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

Anonymous Referee #1

Summary

The authors perform a variety of methods to understand the atmospheric response and surface response to solar changes. They mix observations, reanalysis and model experiments. The study culminates in the modelling assessment, where induced momentum (analogous to solar forcing) is directed into experiments to assess the surface response. The manuscript is well written, and allows the reader plenty of spin up time in on solar influence on climate. The methods seem sound, although more discussion over different methods would help guide follow on research from this study.

My main criticism is that the paper has a lot of repeated material already in the literature. In fact up to figure 8 there is nothing substantially new. I think the most exciting parts of the paper certainly come from the model experiments, which culminate in figures 10 and 11. If I were the authors, I would have expanded this section more (at the expense of the early sections), or simply submitted it is a shorter letter based article.

Nevertheless, as the authors have submitted the paper as it is, it may be OK to publish, although I believe it needs to be made far more clear that Figures 1-8 are mainly reproducing previous work. Therefore the manuscript needs to be better referenced, and clear in the discussion of the paper structure.

==> To make the point of the present study clearer, the title has been changed from "How can we understand the solar cycle signal ..." to "How can we understand the global distribution of the solar signal".

Similar solar signal in the surface temperature as in Fig. 1 can be found in previous papers. Many studies, however, focus on regional aspects. For instance, the paper by Meehl et al. (2008) illustrates only the Pacific sector, whereas that by Gray et al. (2013) focusing on the Atlantic sector, uses a map centered on the Greenwich meridian. In the case of Zhou and Tung (2010), having no interest in the spatial structure, a map according to a convention starting from 0° to 360° longitudes is used. If the results of the analysis were very similar, they are presented differently, in a way to what authors aim to study or demonstrate. In the present study, we investigate global features including a connected variation in the solar signal from the Pacific to the Atlantic sector. Therefore we need to produce figures appropriate for our study.

To indicate what is new in this paper, the following sentences were added. "It should be noted that most of the previous work investigated processes producing solar signals on the Earth's surface in a specific region. Little was done to understand the overall aspect of solar signals on the entire Earth's surface."

We also added references to previous similar MLR studies using meteorological reanalysis data: "The results of similar MLR analyses using meteorological reanalysis data have also been published (e.g., Frame and Gray, 2010; Chiodo et al., 2014; Mitchell et al., 2015a)."

General Comments

- Make the paper clearer as to what is new and what is not.

==> As mentioned above, most of the previous studies focus on regional aspects of solar influence if not a globally averaged temperature. What is new in this paper is a study of the processes which produce a global distribution of the solar signal in the surface temperature in the extratropics of the NH and SH, as well as in the tropics.

- 2. There are inherent issues with some of the analysis that the authors use. Namely that solar signals can interact in a nonlinear way with the atmosphere and even more so the surface. We know some of the responses are non-linear, and therefore multiple linear regression may not be the most appropriate tool. My feeling is the MLR is probably OK for some assessment of the surface, but the issues with it should certainly be addressed in a standalone paragraph. For instance, machine learning methods (Blume et al, 2012) which are naturally non-linear, optimal detection (Stott et al, 2003; Mitchell, 2015) which gets around some of the non-linearity by using model predicted responses as the regressors (they also do not assume noise free regressors, another issue with the standard MLR), and final non-linear attribution, (Kuchar et al, 2015). The latter study does some comparison with the MLR technique as well, although does not directly address your analysis.

==> We revised section 2.2 by adding the following paragraph to indicate the limitations of the MLR method as suggested by the reviewer:

"The use of the MLR approach to separate the contribution of different factors on climate variability has inherent limitations that should be kept in mind when analyzing the results. The MLR particularly relies on several assumptions that may not be valid in all cases. However, composite analysis of the monthly mean data based on two or three levels of solar activity (e.g., Kuroda and Kodera, 2002; Lu et al., 2011) also produces similar solar signals as those obtained from the MLR method (Figs. 5, 6). Therefore, in spite of the limitations, the MLR method may be useful to get approximate solar signals (Kuchar et al., 2015). We note that highly non-linear responses can be produced through the interaction between different forcings: for example between ENSO and solar signals (Marsh et al., 2007), solar and QBO signals (Matthes et al., 2013), as well as volcanic and solar signals (Chiodo et al., 2014). Usually this kind of interaction occurs at a specific location and time, which needs to be investigated in separate studies. Sophisticated attribution methods which can account for non-linearity have been used, such as machine learning methods (Blume et al, 2012), or optimal detection (Stott et al, 2003; Mitchell, 2016). Although these methods allow advanced statistics to be established, their limited interpretive capacities make it difficult to study physical mechanisms. "

- 3. From the title I thought it would be a rather different paper. I do not think you have answered the question ‘how can we understand. . .’, I think you have simply performed an analysis of the surface response. I would therefore change the title.

