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Interactive comment on “The stratospheric response to external factors based on MERRA data using linear multivariate linear regression analysis” by M. Kozubek et al.

M. Kozubek et al.

kom@ufa.cas.cz

Received and published: 13 February 2015

We would like to thank the reviewer for his/her constructive comments. By addressing these issues, we think the manuscript has been greatly improved.

General comments: The study addresses the topic of solar variability and its influence on Earth's atmospheric variability. In particular, the study addresses the influence of the 11-year solar cycle on atmospheric temperature and winds. The authors employ a multiple linear regression analysis of the MERRA reanalysis data to attribute patterns in temperature and winds to the solar signal, to particular modes of variability such as

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the NAO, ENSO, ENSO Modoki, and the QBO and to the volcanic signal. The main results of the study seem to be:

1) The solar signal in the MERRA data is similar to the one found in ERA-40, but the positive temperature anomaly in the upper stratosphere extends over all latitudes in MERRA, while in ERA-40 it is restricted to the tropics. (The shown plots suggest that the positive temperature anomaly in the lower stratosphere is weaker in MERRA than in ERA-40, but this result is not mentioned in the article.)

Answer: These features are discussed in the paper now. Some new regressors were added (more variable like CO₂, fixed volcanic aerosols data problem) into the model so the results are slightly different from previous version.

2) The volcanic signal is not significant in MERRA. While the topic itself seems relevant and the study seems novel in the sense that this specific analysis of the MERRA data has not been published before, the authors fail to provide a clear motivation for this study. Why is it important to analyze this specific data? What was the problem with previously used data or methods? What is the motivation to apply this particular method to the MERRA data? Although the authors provide a somehow structured overview of earlier studies in the Introduction section, the shortcomings or inconsistencies of previous studies are not clarified. Furthermore, it is not clearly conveyed how the present study can contribute to solving an existing problem or answering previously unsolved questions. Similarly to the missing clear motivation, also the focus of the study remains unclear. Is the focus on the influence of the solar cycle on stratospheric variability, or on the detectability of solar cycle signals in reanalysis data, or on the evaluation of the MERRA reanalysis?

Answer: We have extended the introduction and described the motivation and considered scientific questions with more details.

The finding that there is no volcanic signal in the MERRA data is very remarkable, since this signal is clearly detectable in observations and also in other reanalysis data

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(Crooks and Gray, 2005). In addition, this finding seems to contradict the findings by Chiodo et al (2014) who do find a significant volcanic signal not only in their model simulations but also in the MERRA reanalysis. The authors should consider double-checking the corresponding calculation and plots on which they base their conclusion. (Unfortunately the units are not given for any of the plotted quantities.) In addition, the authors do not provide a reasonable explanation of the missing volcanic signal and what this finding could hint at.

Answer: We have identified an error in the volcanic aerosol dataset. The updated results show clear volcanic signal in the MERRA data confirming the result of Chiodo et al., (2014). The unit are described in the text (results section) now.

The lines of argument and the interpretations that are given in the article are not easy to follow. Also it is not clear to me what the conclusions are and how the authors drive at some of their statements. Most results of the statistical analysis are just described without a proper interpretation what the results imply, e.g., for a better understanding of atmospheric interactions, for the consistency or usability of the MERRA reanalysis or of other reanalyses, or for the validity of a certain method. Although the results are partly discussed, put into context, and compared to previous studies, it is hard to follow the lines of argument.

Answer: We have added more discussions of the results emphasizing their implications for the atmospheric dynamics.

While the applied method seems reasonable, the authors should be more specific then describing the analysis they actually performed and what the differences to the methods used in previous studies are.

Answer: We have extended the data section adding the comparison with the previously applied methods.

The overall presentation should be improved substantially. In particular, the language

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and the figures should be improved. In some paragraphs the language is so poor that it is impossible to follow what the authors would like to say. In the results section it is hardly possible to follow the description of the individual signals by looking at the plots, since the units of the plotted quantities are not provided.

Answer: We have improved the text and figures (bigger labels, symmetric colour bar). In particular, we have reformulated the result description and discussion section. We have asked a native speaker to read and correct the text.

Overall the study seems reasonable, but the present article needs substantial revisions before it can be published.

