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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.

Anonymous Referee #2

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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

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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 green house 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.

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. 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). 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).

3. The literature, data set description, and regression description is not very in depth, and some times 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.

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

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