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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.

Anonymous Referee #1

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

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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.)

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?

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 (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

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what this finding could hint at.

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 arrive 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.

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

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

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 atmo-

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spheric 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.)

- What is the advantage of using reanalysis data compared to observations? What is the advantage of the MERRA reanalysis compared to other reanalyses? (Other reanalysis are also available for pressure levels up to 0.1hPa.)

- Why do the authors analyze meridional wind?

- 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).

- 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 seems to be a relevant issue in this context.

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

- 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 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.

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

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

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

- 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?

P 23892:

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

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

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).

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

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

P 23895:

L3: "Dobson-Brewer circulation" -> Why is this mentioned here and never appears again later in the article?

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

L17: The source of the EMI data is missing.

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

L20: The source of the volcanic data is missing.

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?

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.

L20: "Negative signal can be found..." -> From looking at the plot (Fig.2 bottom) this signal seems to be insignificant.

P 23897:

L3: "Surprisingly..." -> If this is surprising, why is it not discussed what this finding could imply?

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

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

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

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

P 23898:

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

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

Technical corrections:

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

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

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

- 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.)

- The font size of the labels should be enlarged.

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

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