

Many thanks for your comments and suggestions on our manuscript. Here is our answers (==>).

#### Anonymous Referee #2

This paper studies the 11-year solar cycle signal in Earth's surface using historical datasets and the surface evidence is further supported by the zonal mean vertical profile using ERA-Interim and previously archived model simulations. The authors present many surface and zonal mean quantities that are composited between high and low solar years. Although majority of the results presented here are known or previously published, this paper could still be informative because it provides an up-to-date and comprehensive summary of the atmospheric response to the 11-year solar cycle in the observational data sets.

The authors have attempted to examine the dynamical mechanism by which the 11 year solar cycle signal is transmitted from the tropical upper stratosphere to the surface. They suggest that the observed surface signals are largely resulted from circulation changes in the upper stratosphere through downward migration of zonal mean anomalies and changes in the stratospheric mean meridional circulation. The authors' argument on this point is demonstrated mostly by using a model simulation where westward and eastward momentum forcing was applied to the entire column of the winter stratosphere polar vortex. The initial solar UV forcing however normally confines to the subtropical upper stratosphere, i.e. above 10 hPa. Thus, it differs significantly from the strong and weak polar vortex cases in their model simulation. Firstly, the solar UV effect at lower latitudes must be transmitted to middle to high latitude to produce a definite stronger vortex, which is not always easy in the real atmosphere. This is clearly demonstrated by the different responses in the SH and NH during winter sessions.

The authors present no diagnostics of the wave-mean flow interaction or meridional circulation in the stratosphere based on observation or reanalysis data. Only if the wave forcing diagnostics from reanalysis data sets match those from their model simulations, the proposed mechanism can then be stated as the main mechanism for the solar signal seen in the SSTs or SATs. I therefore find that this part of the paper is not entirely convincing. The rather strong statement made by the authors about the cause and effect regarding the link between the surface signal and this mechanism should be tuned down and presents as one of the contributing mechanisms instead. If not, please provide additional supporting wave-mean flow interaction diagnostics using ERA-Interim or other reanalysis data sets.

The results are appropriate for ACP and the structure of the paper is sound. The clarity of paper may be improved by reducing the lengthy discussion. I have several specific comments that I would like to see addressed before the paper is published.

==> The aim of the present study is to understand the global distribution of the solar signal on the Earth's surface. Therefore, stratospheric processes, such as wave-mean flow interaction are not investigated in the present paper, but results of previous studies are refereed.

#### Major comments:

- Lines 22-24. Abstract. As I stated previously, these statements are too strong given the momentum forcing applied in the model simulation differ largely from the actual solar UV forcing.

==> The sentences have been modified according to the reviewer's comment.

"Although the momentum forcing differs from that of solar radiative forcing, the model results suggest that stratospheric changes can influence the troposphere not only in the extra-tropics but also in the tropics through i) a downward migration of wave–zonal mean flow interactions and ii) changes in the stratospheric mean meridional circulation."

- 2. It appears to me that the atmospheric or tropospheric response in their model simulation (Figure 10) can only explain the early winter behaviour of the solar signal in the NH. It fails to explain the high latitude warming signature in the late NH winter and in SH spring and no signal in SH winter (Figure 6).

==> When a stronger westerly jet extends from the stratosphere to the troposphere in late winter, tropospheric planetary waves propagate upward along a stronger westerly jet. Then, zonal winds in the upper stratosphere are decelerated and a warming occurs in the polar middle stratosphere. This means that polar warming in late winter is rather a stratospheric response to a tropospheric circulation change. Here, we focus on the downward penetration of stratospheric influences. Therefore the absence of this feedback from the troposphere is not crucial to understand stratospheric impact on the troposphere.

- 3. Lines 5-30, Page 4. MLR may be quite useful in studying a system in which the dependent variables are linearly related to the predictors in time. The assumption may hold for annual mean fields but will not be applicable for the seasons where nonlinearity dominates. In NH winter, for instance, the authors have suggested that the stratospheric response to the 11-year solar UV cycle in early and late winter flips the sign. This suggests nonlinearity and may result in cancellation of solar signal there when a linear regression model is applied. It would be helpful to the readers if the authors make this point clearer.

==> We agree with the reviewer that the use of the MLR to derive seasonal signals is not always relevant due to nonlinear processes and requires additional care. Indeed, it is shown that the stratospheric 11-year solar cycle response rapidly evolves in the Northern Hemisphere winter (Fig. 5). In this case, where the seasonal march is crucial to understand the physical processes leading to the propagation of the solar signal throughout winter, we show only individual months and not the seasonal signals. In our study, seasonal signals are essentially shown for atmospheric and ocean surface variables to focus on the seasonal variation of climate variability modes. The only exception we made for the stratosphere is in Fig 6a where we show the averaged response for two consecutive months (Nov/Dec for NH and Jul/Aug for SH) which correspond to the “radiatively controlled” stage of the seasonal march of the solar signal and the signature for the two months is similar. We thus made the point clearer in the text (in section 2.2, last paragraph) Section 2.2 was also expanded to discuss the MLR limitations as requested by reviewer #1.

