Atmos. Chem. Phys. Discuss., 14, C12790–C12816, 2015
www.atmos-chem-phys-discuss.net/14/C12790/2015/

Atmospheric Chemistry and Physics Discussions

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

A. Kuchar et al.

kuchara@mbox.troja.mff.cuni.cz

Received and published: 25 March 2015

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

Author's response and changes in 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.

General comments

There are a lot of different techniques being used simultaneously in this paper. Overall I feel like there needs to be more in depth explanation of what is precisely done. Also, more intermediate results in the methodology chapter should be presented. It is, e.g.,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

not surprising to see that all three statistical methods lead to similar results given only monthly data were used. The true power of nonlinear methods only comes into play when having lots of events available. Daily data would be a good starting point. In my opinion, there should be a few time series figures demonstrating the hind-casting abilities of the different methods. Also, the authors should consider combining all three methods and averaging them in a weighted fashion based on their forecast abilities.

We have considered this general comment as a relevant and corresponding experiments were done. However, these suggestions seem to be out of range of our paper. Thus no relevant changes in the paper were considered according to this comment.

The hindcasting abilities of the different methods was demonstrated daily and monthly zonal means of ERA-Interim temperature time-series between 25°N a 25°S in 1 and 50 hPa. All statistical models were trained with data from training period 1979-2006 to set up the tunning parameters. The tunning was done by 5-fold crossvalidation. The model configuration with the highest correlation calculated for the crossvalidated subsets was used for testing period 2007-2013.

The results in form of coefficient of determination are presented in the Fig. 1. From these table it is quite obvious that the referee's statement about power of nonlinear methods when having lots of events, i.e. using daily data is valid only for SVR. The neural networks in our case are not so powerful as was expected. This could be affected by the fact that we use only one layer of hidden neurons and therefore the training was not conducted so properly as in the SVR training.

Using two layer neural networks we achieved even better performance. However, coefficient of determination based on daily data is still lower. The comparably worse performance of MLPs may be a result of their increased vulnerability to inhomogeneities and breaks in the data, due to high complexity of the respective transfer function combined with its nature as global mapping in the space of predictors

ACPD
14, C12790–C12816, 2015

Specific comments

(1)P30881,L23 to P30882,L5 This paragraph is in my opinion too early, it should be placed behind the general explanation of the mechanism (currently L6 to L18).

Your comment was considered as relevant and moved behind the paragraph “The ozone and temperature perturbations associated....”.

(2)P 30882, L19 to L25 This paragraph seems to have nothing to do with the rest of the analysis and should be omitted.

This part of the paragraph was omitted: “Observational and modeling studies over the past two decades have fundamentally changed our understanding of wave processes and the coupling between the middle atmosphere and tropospheric conditions (Gerber et al., 2012). It has been shown that the stratosphere plays a significant and active role in tropospheric circulation on various time scales (Baldwin and Dunkerton, 1999; Lu et al., 2013; Solomon et al., 2010). A deeper understanding of the mechanisms of communication between the middle atmosphere and troposphere contributes to better climate change predictions. However, a number of questions about the coupling processes with regard to solar signal perturbation have to be answered.”

(3)P30883,L2 The TIM/SIM data (Harder et al., 2009) mentioned here are currently checked and corrected for possible instrument degradation. These data should be considered as the upper boundary of possible solar spectral irradiance variations, whereas the NRLSSI data by Lean (2005) which are widely used in chemistry climate models give a kind of a lower limit. An appropriate statement should be made in the text. There is a review on this issue by Ermolli et al. (2013) which should be cited here.

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

ACPD
14, C12790–C12816, 2015

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).

(4)P30884,L13 The introduction ends somewhat abrupt without a statement on the intention nor the focus of the present paper. This should be added here. Furthermore, a short outline of the paper should be given, e.g. "In section 2 the datasets are presented, in section 3 the analysis methods are described ..."

The paragraph with a short outline was added. See below.

The paper is arranged as follows. In section 2 the used datasets are described. In section 3 the analysis methods are presented along with regressor terms employed in the regression model. Section 4 is dedicated to the description of the annual response results. In subsection 4.1.1 solar response in MERRA reanalysis is presented. Next, in subsection 4.1.2 other reanalyses are compared in terms of SC. Comparison of linear and nonlinear approaches is presented in subsection 4.1.3. Section 4.3. describes monthly evolution of SC response in the state variables. Section 5 is aimed at dynamical consequences of the SC analysed using the EP-flux diagnostics.

(5)P30885,L20ff I am missing explanations of the various parameters used in equation (1) such as z, phi, and lambda.

The explanation of these parameters was added.

(6)P30886,L10ff The QBO factors should be calculated from the data of each reanalysis and then used together with the respective data set. Computing them just from MERRA seems inconsistent. Why are the QBO factors computed using the regression model itself? Statistical features that serve as input to a supervised method should be

ACPD
14, C12790–C12816, 2015

independent. I would recommend to compute the QBO factors from a Principal Component Analysis of equatorial, deasonalized zonal mean zonal wind anomalies. The QBO factors were calculated from the data of each reanalysis and this fact was also mentioned within the manuscript. However, the results haven't changed significantly. See Figs. 2 and 3 for ERA-Interim and Figs. 4 and 5 for JRA-55.

As was already pointed out in the text this approach follows the paper by Frame a Gray (2010) to avoid contamination of the QBO regressors e.g. by the solar signal as well as the other regressors. However, the contamination needs not to be just linear and stationary as is considered in linear regression approach. The regression analysis has been repeated with the QBO regressors computed from a PCA of equatorial, deseasonalized zonal mean zonal wind anomalies. For this purpose the MERRA reanalysis was used. The differences between solar regression coefficient results from revised and original analysis are presented in Figs. 6 and 7. It is obvious that the amplitude of the solar signal changes according to the changes in QBO regressors, but the pattern is still the same. These changes can be considered as statistically significant.

In addition, the correlation analysis between the regressors did not reveal any remarkable changes, only the correlation changes either between QBO2 and TREND, EESC and SAOD or QBO3_new and TREND are worthy of notice since these correlations are statistically significant for p value < Âă0.05. See correlation-matrix in Fig. 8.

(7)P30887,L10 Why was the NAO included as a regressor variable? It could be that the NAO is not independent from other regressors, such as the solar cycle or ENSO, so you introduce possible errors in the regression model. Did you test whether the solar regression coefficient changes markedly when you include or leave out the NAO regressor? Please, discuss this in the text. What does the reference NWS,2013 mean?

NWS was replaced by NOAA since the NAO was downloaded from their website included in the references.

