

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

Reply to Reviewer 2:

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

This paper presents a summary of the SOLCHECK project, highlighting the modeled atmospheric and surface climate impacts of solar forcing (encompassing both irradiance and solar energetic particles). First, using three different models and an extensive number of experiments, they conclude that the 11-year solar cycle robustly impacts the upper tropical stratosphere, but this signal does not robustly propagate to the lower atmosphere, due to a (claimed) dependence on the dynamical state of the stratosphere. Second, the authors suggest that the climate system shows increased sensitivity to solar forcing under future conditions, compared to pre-industrial state. Third, they suggest that extreme solar storms in the future could have substantial effects on surface UV. The subject of the paper is relevant for the journal's readership and wider climate community, as it concerns the long-standing problem of our limited understanding of the effects of solar variability on climate. Also, there is clearly some merit in presenting results from three very different models.

While some of the conclusions of the paper are indeed supported by the evidence provided by the authors (e.g. the robustness of the tropical stratospheric response in temperature and ozone to the sunspot solar cycle - Fig. 2 - as well as the chemical impacts of SPE events - Fig. 1), some are not supported at all (e.g. there is no demonstration of the claimed "sensitivity of the 11-year solar signal to the dynamical state of the stratosphere", nor the "potential impacts of extreme solar storms in a future climate"...!). This makes parts of the paper weak and quite speculative. I would strongly suggest addressing the (major) shortcomings detailed below, and either (1) rephrase things to make results more in line with the evidence provided so far or (2) tone down statements in case additional evidence cannot be provided. I also have reservations about the setup of the GSM experiments, and therefore, I am not convinced about the claimed "increased sensitivity" under future conditions (see specific comments below).

Last but not least, I strongly urge the authors to take care of essential aspects, such as the quality and labeling of Figures (many are quite blurry and/or have wrong labels!), and also including a Table of the experiments. Only if these shortcomings are addressed, this paper could be reconsidered for publication.

SPECIFIC COMMENTS:

L19: actually, models have made great strides in representing all the essential processes involved in the atmospheric and chemical response to the solar cycle. Some of the latest generation of CCMs include, for example, particles forcing, spectral solar irradiance, interactive chemistry. The problem may not only lie in models deficiencies (although models are by far not perfect... but some are useful!) but rather in the very small magnitude of the solar signal, relative to the internal variability. This could/should be acknowledged here.

We fully agree. The challenge of identifying solar imprints in the climate system is multifaceted and relates to model deficiencies, small signal amplitudes, and the repeated use of similar analytical strategies. We have added a comment on this issue.

L24-25: while I agree on the first part of this sentence, I strongly disagree with the latter one ("the tropospheric... stratosphere"); this has not really been shown in the paper. Fig. 3 does not really support this, as wind changes there are not really in phase with the solar cycle and there is no robust evidence that the downward propagation depends on the "background state of the stratosphere". More targeted experiments (e.g. testing the impact of the 11 year solar cycle under different initializations resembling different vortex states, for example....) with MANY more ensembles would be needed to demonstrate this point.

A more detailed investigation of the claim that solar signals in the middle atmosphere and their subsequent downward progression into lower atmosphere are modulated by the mean state of the middle atmosphere is provided in the SOLCHECK publication by Huo et al. (2025). However, we agree that, at this stage, our statement is not sufficiently supported by the analysis presented in the manuscript. We have therefore added a simple but illustrative analysis to better substantiate this point.

In general, the downward progression of the solar cycle signal through the atmosphere can be understood in terms of three key regions:

1. the tropical upper stratosphere, where the initial solar signal is generated;
2. the polar vortex region during the respective winter season, where the atmospheric response is modulated by the prevailing mean state of the middle atmosphere, including adjustments in temperature gradients and dynamical circulation; and
3. the tropospheric domain, onto which the signal from above may project and where top-down-related tendencies may potentially become apparent.

In the new Supplementary Figure 1, we will show four ensemble members from our CCM integrations with full solar forcing: two from the EMAC model and two from the FOCI model. As can be seen, all four ensemble members show a clear in-phase relationship with the solar cycle, indicating that the initial solar signal is present in each of these members. However, when considering latitude–height cross sections for these ensemble members during December–February, a more differentiated picture emerges. Two ensemble members show a response that is broadly consistent with the so-called top-down mechanism, as shown in Supplementary Figures 2 and 3. In contrast, the other two ensemble members show a response of opposite sign, namely a weakening of the polar vortex under solar maximum conditions, as shown in Supplementary Figures 4 and 5. This is opposite to what would generally be expected from the top-down mechanism.

With regard to the reviewer's suggestion to use more differently initialized ensemble members, we would like to clarify that all ensemble members were initialized from slightly different initial conditions, as is standard practice for larger ensemble simulations. We also note that, across our model integrations, a top-down-like response occurs in 3 out of 6 EMAC members, 6 out of 10 FOCI members, and 5 out of 10 MPI-ESM members. Thus, approximately every second ensemble member shows some indication of a top-down mechanism.

We consider this to be a robust result, as it indicates that the occurrence of a top-down-like response is not systematic across all members, but depends on the background state and internal variability of the middle atmosphere. Therefore, we do not expect that simply increasing the number of ensemble members would fundamentally alter this conclusion.

L27-28: I am not sure the method employed in the GSM experiments is correct. Are the authors really imposing the same exact TSI perturbation under 1850 and 2100 conditions, or are they employing a scaling of TSI/SSI background conditions (see my specific comments below on this)? It may well be that a different (absolute) TSI reduction is being employed under both conditions (being larger under 2100!), which would invalidate the conclusion about the "enhanced sensitivity" of future climate to solar variations.

