

## ***Interactive comment on “Solar cycle in current reanalyses: (non)linear attribution study” by A. Kuchar et al.***

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We would like to thank the reviewer for his/her comments.

Author's response and changes in the manuscript are included below. In addition, you can find the revised manuscript as supplement where the particular changes are highlighted together with the comment referring to them.

(1) It is found that the 11-year ozone response in the tropical upper stratosphere differs greatly between the MERRA and ERA-Interim datasets (Figures 1m and 2m) and that neither response resembles that derived from observations, i.e., there is no double-peaked response. The annual mean upper stratospheric ozone response is decidedly negative for ERA and is slightly negative for MERRA, which is inconsistent with the ef-

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fects of 11-year solar UV forcing. For ERA, the negative response is most pronounced (up to 5%) at polar latitudes. In contrast, analyses of merged SBUV ozone data yield a positive response in the upper stratosphere with more pronounced positive maxima at polar latitudes. Analysis of the seasonal dependence of the polar maxima show that they occur mainly in the summer season in each hemisphere (see, e.g., Figure 1 of Tourpali et al., JGR, v. 112, D12306, doi:10.1029/2006JD007760, 2007). Obviously, therefore, there is an issue with the assimilation of ozone in the reanalysis datasets. In the text (p. 30891), the pronounced negative polar ozone response is interpreted as “connected with a higher destruction of ozone during the solar maximum period and consequent heating of the region.” This is possible since increases in temperature lead to increased ozone losses because of the temperature dependence of the reaction rates that control the ozone balance. Would this interpretation require that the assimilation model had interactive ozone chemistry? Please expand the discussion of this interpretation. In the case of MERRA, SBUV ozone profiles are assimilated while, in the case of ERA, no solar cycle variation of ozone is passed to the forecast model. There is also no solar cycle in irradiances passed to the radiative part of the forecast model for any of the three reanalysis datasets considered here. So, no direct solar-induced increase in ozone production would be expected even if the assimilation model has interactive chemistry. The SBUV ozone profiles have very low vertical resolution and may yield an 11-year ozone response that is biased toward higher altitudes compared to SAGE observations, which have much better (1 km) vertical resolution. So, assimilation of the SBUV profiles in the MERRA system may not have produced a realistic 11-year ozone variation in that reanalysis dataset. How does the lack of a realistic upper stratospheric ozone variation affect the value of the reanalysis datasets for investigating 11-year dynamical responses? This should also be discussed somewhere in the manuscript. The paragraph below was added to clarify this in the manuscript.

The negative response could be interpreted as a consequence of temperature rise leading to increased ozone losses because of the temperature dependence of the reaction rates that control the ozone balance in the upper stratosphere. This inter-

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pretation does not require that the assimilation model had included interactive ozone chemistry since in the model used for ERA-Interim the ozone as a prognostic variable is relaxed towards a photochemical equilibrium for the local value of the ozone mixing ratio, the temperature, and the overhead ozone column (Dee et al., 2011). An additional term is used to parameterize the heterogeneous chemistry. This fact together with the finding that the temperature and ozone are highly negatively correlated in the upper stratosphere, e.g. -0.93 for zonal mean between 15°S and 15°N in 1 hPa, provide reasonable explanation of the negative ozone response to the SC which is driven by temperature variability in the upper stratosphere. In the case of MERRA, while SBUV ozone profiles are assimilated with solar cycle passed to forecast model (as ozone analysis tendency contribution), no solar cycle was passed to the radiative part of the model. The same is also true for ERA-Interim and JRA-55 (see descriptive table of reanalysis product on SC in irradiance and ozone in Mitchell et al. (2014a)). Among other tendencies the dynamics and chemistry components also contribute to total tendency of ozone. These two tendencies prevent any variations in ozone analysis tendency though. Thus periods longer than 1 year are filtered out in the upper stratosphere. Only annual and semi-annual cycles are included. The SC-like periods seem to be diminishing approximately from 5 hPa except in the polar regions from both hemispheres. The negative correlation -0.93 between the tendency of dynamics and chemistry and tendency from analysis for zonal mean in the tropical upper stratosphere confirms this statement as well. This negative correlation roots from anti-phase relationship between the tendency from dynamics and chemistry. Therefore despite the fact that the analyzed ozone should contain a solar signal, the signal is very weak and is compensated by internal model variability in terms of dynamics and chemistry. Since the SBUV ozone profiles have very low vertical resolution this may also affect the ozone response to the SC in the reanalysis. These facts should be also taken into account in case of monthly response discussion of particular variables in the section 4.2.