Finally, we also compared different MLR techniques to derive seasonal signal, i.e. by averaging the monthly fields before applying MLR and deriving directly “seasonal coefficient” (as we formerly did) vs. by first deriving the monthly coefficient and then averaging them to obtain the seasonal response. Both methods gave very similar results (see below).

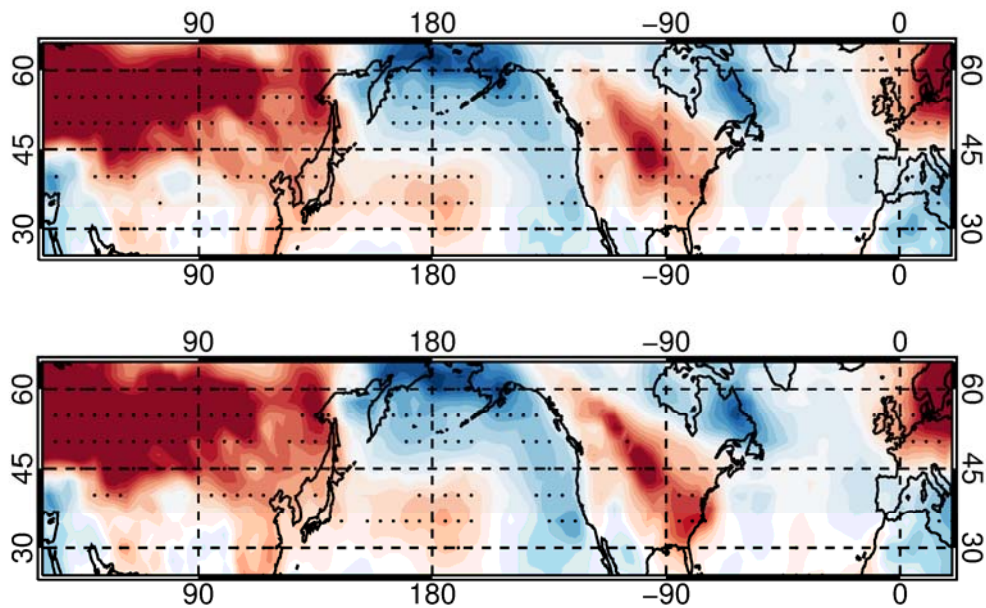
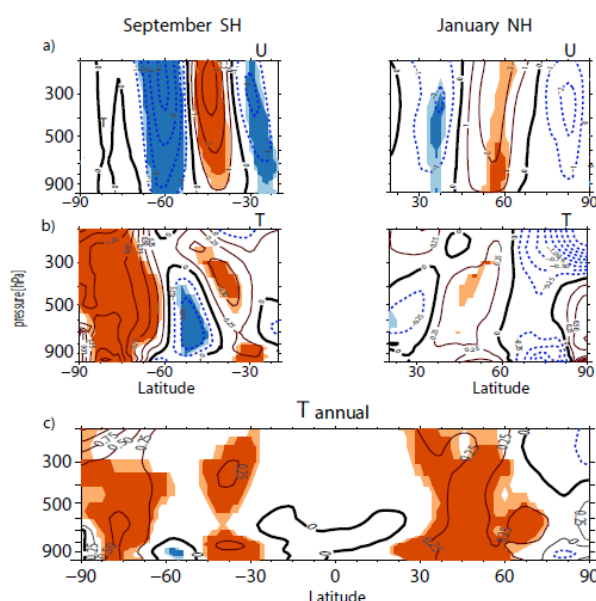


Figure. DJF-averaged solar regression coefficient of the surface temperature for two different seasonal MLR methods: (top) averaging the monthly fields before applying the MLR and (bottom) deriving the monthly coefficients first and averaging them to obtain the seasonal signal.

- 4. From Figures 5 and 6, it is not clear to me how the surface temperature and circulation patterns are so surely linked to the stratospheric circulation anomalies, as the way presented by the authors. In both hemispheres, little solar signals can be found in the polar temperature during middle winter (see Figure 6). In the NH, the mid-latitude troposphere and lower stratosphere show to be weakly warm in Nov-Dec, Jan and Feb while the polar region flips from cold to warming from Nov to Feb. Thus, why the upper level “causing” signals are effectively weaker than the “responding” signals near the surface? Or to what extent these winter temperature anomalies shown in Figure 6 contribute to the annual mean anomalies shown in Figures 1 and 4?

