Response to Reviewer #1's Comments

Thank you for providing valuable comments that improve the original manuscript. We tried our best to improve the manuscript based on your suggestions.

Major comments:

This paper examines SSW in several reanalysis datasets and reaches two main conclusions:

1. That the various datasets yield pretty much identical identifications of SSW, as well as their classification into type-1 and type-2 SSW;

2. That the EP flux divergence makes the most important contribution to w at high latitudes as determined from a generalized version of "downward control"; and*

2a. That the resulting warming of the polar cap occurs due to "advection" [actually adiabatic warming] by the induced TEM circulation.

Neither of these findings is surprising and the second is definitely not new. One can find a similar description in Andrews et al (1987, and references therein). Some discussion of the role of gravity wave drag is presented, but found to be small. There is a weak attempt to relate the findings to the work of Albers and Birner (JAS, 2014) but it is unconvincing. All in all, there appears to be no change in this version of the paper compared to the version submitted for preliminary review. Therefore, I am afraid I still cannot recommend the paper for publication in ACP.

→ As we know, this is the first study that examines the contribution of each wave forcing to the temperature change during the evolution of SSWs, based on the generalized downward control principle.

During the revision process, the relative magnitude of GWD to total wave forcing (EPD+GWD) is calculated and a new result is included as Fig. 4 in the revised manuscript. It is found that GWD/(EPD+GWD) averaged in 60° N–70° N is up to 90% in the upper stratosphere before warming, especially for the Type-2 cases. After Lag = 0, there are several heights and times of which GWD/(EPD+GWD) is more than 50% in the whole stratosphere for both cases. This implies that contribution of GWD to SSW is rather large locally. [Page 6, line 28–33]

Minor comments:

1) (1,32) "waves are broken": "waves break" would be preferred. That aside, wave breaking need not be the only dissipation mechanism in the case of planetary Rossby waves, whose group velocity is slow enough that they can be affected by thermal dissipation. In fact, Matsuno (1971) never discussed wave breaking; that concept came much later, with McIntyre and Palmer's paper in Nature (1983).

→ Thank you for pointing out this. We modify the sentence to "critical layer interaction", which was presented in the Matsuno (1971). [Page 1, line 33–34]

2) *(3,12) Section 3: The material on the downward control (DC) definitions of v*, w* logically should follow what is now section 3.2, since the DC equations are derived from the TEM set presented in 3.2. In addition, Eqs. (1)-(4) should be moved after Eq. (9) in what is now Sec. 3.2, since they are definitions of terms in that equation. Section 3 should be reorganized, such that the present Section 3.2 becomes Section 3.1 and the material on DC becomes a short Section 3.2.*

   → We agree with you, and it is reorganized, as suggested. [Page 3, line 16–page 4, line 27]

3) *(5,7) "For Type-1 (composite mean)…": It should be mentioned here that the composites are shown on the two lower, RHS panels of Fig. 1.*

   → We already mentioned this in the original manuscript. [Page 5, line 3] [Page 5, line 15 of the revised manuscript]

4) *(5,39) "Figure 3 shows…": Here and in the figure caption you need to note that what is shown are composites for Type 1 and Type 2 (and their difference).*

   → It is modified in the revised manuscript, as suggested. [Page 6, line 9–13 and page 20, line 1]

5) *(6,16) "A large proportion": From Fig. 3, GWD is -2 to -5 m/s, but EPFD is -10 to -20 m/s, so GWD is at most 20-25% of EPFD, and that only near 1 hPa. I would not consider this a "large" fraction. Perhaps you should just state the numbers and let the reader decide.*

   → To quantify the fraction of GWD, percentages of GWD to the total wave forcing (EPD+GWD) for Type-1 and Type-2 SSWs are calculated during the revision process and shown in a new figure (Fig. 4) of the revised manuscript. From Lag = –15 to –10, GWD contributes more than 20% of the total wave forcing above 10 hPa in both types of SSWs, and high percentage (> 70%) is also observed, especially for Type 2. This is consistent with Fig. 2 of Albers and Birner (2014) which shows that the percentage of GWD to the total wave forcing above 10 hPa in the 60° N–70° N is about 20–80% at Lag –30 to –5. [Page 6, line 28–33]

6) *(6,40) "The GWD forcing anomaly has different structures…": I don't quite see this. If one compares 3b and 3d one sees similar behavior: Not much contribution before the key date (lag=0) and a positive contribution after the key date. What is remarkable here is how different this looks compared to the original composites (without subtracting climatology). One interpretation of this is that GWD does not differ much from its climatological value before the key date, but after the key date GWD is suppressed, such that the difference from climatology is positive. This behavior is consistent with the idea that the reversal of the wind inhibits GW propagation and thus reduces the GWD.*

   → As we mentioned in the original manuscript, the difference between the GWD anomaly for two types of SSW is particularly large at Lag = –15 in the upper stratosphere, where statistical significant positive (Type-1) and negative (Type-2) anomalies exist. As shown in Fig. 3e, the difference between the two types of anomaly fields also significantly large. Therefore, it can be said that GWD anomaly for two types of SSW has different structures. When the lag time extends to ±25 days from the original ±15 days, we found (Fig. A1) significant negative GWD anomalies in the upper stratosphere between Lag = –25 days and –15 days exclusively for Type-2. Because it is hard to see the contours and arrows clearly, if we extend the time series, we decided to keep the current lag time (Lag = ±15 days).

