

# ***Interactive comment on “Residual Mean Circulation and Temperature Changes during the Evolution of Stratospheric Sudden Warming Revealed in MERRA” by Byeong-Gwon Song and Hye-Yeong Chun***

## **Anonymous Referee #3**

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This study uses a modern reanalysis (MERRA) to compute and composite forcing terms in the transformed Eulerian mean (TEM) zonal wind and thermodynamic equations about sudden stratospheric warmings (SSWs). The authors separate the SSWs into Type-1 and Type-2 events based on relative sizes of wave-1 and wave-2 polar geopotential height anomalies. The composites demonstrate that the planetary-scale wave activity flux is the dominant forcing term in both the TEM zonal wind and thermodynamic equations. Uppermost stratospheric gravity wave drag and middle stratospheric diabatic heating are meanwhile shown to be small, though non-negligible. I believe the authors present a clean analysis that stays on point with the paper's theme.

There are however a few points that I'd like the authors to edit or address to boost the quality of the manuscript.

Principally, I'm not sure what the added value of separating the events into two types is. While comparison of the two types seems to be a large portion of the analysis, there is not much discussion on the implications of these results. Events are often separated in studies of SSWs, but the reasons need to be made clear. I don't believe the authors have amply done this in the introduction or summary. I think a more thorough discussion of why the authors did what they did and how it fits into the literature will greatly aid the manuscript.

There are also a few analysis steps by which the authors could address this problem. Firstly, the authors could show an 'all SSW' composite for each part of the analysis. In this way, the manuscript would analyze the residual mean circulation from MERRA in all SSWs and concurrently show the results for one way that SSWs are separated.

Secondly, the authors could (and I believe should) show significance of the anomalies for each event type. While the significant difference between Type-1 and Type-2 is important, so too is their own significant difference from zero. Especially given the small sample size, this will better inform the reader as to which composite structures agree with each other.

Given the scope and work required for these changes, I recommend that the manuscript be returned for major revisions.

Minor comments:

Page 2, line 23: I think you should state that it has not been done with the generalized downward control principle.

Page 2, line 41: how is the climatology calculated? This will be important information if others wish to reproduce or adapt your results.

On reproducibility, thank you for including a table of SSW dates. This step is often

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overlooked for SSW studies.

Page 5, line 42: what is the reason you average the forcing over a different latitude band than the residual forcing terms?

Page 7, line 19: though the amplitude may be small, the residual term has a broad region of significant difference. Do the authors have any insight as to why that may be?

Page 9, line 21: model level data from ERA-Interim is used, but Table 1 indicates pressure-level data is used (i.e., shouldn't ERA-Interim have 60 levels?).

On the figures: since so many panels are included in each figure, the panels will be quite small when published. This will make seeing the small regions of significance hard to see. I'm not sure the best way to do this, but the authors may consider altering their figures to better show hatched regions. This is especially true over regions with dark blue contour filling.

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[Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-729, 2016.](#)

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