The SOLCHECK Project: A State-of-the-Art Investigation into the Imprints of Solar Variability Across Multiple Timescales

Reply to Reviewer 1:

We would like to thank the reviewer for his or her time and valuable feedback. Our responses are listed below in blue.

As summarised in the abstract, this manuscript reports climate model simulations designed to assess the “influence of solar variability on the atmosphere from the pre-industrial era to the present and future.” This is a very well-written article reporting a well-intentioned study by the authors, many of whom have a solid history in studies of this type. However, as explained further below, whether intentional or not, a somewhat unfair advantage is given to the effect of particle precipitation through emphasis on a “worst-case” SEP event occurring only once every few millennia. In contrast, the 11-yr solar UV forcing adopted for the simulations is severely diluted and underestimated, leading to no modeled top-down influence on tropospheric climate at all! Prior work on the latter mechanism is also not adequately reviewed or taken into consideration. Major revisions are needed prior to publication.

1. In section 3, possible impacts of the 11-yr solar cycle on stratospheric and tropospheric dynamics are investigated. As shown in Figure 2, a clear, but rather weak, 11-yr solar signal in ozone and temperature in the tropical middle and upper stratosphere is obtained in the three considered models. In section A2.2 (lines 377-378), it is noted that the assumed solar spectral irradiance forcing is “based on the CMIP6 solar forcing dataset (Matthes et al., 2017)”, which is an average of the NRLSSI2 and SATIRE spectral irradiance models. The resulting effect of 11-year solar forcing on the stratosphere using this SSI dataset was investigated by Matthes et al. using two “state-of-the-art” chemistry-climate models, CESM1-WACCM and the ECHAM/MESSy atmospheric chemistry model (EMAC). One of these models, EMAC, appears to be nearly the same as that used in the present study. As shown in their Figure 8, to assess the impact of the 11-year solar cycle, differences between perpetual solar maximum and minimum experiments on annual mean profiles of shortwave heating rates, temperature, and ozone concentrations averaged over the tropics (25S to 25N) were calculated. The modeled solar flux during cycle 22 for November 1989 was used for solar maximum and that for November 1994 was used for solar minimum. This was representative of cycles occurring during the last half of the 20th century and corresponds to a difference of about 150 units of F10.7 solar flux. As seen in their figure, the shortwave heating rate change for ECHAM peaks at about 0.20 K/day at 1.5 hPa while that for WACCM peaks at about 0.22 K/day at 2 hPa. Their temperature change peaks at about 1 K for both models at 2 and 1 hPa. Their ozone change peaks at about 2.5% for both models at 35-40 km altitude. For comparison, the ensemble mean EMAC shortwave heating change between solar maxima and minima estimated in the present manuscript (Figure 2a) is only 0.1 K/day and the temperature change in the upper stratosphere is only 0.3 K. The

EMAC ozone change shown in Figure 2c is given in ppmV and peaks at about 0.12 ppmV at 10 hPa. At 3 hPa (~ 40 km), the change is about 0.06 ppmV, which is less than 1% (for a climatological mean of ~ 7 ppmV). So, overall, the minimum to maximum changes shown in Figure 2 are much smaller (by factors of 2 to 3) than those estimated by Matthes et al. (2017) using a comparable climate model but applying larger UV forcing typical of recent strong solar cycles.

