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

Anonymous Referee #4

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General comments

The manuscript entitled "Solar cycle in current reanalyes: (non)linear attribution study" by A. Kuchar et al. addresses a currently debated issue, namely the influence of the 11-year solar cycle on the middle atmosphere. Thus, from a topical point of view it is of interest to ACP.

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., 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

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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.

The presentation and description of the results is very detailed on the one hand, maybe overly detailed given the short time series (35 years) analyzed. On the other hand there are a number of statements that are not really supported by the shown figures. This, together with the very small and difficult to read figures is a major deficiency of the manuscript. I would also strongly recommend to include a discussion of possible aliasing errors between the solar and the volcanic influence as well as a caveat concerning the general use of reanalysis data for 11-year solar cycle studies.

I recommend publication in ACP provided the manuscript is subjected to major revisions.

Specific comments

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).

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

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.

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 ..."

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

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 independent. I would recommend to compute the QBO factors from a Principal Component Analysis of equatorial, deasonalized zonal mean zonal wind anomalies.

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?

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

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.

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

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

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

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.

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 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.

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

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.

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.

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.

P30893,L9ff From the above explanations I cannot follow this conclusion. Please, clar-

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ify this in the text.

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.

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.

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

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.

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.

P30897, L12 to L14 At this point Sudden Stratospheric Warmings 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.

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

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

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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.

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.

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.

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.

Technical comments:

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 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?

A native English speaker needs to proofread this paper.

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"

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

linear regresion approach".

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 [gpm]?

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

P30887, L25f there is twice "in our case" in this sentence.

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

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

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