The paragraph related to the robustness of solar regression coefficient was added in

ACPD
14, C12790–C12816, 2015

section Methodology. See paragraph and documenting Fig. 6,9,10 and 11.

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 figure 8.

(8)P30887,L27 What does the author mean by "highly complex"? The MLP is in fact a fairly simple mathematical construct.

This collocation was omitted in the manuscript.

(9)P30888,L9 There are no words about the hyperparameters of MLP and SVR. Which values do they have and how were they determined? Also, which algorithm is used to optimize the MLP? Which SVR is used, epsilon or nu SVR? It would also be interesting to know which software libraries were used in this study.

All information suggested above was added to the manuscript. Software libraries were mentioned in the acknowledgement section.

(10)P30888,L15 Lack of explanatory power? What is meant here?

The sentenced was revised. See below.

The earlier mentioned lack of explanatory power of the nonlinear techniques in terms of complicated interpretation of statistical models (Olden, 2002).

(11)P30888,L21 Using the relative impact based on the median is ok for this study. However, an even better approach would be to average across relative impacts based

ACPD
14, C12790–C12816, 2015

on quantile variations. The author should consider looking at the featureimpact Python package which implements this approach.

We considered relative impact based on quantile variations via the feature impact Python package, which implements this approach. The results of relative impact for monthly zonal mean of ERA-Interim temperature between 25°N a 25°S in 1 and 50 hPa are presented in Figs. 12 and 13. Linear regression, Neural networks and SVR have been used in the left, middle and right figure respectively. Blue line corresponds to original approach based on the median and red line correspond to quantile-variation approach. The relative impact values of solar regressors are almost the same through all methods. However, the relative impact of other regressors may differ. The relative impact of solar regressor based on the median seems to be robust in all cases. Therefore, we conclude it is convenient to use this approach, although the approach based on quantile variations would appear even better, especially in case of other regressors.

Based on the statements above no revisions was not considered in this point since we have already discussed quartile variation approach in our paper.

(12)P30888,L23 y-y_k is the difference and not the variance of it.

This sentenced was revised. “variance of” was added in front of difference.

(13)P30889, L11 to L13 What is the average difference between the solar maxima and minima in the period 1979-2013 in terms of F10.7 solar radio flux units? Please state clearly the value in the text and in the figure captions. In the literature the commonly used value is 100, sometimes 130 units.

The signal is expressed as the average difference between the solar maxima and minima in the period 1979-2013, i.e. normalized by 126.6 solar radio flux units. And this part was also included in the manuscript.

(14)P30891, L20 The negative ozone response in the ERA-Interim dataset needs some further explanation. Higher destruction of ozone at solar maximum as stated in the text

ACPD

14, C12790–C12816, 2015

should become the dominant process higher up in the mesosphere (due to enhanced water vapor photolysis generating OH which in turn depletes ozone). And what is meant by "consequent heating“? Should it be cooling? Please clarify this in the text.

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 interpretation 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 5 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 10 included. The SC-like periods seem to be diminishing approximately from 5 hPa except in the polar regions fro 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

ACPD
14, C12790–C12816, 2015

relationship between the tendency from dynamics and chemistry. Therefore de15 spite 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.

(15)P30891, L27 It is not entirely clear which dataset the authors are talking about in this paragraph.

"solar signal" was replaced by "ozone response".,

(16)P30892, L22 Do you mean southern hemisphere? From the figures I cannot detect any relative impact signal in temperature exceeding say 30% in the northern hemisphere.

You are right. "southern"was replaced by "northern".

(17)P30892,L25 This is the first mention of volcanic signals being important in the lower stratosphere. In my opinion, this needs some further discussion. Given the shortness of the examined time series (1979-2013, i.e. 35 years) and the fact that two major volcanic eruptions happened with about 10 years difference and, thus, were aligned to maxima in the 11-year solar cycle it is possible that there are some problems with the attribution. This is e.g. discussed in a recent paper by Chiodo et al., ACP, 2014 also in view of the length of the considered time series, using a chemistry climate model. However, the possibility of aliasing must be mentioned in the text and some appropriate citations should be included.

The paragraph below was added regarding this comment.

However, the results presented by Chiodo et al. (2014) suggest the contribution of solar cycle variability could be smaller since two major volcanic eruptions are aligned with so-

ACPD
14, C12790–C12816, 2015

lar 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).

(18)P30893,first paragraph This paragraph explains what has not been done in a lengthy way. I would suggest to substantially shorten this or to omit it.

First sentence was omitted.

(19)P30893,L9ff From the above explanations I cannot follow this conclusion. Please, clarify this in the text.

This paragraph of the manuscript was revised to be more clear than before.

(20)P30894, L8 From Figure 4d I cannot see any downward propagation of a temperature anomaly. I can only see a positive temperature anomaly that extends further down into the stratosphere compared to the January situation, at least at low latitudes. Do you mean this? Or do you mean something that happens from February to March? Please clarify this in the text.

“propagates downward” caused a misunderstanding and was replaced by “extends further down”.

(21)P30894, L14 I can guess from the Figure 4d and 4h that the anomalies reach tropospheric levels. But strictly spoken this is not shown in your figure.

The citation of Mitchell at al., 2014 paper was placed here since they included also tropospheric levels in the regression analysis.

(22)P30894, L28 Geopotential height anomalies are not shown in Figures 4 and 5. Please state this in the text.

ACPD
14, C12790–C12816, 2015

The additional figures of geopotential height anomalies were included.

(23)P30895, L3ff This is a whole paragraph about something that is not shown (no October panels in Fig 4) and it reappears in the conlusions section. I suggest omitting these sentences.

“only November shown” statement was included in this place. Although the figures of monthly response does not include October we believe these differences in the lower mesosphere should be pointed out since the model top could play a role in this case.

(24)P30896, L24 Again, geopotential height anomalies are not shown in Figure 4. The authors discuss the shape of the vortex in this section without showing geopotential height results, this is somewhat strange. I suggest either inclusion of additional figure panels or a restriction of the discussion to the shown variables.

The additional figures of geopotential height anomalies were included.

(25)P30897, L12 to L14 At this point Sudden StratosphericWarmings are mentioned for the first and only time. This is done in a way that the reader can gain the impression these warmings happen only in February which is not true. Did you check the occurrence rates and the seasonal distribution of SSWs in the MERRA dataset? If not, please omit this sentence. This applies also to the last half sentence in the abstract.

The sentence was omitted. The last sentence in the abstract was changed to: “The hypothetical mechanism of a weaker Brewer Dobson circulation at solar maxima was reviewed together with discussion of polar vortex behaviour.”

(26)P30898, L2 What do you mean by "latitudinal coordinates“? Please, reformulate and clarify.