==> As mentioned above, the title has been modified as follows. "How can we understand the global distribution of the solar cycle signal on the Earth's surface?"

Minor comments

- P1L27: This sentence does not fit so well. In longer term studies (of centuries) solar influence on climate has been known about for a while. Do you mean just short term?

==> According to the reviewer's comment, we added the phrase "especially that of the 11-year solar cycle," for more precision.

- P2L11: 'global mean temperature' to you mean 'surface temperature'?

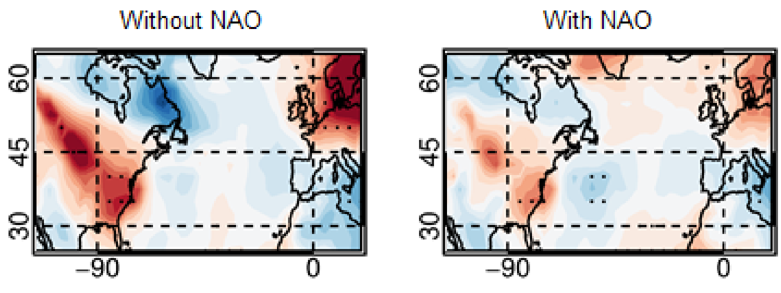
==> We changed to global mean "surface" temperature.

- Section 2.2: Here I would address my General Point 2.

==> We followed referee's suggestion. See answer for General Point 2.

- P4L14: A number of studies use additional regressors. Maybe a point or two on why these are OK.

==> For the MLR, we used indices which describe climate variability factors that have been demonstrated to have a significant impact in the middle atmosphere and at the surface, i.e. solar forcing, volcanic aerosols, ENSO, QBO (x2), and anthropogenic forcing, and which have been extensively used in many model and reanalysis solar-related studies [e.g., Chiodo et al., 2014 ; Mitchell et al., 2015a,b]. Some studies used additional regressors allowing to account for NAO variability [e.g. Haigh et al., 2005] or a third QBO term [e.g. Kuchar et al., 2015]. After testing, Kuchar et al. [2015] confirmed that the "The solar regression coefficient seems to be highly robust since neither the amplitude nor the statistical significance field was changed significantly when NAO or QBO3 or both of them were removed" <u>in the stratosphere</u>. However we notice that using the NAO index in the MLR to examine the surface climate is somewhat misleading from a physical point of view since it is quite well established that the solar signal modulates the NAO [e.g. Kodera, 2003] at quasi-decadal timescales. For instance, we repeated the MLR analysis for the surface temperature with (left panel) and without (right panel) the NAO index as a regressor. The solar regression coefficient is shown in Figure below.



It is obvious that the solar signal in the North Atlantic sector (quadrupolar temperature pattern around the North Atlantic basin) becomes weaker when the NAO index is added in the MLR because part of the signal is projected onto the NAO regression coefficient.

According to the comment, we added the following sentences. "The Arctic Oscillation (AO) or the North Atlantic Oscillation (NAO) is climate mode which is partly driven by solar variability as will be shown later. Hence, it is not relevant to include its index in a MLR model which aims at examining the solar cycle effect on surface climate."