(2) The derived upper stratospheric temperature response in all three reanalysis  
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datasets (Figures 1a, 2a, and 3a) is less than accurate due to the existence of large offset errors occurring at times when the input satellite radiance data and/or the assimilation scheme changed (McLandress et al., 2014). This problem is briefly noted on p. 30884, line 9, but it is not considered to be a major issue. Also, no attempt is made to correct or adjust the reanalysis temperature data prior to the analysis. Such retrospective adjustments are probably next-to-impossible for MERRA and JRA but could have been attempted for ERA using the procedures developed by McLandress et al. However, the McLandress et al. study only considered discontinuities occurring in 1985 and 1998. As noted by them, another discontinuity occurred during 1979 that would also need an adjustment if the time series begins in that year. But, at a minimum, the offsets in 1985 and 1998 could have been corrected. In the revised paper, please (a) apply the necessary adjustments and repeat the analysis for the ERA data; and (b) add statements to the discussion and conclusion sections pointing out the likely errors in the temperature results resulting from these unphysical temperature discontinuities.

The regression analysis has been repeated with the adjustments in 1985 and 1998. For simplicity the fundamental regressors were involved: trend, solar, saod, qbo1(2), enso. The differences between particular regression coefficient without adjustment and with adjustment are illustrated in the Fig. 1.

First of all, the adjustment dataset by McLandress et al. 2013 were downloaded and applied for the 1, 2, 3, and 5 hPa temperature field of the ERA-Interim dataset. As regards solar regressors the most pronounced differences are apparent in higher latitudes and especially in 1 hPa. However, the regression coefficients decreased by about 50 % when using adjusted dataset instead of unadjusted dataset these differences are not statistically significant in terms of 95% confidence interval. The difference in tropical latitudes is about 0.2 K/(Smax-Smin). Trend regressor reveals large turnaround from positive trend to negative trend in these levels. Another regressors do not reveal any remarkable difference.

In the revised paper the statements about the results of repeated analysis with adjust-

ments were added, see below. However, we keep the results from original analysis in order to refer and discuss the accordance and difference between our results and results from Mitchell et al., 2014, where no adjustment has been considered as well.

This paragraph was added: However, upper-stratospheric temperature response could be less than accurate due to the existence of discontinuities in 1979, 1985 and 1998 (Mclandress et al. 2013) coinciding with solar maxima. Therefore, the temperature response to solar variation may be influenced by these discontinuities in the upper stratosphere. The revised analysis with the adjustments from Mclandress et al. (2013) showed in comparison with the original analysis without any adjustment that the most pronounced differences are apparent in higher latitudes and especially in 1 hPa. However, the regression coefficients decreased by about 50% when using adjusted dataset and the differences are not statistically significant in terms of 95% confidence interval. The difference in tropical latitudes is about 0.2 K/(Smax-Smin). The trend regressor  $t$  from Eq. (1) reveals large turnaround from positive trend to negative in the adjusted levels, i.e. 1, 2, 3 and 5 hPa. Other regressors do not reveal any remarkable difference. The results in Figs. 1(b,e,h,k) and 2 from raw dataset were kept in order to refer and discuss the accordance and difference between our results and results from Frame and Gray (2010); Mitchell et al. (2014a), where no adjustment has not considered as well.

(3) There is no mention in the manuscript of the possibility that the calculated linear solar regression coefficients are affected by aliasing from the effects of strong volcanic aerosol injection events (El Chichon and Pinatubo) occurring following the cycle 21 and 22 maxima, respectively. The record is short (35 years) and these two fortuitously placed injection events are unique to this time period. They could have produced decadal-scale variations in the stratosphere that would not be entirely orthogonal to the solar forcing variable (the 10.7 cm radio flux). So, there could be some mixing of the volcanic and the solar regression coefficients. The most well-known possibility is that part or all of the 11-year lower stratospheric response of ozone and temperature de-

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rived from observations is a consequence of such aliasing (Solomon et al., JGR, 1996; Lee and Smith, JGR, 2003). Austin et al. (2008) concluded that this was not likely to be true for the chemistry climate models considered by them because the solar regression coefficients over the 1960-2005 period did not change much if an aerosol term was included or not in the regression model. However, Chiodo et al. (ACP, v. 14, p. 5251, 2014) have recently tested in more detail one such chemistry-climate model (WACCM 3.5) by carrying out simulations with and without volcanic aerosol forcing. They find that, at least for this specific model, the apparent solar-induced ozone and temperature responses in the lower stratosphere largely disappear in the simulation with no volcanic aerosol forcing. Thus, at least for WACCM 3.5, the solar-induced lower stratospheric response appears to be due almost entirely to the aliasing effects of the two eruptions. On the other hand, it is known that some CCMs overestimate ozone losses during high aerosol loading periods, causing a larger aliasing effect on the solar response than would occur when analyzing observations (Dhomse et al., ACP, 2011). At least some coupled climate models (e.g., MIROC-ESM-CHEM; Watanabe et al., Geoscientific Model Development, v. 4, p. 845, 2011) produce solar-induced lower stratospheric responses that are not strongly affected by aliasing from the El Chichon and Pinatubo eruptions. So, the answer to the question of whether or not the observationally estimated lower stratospheric response is strongly affected by volcanic aerosol aliasing unfortunately appears to depend on the model that is employed to simulate the climate system. Even the upper stratospheric solar response could be affected by such aliasing since the dynamical evolution of the entire stratosphere in winter was affected by these major eruptions. Further work is needed to resolve this issue. In the meantime, one should be careful to note the possibility that the lower stratospheric solar response derived from observational datasets could be affected by such aliasing. This should be done at appropriate places in the paper with appropriate added references.

