Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-729-RC1, 2016

Atmospheric Chemistry and Physics Discussions
Open Access
EGU

ACPD

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 #1

Received and published: 9 December 2016

Referee report on "Residual Mean Circulation and Temperature Changes during the Evolution of Stratospheric Sudden Warming Revealed in MERRA"

by B.-G. Song and H.-Y Chun

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;

Printer-friendly version

Discussion paper

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.

Specific comments (page and line number):

(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).

(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.

(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.

(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).

(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.

(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.

(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.

(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.

(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.

(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.

(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.

(10.,3) “EPD is the most significant contribution. . .”: Again, this is hardly news.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-729, 2016.