“coordinates” was changed to “component”.

(27)P30898, L19 and P30899, L3 It is sufficient to state once that you used the last generation of reanalysis datasets.

ACPD
14, C12790–C12816, 2015

“last generation of reanalysed data” was changed to “three reanalysed datasets: MERRA, ERA-Interim and JRA-55” in the conclusion section.

(28)P30899, L1 Given the short analysis period (35 years), I would not write "robust“, also in view of a possible aliasing issue with the volcanic eruptions.

“can be considered sufficiently robust and they” was deleted from this sentence.

(29)P30899, L2 Frame and Gray used ERA data, right? So please do not call it an observational study since it is a reanalysis study. This does also apply to the same sentence in the abstract.

“observational” was changed to “attribution”.

(30)P30900, L18 to L20 The sentence starting with "The main part ...“ can in my opinon be deleted. Otherwise you should add in a concise way the essence of the dynamics discussion.

“The main part deals with the solar influence on northern winter conditions nevertheless, southern winter anomalies were also discussed.” was deleted.

(31)P30901,L2 Please reformulate "So it would be desirable ...“ into "Hence, it would be interesting to ...“ Otherwise the question arises why you didn’t do it, immediately.

“So it would be desirable” was reformulated to “Hence, it would be interesting”.

Technical comments:

(1)The figures are very difficult to read due to their smallness and their design and should, therefore, be substantially enlarged or even re-designed in a different way, e.g., with contours on top of the color fields. Maybe statistical significance can be included in a different way (e.g. as bold white contour) as it is really difficult to see the color behind the hatching and then to get an impression on the magnitude of the significant signal. This is especially a problem in Figures 4 and 5. Another suggestions for Figures 4 and 5: Why not concentrate on the respective winter hemisphere and show results from the

ACPD
14, C12790–C12816, 2015

equator to the winter pole or possibly from 30° at the summer hemisphere to 90° in the winter hemisphere? And a question concerning the scaling of the EP-Flux arrows: The arrows show predominantly horizontal anomalies, did you apply the scaling only to the horizontal component?

The paper figures was 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. The scaling was applied for both components. However, the vertical component of residual circulation was multiplied by 1000, thus the units are in 10ˆ{-3} Pa/s.

(2)A native English speaker needs to proofread this paper.

The manuscript was proofread before ACP discussion. In this phase the authors paid more attention to correct way of writing. Before prospective publishing the paper could be proofread again.

(3)Maybe the paper title should be formulated more precisely, e.g. "The 11-year solar cycle in current reanalyses: A (non)linear attribution study of the middle atmosphere“

The paper title was changed to “The 11-year solar cycle in current reanalyses: A (non)linear attribution study of the middle atmosphere”.

(4)Please rename in the abstract (P30880,L5) the "traditional linear approach“ as "multiple linear regresion approach“.

“traditional linear” was replaced by “multiple linear regression” in the abstract.

(5)Temperature differences are given in Kelvin, not in #C as temperature itself. Please change this in the text and in the figure captions.

The unit of geopotential height is given as meters [m]. Isn’t it geopotential meters

ACPD
14, C12790–C12816, 2015

[gpm]?

The units were revised in these cases.

(6)Please consider reformulating the section headlines to remove the parentheses, e.g. 4.1.1 Annual response – Comparison with JRA-55 and ERA-Interim.

This section headline without parenthesis was considered as more convenient.

(7)P30887, L25f there is twice "in our case“ in this sentence.

“in our case” was deleted once.

(8)P30894, L1 Do you really mean "alternation“ in the sense of reversal or "alteration“ in the sense of change?

“alternation” was changed to “alteration” because it was supposed to mean in the sense of change since regression coefficients express relative change between Smax and Smin.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C12790/2015/acpd-14-C12790-2015-supplement.pdf

---

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

1hPa:

	Monthly		Daily	
	Train	Test	Train	Test
LREG	0.135902128359	0.188644744567	0.0999010739788	0.119092463972
SVR	0.139631982558	0.123990201857	0.731505412416	0.671168700694
NNET	0.697032804235	0.561393545156	0.299611435909	0.289565371261

50 hPa:

	Monthly		Daily	
	Train	Test	Train	Test
LREG	0.0389747054139	0.151568481135	0.02337480342	0.0305293568918
SVR	0.0604949228305	0.0825654167504	0.734330755984	0.657803351645
NNET	0.789019942167	0.51132516853	0.346617586523	0.369187889736

NNET 2HL	0.967575411623	0.461844164912	0.742810213875	0.708926970146

Fig. 1. Coefficient of determination of particular hindcast experiments for 2 pressure levels.

