

This manuscript presents a comprehensive evaluation of the 11-year solar cycle imprints from the middle atmosphere to the surface, especially the so-called “top-down” mechanism, based on 10-member CMIP5 historical simulations from the MiKlip (MPI-ESM-HR). Considering the diverse and debated conclusion on this topic in the existing studies, I think this paper is worth to be published after a minor review. The following are my comments on this manuscript:

Lines 17-18: “.....on the North Atlantic Oscillation might lead to”, I would suggest writing in a way like: “.....on the sea level pressure in the North Atlantic might lead to a synchronization of the North Atlantic Oscillation via the 11-year solar cycle”

Lines 32-33: “.....with opposite sign in response.....” This also suggests the large internal variability could veil the possible solar imprints.

Lines 93-104: all these studies are based on the same climate model, i.e. CESM-WACCM, of course, different versions, and different forcings. So even using the same climate model, the detected solar imprints or the interpreted results also could be controversial.

Lines 134-135: I guess the sea ice module is included in the MPIOM, right? Please add a few words here.

Line 142: “We focus on the period 1880-1999” Why did not use all model years (1850-2005)?

Line 160: How did you perform the EOF analysis on the ensemble members and the ensemble mean? Did EOF on stitching data (i.e. 1200 model years) or did EOF on each member separately, but keeping the phases (sign in EOF1) the same as the observation? I ask this question because the EOFs could be opposite signs, if performing the EOF separately, need to double-check to make sure the patterns of all the members are in the same phase.

Section 2: Significance level is shown in the lead-lag correlation and MLR analysis results, but the method description is missing. Considering after a band-pass filter, the effective degree of freedom of the time series will reduce a lot, so it’s necessary carefully calculate the EDF and describe the method in this section.

By the way, how did you assess the robustness of the solar imprints?

Line 174: “180 SSN”, here hard to understand this term for a reader not familiar with the MLR, please explain more.

Line 176: “the solar forcing lags the model data”, lags? for investigating the lagged response in climate, should solar forcing **lead** the model data, right? Did I misunderstand something?

Lines 175-176: Are the other predictors keeping the same as the lag 0 when shifting the solar time series?

Line 207: “period from 1850-1999” the period from 1850-2005?

Line 209: “pretty high minimum” do you mean compared to the SC19? I think the response of SWHR to the minimum of SC20 fits quite well, but a higher response to the maximum of SC20. So, please clarify your statement in this sentence.)

Figure 1: a) and b) are missing in the figure. And I would expect a similar figure as Figure 1, but for temperature in the tropical mean (supplementary figure or panel into Figure 1), this figure will nicely show the sensitivity of the temperature response to the solar cycle amplitude.

I also suggest plotting the original SSN time series and the scaled SSN time series used in MLR (180 SSN?), as a supplementary figure.

Line 219: “a robust” How did you define this?

Line 223: “a relative short time series of satellite...” maybe provide a specific time period? I think this period probably includes different solar cycles from the simulated time series. Repeating the model analysis in the same data period as the satellite would help to compare

Figure 2: A very brief caption. Please add more details. Like, is it a result of MLR? or a composite of annual mean zonal mean temperature in the solar maximum years? If it's the latter, how did you define the solar maximum years (maximum of SSN of each solar cycle I guess)? What does it look like in the solar minimum?

It seems the second warming is absent in your simulations.

Section 4: a very long paragraph, that needs a break somewhere.

Line 256: What does it imply, if the SSN is below the SC14 maximum? How strong the solar cycle could be if it's above the SC maximum? I think a figure of the original SSN time series will help to give the reader directly the impression.

Line 266: please add a reference for this statement.

Line 270: As the “dynamical response of the PNJ” is directly related to the meridional temperature gradient, I'm curious, can the solar-induced TST warming really increase the poleward temperature gradient? I think it's not the case for some ensemble members, at least for the EM4.

Figure 4: same as Figure 2, the caption is too brief. Is it a result of MLR? MLR performed on ensemble mean or averaged regression coefficients of all the members?

Horizontal components of the E-P flux are hard to see in the bottom row of Figure 4, is it due to a very **less reflection? Maybe scaling it to show more clearly.**

Line 339: “insulation” Do you mean “insolation”?

Lines 370-371: I think the temperature response in EM4 is very different from EM1, it's almost the opposite state in the Pole region.

About Figure 5, no significant temperature response in the tropical stratopause and no response of the lower mesospheric subtropical jet in EM4. Is the warming migration to the high latitude in EM1 due to solar insolation? If yes, why it disappears in EM4?

Line 384: “Drews et al., (2022)” => “Drews et al. (2022)”, they used a different climate model (CESM-WACCM)

Lines 413-415 and Figure 6: did a “top-down” mechanism show in observational data? An anomalous zonal mean zonal wind in the troposphere and surface in Feb?

Line 444-446: I guess that’s why we need some positive (slow) feedback from the ocean. But I’m not sure your model can simulate this. By the way, if the “top-down” signals are not robust, how can we expect the surface response?

Figure 7: I suggest adding the surface wind or surface zonal wind in Figure 7. Same comments on the caption as above.

The spatial pattern of solar imprints in SLP may have different active centers from the EOF1 (NAO). Could you please compare them? Is the EOF1 (NAO) in your model the same as the observation?

Lines 458-463: this should be in the method section.

---best,

Wenjuan