

## ***Interactive comment on “How can we understand the solar cycle signal on the Earth’s surface?” by Kunihiro Kodera et al.***

### **Anonymous Referee #2**

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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

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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.

Major comments:

1. 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.
2. It appears to me that the atmospheric or tropospheric response in their model sim-

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ulation (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).

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 non-linearity 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.

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?

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 tropic 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?

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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.

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.

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.

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.

Minor comments:

1. 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”.

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

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which is the polar vortex which initializes at lower latitudes in early winter.

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

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

6. Line 16, page 4. “predictorand” -> “predictors”.

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.

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