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Interactive comment

Interactive comment on "Relation Between the Interannual Variability in the Stratospheric Rossby Wave Forcing and Zonal Mean Fields Suggesting an Interhemispheric Link in the Stratosphere" *by* Yuki Matsushita et al.

Anonymous Referee #1

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Review of "Relation Between the Interannual Variability in the Stratospheric Rossby Wave Forcing and Zonal Mean Fields Suggesting an Interhemispheric Link in the Stratosphere" by Matsushita et al.

General comments

This study uses MERRA-2 reanalysis to investigate the interannual variability in the stratosphere during the austral solstice season. The primary result is that there is a correlation of the 3-month average EP flux divergence in the winter midlatitude strato-

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sphere with zonal mean fields in the tropics and summer hemisphere.

On the positive side, the paper shows that the seasonal mean anomalies of wave processes in the winter are correlated with anomalies in the zonal average dynamical fields in the tropics and low latitudes of both hemispheres. This type of pattern has been seen before in simple models of steady-state conditions and in a few analyses of observations, as cited in the manuscript. The new contribution of the present investigation focuses on the correlation patterns from a 38-year set of analyses. The authors are careful not to over-sell their results; since this is a study based on reanalysis, the ability to explore mechanisms is limited. However, they are able to link the signals they see to the expected results from a simple model.

The separation of the results into the period in which MERRA-2 includes or does not include assimilation of MLS is an important and useful component of the investigation.

While I do not have any major concerns about the analysis or about the explanations given by the authors, I wonder why the scope of the investigation was so narrow. Since all of the results use averages over the same 3-month portion of the year and only consider interannual variability of this a single time of year, this reader felt like the picture was incomplete. The comments below describe the questions I was left with after reading the manuscript.

Major comments

1. Why were 3-month averages used in the analysis? If shorter periods, such as individual months, were used, there would be a larger set of cases for investigating correlations. Did the authors probe the data to see whether correlation signals were stronger or weaker for averages over a subset of a season?

There is only a limited discussion of mechanism and I was unable to determine exactly what your interpretation is. The downward control mechanism that you refer to is valid for steady state (your three-month averages should be sufficient to satisfy this) but does not give a circulation that extends very far away from the region of the forcing (see any of the steady state figures in Haynes et al 1991). It is plausible that a dynamical mechanism would have a timescale shorter than the season-spanning three-month period and still affect the season-average circulation. It seems that you do not support this interpretation since you say (l. 170-171) that the pattern you see is not a result of the same processes that cause SSW.

Since your analysis is limited to a single season, separated by nine months that are ignored, timescales of up to one year would be consistent with the results. Is external forcing responsible? You show that solar cycle forcing, which had been proposed in earlier studies, is not consistent with their results. Another “external” variation with a long timescale is the QBO; this can have an impact on circulation in the low latitude stratosphere. Looking at periods covering subsets of the three-month average would give some information about whether the signal has a timescale shorter than a year.

2. Related to the above, the definition of winter as June, July, August appears arbitrary. Wave forcing in the southern winter is spread over a long period from May to November or December. Why were these months chosen?

3. The statement (l. 26-27) that “the Rossby wave forcing in the winter extratropics cannot directly drive the cross-equatorial flow” needs more explanation since the results suggest to me that the Rossby wave forcing is driving the flow. Do you mean that this can only happen in certain circumstances that depend on the presence of an angular momentum gradient? Since there will generally be a region in the tropics where the gradient of M disappears, you seem to be implying that this wave driven circulation is not possible. Also, the statement later on (l. 142) seems to be saying something quite different: that the Rossby wave forcing is necessary to drive a circulation if there is an angular momentum gradient but a flow can exist without wave forcing if there is no angular momentum gradient. Please clarify.

Likewise, I found support lacking for your conclusion that “The cross-equatorial residual

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mean flow is not directly driven by the Rossby wave forcing but indirectly maintained by the weak and small meridional gradient of the absolute angular momentum around the equator.” Isn’t it possible that the wave activity in the winter hemisphere, and the circulation response to it, is affecting M? Could you find cases where the wave forcing is high but M is large and vice versa? Otherwise, the relative impacts of these two processes cannot be separated.

4. It would be interesting to compare the opposite time of the year (northern winter) to determine whether a similar correlation exists then? Finding such a correlation would provide support that there is a physical mechanism rather than a chance correlation.

Minor comments

1. Some more description of the analysis is needed in Section 2. It took me a while to figure out that, when you discuss standard deviation, you mean only the standard deviation of fields that have already been averaged for the three months. The analysis for the wave amplitude is not clear – did you average daily amplitudes or daily Z’?

2. (l. 104) This sentence is not clear; Figure 1b does not show wave forcing. In fact, I wasn’t sure what you are referring to in this entire paragraph. As far as I can tell, you have used the term “wave forcing” to mean something very specific (EP flux divergence in Area A) but it does not fit with the usage here.

3. (l. 113 ff) “This indicates that the wave forcing in the SH affects the mean fields in the low latitude region of the NH”. Be careful here. Correlation does not mean causation. You need more evidence to say that one timeseries is affecting the other, rather than vice versa or both responding to some other forcing.

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