Specific comments: -

The title and also the wording in the abstract and main text should be made more specific. The title and also parts of the abstract and main text suggest that the stratospheric response to external forcings (although the word “factors” is used in the title) is studied. In fact, the two forcings that are studied are the solar and the volcanic signals, the latter one of which is dropped in the analysis because of a presumably weak and insignificant signal. Since the study seems to be focused on the attribution of atmospheric patterns of variability to the external solar forcing and to dominant patterns of variability (NAO, ENSO, QBO), the title, abstract and main text should reflect this. Using the words “external” and “internal” might be additionally misleading in this context if not made specific. (The sun and volcanoes are external to the atmosphere; NAO, ENSO, and QBO are internal to the atmosphere; NAO and ENSO are external to the stratosphere; QBO is internal to the stratosphere.)

Answer: The title and abstract have been changed to make them more relevant to the considered problem.

- What is the advantage of using reanalysis data compared to observations? What is the advantage of the MERRA reanalysis compared to other reanalyses? (Other re-

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analysis are also available for pressure levels up to 0.1hPa.) - Answer: The reanalyses products cover entire globe and there are no gaps in the datasets so they fit for the analysis of the variability modes better than observations. There are differences of the MERRA dataset in comparison with ERA-interim. Therefore, the analysis of the variability modes in MERRA and their comparison with similar results obtained from other products helps to better understand the level of uncertainties.

- Why do the authors analyze meridional wind? - Answer: The meridional wind is very important part of the stratospheric dynamics (Dobson-Brewer circulation is mainly meridional circulation), so we can analyse the impact of different parameters on it. It is added in the text.

- The authors should specify in what way the method used in this study is equal or different to the method used in Crooks and Gray (2005). - Answer: It is the same method but we added more exploratory variables in the regression model than Crooks and Gray. It is explained in the text now.

- In the introduction section potential volcanic signals are not mentioned at all, although the aliasing problem / mutual masking of the solar and the volcanic signal seem to be a relevant issue in this context.

Answer: We have explored the problem with volcanic dataset and it is fixed now. The results confirm previous studies. The possible impact of volcanic eruption is described in the introduction section,

- I would have expected the results section to start with the solar signal. Instead, the solar signal is treated last.

Answer: The result section has been reformulated and solar signal is discussed at the beginning.

- The comparison to previous studies could be made more consistent if all (or most) results were compared to the same study. E.g. one could compare temperature and

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zonal wind signals for solar, ENSO, NAO and QBO to the ERA-40 results by Crooks and Gray (2005). Instead, it seems that the authors compare the ENSO results to results of model studies, the NAO results to observations, and the QBO results to observational and reanalysis studies.

Answer: The discussion section has been rewritten but we still discuss the influence of different phenomena separately and compare them with different studies because not all studies include all phenomena.

- The meridional wind signal of ENSO Modoki is not discussed, but the plot is shown.

Answer: The discussion of the meridional wind variability has been added.

- Why do the authors not mention the weaker positive solar signal in the lower stratospheric temperature compared to ERA-40?

Answer: This discussion has been added to the paper.

- A discussion of the limitations of a linear regression model is missing.

Answer: The discussion about the limitations has been added to the text.

- I cannot follow the authors' interpretation of the results concerning ENSO Modoki in the conclusion section. What does it mean that the ENSO and ENSO Modoki signals are different besides the trivial fact that they affect different regions?

Answer: Classical ENSO affect tropics and subtropics mainly at about 100 hPa. ENSO Modoki strongest signal is found not only in tropics but also about 50°S up to 10 hPa so it can affect stratospheric dynamics (wave activity) and chemistry in the higher latitudes. According to Garfinkel, 2012 there is possible teleconnection between these two phenomena, which should be considered when we would like to analyse general changes in dynamics.

P 23892: L10: Why "mainly"? As far as I see the study is based solely on MERRA data.

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Answer: We have removed mainly from the text.

L12: “We do not find: : :” -> rather start with the things one does find and then compare them to other studies

Answer: We have rephrased this sentence.

P 23894: L16: “Chiodo et al. (2014) found out that: : :” -> Might be a bit too general. The statement should be restricted to the lower stratosphere (and 45 yr simulations).

Answer: We have rephrased this sentence.

L22: I could not find the comparison to all mentioned reanalyses.

Answer: Now the only reanalyses, which are used in the text is mentioned.

L22: “The main advantage: : :” -> The mentioned advantage is also true for other reanalyses.