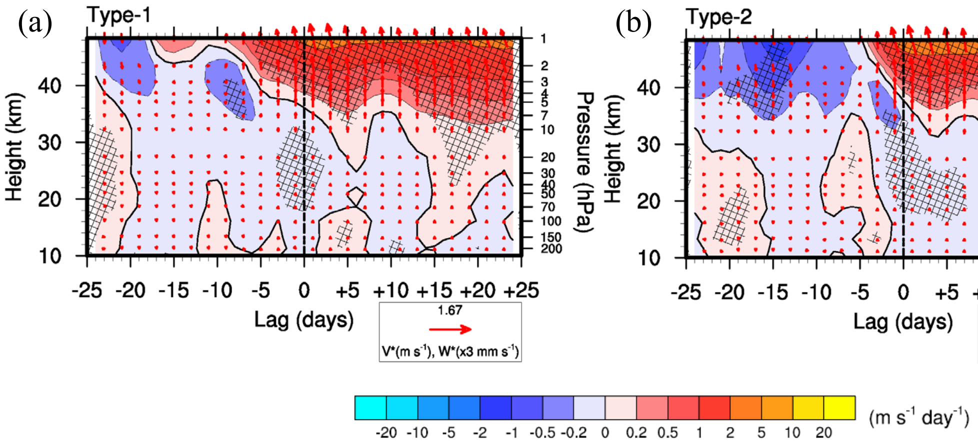

Figure A1. Time-height cross section of GWD anomalies (shading) averaged over 60° N–70° N and the residual mean velocity anomalies (red arrow) averaged over 70° N–80° N, induced by GWD for (a) Type-1 and (b) Type-2 SSW events. The hatch patterns denote statistical significance at 90% confidence level.

7) *(7,13) "Although the magnitude…": This is stated without proof and is unconvincing. It is not at all clear from what is shown here that the small forcing due to GWD is important for the generation of SSW. One could equally argue that GWD is responding to the underlying zonal-mean zonal wind, which has been modified due to other causes.*

→ We agree with the reviewer's criticism, and delete this statement in the revised manuscript. Although we are currently working on the indirect effect of GWs on SSW, it is not proper to state in the current manuscript without concrete results.

8) *(7,42) "temperature advection": This is contributed mainly by w*S (S = kappa T/H), so it is actually adiabatic warming or cooling due to vertical motion. As is well known, this is the principal mechanism whereby a sudden warming warms the polar stratosphere.*

→ Yes, it is correct. The adiabatic warming/cooling rate term is dominant among the terms included in the "temperature advection" term in the original manuscript. To avoid any confusion, "temperature advection" is changed to "adiabatic heating rate" in the revised manuscript.

9) *(8,13) "adiabatic heating": I believe you mean "diabatic" (third column of Figure 6). If so, note that this is really a response to the temperature change brought about by dynamics (adiabatic effects–what you call "advection"). It is not a driver of the SSW.*

→ Thank you for pointing out this typo error. Yes, it is diabatic heating. As we know, the anomalous diabatic heating is a forcing to determine the temperature change, and it is a forcing to derive zonal-mean zonal wind tendency and residual circulation [see Eq. (3.5.7) and (3.5.8) of Andrew et al. (1987)]. Figures 9 and 10 show that diabatic heating has a similar structure (cooling in poleward of 60° N) before and after the warming, while its contribution to the temperature change differs as the adiabatic heating changes dramatically before and after the warming. It is not simply a response to the temperature change accompany the sudden warming. If our understanding is not correct, please let us know. We will make a further revision regarding this part latter.

10) (9,16) "To summarize": This is the main finding of the paper, but it is neither new nor surprising. And, once again, "anomalous cooling" is a response (IR relaxation) to the temperature changes that accompany the sudden warming.

→ Many part of the results, such as dominancy of EPD forcing in TEM equation during SSWs, are similar to the previous studies, which cannot be different as long as our analyses are correct. As mentioned earlier, however, this is the first study on the contribution of each wave forcing to the temperature change during the evolution of SSW, based on the generalized downward control principle. In addition, as answered to the previous comment (#9), we do not think that the diabatic heating considered here is the response of SSW but the forcing to contribute to temperature change during the evolution of SSW.

11) (9,24) "results … not specific to just one data set": This is useful to know but not particularly surprising insofar as all of the reanalyses ultimately rely on the same observational data.

→ Yes, it is correct that recent reanalysis data sets can produce similar structure of the large-scale wind and temperature fields, in general, based on similar observational data. However, it is worth to check the robustness of the current results based on MERRA with different data sets, mainly because small differences in wind and temperature can induce significant differences in wave forcing terms and residual circulations induced by the wave forcing, which are calculated from high-order derivatives. This point is included in the revised manuscript. [Page 9, line 41–43]

12) (10,3) "EPD is the most significant contribution…": Again, this is hardly news.

→ Again, the current result is consistent with previous ones, although we used GWD information as well, which is not commonly used in previous studies. The magnitude of GWD provided from MERRA is not larger than EPD. However, as shown in the new figure (Fig. 4), the ratio of GWD to total wave forcing (EPD+GWD) is up to 90% at some heights and times before the warming, especially for the Type-2 SSWs, and this implies that GWD is a non-negligible forcing for driving for SSWs. The statement is modified based on the new figure.

Reference:

Albers, J. R., and Birner, T.: Vortex preconditioning due to planetary and gravity waves prior to sudden stratospheric warmings. J. Atmos. Sci., 71, 4028–4054, doi: 10.1175/JAS-D-14-0026.1, 2014.

Andrews, D. G., Holton, J. R. and Leovy, C. B.: Middle Atmosphere Dynamics. Academic Press, 489 pp, 1987.

Matsuno, T.: A dynamical model of stratospheric warmings. J. Atmos. Sci., 28, 1479–1494, doi: 10.1175/1520-0469(1971)028<1479:ADMOTS>2.0.CO;2, 1971.