2. Part of the reduced UV forcing assumed in the present study is due to fact that the results in Figure 2 are composite averages for time-progressive simulations extending from 1850 to 2014. Also, rather than taking an absolute difference between monthly means at solar maximum and minimum to define the spectral irradiance change, 3-year smoothed DJF means of F10.7 were used (section A2.2, lines 391-393). A plot of the assumed F10.7 variation is shown in Figure 3a of the manuscript. During the early part of the record, the highly smoothed solar cycle variations were much weaker than during the latter half of the 20th century, which effectively dilutes the solar UV forcing. According to this plot, the average change in smoothed F10.7 from solar minimum to maximum appears to be no more than 70 or 80 flux units, i.e., about half of what was assumed in the time-slice simulations of Matthes et al. (2017). Consequently, the simulated stratospheric impacts of 11-yr solar forcing shown in Figure 2 are much weaker than is appropriate for most solar cycles occurring during the last 70 years, greatly reducing the ability of these simulations to capture the top-down mechanism.
3. Prior work on the top-down UV forcing problem (other than that of Matthes et al. (2017)) is also not adequately reviewed and taken into consideration in the manuscript. The comprehensive review of Gray et al. (2010; https://doi.org/10.1029/2009RG000282) is only mentioned in passing in line 52 in the context of TSI variations but is misspelled and is not in the reference list. The SPARC-sponsored project SolarMIP from a decade ago is not mentioned and the three primary publications of that project (Mitchell et al., 2015, https://doi.org/10.1002/qj.2530; Hood et al., 2015, https://doi.org/10.1002/qj.2553; Misios et al., 2016, https://doi.org/10.1002/qj2695) are not referenced or discussed in the manuscript, a major omission. For example, adopting a solar minimum to maximum change in F10.7 of ~130 flux units, it was found using multiple linear regression that three high-top CMIP-5 models with interactive ozone chemistry simulated an ozone change in the middle to upper stratosphere of up to 3% , a temperature change near the tropical stratopause of ~ 1 K, and November – December zonal wind changes of up to 6 m/s near 50°N, in approximate agreement with the results of Matthes et al. (2017).
4. To correct these issues, there are several alternatives. The best solution would be to simply remove section 3 from the manuscript, including the study of impacts on prediction skill, which are unlikely to be accurate given the lack of a top-down dynamical response. The remainder of the manuscript, including the SEP simulations (section 2) and the effect of greenhouse gas increases on climate impacts of an hypothesized GSM (section 4), could be retained to make a viable paper. Otherwise, one would need to begin by comprehensively reviewing prior work on the top-down solar UV forcing problem, including at least the publications noted above as well as others. These results would need to be considered throughout the manuscript, including in the conclusions section and in the abstract. In addition, more simulations would need to be performed using a more realistic solar flux variation. One should start with time-slice experiments using EMAC with a 10.7 cm solar flux change of 150 units to reproduce the results of Matthes et al. (2017). Then, time-progressive simulations using the same model should be performed with results analysed using multiple linear regression. Monthly or weekly

> mean solar fluxes should be used rather than 3-month smoothed averages. Daily solar fluxes would be a further improvement since daily fluctuations can be quite large. Analyses of zonal winds during austral winter as well as boreal winter would be needed as noted in comment 9 below.

We thank the reviewer for this very detailed and constructive feedback on our treatment of the 11-year solar cycle. First of all, we want to point out that it was not the intention of the SOLCHECK project to study the relative importance of the climate impact of the 11-year solar cycle versus SEP events. We agree that for such a comparison, a more conservative SEP event would be more appropriate as model input. The goals of SOLCHECK were rather to address open research questions which are of ongoing scientific interest either because the underlying mechanisms are still not fully understood, such as the top-down effect of the 11-year solar cycle, or because they might become relevant in the future, such as a worst-case SEP event or a Grand Solar Minimum.

Since comments 1–4 all address closely related aspects of the modelling, analysis, and interpretation of 11-year solar cycle imprints, we provide a combined response to these points below. We understand the reviewer's main points of criticism as follows:

Point 1: The simulated 11-year solar signal is much weaker than expected, especially compared with Matthes et al. (2017); changes in shortwave heating, temperature, and ozone are smaller by roughly factors of 2–3.

We appreciate these detailed comments. However, we respectfully disagree with several of the points raised by the reviewer.

It is not the intention of the authors to repeat the experiments published by Matthes et al. (2017), for several reasons. Their study followed a different objective and experimental strategy, namely idealized atmosphere-only time-slice experiments with fixed solar-minimum and solar-maximum conditions based on one selected solar cycle. While this approach is useful for testing SSI datasets and isolating mechanisms, it also enhances signal detectability and reduces the influence of internal variability. In addition, the atmosphere-only setup excludes coupled-ocean feedback processes and the bottom-up pathway. The SOLCHECK project addresses a different question: Do solar-cycle signals remain detectable and robust under more realistic transient conditions, including multiple weak and strong solar cycles. Repeating Matthes et al. (2017) would therefore mainly reproduce an already established sensitivity experiment, rather than add new insight to the specific aims of our study.