The paragraph below was added regarding this comment.

However, the results presented by Chiodo et al. (2014) suggest the contribution of solar

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cycle variability could be smaller since two major volcanic eruptions are aligned with solar maximum periods and also given the shortness of analysed time series (in our case 35 years). These concerns related to the lower stratospheric response of ozone and temperature derived from observations has already been raised (e.g. Solomon et al., 1996; Lee and Smith, 2003). However, another issue is whether or not the lower stratospheric response could depend on the model employed in the simulations Mitchell et al. (2015).

(4) In the monthly analyses shown in Figures 4 and 5, by far the largest apparent solar response occurs in February at high northern latitudes in the form of a lower stratospheric warming, a mesospheric cooling, and an associated weakening of the zonal wind (polar vortex). This apparent response has been found in previous analyses of the ERA data (e.g., Frame and Gray, 2010). It is possible that this response is indeed solar-induced. For example, Gray et al. (J. Atmos. Sci., v. 61, p. 2777, 2004) suggests that the negative zonal wind response in late northern winter may be caused by an increased likelihood of major stratospheric warmings later in the winter under solar maximum conditions when the polar vortex in early winter is stronger, on average, and therefore less susceptible to disruption. In this manuscript (p. 30894), the February negative zonal wind response is regarded as real on statistical grounds alone: "In February, the intensive stratospheric warming and mesospheric cooling is associated with a more pronounced transition from winter to summer circulation attributed to the solar cycle (in relative impact methodology up to 30%)". However, one problem with this conclusion is that general circulation models have not yet successfully simulated the strong final warming in February under solar maximum conditions (e.g., Schmidt et al., JGR, v. 115, doi:10.1029/2009JD012542, 2010). Also, there is no similar observed response in late winter in the southern hemisphere. Given the short (35-year) record, it is possible that this response is not really solar but is instead a consequence of internal climate variability or aliasing from effects of the two major volcanic eruptions. Please revise the discussion to note this possibility.

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This was discussed in the paragraph below within the revised manuscript.

However, GCMs have not yet successfully simulated this pattern (e.g. Schmidt et al., 2010; Mitchell et al., 2015). Due to the short (35-year) time series, it is possible that this pattern is not really solar in origin but is instead a consequence of internal climate variability or aliasing from effects of the two major volcanic eruptions aligned to solar maximum periods Chiodo et al. (2014).

(5) The Introduction does not really explain what will be done in this manuscript and why it is necessary. It consists of a general and rather lengthy review of the topical area of solar cycle forcing of the stratosphere, including observational and model results. This review includes some material that could be left out and is not written in a way that explains what the outstanding questions / issues are. It never says what the objectives of the present work are and why they need to be addressed. Why is it necessary to consider non-linear methods in addition to linear multiple regression? Why is it necessary to investigate whether solar responses derived from assimilated reanalysis datasets are consistent with previous analyses of observations alone (e.g., whether a double-peaked response can be extracted from the reanalyses)? What will be done in this manuscript that is different from previous work? Please revise.

The Introduction was revised regarding to this comment. See added paragraph below.

Under this framework the paper by Mitchell et al. (2014a) has been published where 9 reanalysis datasets were examined in terms of 11-year SC, volcanic, ENSO and QBO variability. Complementing their study, we provide comparison with nonlinear regression techniques here, assessing robustness of the results obtained by Multiple Linear Regression (MLR). Furthermore, EP-flux diagnostics are used to examine solar-induced response during winter season in both hemispheres, and solar-related variations of assimilated ozone are investigated.

(6) Abstract, lines 17-20: "Furthermore, the seasonal dependence of the solar response was also discussed mainly as a source of dynamical causalities in the wave

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propagation characteristics in the zonal wind and the induced meridional circulation in the winter hemispheres.” This sentence is not clear. Please re-write or leave out. Also, in the next sentence, please insert “at solar maximum” after “Brewer-Dobson circulation”.

The abstract was rewritten in this way.