ACPD

14, C12790–C12816, 2015

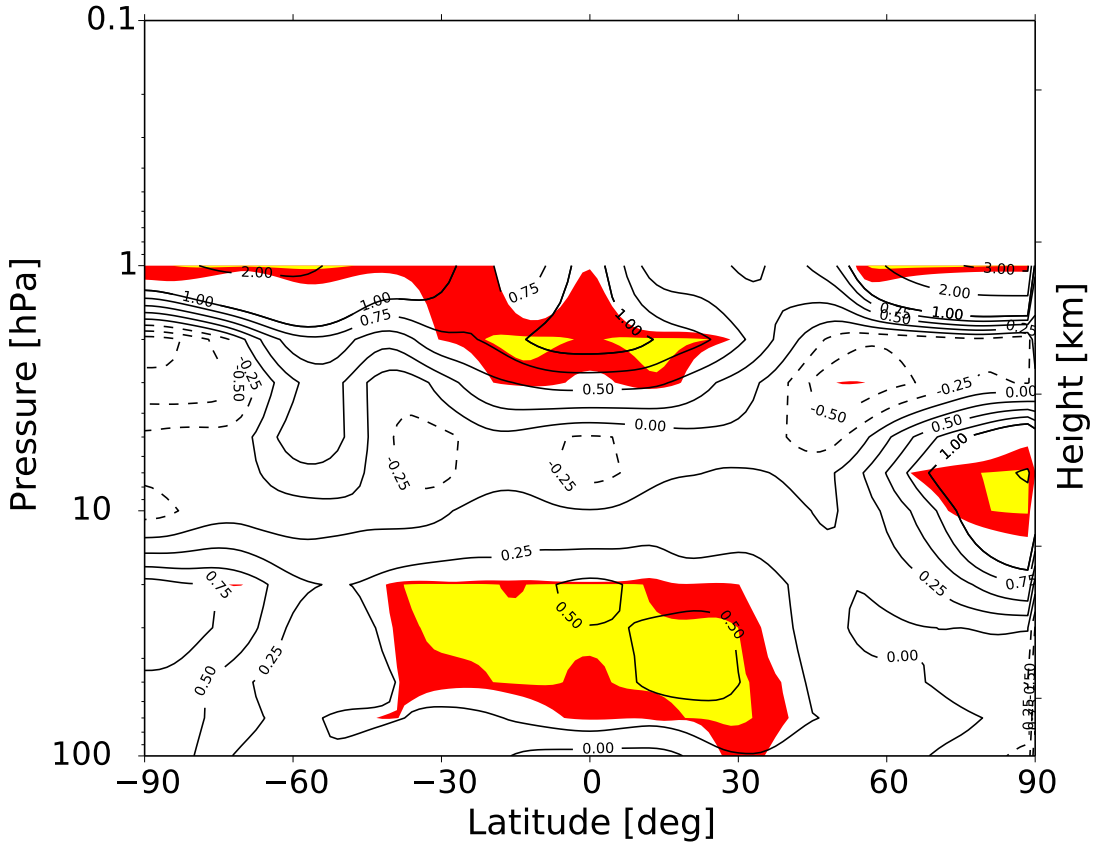

Fig. 2. Temperature regression coefficient for ERA-Interim. The QBO factors were calculated from the MERRA.

ACPD
14, C12790–C12816, 2015

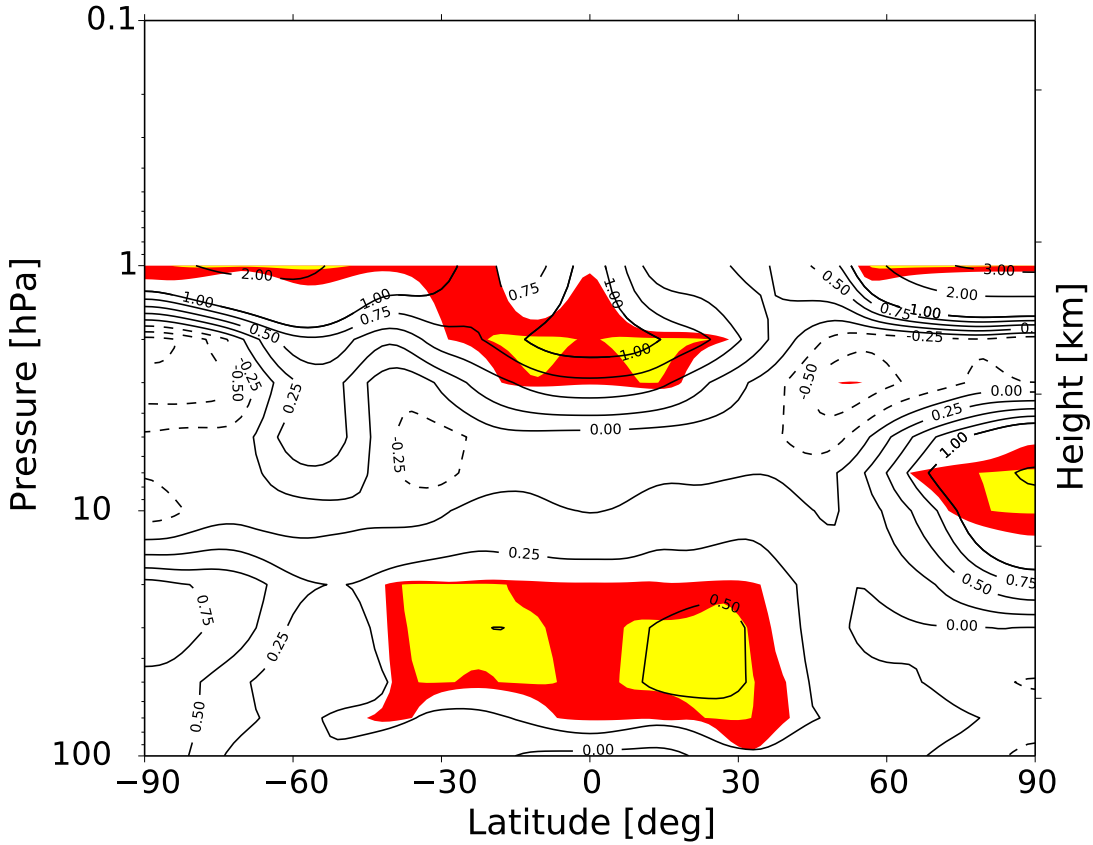

Fig. 3. Temperature regression coefficient for ERA-Interim. The QBO factors were calculated from the ERA-Interim.