Furthermore, we do not agree that the constant solar-maximum forcing used in Matthes et al. (2017), based on solar cycle 22, is necessarily representative of the common or recent era. In the reviewer's comment, the solar-maximum conditions of cycle 22 are treated as a suitable reference for typical solar-cycle amplitudes. However, in our view, this assumption is problematic. Since 1850, covering approximately solar cycles 10 to 25, the majority of observed solar cycles have shown weaker amplitudes than solar cycle 22. In fact, 12 out of these 16 solar cycles, including the most recent cycles that best represent the current state of solar activity, have lower amplitudes than the cycle used as reference in Matthes et al. (2017).

Therefore, we do not interpret our results as a dilution of potential top-down signals. Rather, they indicate that earlier idealized studies using strong, fixed solar-maximum conditions may have enhanced the detectability of the top-down response relative to what can be expected under more realistic transient solar forcing. Our simulations are designed to assess this more realistic situation, including both weaker and stronger solar cycles, and therefore provide a complementary perspective on the robustness and detectability of solar-cycle imprints in the atmosphere.

To address the reviewer's concerns and to guide the reader towards a better understanding of the experimental setup and the interpretation of our results, we have revised the manuscript in several ways:

First, we now provide a clearer description of our simulations, the motivation behind them, and how and why they differ from previous modelling approaches, such as Matthes et al. (2017).

Second, we have extended the analysis presented in the manuscript. In addition to showing the ensemble-mean response over all solar cycles, including both weak and strong cycles, we now also analyse periods of weaker and stronger solar activity separately, as applied for example by Drews et al. (2021).

Third, to allow for a more direct comparison, we have made the presentation of the results more consistent with previous studies. For example, we now show the ozone response in percent (see updated Figure 2).

Overall, we believe that these revisions provide the reader with the necessary information to understand how differences in the amplitude of the solar cycle affect the strength of the simulated response, and to assess the results in the context of previous work.

Point 2: The applied solar forcing likely dilutes the signal, because the analysis uses time-progressive simulations from 1850 to 2014 and 3-year smoothed DJF F10.7 values, yielding a solar minimum-to-maximum difference of only about 70–80 sfu instead of approximately 130–150 sfu.

This comment appears to be based on a misunderstanding of our experimental setup and analysis procedure that most likely arose from an insufficiently clear description of the experiments. Our experiments do **not** use smoothed solar forcing. The forcing includes the full range of variability over the complete period of interest. More specifically, we use daily mean values from the datasets recommended for CMIP6 simulations, following Matthes et al. (2017). Only the model output is smoothed, namely the time series used to illustrate the tropical initial response to the solar cycle. Applying such smoothing to improve the visibility of potential solar-cycle signals is standard practice, and here we largely follow the approach of Drews et al. (2021). To avoid further misunderstanding, we have added a more detailed description of the solar forcing and of the smoothing procedure in the methods section (Appendix A2.2).

Point 3: Important prior work on the top-down UV forcing mechanism is insufficiently discussed, especially Gray et al. (2010) and the SolarMIP studies by Mitchell et al. (2015), Hood et al. (2015), and Misios et al. (2016).

We agree with the reviewer's comment and have revised the manuscript to include additional relevant literature to illustrate in more detail the progress in simulating the top-down process over time.

Point 4: The validity of Section 3 is questioned, with the reviewer suggesting either removing it, including the prediction-skill analysis, or substantially revising it through a broader literature review and additional simulations using more realistic solar-flux variability.

As discussed in our responses to points 1–3, some of the reviewer's comments were based on misunderstandings that likely resulted from an insufficiently clear description of the experimental setup and subsequent analysis. We have therefore revised the relevant parts of the Methods section (Appendix A2) to clarify the applied solar forcing, the model setup, and the analysis procedure.