We understand the reviewer's concern, which likely results from an insufficient description of the solar forcing applied in our sensitivity experiments. Since our aim is to test the response of the climate system to centennial-scale solar perturbations under different background conditions, all reference simulations, namely REF1850, REF2020, and REF2100, use identical solar forcing. Specifically, these simulations are driven by the non-transient pre-industrial solar forcing described in Matthes et al. (2017).

Importantly, the reference simulations do not use transient solar forcing, nor do they use period-specific mean solar forcing, such as averages over 1850–1870, 2080–2100, or any comparable time interval. Similarly, all Grand Solar Minimum simulations, namely GSM1850, GSM2020, and GSM2100, use the same solar forcing representative of a strong Grand Solar Minimum, as provided by Egorova et al. (2018). In both sets of experiments, that is, the REF and GSM simulations, both TSI and SSI were adjusted consistently.

Therefore, because the solar forcing is identical within each experiment type across the different time-slice simulations, the differences between the 1850, 2020, and 2100 cases reflect the modulation of the solar-forcing response by the respective background conditions, in particular greenhouse gases and ozone-depleting substances. We have revised and expanded the description of the experimental setup to make this point clearer.

L30: this point is only really shown briefly for KASIMA, but not for the other 2 models and it's unclear how sizable the UV perturbation is, compared to UV perturbations due to e.g. cloud and surface albedo changes are. That is: would this UV change be detectable in the real world?

Unfortunately, UV output data are not available for the other model simulations, so a comparison of the UV perturbation due to the SPE with cloud or albedo changes is not possible at the current stage.

L70: again, I am not convinced about this point, as the evidence provided by the authors is lacking in this respect.

Please refer to our response above.

L75: please also add Sukhodolov et al., 2017 here: <https://www.nature.com/articles/srep45257>

Done.

L89: why 19 members, and not... for example... 30 or 40? Deser et al., has shown in many papers that one needs really large ensemble sizes to study circulation metrics

The addressed model simulations used the high-top EMAC-B model configuration with 74 levels up to about 200 km height. To account for the high wind speeds and fast ion chemistry in the thermosphere, this model version needs to be run with a very short time step of maximum 1.5 minutes. Given the resulting enormous computer time requirements, only 19 simulations could be completed. We think, however, that a 19-members ensemble should produce a robust signature of the prescribed extreme SPE scenario.

Fig. 1: I think there are several major issues with this Figure, starting with the labels being wrong, the colorscale of panels b-e-h being not optimal, and ending with a wrong description in the caption (where are the UVI panels for EMAC...? what is the difference between the late October 2003 event and the Halloween storm of October 2003 described near L95...? I guess you rather mean the "extreme" SPE for the middle row...? I also believe many, if not all, the panel labels are wrong!)

We are very sorry for the confusing panel labels. The figure has been corrected. UVI panels for EMAC are not available due to missing UV model output. The late October extreme event is an extreme SPE similar to the 774/775 SPE event, while the Halloween event occurred in late October 2003. The caption has been clarified.

L115: I totally agree with this conclusion, and I think it should be highlighted in the conclusions' and abstract!

Thank you. Done.

Figs.2 and 3 are VERY blurry. Please improve the quality of these Figures

The figures have been replaced.

L149: can you please clarify what you mean with "internal model dynamics"?

Changed to: "suggesting that the representation of dynamical processes in individual models plays a key role."

L151/L160: I would recommend to analyze and consistently show that this downward progression of the signal is robust in observations, before blaming the models for not "capturing" / "reproducing" it

We agree that restricting model results to relatively short observational periods is problematic, since observations represent only a single realization of the climate system rather than an ensemble. We have therefore added a comment addressing this issue.

L153-155: this is not really convincing - as there is no robust evidence of any "phase locking". To support this, the authors would need to analyze the signal in detail and use advanced techniques such as wavelets or similar...

We have now included additional analysis; please refer to our comments above.

L190: can the authors explain why the inclusion of the solar forcing would worsen the skill in certain regions of the SH hemisphere? Also, why is the CSS so patchy? Could it be that this metric underestimates the amplitude of "unforced" variability?

We agree that the interpretation of skill scores is problematic at this stage, since the main conclusion of the paper is that there are limitations regarding the general validity of the top-down mechanism. Therefore, a discussion of potential implications for forecast skill would be too speculative in the context of the present analysis. We have therefore removed this analysis.

L207-223: I am a bit worried about the set-up of the GSM experiments. Can the authors verify that the absolute TSI variation they are employing in 1850 and 2100 conditions is the same? If not, part of the basic-state dependence would not really be such, but rather, it would be a dependence on the TSI forcing being employed

Please refer to our detailed comments above.

L218-223: this would need to be analyzed and shown quantitatively. In the present form, all this discussion feels quite speculative.

We have now included additional analysis; please refer to our comments above.

Fig 5 - again, all panel labels seem to be wrongly references in the Figure caption.

The figure caption has been corrected.

A table of simulations has been added.

References

Egorova, T., Schmutz, W., Rozanov, E., Shapiro, A. I., Usoskin, I., Beer, J., Tagirov, R., and Peter, T.: Revised historical solar irradiance forcing, *Astron. Astrophys.*, 615, A85, <https://doi.org/10.1051/0004-6361/201731199>, 2018.

Huo, W., Spiegl, T., Wahl, S., Matthes, K., Langematz, U., Pohlmann, H., & Kröger, J. (2025). Assessment of the 11-year solar cycle signals in the middle atmosphere during boreal winter with multiple-model ensemble simulations. *Atmospheric Chemistry and Physics*, 25(4), 2589-2612.