(7) P. 30881, lines 10-12. “Gray et al. (2009) have shown, with the fixed dynamical heating model, that the response of temperature in the photochemically controlled region of the upper stratosphere is approximately given 60% by direct solar heating and 40% due to indirect effect by the ozone changes.” This statement is a simplification of what is shown in Figure 2 of Gray et al. (2009). In fact, the contribution from the indirect effect of the ozone changes varies from nearly zero in the equatorial middle stratosphere to 60% near the equatorial stratopause. It is a strong function of position, depending on what the solar-induced ozone change is, which can vary strongly with season.

The statement was revised and the area with this proportion limited to the upper tropical stratosphere.

(8) P. 30881, lines 20-22. This sentence refers to the confirmation of the double-peaked vertical structure in the simulations analyzed by Austin et al. (2008). Please revise based on Comment 3 above.

This sentence was revised according to this comment. See below.

The observed double-peaked ozone anomaly in the vertical profile around the equator was reproduced, nevertheless the concerns about physical mechanism of the lower stratospheric response was expressed Austin et al. (2008).

(9) P. 30882-83, lines 27-. This brief summary mentions the work of Ineson et al. 2009 and Harder et al. 2009. However, a more recent detailed review of solar spectral irradiance variability has been given by Ermolli et al. (Atmos. Chem. Phys., v. 13, p.

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3945, 2013). They discuss, for example, that the Harder et al. measurements from the SORCE satellite may have been affected by instrument degradation with time and so may be too large in the UV. They conclude that the SORCE measurements, which are currently being re-calibrated by the SORCE team, probably represent a liberal upper limit on the true SSI variation while proxy-based SSI models such as the NRL model represent a lower limit. Please revise to bring this up to date.

The paragraph below was added according to this comment.

However, the measurements by Harder et al. (2009) from SORCE satellite may have been affected by instrument degradation with time and so may be overestimated in the UV (Ermolli et al., 2013). They have also concluded that the SORCE measurements probably represent the upper limit in the magnitude of the SSI variation. Consequent results of GCMs, forced with the SSI from the SORCE measurements, have shown larger stratospheric response than for NRLSSI dataset. Thus, coordinated work is needed to have reliable SSI input data for GCM simulations (Ermolli et al., 2013), and also to propose robust conclusions concerning solar cycle (SC) influence on climate (Ball et al., 2014).

(10) P. 30883, lines 10-13. These two sentences should be combined into one.

These two sentences were combined into one.

(11) P. 30884, line 20. Please change to: ... were analyzed on a daily ... P. 30885, first line: Please insert “For example,” before “the Brewer-Dobson ...”. P. 30885, line 6: Please change “Except for” to “In addition to”.

All suggested changes were realized.

(12) P. 30885, lines 6-14. This whole paragraph seems out of place in a section on Datasets. Please move it to either the Introduction or to section 4.2. In our opinion this paragraph forms a bridge between Introduction and Methodology sections. Based on that this paragraph would not be moved.

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(13) P. 30885, Eq. 1. This seems to be a standard regression model except for the NAO term. Is the NAO really independent of the other terms? Or, does it depend partly on ENSO and on the solar cycle? If the latter, then this may introduce errors in the results since there will be mixing of coefficients. Please either remove this term from the model or discuss the issue of independence and whether an NAO term is needed.

The paragraph related to the robustness of solar regression coefficient was added in section Methodology. See paragraph and documenting figures 2,3,4 and 5.

The robustness of solar regression coefficient has been tested in terms of including or excluding particular regressors in the regression model, e.g. NAO term was removed from the model and resulting solar regression coefficient was compared with the solar regression coefficient from original regression model. The solar regression coefficient seems to be highly robust since either the amplitude or statistical significance was not changed when NAO or QBO3 or all of them were removed. However, cross-correlation analysis reveals that the correlation between NAO and TREND, SOLAR and SAOD regressors is statistically significant.

See correlation matrix Fig. 6.

(14) P. 30886. Use of the 10.7 cm flux is acceptable for the solar proxy. However, the results are presented as solar max minus min values in the figures. What is the corresponding difference in the 10.7 cm flux? Is it 100 flux units? Please state this or, otherwise, it is not possible to convert the coefficients in the figures to actual numbers per change in the solar flux. It should also be stated in this paragraph that the 10.7 cm flux is a proxy for solar ultraviolet variations at wavelengths (200-300 nm) that are important for ozone production and radiative heating in the stratosphere.

The information about normalization was added. The statement “for solar ultraviolet variations at wavelengths 200-300 nm that are important for ozone production and radiative heating in the stratosphere” was added as well.

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(15) P. 30887, line 13. Please define NWS, either here or in the reference list. Line 25: Perception. NWS was replaced by NOAA since the NAO was downloaded from their website included in the references.

(16) P. 30888, line 2. feedforward should be feed-forward and backpropagation should be back-propagation.

These words were changed in this way.

(17) P. 30889, first line. change to: ... from purely practical ...  
“pure” was changed to “purely”.