However, we respectfully disagree that the section on the 11-year solar cycle should be removed. In our view, this section is an important part of the manuscript because it shows that, when applying realistic transient solar forcing in ensemble simulations with chemistry–climate models and an Earth system model, additional questions arise regarding the detectability and interpretation of solar cycle signals. The results obtained from advanced models and simulation setup may partly question conclusions drawn from studies using more individualized forcings and idealized model setups. We believe that documenting these results is necessary and represents a useful step towards a better understanding of the role of the 11-year solar cycle in the atmosphere. The data produced within the SOLCHECK project are freely available, and we would like to invite the wider solar–climate community to use these simulations to further investigate open questions regarding the importance of the solar cycle, for example for tropospheric weather regimes.

At the same time, we agree with the reviewer that the prediction-skill analysis does not add sufficient information in its current form. Since we do not find robust solar imprints that can clearly be attributed to the so-called top-down mechanism, a detailed assessment of prediction skill would require substantially more analysis. We have, therefore, removed this part from the manuscript.

5. Regarding section 2, the abstract and the description in section 2 do not make it clear whether UV dose effects on human health of "worst-case" events such as that believed to have occurred in 774-775 would be limited mainly to high polar latitudes (> 70° where the ozone loss occurs) or whether effects would be substantial at middle and lower latitudes where most of the population resides. For example, the description of Figure 1 in section 2 and the figure caption do not mention that the NO_y and ozone calculations are averaged over latitudes > 70°N. One has to look at the labels on the figure to find this. According to lines 109 – 110, "The long-lasting ozone reduction increases UV erythema dose by <5% at mid and low latitudes (Fig. 1c) …" However, according to the green shading in Figure 1c, the KASIMA change in UV exceeds 30% at 20° – 40°N. So this implies substantial effects outside of the polar regions. Similarly, in the Conclusions (lines 240-241), "Ozone loss exceeded 40% around 40 km, with effects on UV radiation and mid-stratospheric temperatures …" But the latitudes where this occurs are not specified (see comment 7 below). Please revise the abstract and the conclusions section to make these descriptions clearer.

This appears to be a misunderstanding. UVI in Figure 1c refers to the UV Index, not to the percentage change in UV dose. The UV Index typically reaches maximum values of around 10. Accordingly, the larger absolute UVI changes at lower latitudes can be explained by the smaller solar zenith angle in these regions. In contrast at high latitudes, where the largest total ozone changes occur, the baseline UVI there is already low due to the larger solar zenith angle. Therefore, even comparatively large ozone changes at high latitudes do not necessarily translate into equally large absolute changes in UVI. More explanation has been added to the text.

All panels in Figure (except for Figure 1c) indicate in the caption on top that the results shown refer to latitudes > 70°N.

Further comments:

6. The sentence in lines 74 to 76 should be separated into two sentences.

Done.

7. In Figure 1, all parts except part (c) are for latitudes > 70N. But part (c) shows the largest increase in UVI at middle and low latitudes. Presumably, this is because the ozone loss in the upper stratosphere also affects the ozone concentration at lower altitudes where dynamical transport can transfer it to lower latitudes. If this is the case, please add text to describe this. Also, a plot of the total ozone change similar to that of Figure 9 of Reddman et al. (2023) would help the reader to understand this better.

We refer the reviewer to our response to Point 5 above.

8. Figure 2c is a plot of the ozone change in ppmV. However, most previous studies (e.g., Matthes et al., 2017) have shown the change as a percentage of the climatological mean. Please replace this figure with one showing percent change. This would produce a peak at a somewhat higher altitude.

The figure has been replaced and now shows the ozone change in percent.

9. Ability to detect an 11-year zonal wind signal could be enhanced in several ways. Doubling the solar cycle UV forcing amplitude in the FOCI experiment was a reasonable attempt but 11-yr zonal wind responses have been detected in other model simulations without resorting to this (e.g., Matthes et al., 2017, their Figure 10 using EMAC and WACCM; Hood et al., 2015, their Figures 10 and 11). Note that a modeled zonal wind signal may be more easily detectable in austral winter due to a more stable polar vortex in that hemisphere.