==> To understand the solar signal, the overall features are investigated by combining the tropospheric part of Figs. 4, 5 and 6 in Fig. 14. The solar signal in the tropospheric temperature field is relatively small in January in the NH and September in the SH (Fig. 6; Fig. 14b). It should be noted that this is a period of transition; the tropospheric temperature signal is induced by the downward penetration of zonal wind anomalies. Therefore, more statistically significance should be expected in the zonal wind field (Fig. 5; Fig. 14a). A pair of warming and cooling is formed at both sides of the axis of the zonal mean zonal wind anomaly consistent with the thermal wind relationship. Therefore, the temperature signal is physically consistent even though the statistical significance is low. It is also shown in Fig. 7 that the surface temperature signal induced during the winter can be maintained and even amplified through an interaction with the ocean. Therefore a stronger statistically significant signal is found in the annual mean temperature field. In contrast, the annual mean zonal wind signal is less significant (Fig. 4). A possible role of ocean feedback to enhance stratospheric impact is also discussed in Yukimoto and Kodera (2007) and Misios and Schmidt (2013). The active role of the ocean is also found in the model experiment in Fig. 10 that although no external forcing

is applied in the summer hemisphere, anomalous mid-latitude warming and wave activity persist in the troposphere, in particular in the SH.



The above text and Figure were added in the revised version.

- 5. Line 31, section 3.4, page 8. Tropical solar signals appear to be important in this paper and the authors have devoted an entire subsection for it. However, in the abstract, it states “no warming in the tropics”. Somehow, I feel that the authors need to provide the reason as why the tropical solar signals need to be specifically discussed given the most significant solar signals are found in the middle latitudes (See figure 1). Also, in what way the tropical solar signals are connected to the dynamical mechanism by which the 11 year solar cycle signal is transmitted from the tropical upper stratosphere to the surface?

=> It is rephrased as "no overall tropical warming". The amplitude of the temperature variation is small in the tropics. However, in the tropics, change in precipitation (or vertical velocity) is much more important. Figure 4c indicates a shift of the raising branch of the Hadley circulation, of which importance is evident.

A possible process producing a tropical tropospheric effect is described in the text Page 10 line 24-30 of original paper: "Previous model studies (Thuburn and Craig, 2000; Kodera et al., 2011) showed that changes in stratospheric meridional circulation affect tropical convective activity through changes in static stability in the tropical tropopause region (Eguchi et al., 2015). In the present experiments also, suppression of equatorial ascending motion occurs in the troposphere in connection with the reduction of stratospheric mean meridional circulation change, as can be seen in the residual circulation differences in Fig. 10c.

- 6. Figure 12c is rather sudden and thus potentially confusing because the wave forcing and residual circulation anomalies in late winter are not supported by any of the analysis presented earlier in the manuscript based on either data or model simulations.

=> This is based on the results in Kodera and Kuroda (2002) and Matthes et al. (2006).

The sentence has been modified as follows."we show these two stages schematically in Fig. 13 based on previous studies (Kodera and Kuroda, 2002; Matthes et al., 2006; Matthes et al., 2013) ".

- 7. Lines 5-8, page 13. I cannot see the reason why a longer lasting radiatively controlled stage in the subtropical SH upper stratosphere can lead to an anomalous weakening of the stratospheric jet and warmer polar stratosphere (Figures 5 and 6). It appears to me that the argument based on dynamical versus radiative control is definitely valid in part but it remains not sufficient to explain all the stratospheric anomalies.

==>In the SH, a weakening of the stratospheric jet and a warmer polar stratosphere becomes evident in September "near the equinox", when differential solar forcing becomes small. Then, planetary waves propagate in weaker winds in the stratosphere and produce polar warming in October.

- 8. Lines 21-34, page 14. These sound much like results rather than discussion and concluding remarks. Suggest moving to an earlier section instead. As I have stated before, the composite difference estimated from the simulated weak and strong polar vortex are not exactly representative to actual solar UV forcing. First, the solar UV forcing has much smaller magnitude. Second, the solar UV effect is located much higher in altitude than the model simulation assumed. As a result, the solar UV effect should be much weaker than what has been suggested by the model simulation.

==> According to the reviewer's comment, this part has been moved to a new section 5. Centennial scale variation. To conform to this change, the following sentences are added in Introduction and Discussion.

#### Introduction

"To get insight into a centennial solar variation such as the Maunder minimum, the effect of centennial scale stratospheric circulation changes on the troposphere is briefly studied in section 5."

#### Discussion

"It should also be noted that centennial circulation changes produced in the stratosphere can affect global mean surface temperature through changes in the Earth's surface condition without changes in total solar irradiance."

- 9. Some of the fields are quite messy (e.g. Figure 4b,c; Figure 6) or not statistical significance is shown (e.g. Figure 1a). Some of the features are not statistically significant but are discussed as the cause for the surface anomalies. I suggest that the discussion around these figures/features needs to be more careful.

