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Atmospheric Chemistry and Physics Discussions

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

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

The paper considers one of the more recent reanalysis data sets, generated by NASA, to look at signals in the stratosphere that are in line with ENSO, the QBO, volcanic eruptions, and the solar cycle. The chosen analysis tool is linear regression, and the authors use all available data. Unfortunately I believe the paper fails on a number of levels to bring anything meaningful to the scientific literature. As such I have to

recommend a rejection. My reasons are as follows:

1. The regression analysis does not seem to be applied properly. At least in the way it is described in the paper. The authors do not show all the regression terms, there is no long-term trend mentioned (i.e. to do with stratospheric cooling from ozone or greenhouse gases). There is likely cross correlation within the predictors. No mention of this is made with respect to the two ENSO terms. What's more, I do not even believe the volcanic regression is correct as they have plotted it. The volcanic signal is strong in MERRA, and highly significant in the lower stratosphere. As their regression does not show this, it makes me suspect to the rest of the analysis.

Answer: The reviewer is right about the response to the volcanic eruptions. We have found a problem with volcanic dataset. Now it is fixed and results agree better with the previous studies now. We have also added the long-term trend related to GHG increase. In the revised version all terms (excluding QBO 50, where is noted that the results are very similar to QBO 10, are shown. Of course that we can evaluate the predictors cross-correlation but as you can see from Figure 1 the correlation should not be a problem in our case. We have added a discussion about the cross correlation to the text.

2. Are the results adding anything new? I think not. All the authors do is perform regression on a different data set. Personally I do not think that repeating analyses just with a different data set warrants publication. I would urge the authors to try something different with this data set, and attempt to republish.

Answer: We understand the point, but cannot really agree on this. First of all, there were many publications analysing the results of regression on different data sets. For example, Crooks and Gray (2005) studied modes of variability in ERA-40 dataset and Frame and Gray (2010) applied the very same approach to the same data send extended to 2008. In general they confirmed previous results. In our paper we analyse variability modes in MERRA dataset, which differs in many aspects from other available

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reanalysis products. It is true that we applied well-known technic but we have added a new explanatory variable (EMI), which was not applied before. We have found out that the maximum temperature response to the solar variability occurs in the lower mesosphere in contrast to the results of ERA-40 data, which is also a new and important finding.

It seems that the main argument for using MERRA is that it provides a long time series, which is complete up to 0.1 hPa (this is wrong in the paper, MERRA actually provides data up to 0.02 hPa, just on model levels).

Answer: The MERRA reanalysis provide data up to 0.1 hPa (even it could be calculated up to 0.02 hPa). ERA-interim data are publically available up to 1 hPa. We think that analysis of levels above 1 hPa is very important for understanding the impact of for example solar irradiance on the atmosphere.

But this sort of regression has already been performed on ERA-40 and ERA-I, and the differences noted. In the papers that deal with the ERA products, the analysis is much more in depth (i.e. looking at seasons, individual months, step changes, etc).

Answer: The reviewer's statement is controversial. If there were analysis and comparison of ERA-40 and ERA-I and the differences were noted why the same should not be done for MERRA data, which differ from ERA-40 and ERA-I?

We think that analysis and comparison of the atmospheric variability modes extracted from the different reanalysis data sets is necessary and very instructive for many reasons. First of all, it allows establishing the robustness and the uncertainty level of the results. This is very important for the proper evaluation of the models. Secondly, it could help to evaluate the quality of any new reanalysis products.

The necessity of extensive intercomparison, validation, and evaluation of different reanalysis products has been recognized by the community end special SPARC Reanalysis Intercomparison Project (S-RIP) was initiated to address these problems (Fuji-

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wara M, Jackson D. 2013. SPARC reanalysis intercomparison project (S-RIP) planning meeting, 29 April – 1 May, 2013, Exeter, UK. SPARC Newsl. 41 52. http://www.sparc-climate.org/activities/reanalysis/.

It is possible to evaluate monthly or even 10-day mean but even the analysis of annual mean could reveal the basic problem and show where to go further. On the other hand we are not sure that the analysis of higher time frequency data is more instructive because the increase of noise level.

3. The literature, data set description, and regression description is not very in depth, and sometimes is wrong in places. It also feels very much like the authors have not thoroughly evaluated their data, and their analysis technique. The written language needs to be significantly tightened up, there is even a glaring mistake in the title.

Answer: The dataset section was redone and now it should be better explained. The paper was checked by native speaker.

I do not feel that more specific comments are helpful at this stage.

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