Answer: We have rephrased this sentence (added information about other reanalyses).

P 23895: L3: “Dobson-Brewer circulation” -> Why is this mentioned here and never appeal again later in the article?

Answer: In the improved version it is discussed in the discussion sections.

L12: “used by Gray et al., 2005” -> Ref. should be Crooks and Gray, 2005

Answer: Corrected.

L17: The source of the EMI data is missing.

Answer: The reference has been added to the data section.

L18: Why do the authors choose QBO at 50 and 10 hPa? Why is the 10 hPa QBO not mentioned in the analysis?

Answer: The results for 50 and 10 hPa are very similar (but shifted in time) so we have

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shown only 10 hPa and mention that 50 hPa are similar.

L20: The source of the volcanic data is missing.

Answer: The reference has been added to the data section.

L26: “We have tried two ways: : :” -> Which of the two was finally used for the analysis? Both? Is the Durbin-Watson test the same as the one used by Frame and Gray (2010)? Is the Box-Jenkins method the same as the one used by Chiodo et al. (2014)? The authors should be more specific here and also specify whether their method removes autocorrelation or detects it. Furthermore, was the autoregressive term used in the final regression or not?

Answer: We have tried two ways to remove autocorrelation. First, we used Durbin–Watson test for auto-correlation treatment of our analysis (used by Frame and Gray, 2010). Second, we followed a Box-Jenkins pre-whitening procedure [Box and Jenkins, 1980] used by Chiodo et al., 2014. Finally we used the first method which helps us to remove autocorrelation and that is why we do not use the autoregressive term in the final regression. This is now explained in the paper.

P 23896: L22: “We cannot find any significant response: : :” -> The crosses in the plot (Fig.2 top) seem to indicate that there is a significant response.

Answer: After the recalculation the results differ from the previous version. L20: “Negative signal can be found: : :” -> From looking at the plot (Fig.2 bottom) this signal seems to be insignificant.

Answer: After the recalculation the results differ from the previous version.

P 23897: L3: “Surprisingly: : :” -> If this is surprising, why is it not discussed what this fading could imply?

Answer: The discussion has been added to the text (discussion part).

L6: “These results confirm: : :” -> I cannot follow this discussion about NAO.

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Answer: The sentence has been rewritten and now it should be clear.

L19: “The positive signal: : : could be connected: : :” -> Not clear what this means.

Answer: The statement has been reformulated and now it should be clear.

L23: “ENSO Modoki is: : :” -> This introduction of ENSO Modoki should rather go to the introduction section.

Answer: This paragraph has been moved to the data section.

L27: “We identify: : :” -> I cannot follow this from looking at the plot (Fig.5 middle). Both signals at 1hPa seem to be insignificant.

Answer: After the recalculation the results differ from the previous version.

P 23898: L9: “In the zonal wind analysis: : :” -> From looking at the plot (Fig.6, bottom) this signal seems to be insignificant.

Answer: After the recalculation the results differ from the previous version.

L12: “The analysis of meridional: : :” -> From looking a the plot (Fig.6, top) all signals from 1hPa and higher seem to be insignificant.

Answer:After the recalculation the results differ from the previous version.

Technical corrections: Figure 1: Legend could be replaced by putting labels/annotations directly at the corresponding lines.

Answer: We have improved the figure according to the reviewer suggestion.

Figures 2-7: - The colorbars and scales are misleading; a different choice would facilitate reading the plots. -> colorbar symmetric around zero and meaningful adjustment of min/max values to be able to compare plots more easily

Answer: The colorbars has been improved (they are symmetric around zero, but it not possible to have the same min/max for all figures because there are big differences

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between them). We have plotted zero signal with the same colour for all figures.

- In the caption it says that there are left and right panels, but they are top and bottom.

Answer: The problem appears during typesetting so it is changed now.

- The units of the shown quantities should be mentioned/included in the plots or captions. (In the present manuscript the units are not even given in the text.) In addition, it should be mentioned in the plots or captions that zonal means are shown. (The information is only given in the text.)

Answer: The units are mentioned in the text but the plot show only regression response (signal). The information about zonal mean has been added to the captions.

- The font size of the labels should be enlarged.

Answer: We have enlarged the size of labels

Figure 6: The mid panel seems to be cut at the right border. (labels are not fully visible)

Answer: This problem has been eliminated.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 23891, 2014.

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