==> As discussed in the paper, solar signal is characterized by its global distribution. We consider that we should not put too much importance on local variables. In this respect, the way that Zhou and Tung (2010) made to test the statistical significance of the global solar signal as in Fig. 1a, may be better adopted to this problem.

Minor comments:

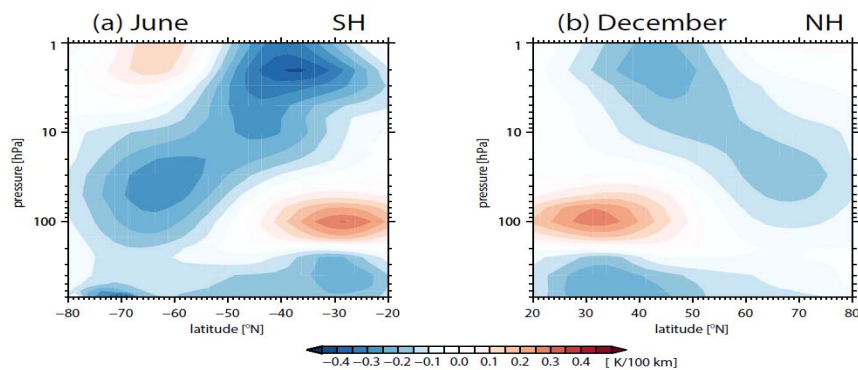
- Line 11, abstract. “no warming in the tropics”. This is not clear. “No warming” could imply either “cooling”, “no signal” or “complex signal with longitudinal variation”.

==> According to the comment, the phrase was modified as "no overall tropical warming".

- 2. Line 14, abstract. “the subtropical jet”. The term is not clear. The subtropical jet in the atmosphere often refers to the tropospheric subtropical jet. Here, the authors refer to the upper stratosphere subtropical jet. Climatologically speaking, there is no subtropical jet in the stratosphere anyway. There is only one jet in the stratosphere which is the polar vortex which initializes at lower latitudes in early winter.

==> Study on the subtropical jet in the middle atmosphere is rare and may not be well known. We therefore introduced the following explanation and figure about two different nature of westerly jets in the middle atmosphere.

" It should be noted that there are two kinds of westerly jets in the middle atmosphere. Figure 13 displays the climatological poleward temperature gradient during winter solstice (Jun in the SH and December in the NH). The meridional temperature gradient is large in the subtropics of the upper stratosphere due to solar UV heating, while in the lower stratosphere, the gradient is large in the polar region due to strong longwave cooling. They are respectively connected to the subtropical and polar night jet. Poleward and downward penetration of solar signals in the middle atmosphere occurs through interaction between these jets and planetary waves propagating from the troposphere."



- 3. Line 1, page 2. “amplify” -> “act to amplify”.

several mechanisms have been proposed that amplify the initially small solar effect

==> Corrected as "act to amplify"

- 4. Line 13, page 2. Zhou and Tung (2010) not cited in the reference list.

==> It is located at the end of the reference list.

- 5. Line 26-27, page 2. “Because solar signals in SLP data are inconsistent, probably due to the temporal and spatial limitations of the data, we instead study pressure or geopotential

height fields . . .". It is confusing firstly because the SLP is pressure, isn't it? Also, it is known that solar signal tends to wax and wane with the different periods under consideration. Would it be better that we admit that we still do not understand why it happens rather than blaming the data quality. The wax and wane can also be found in modern data sets such as ERA-40 or ERA-Interim.

==> According the comment, we rewrote the sentences as follows.

" Because sea surface temperature (SST) is more persistent than the sea-level pressure (SLP), long-term variations can be more easily detected in the temperature field. Therefore, we investigate mainly surface temperature variation from the historical data, complimented by pressure or geopotential height fields with a modern dataset. "

- 6. Line 16, page 4. "predictorand" -> "predictors".

==> corrected

- 7. Line 19-20, page 7. "The differences in the latitudinal structure of the warming suggested. . .". This is not clear especially from the annual mean field. These statement can only be said when other dynamically quantities are also analysed. Suggest to remove or cite references to support such claim.

==> According to the comment, the sentences have been modified as follows.

"Previous studies suggest that the solar signal in the tropical lower stratospheric temperature is mainly induced through a modulation of the stratospheric mean meridional circulation or the Brewer-Dobson circulation (e.g. Kodera and Kuroda, 2002; Hood and Soukharev, 2012). Inspection of Figs. 5 and 6 reveals that the warming in the middle and lower stratosphere is produced in association with very sharp zonal wind anomalies. In fact, such strong meridional gradients of the zonal winds could not be produced by latitudinal difference of the radiative heating rate which mainly depends on the solar zenith angle."