ACPD
14, C12790–C12816, 2015

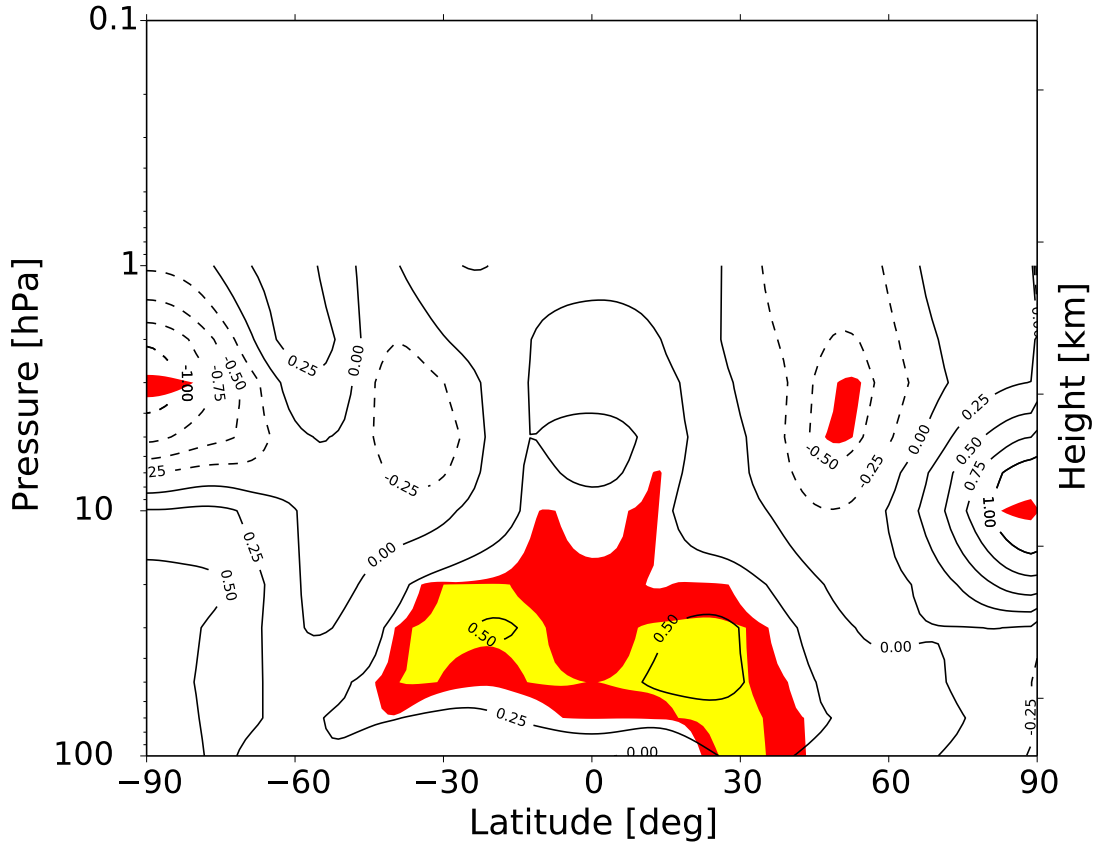

Fig. 4. Temperature regression coefficient for JRA-55. The QBO factors were calculated from the MERRA.

ACPD
14, C12790–C12816, 2015

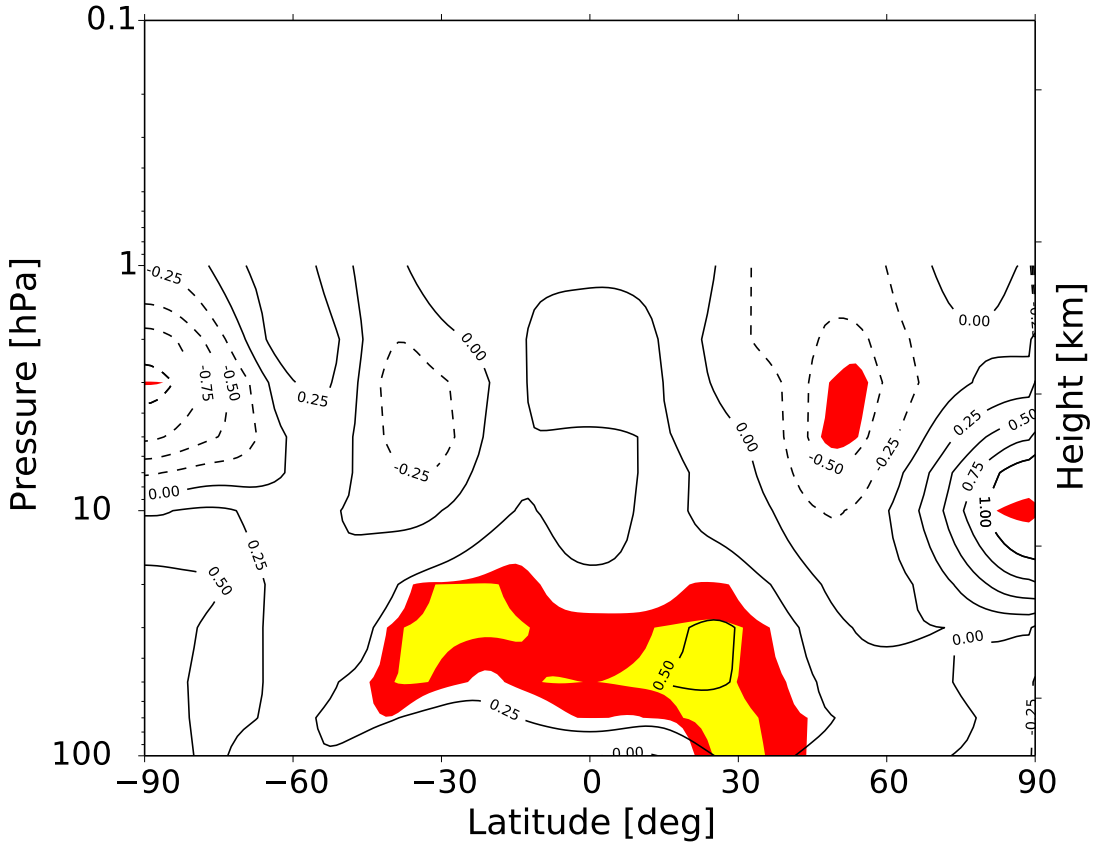

Fig. 5. Temperature regression coefficient for ERA-Interim. The QBO factors were calculated from the JRA-55.