(18) P. 30889. Here, the figures are discussed for the first time. Looking at the figures, the small size makes them difficult to read. Also, the hatching to indicate statistical significance makes it difficult to determine exactly what the underlying color is. I am not sure what to do about this but the authors should consider a different presentation method. Would it be possible to enlarge by a factor of 2-3 the regression coefficient plots while leaving the RI plots (which are less illuminating) at a small size?

The paper figures were completely revised. Regression coefficient figures were put together for all reanalyses and relative impact figures were drawn for each reanalysis. Monthly response figures were also revised – temperature, zonal wind geopotential height response is expressed by contours and results are shown from 30° at the summer hemisphere to 90° in the winter hemisphere as was suggested above.

(19) P. 30891, lines 7-9. “The largest discrepancies can be seen in the upper stratosphere and especially in the temperature field ...”. It should be noted here that this could be at least partly because the discontinuities in the reanalysis temperature data are most pronounced in the upper stratosphere. It will be interesting to see how the ERA-Interim results change after the discontinuities are minimized using the McLandress procedure.

See your comment 3.

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(20) P. 30895, lines 3-7. This sentence should be divided into two sentences. The second sentence should begin with: While, in the MERRA . . .

These two sentences were divided.

(21) P. 30895-30898 - Dynamical effects discussion. Overall, this is a valuable and detailed description of the dynamical processes that are implied by the monthly linear regression results. In particular, as stated at the bottom of p. 30895, the coupled solar-induced anomalies of ozone, temperature, geopotential, and E-P flux divergence support the hypothesis of a weaker BDC near solar maxima, consistent with the previous interpretations of Kodera and Kuroda (2002) and Matthes et al. (2006). However, it is also stated on p. 30895, lines 7-9, that an effort is made in this section to “deduce the possible processes leading to the observed” solar-induced anomalies. I am not sure that this section really achieves this goal. It is more a description of what is happening dynamically rather than why. You are right it is more a description. Thus “deduce” was replaced by “describe”.

(22) At the end of section 5 (p. 30898), it is noted that the weakening of the BDC is apparently not as well established in the SH winter as in the NH winter. It is then stated that this could help explain why the temperature response in the equatorial lower stratosphere is larger in August during SH winter (about 1 C) than it is in December for NH winter (about 0.5 C). First of all, although the lower stratospheric temperature response in SH winter (Figure 5d) does appear to be larger than during NH winter (Figure 4d), it is quite impossible to read the amplitudes of the response from these figures (see comment 18 above). More importantly, if the slowing of the BDC is less in the SH winter, then why is the lower stratospheric temperature anomaly larger at that time?? Again, the discussion in this section is useful for describing what is happening but does not really address the why question. To address the why question, diagnostic analyses of model data are probably required.

This part was completely revised since the statements about BDC and temperature

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anomalies were inconsistent.

(23) Conclusions section (p. 30898). Please add a caution to the reader who may otherwise think that the reanalysis datasets are free of errors and that it is straightforward to evaluate the 11-year solar response using these datasets. In particular, please note again the existence of large discontinuities in the temperature record occurring in 1979, 1985, and 1998 that will translate into errors in the derived solar coefficients.

The caution was made in this paragraph, which was added into the Conclusion.

The reanalyses have proven to be extremely valuable scientific tools (Rienecker et al., 2011). On the other hand, they have to be used with a caution for example, due the existence of large discontinuities occurring in 1979, 1985 and 1998 (McLandress et al., 2013) that translated into errors in the derived solar coefficients. For instance the revised anal15 ysis with the adjustments from McLandress et al. (2013) resulted to 0.2 K/(Smax-Smin) difference between regression coefficients in tropical latitudes of the upper stratosphere.

(24) P. 30899, lines 8-9. Again, the statement that the Austin et al. (2008) results confirmed the double-peaked structure is a bit of an exaggeration. Please revise (see comment 3).

“even confirmed by the coupled chemistry climate model simulations (e.g. Austin et al., 2008)” was replaced by “in spite of that the concerns about physical mechanism of the lower stratospheric response was expressed (e.g. Austin et al., 2008)”.

(25) P. 30900, some English corrections: Line 2: ... which investigated the solar ...; Line 15: ... show an ability to simulate the ...; Line 19: ... on northern winter conditions; nevertheless, southern winter ...