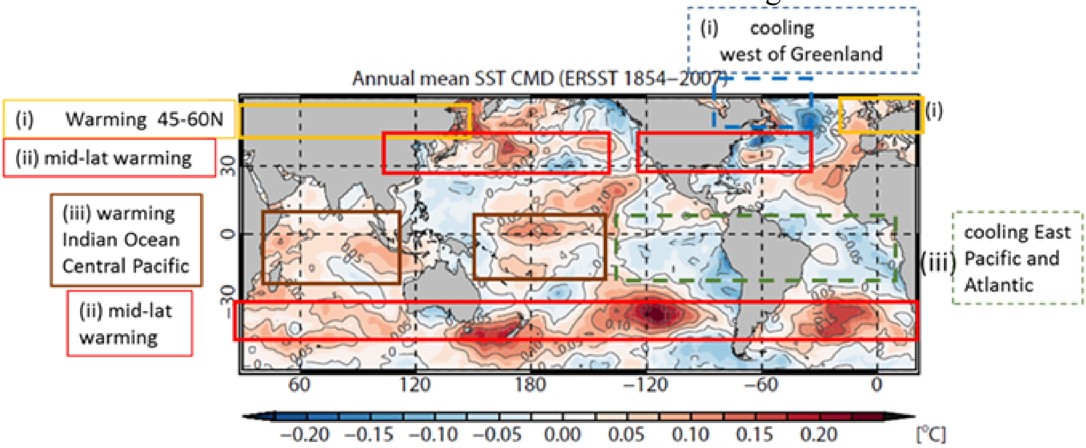
- P5L4-5: Maybe cite some papers that look at solar influence on climate using the Tiao method.

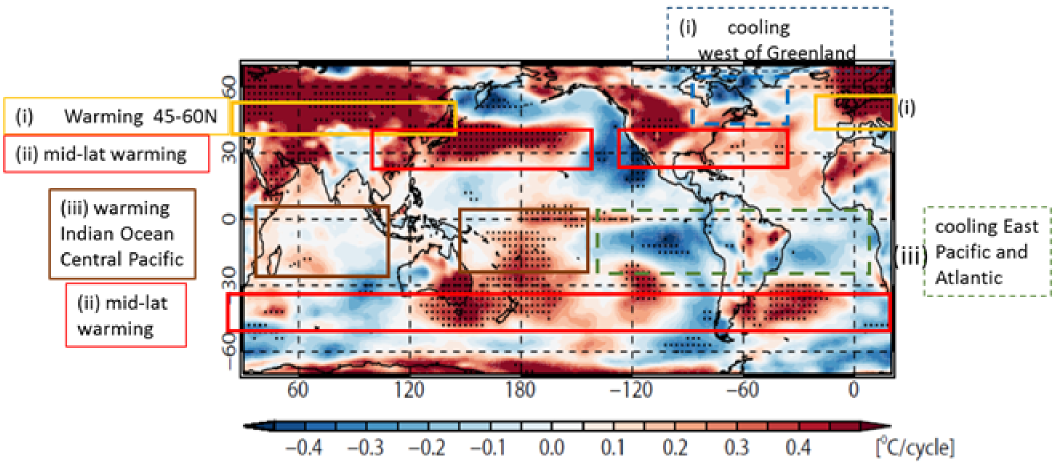
==> We added the following sentence: "The application of the Tiao et al. method can be found in several papers examining the solar signal (e.g., Austin et al. , 2008 ; Mitchell et al. , 2015b)"

- P5L27-30: It is not clear to me exactly where you refer to here. Is it literature, or is it panels a and b? If the latter, I still do not see all the features that are mentioned.

P5L27-30: "Common features in the spatial structure of the solar signal in surface temperatures include i) (sub-polar regions): warming around 45° -60° N over the Eurasian continent and cooling west of Greenland; ii) (mid-latitudes): warming over the ocean basins around 30° -45° latitudes in the Northern Hemisphere (NH) as well as in the Southern Hemisphere (SH); iii) (tropics): 30 warming over the Indian Ocean and the central Pacific, and cooling in the East Pacific and the Atlantic, particularly in the SH."

==> The feature described in the text can be found in the Figures shown below.



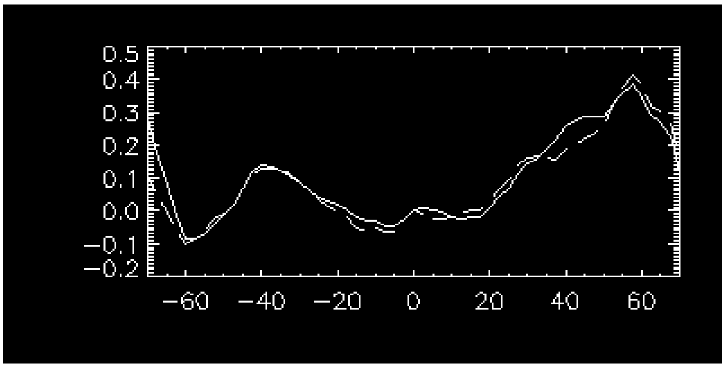


- P6L5 2-3 years should probably be 2-4 years. In the literature it is often written in both ways, but I think if you look at the figures in the relevant literature, the signals at 2 years are as large as those at 4 years (with the signal max at 3 years).