ACPD
14, C12790–C12816, 2015

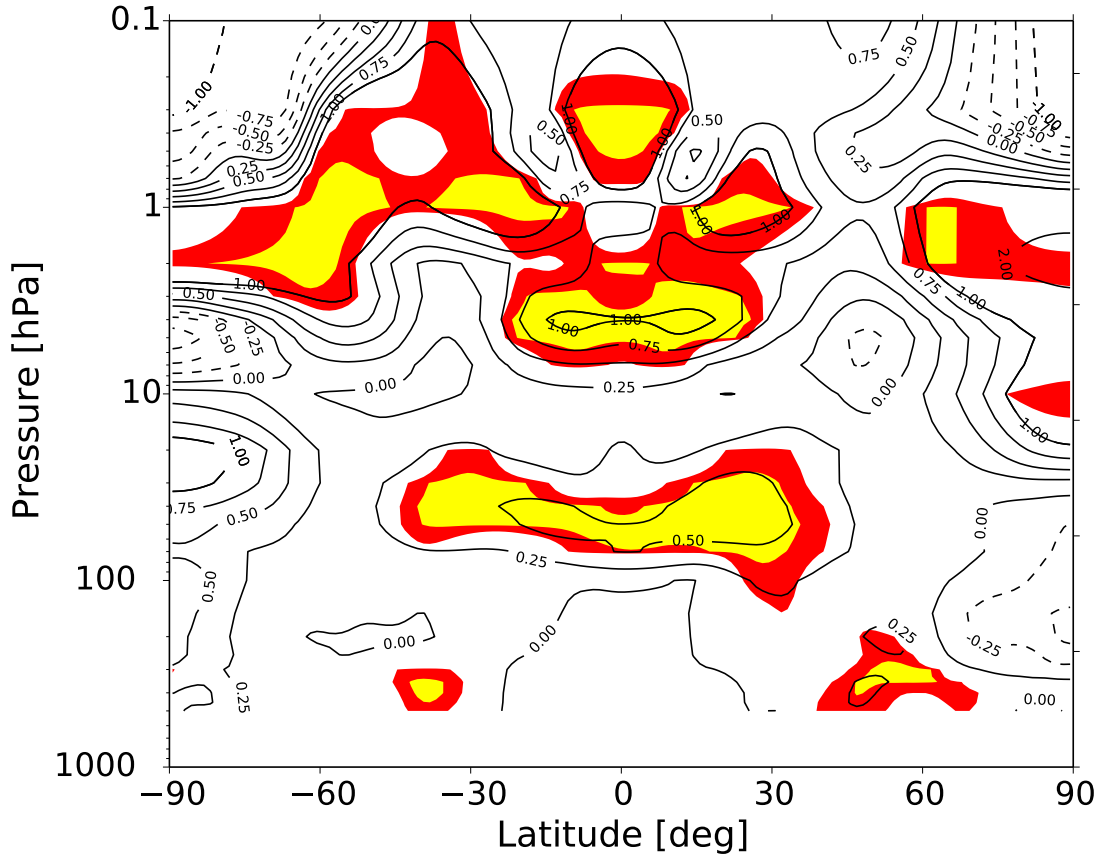

Fig. 6. Temperature response to SC for MERRA. The QBO factors were computed by the approach following the paper by Frame and Gray (2010).

ACPD
14, C12790–C12816, 2015

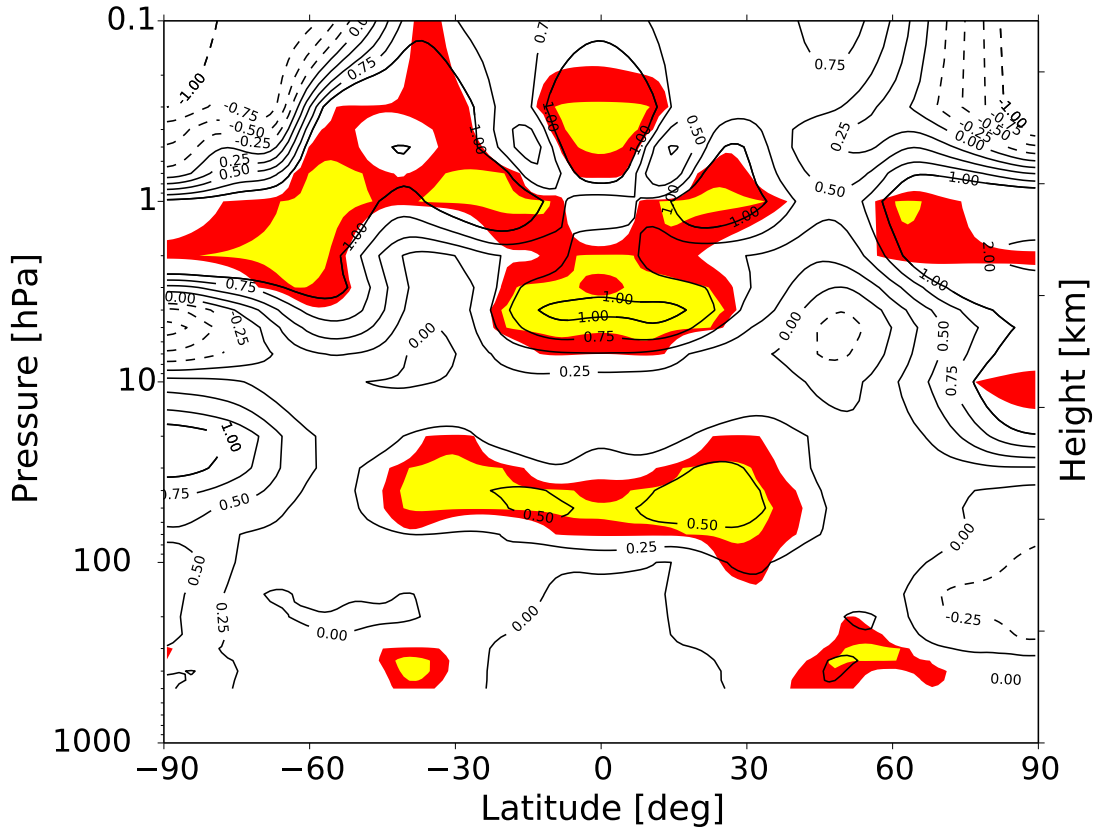

Fig. 7. Temperature response to SC for MERRA. The QBO factors were computed from a PCA of equatorial, deseasonalized zonal mean zonal wind anomalies.

ACPD
14, C12790–C12816, 2015

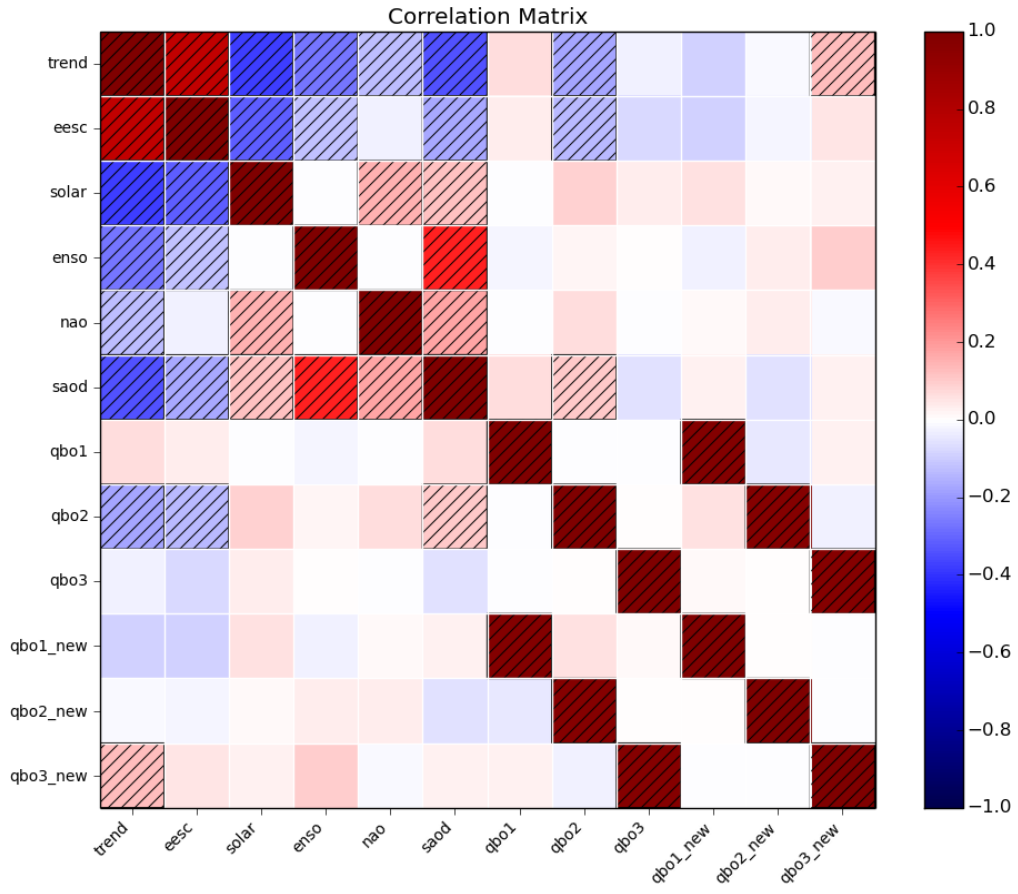

Fig. 8. Cross-correlation matrix of regressors for MERRA. QBO1_new, QBO2_new and QBO3_new computed from a Principal Component Analysis of equatorial, deasonalized zonal mean zonal wind anomalies only.