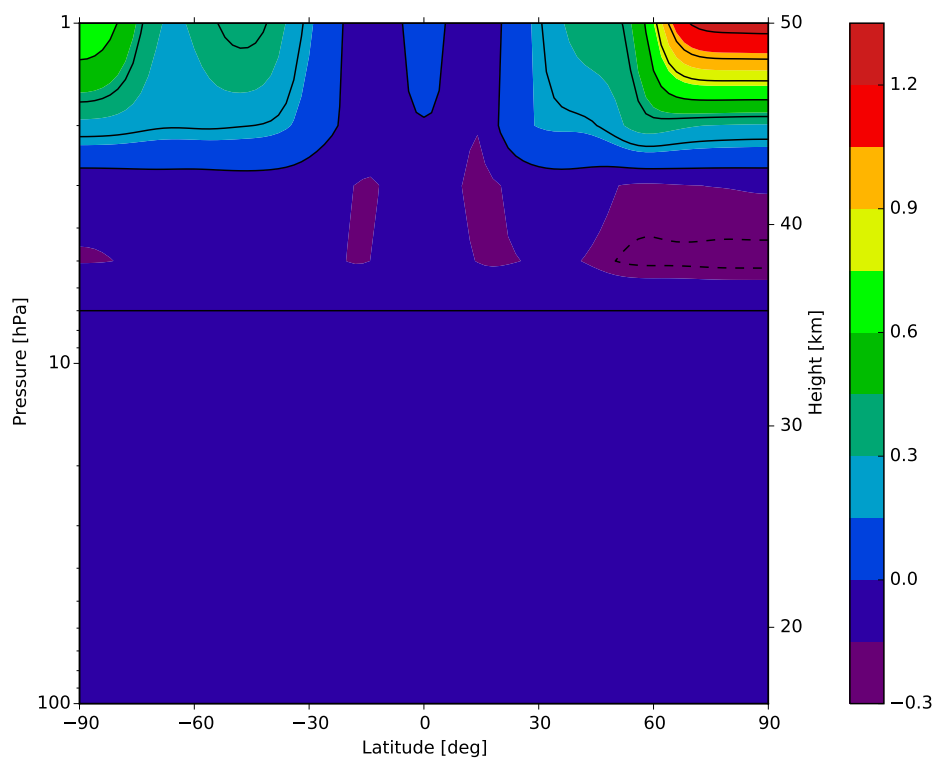
First two issues were changed. However, the sentence “The main part deals with the solar influence on northern winter conditions nevertheless, southern winter anomalies were also discussed.” were deleted based on the comment 30 of ref. #4.

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Please also note the supplement to this comment:  
<http://www.atmos-chem-phys-discuss.net/14/C12756/2015/acpd-14-C12756-2015-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 30879, 2014.

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**Fig. 1.** Difference between solar regression coefficient of non-adjusted and adjusted ERA-INTERIM dataset. Contour lines are drawn with step 0.2.

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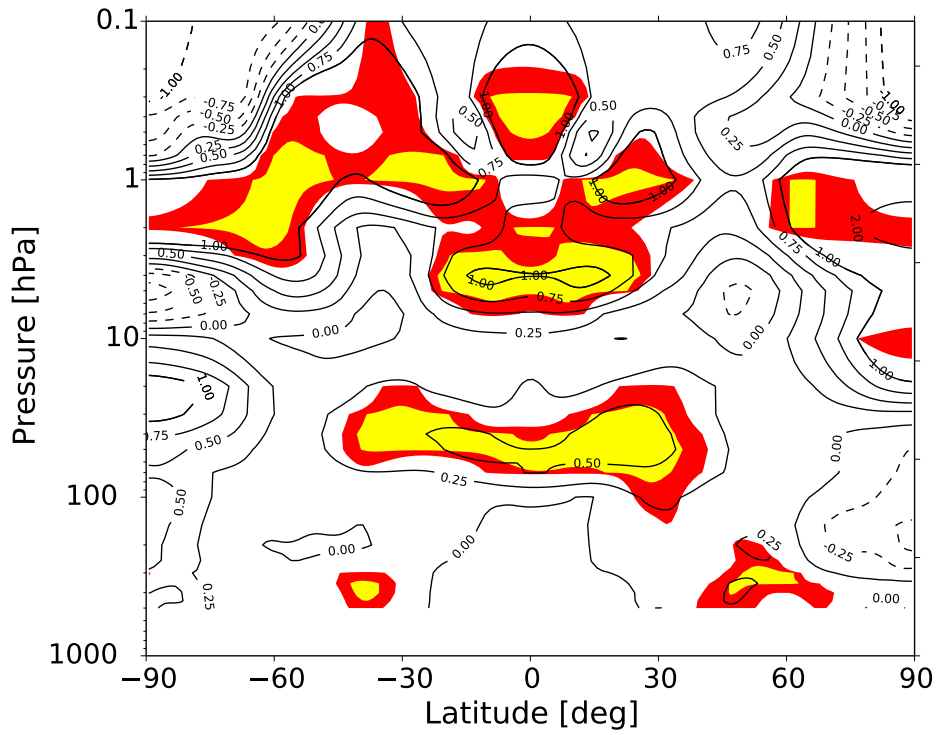


Fig. 2. Temperature response to SC for MERRA. All regressors included.