In our experiments with doubled solar cycle amplitude clearer zonal wind signals emerge mainly in the tropics, where the initial solar signal is generated. However, even these sensitivity experiments do not lead to a more robust zonal wind response in the polar vortex region. We, therefore, conclude that increasing the amplitude of the solar forcing alone does not necessarily improve the detectability of the dynamical response associated with the top-down mechanism. At the same time, we note that such experiments are highly idealized and should therefore be interpreted with caution, particularly in light of the reviewer's emphasis on the importance of applying realistic solar forcing. We have added a short discussion of this aspect to the manuscript and now also cite the relevant literature listed above.

10. In Figure 3, it is very difficult to distinguish the FOCI lines from the F10.7 lines (both gray).

Done.

11. In Figure 3a, there is a strong stratopause temperature dip in ~ 1993. Could this be related to dynamical effects of the Pinatubo eruption? Were volcanic aerosol injections included in the model setup?

This is possible, and we have added a comment on this in the manuscript. The model setup accounts for changes in optical properties due to aerosol injection following volcanic eruptions. This information has now been included in the Methods section (Appendix A2).

12. On p. 9, the last sentence (lines 190-191) is very difficult to understand. Please re-phrase.

Done.

13. Discussion of Figure 5 in lines 209-223 on p. 11: Presumably these effects are all a consequence of TSI variability and are not affected by the stratospheric effects of UV forcing. If so, please state that this is the case.

As stated in the manuscript, both TSI and SSI were adjusted in these experiments to represent Grand Solar Minimum conditions. The effects shown here therefore represent an integrated response that includes both top-down and bottom-up mechanisms. We have added a corresponding clarification to the manuscript.

14. In Figure 5, the labels (a, b, etc.) are inconsistent with what is stated in the caption and in the text.

We are sorry about the confusion and corrected the figure labels to be consistent with the text.

15. Section 5 (Conclusions). The short paragraph on solar signals of the 11-yr solar cycle (lines 245-249) is not very informative. As requested in comment 4 above, this component of the paper (section 3) either needs to be removed or augmented by carrying out new simulations of the type described there. A detailed comparison with previous results of SolarMIP and Matthes et al. (2017) should be provided.

As stated above, it is not our intention to reproduce the results of Matthes et al. (2017), as those results were obtained from experiments using highly idealized solar forcing. Instead, our aim is to use a more realistic transient modelling framework to identify and discuss open questions regarding the impact of solar variability on the climate system. While idealized solar-minimum and solar-maximum time-slice experiments are highly valuable for isolating specific mechanisms, they necessarily simplify important aspects of the climate system. In particular, simulations without transient forcing and interactive ocean coupling cannot fully represent the complexity of the coupled climate response. We therefore think that our modelling approach provides a complementary perspective to earlier idealized studies. We explain this now in more detail.

16. In section A2.2, the method of determining solar maximum and solar minimum years is described in lines 390-395. Please state what the mean difference is in F10.7 flux units when averaged over all of the cycles considered here. It appears to be ~ 70 or 80 but whatever it is should be stated and the value should be compared to the effective values considered in previous studies (e.g., Matthes et al., 2017).

We refer the reviewer to our detailed response on the solar forcing above.

17. F10.7 is measured only back to about 1947. How was F10.7 determined for years back to 1850? Please explain. Is it possible that the early cycles are being underestimated?

As described in Matthes et al. (2017), for the construction of the variable PI control forcing, F10.7 values prior to 1947 were obtained by multilinear regression to the first 20 principal components of the SSI and application of minor nonlinear adjustments.

18. In the title, “State-of-the-Art” should be replaced with “Chemistry-Climate Model” or just “Modeling”. “State-of-the-art” should only be used to describe the models that are being used. It should be left to others to decide if the modeling study itself is state-of-the-art.

Title has been adjusted to: “The SOLCHECK Project: An Investigation into the Imprints of Solar Variability Across Multiple Timescales”.

Reference

Drews, A., Huo, W., Matthes, K., Kodera, K., & Kruschke, T. (2021). The Sun's role for decadal climate predictability in the North Atlantic. *Atmospheric Chemistry and Physics Discussions*, *2021*, 1-17.