ACPD
14, C12790–C12816, 2015

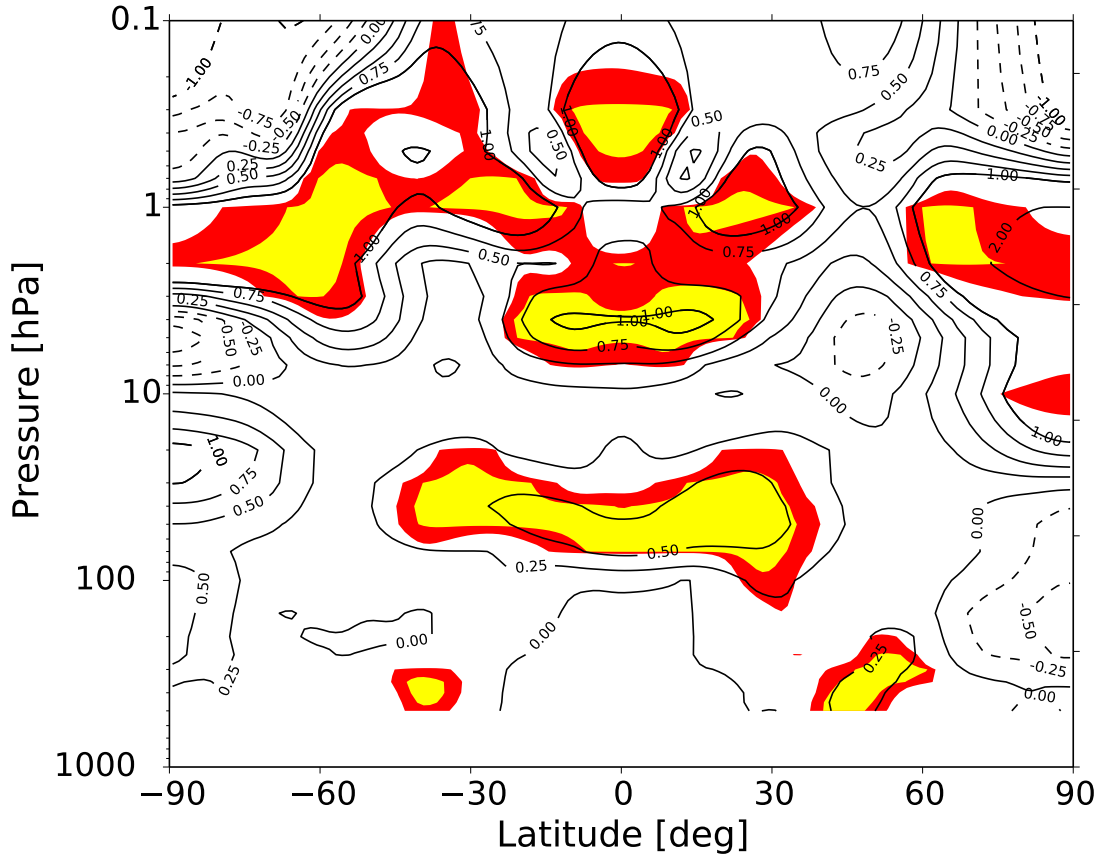

Fig. 9. Temperature response to SC for MERRA. All regressors included except NAO.

ACPD
14, C12790–C12816, 2015

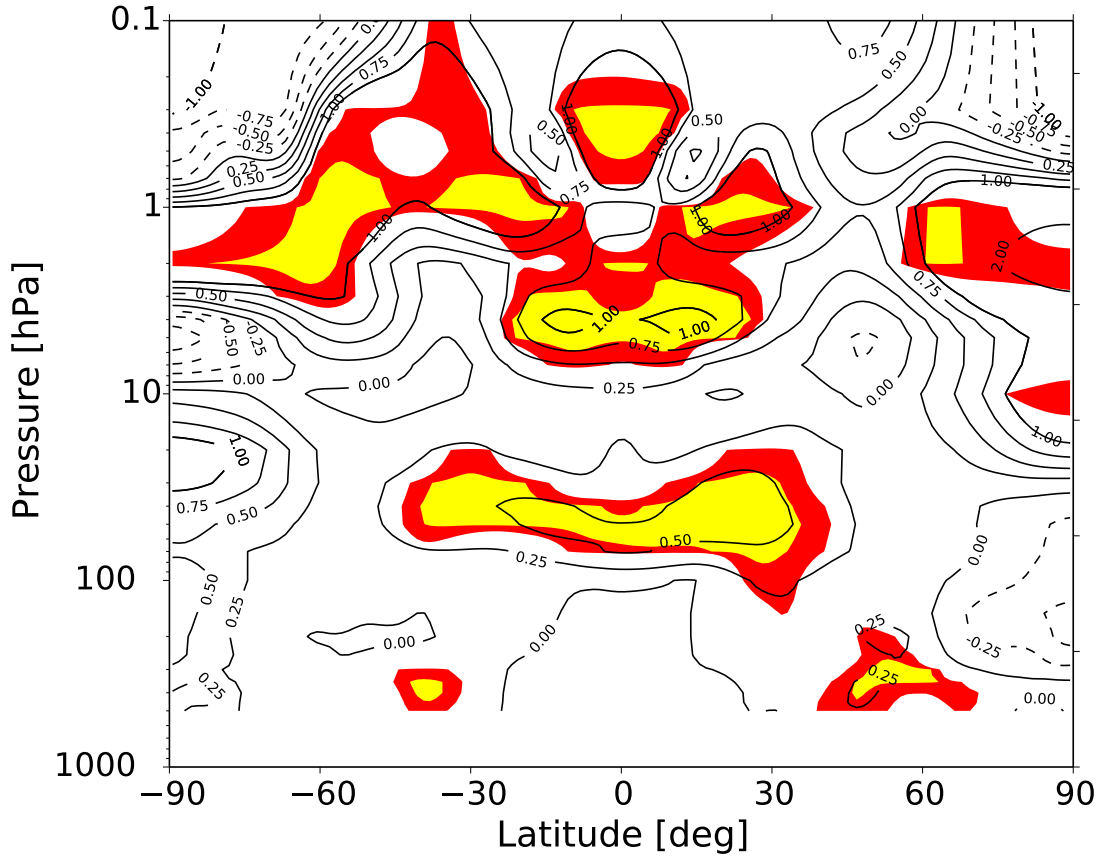

Fig. 10. Temperature response to SC for MERRA. All regressors included except QBO3.