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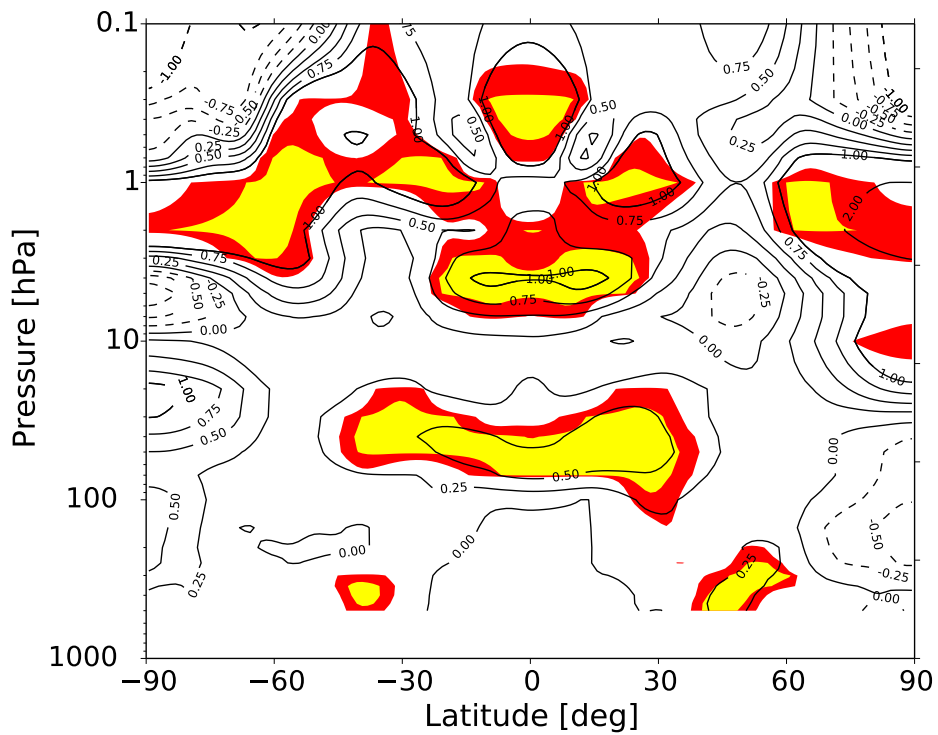


Fig. 3. Temperature response to SC for MERRA. All regressors included except NAO.

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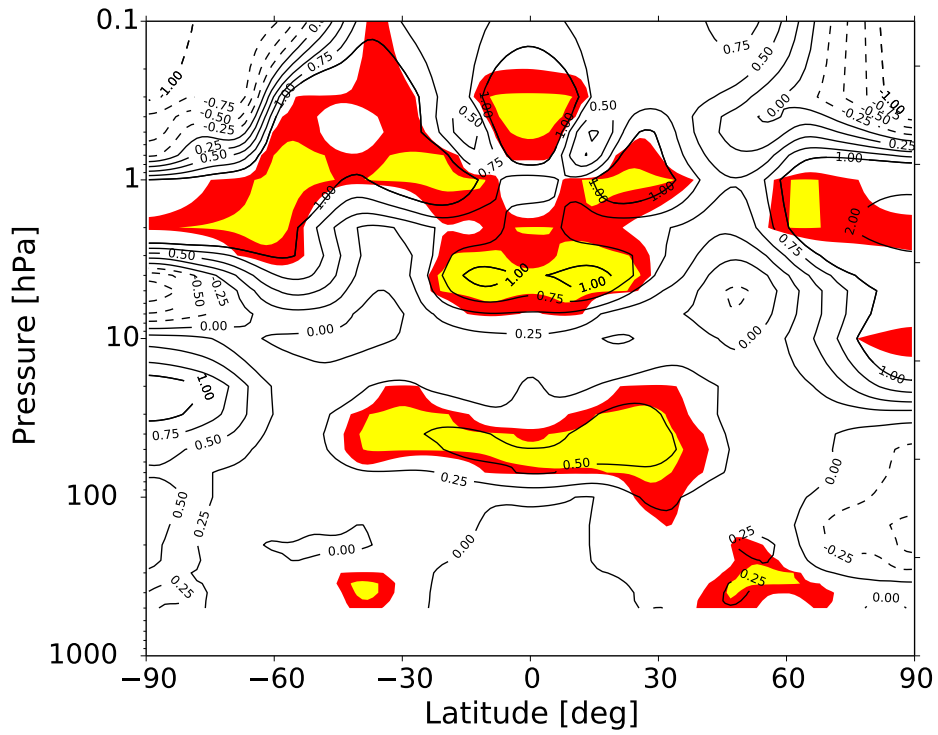


Fig. 4. Temperature response to SC for MERRA. All regressors included except QBO3.