==> Changed to 2-4 years.

- P6L11-22: Is there perhaps cross correlation in the regressors between solar and volcanic? For instance the response seems anticorrelated say at 45N which is a max in solar, and a min in volcanoes.

==> We tested the sensitivity of the solar signal to the volcanic eruptions by removing the volcanic years from the MLR (1982, 1983 for El Chichon and 1991, 1992 for Pinatubo).



Solar coefficients are shown above (as for Fig. 2) with (solid line, like in the paper) and without (dashed line) volcanic eruptions. We conclude that the solar signal derived from the MLR, for this variable, is not strongly affected by volcanic eruptions. The statistical significance (not shown) is also only marginally affected. Moreover Lean and Rind (2008), who used 1889-2006 period historical datasets, obtained results which are very consistent with ours (see their Fig 3).

- P6L32: A forcing of the vortex nearly always leads to a response in the NAM, so why is it remarkable?

==> This phrase simply indicates that the AO and solar signal exhibit very similar structure. According to the comment, the phrase has been modified as "It should be noted....".

- P8L15-16: The temperature response seems very large over Eurasia. Is this real? I find it hard to believe that the temperature response is over 2K. I think this needs to be investigated and discussed more.

==> Interannual variation of winter surface temperature is especially large over Siberia. So, variations of 2K are not surprising. Similar results using different datasets can be found in Chen et al. (2015).

Chen, H., H. Ma, X. Li, and S. Sun (2015), Solar influences on spatial patterns of Eurasian winter temperature and atmospheric general circulation anomalies, J. Geophys. Res. Atmos., 120, doi:10.1002/2015JD023415.

- P10L15-22: Is there a QBO in the model? How does the momentum forcing interact with the QBO. Surely at some points they will not be consistent with the H-T relationship?

==> There is no QBO in the model. It should introduce some additional variability. We think, however, that the average feature of the difference between strong and weak vortex experiments remain similar.

- P11L8-19: The authors are very sure about the casual links here. I think they need to be more speculative about the comments, or back it up with modelling evidence from their model.

  P11L8-19: " These characteristics of the surface response to stratospheric westerly zonal wind changes fit remarkably well to the global solar surface signals from observations (Figs. 1 and 9)."

==> According to the comment, the word "remarkably" has been removed. This sentence simply describes that the global feature of the tropospheric response is consistent.

- P12L13-19: So are the authors suggesting they do not believe the Haigh mechanism? I think it is still important, but the paragraph does not read that way. I would also cite Simpson et al, 2009.

==> The problem of Haigh et al. (2005) and Simpson et al. (2009) is that there is no reason provided for the tropical warming. It is difficult to attribute such warming in the lower stratosphere to direct solar forcing. To clarify our point of view, the sentences have been modified as follows.

"Lower stratospheric tropical heating was proposed as possible origin of the solar influence on the troposphere (Haigh et al., 2005; Simpson et al., 2009). However, the reason for the warming in the tropical lower stratosphere during high solar activity is unclear. It has been shown that such tropical lower stratospheric warming is associated with a downward penetration of westerly anomalies from the upper stratosphere and hence of dynamical origin (Kodera and Kuroda, 2002). "

- P13L13-16: Hood et al only show a subset of models, and not even all the coupled chemistry models from CMIP-5. Are there better (or additional) references that could expand on this point?

==> Hood et al. made a choice according to the reproducibility of the ozone variation in the upper stratosphere, which is the fundamental response to the solar spectra variation. It is natural to exclude such "unrealistic models" which cannot reproduce the fundamental solar influence. There are no other references that could expand on this point.

Figures

- Figure 1: I would make a and b more comparable. Use the same contour intervals and only plot of the oceans. Also use the same latitude ranges.

==> It is to show that in spite of different datasets (historical data or modern reanalysis data, sea surface or surface temperature) and different methods of analysis (composite mean or linear regression), similar results can be obtained. Note also that using the same contour intervals between panels a and b might bring some confusion because it is not exactly the same variables which are plotted (SST vs ST).

- Figure 4: There is a lot of detail in panel c, and it can't really be seen. Can you enlarge it to the size of the other panels.

==> We reduced the number of contours and also converted the vertical velocity from hPa/s to m/day to make the variation in the upper troposphere clearer.