ACPD
14, C12790–C12816, 2015

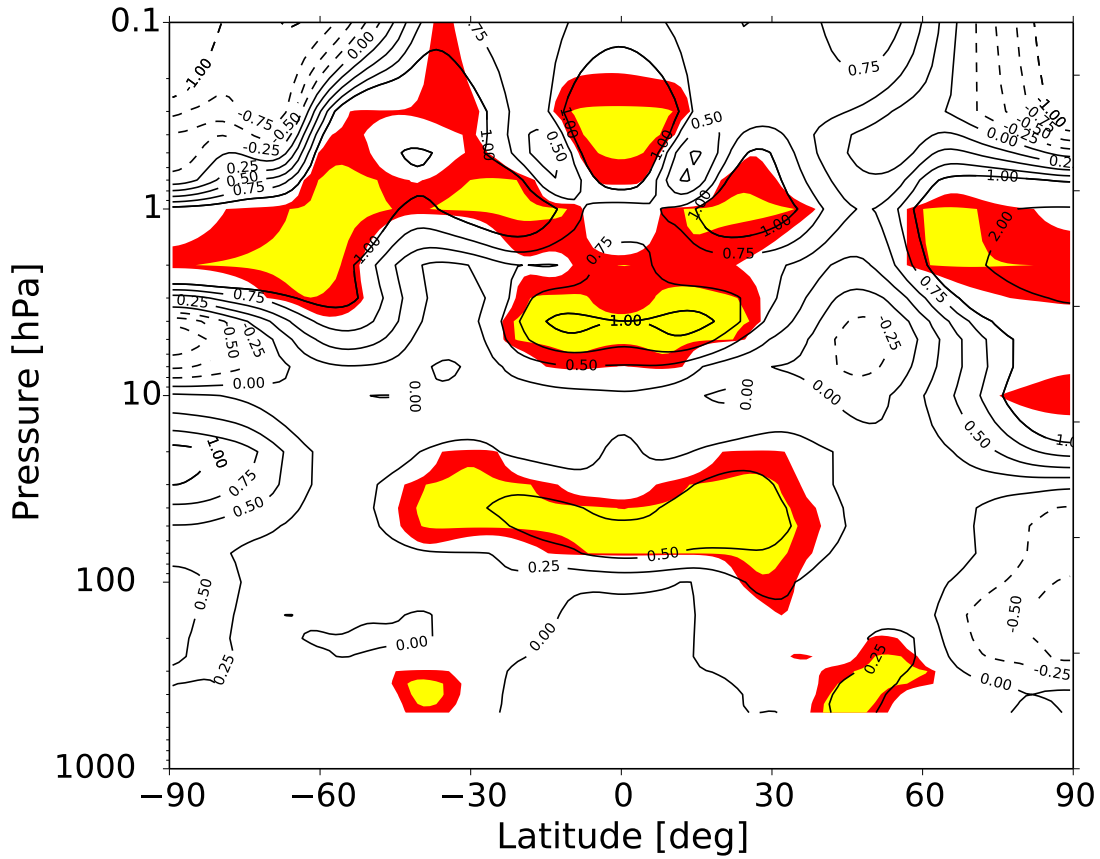

Fig. 11. Temperature response to SC for MERRA. All regressors included except QBO3 and NAO.

ACPD
14, C12790–C12816, 2015

ACPD
14, C12790–C12816, 2015

Interactive Comment

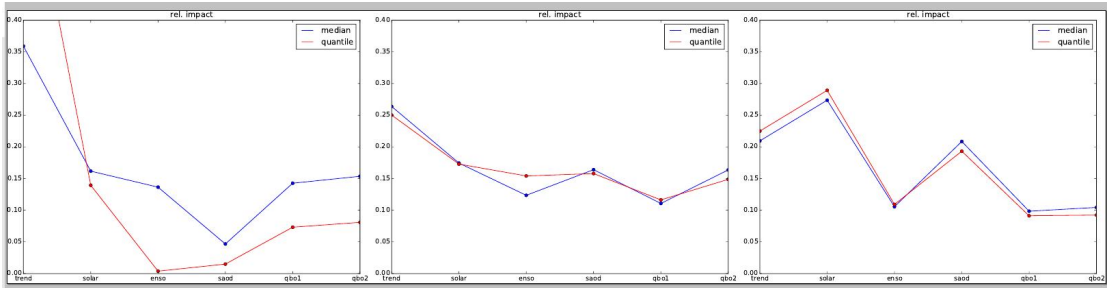

Fig. 12. The results of relative impact for monthly zonal mean of ERA-Interim temperature between 25°N a 25°S in 1 hPa.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

ACPD
14, C12790–C12816, 2015

Interactive Comment

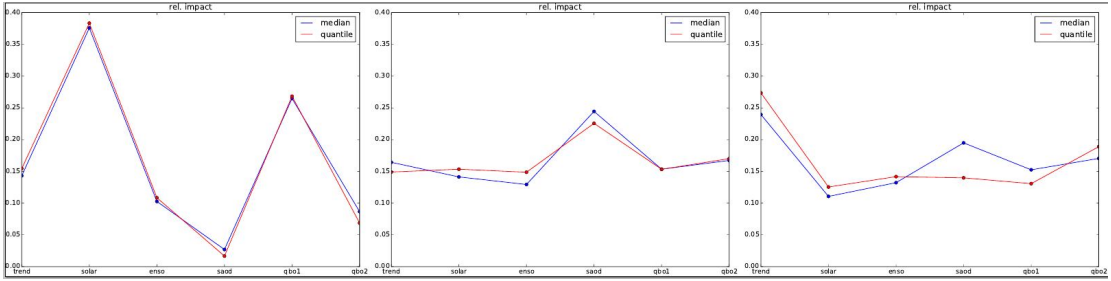

Fig. 13. The results of relative impact for monthly zonal mean of ERA-Interim temperature between 25°N a 25°S in 50 hPa.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper