groups, contributed to the total variability in the simulations. One "treatment" consisted of 45 years of simulations for each of 5 unique SSI data sets (45*5 = 225 elements) of each CCM (WACCM and EMAC); this first treatment then was more of a comparison between the CCM outputs. The second "treatment" consisted of 45 years of simulations for each of the 2 unique CCM's of each SSI dataset; this second treatment was then more of a comparison of the SSI datasets.

Differences in CCM's were found to contribute more to the uncertainty in the upper mesosphere whereas differences in SSI datasets were found to contribute more to the uncertainty in the upper stratosphere and lower mesosphere. However, the majority of the variability in the output was due to "internal variability" in the models.

I find the study of high interest and worthy of publication. I do provide some comments below for consideration. In brief, these focus firstly on the solar irradiance dataset aspect of the study. I find the adoption of a common spectrum from which to baseline differences in the solar cycle 'amplitudes' of the various datasets novel. Secondly, the messaging of the study could be improved to provide the background of why the study was undertaken.

SSI Comments a. The general reader may be unclear why it was necessary to use 5 different SSI datasets, or why you chose the ones you did, or even that there isn't agreement across SSI datasets on longer time scales (observed or modeled) . I would suggest adding a paragraph or two to improve the messaging behind your study, probably in the Intro or in Section 2. b. I agree that TSI observations are relatively short (since 1978) and that SSI observation record is even shorter, nor full spectral coverage, and has time gaps. However, your study does select a relative short period of time to investigate the impacts of SSI over (1989-1994). Therefore, it begs the questions of why that particular time range and not another when full spectrum observations existed (i.e. during the SORCE era) or even partial spectrum observations (265-500 nm) by the AURA OMI instrument. In essence, I'm asking you to more directly draw the line between your "focused" study and the SSI dataset needs of the model intercomparisons studies like CMIP6 which require full spectrum and very long time coverage. This leads to necessary use of modeled SSI datasets, which have differences between them and with observations. It would be helpful to bring the discussion of the Coddington et al and Yeo et al. results (Page 6, line 7 through end of paragraph) in earlier in the section for this reason. c. I do like that you've chosen a single spectrum to adopt as a common baseline for solar minimum conditions. I feel that's quite novel. I am concerned, though, that the manuscript doesn't adequately address how this approach might impact results. You do say that a reference baseline would lead to a certain climatology state (end of page 13 to page 14) and that differences from that baseline, as would occur from using SOLAR-ISS as the reference, would result in a different climatology. However, is it necessarily true that the solar response variations are truly linear from an adopted baseline? Maybe more clear way to ask is whether gas phase reaction rates or water vapor abundances that you mention on page 14 might "bottom out" or "max out" if the baseline climatology/temperature was too high or too low? I would also suggest bringing this discussion up earlier, in addition to where it is in the conclusions. d. In conclusions you also discuss how choosing SC 22 (selected, I understand, because of time range of ATLAS 3 observations) should be reflective of other solar cycles in the 21st century. You examined the irradiances in the Lyman alpha through UV for the various SSI datasets with other solar cycles and found a linear relationship. Was that relationship with TSI magnitude, sunspot number, or something else? In the Coddington et al., 2019 paper you reference, their Tables 3 and 5 show a larger change in integrated SSI (in the 100-200 nm bin) from solar cycle to solar cycle than occurs in differences across some of the datasets you use in your study. Similar to the above comment, you might want to bring this up earlier in Section 2 as well. e. It's possible this is jargon in the CCM community, but is it typical to use phrases of 'solar cycle response' for simulations where the transition from perpetual solar minimum to perpetual solar maximum is quite abrupt?

General comments Page 2, lines 24 – 29: The end of the one paragraph is focusing on the CCM model "spread" caused by differences in spectral resolutions of the shortwave radiation parameterizations or photolysis in the models. The next paragraph begins with different spectral distribution of the SSI data set also impacting CCM models. In the 2nd case, you are referring to the magnitude of the SSI within a spectral bin and not differences in spectral resolution of the SSI observations, but this could easily be confused during the transition of one paragraph to the next.

Page 3, line 3-4: You end with "the effects of the 11-year solar cycle differences in spectral distribution and amplitude…". However, by adopting the common reference baseline spectrum, you have removed the effects of spectral distribution from the study. It's clear from your earlier text what you mean and that it's just an error here.

Page 4, line 12: What type of scaling did you apply to make ATLAS 3 integrate to SORCE TIM TSI? Wavelength independent? "The extended ATLAS-3 spectrum was then scaled to obtain…"

Page 4, line 20: Needs some clarification. The (facular brightening and sunspot darkening) indices themselves do not describe the relationship between sunspots and faculae on the Sun's disk and irradiance. The indices are derived from observations of proxies of faculae and sunspots. It's rather the scaling factors computed from the multiple linear regression of these indices with SSI observations that are used to scale the change in faculae and sunspots into a net, wavelength-dependent, irradiance change.

Page 4, line 23: "The TSI changes are added…" should be "The SSI changes are added.."

Page 4, line 27: While Viereck et al., 2001 is a perfectly appropriate reference for a general discussion of the Mg II index, the correct citation for the University of Bremen Mg II index reference is Snow, M., Weber, M., Machol, J., Viereck, R., & Richard,

E. (2014). Comparison of magnesium II coreâĂŘtoâĂŘwing ratio observations during solar minimum 23/24. Journal of Space Weather and Space Climate, 4, A04. https://doi.org/10.1051/swsc/2014001

Page 5, line 23-24: I am aware that CMIP6 SSI and TSI data are the average of output from NRLSSI2 and SATIRE-S. However, it's unclear to me the relation of this is to your choice of using data from November 1989 and November 1994 in the study?

Page 6, EMAC section: I'm not an expert on CCMs but I find the description of EMAC difficult to read. It doesn't flow as easily as the following section on WACCM, and the acronyms aren't defined. I would suggest some word-smithing to bring it up to the same high quality as the rest of the paper.

Page 7, between WACCM and section and the start of Section 3.1: Again because I'm not an expert on CCM's, it would be nice to have a summarizing sentence or two here as a take home message for the non-expert. Are these suitable models to compare, and are there obvious reasons why their unique setup and execution would lead you to expect differences in their outputs?

Page 7, lines 27-28: One too many of each of the words, "both" and "simulations".

Page 9, Section 7: A general comment in this section is to make it is more clear that majority of the uncertainty, or spread, in the CCM output comes from internal variability in the CCM's. Only a fraction of the model spread can be attributed to differences in SSI datasets or differences in the CCM's themselves. (If I understood correctly).

Page 13, line 20: "...10-40% of the variability of the solar signal [insert of what component, heating rate, temperature, etc.] in the stratosphere and ..."

Page 20, line 8-10: Is there a transition in thought from the sentence ending on line 8 about the distinct differences that appear for SATIRE-T to the next sentence discussing how reduced solar cycle amplitude explain the weaker solar signals in temperature? Does that 2nd sentence also refer to SATIRE-T? If so, Table 1 shows that SATIRE-T has a stronger solar cycle amplitude in the 201-242 nm range, not weaker.

Page 21, line 3-4: You provide support that downward transport of thermospheric photolysis reactants is needed to realistically simulate solar cycle effects. Is this a new finding for the CCM community? It seems to me that you might emphasize the importance of this in guiding CCM model development and directing CCM advances.

Figures: I was finding that the significance hatching in the figures was very difficult to see, particularly in the middle and right hand columns of Figure 1. However, when I look today, it's much clearer on-screen. Perhaps it is just a problem with my printer.

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