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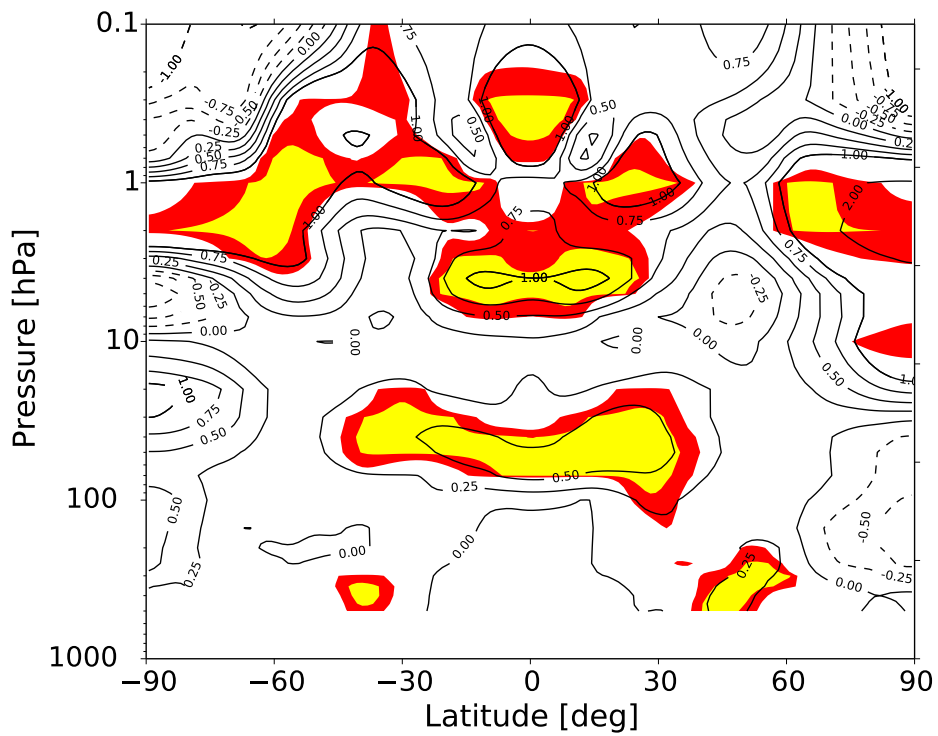
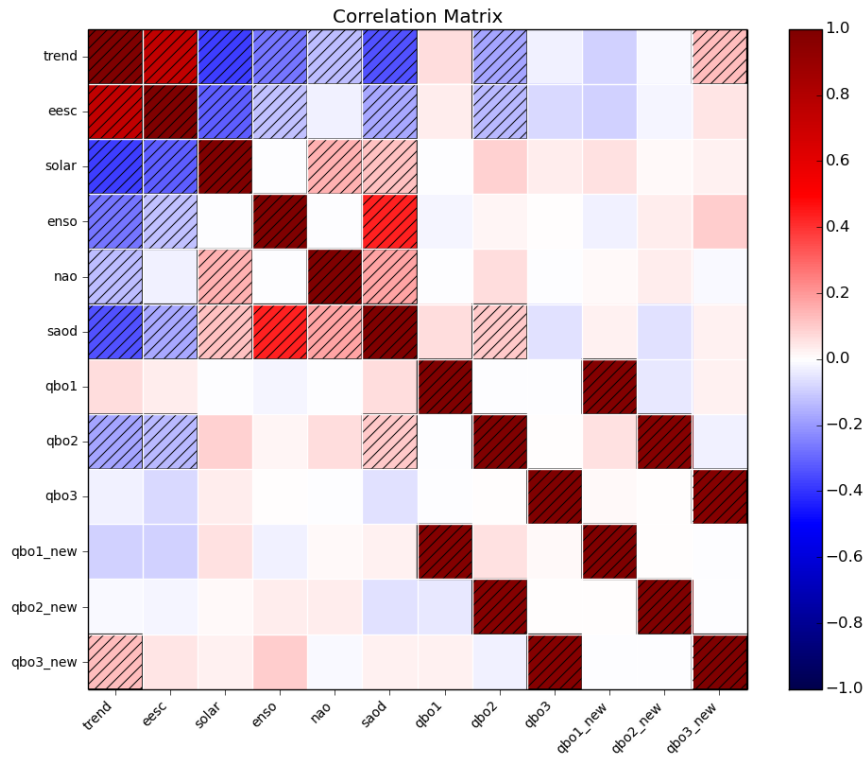


Fig. 5. Temperature response to SC for MERRA. All regressors included except NAO and QBO3.

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**Fig. 6.** Cross-correlation matrix of regressors for MERRA. QBO1\_new, QBO2\_new and QBO3\_new computed from a Principal Component Analysis of equatorial, deasonalized zonal mean zonal wind anomalies only.