The goal of this paper is to improve understanding of the global stratospheric diabatic circulation through isentropes (M) as a metric for the Brewer-Dobson Circulation, by making comparisons to other more commonly-used metrics, including derived tropical upwelling and circulations estimated from water vapor and ozone. The calculation and use of M has certain theoretical advantages to diagnose stratospheric transport in an integrated manner, and it is a good idea to make systematic comparisons to other BDC metrics that are commonly used in the research community. These comparisons can pave the wave for more widespread use of M as a diagnostic tool, as proposed in this paper. I especially like the combination of analyzing reanalysis data sets in tandem with results from a self-consistent chemistry-climate model. This paper will make a valuable contribution and the topic is appropriate for ACP.

While I strongly endorse the goals of the paper, I have a few major comments on the current content, where I think the paper could be improved prior to publication:

1) The current paper focuses on interannual variability in all of the circulation diagnostics. While this is certainly interesting, I suggest also including comparisons of the actual seasonal cycles in various quantities (climatological monthly time series at a few different theta/pressure levels), which can then serve as a context and background for evaluating interannual variability. In order to enhance the understanding of M, it might be useful to include some simple, approximate conversions of the mass flux to equivalent upwelling velocity, to facilitate direct comparisons to the various estimates of tropical upwelling (w^*, w^*_m, w^*_Q, w_{TR}). I expect there will be reasonable overall agreement (with, e.g., a large annual cycle in the lower stratosphere).

2) Most of the interannual variability in the results is obviously due to the quasi-biennial oscillation (QBO); this is clearly seen in the time series in Figs. 5 and 9, and the ozone results in Figs. 7-8. This understanding should be folded into the discussions on comparing the behavior of M and various circulation statistics. For example, the vertical out-of-phase behavior between the lower and upper stratosphere is closely tied to the QBO vertical structure. The patterns of ozone variability (out-of-phase in altitude and latitude) and coupling to meridional circulation are well-known aspects of the QBO signal in ozone (e.g. Bowman, 1989, JAS; Zawodny and McCormick, 1991, GRL; Chipperfield et al, 1994, GRL; Randel et al, 1999, JAS; Tian et al, 2006, JGR). Also, the in-phase vs. out-of-phase ozone-temperature relationships in the lower and upper stratosphere, respectively, are a well-known general result tied to transport and photochemistry. While the M comparisons with the various tropical upwelling estimates were novel and interesting to me, I found the results on ozone (Section 6) to be less valuable for evaluating M as a circulation diagnostic (more of a consistency check with previous results).

Minor comments:

1) In addition to the auto- and cross-correlation diagnostics (Figs. 1-3), it would be valuable to explicitly compare time series of the interannual anomalies in all of the circulation diagnostics, like those included for M and w_{TR} in Fig. 5 (perhaps for time series in the lower and upper stratosphere). This very much helps the reader understand the variability that is

quantified in the correlation diagnostics (and provides a ‘feel’ for the variability among the different diagnostics). Are these comparisons sensitive to the choice of latitude band for the various w quantities?

2) P. 5, line 32: you might include a reference to Abalos et al, 2017, JAS, in regards to trend sensitivity to a tropopause-based vertical coordinate.
3) It would be good to add a few contour labels to the panels in Figs. 1-3 and 6.
4) P.8, lines 25-28: the ‘downward control’ calculations integrate the wave driving multiplied by density, so that in practice the forcing is usually dominated by nearby levels in the vertical (not the entire depth of the stratosphere).
5) You might note that w^*_Q calculations near the tropopause have an uncertainty in the calculations due to neglect of eddy transport terms (Abalos et al, 2012, ACP). Is this what is meant by ‘complications with convergence’ (p. 20, line 19)?
6) The dashed lines relating potential temperature and pressure levels in Figs. 3 and 6 are calculated for an ideal gas, and I guess you mean an isothermal ideal gas. Why not just use a relationship derived from climatological mean values, including realistic temperature structure?
7) I was surprised to see no significant trends in the WACCM diabatic circulation in Fig. 4, given that many models (including WACCM) show small positive trends in tropical upwelling (e.g. Garcia and Randel, 2008, JAS). What do trends in the various WACCM w* quantities look like? If these are different from the WACCM M trends, why is that? Is the QBO variability accounted for in these trend calculations?
8) I do not understand the overlapping 3-level correlation calculations used to derive w_{TR} from the WACCM water vapor fields. Why is such a complicated calculation necessary? How sensitive are the results to different methods of calculation? How does the background annual cycle of w_{TR} compare with the other upwelling estimates (see major comment 1 above).
9) P. 18, line 4: variations in ozone and (potential) temperature are positively correlated in the lower stratosphere because of similarly signed vertical gradients (and long ozone lifetimes), not because of ozone production.
10) P. 20, line 17 and 19: do you mean w^*_Q instead of w*?
11) P. 20, lines 19-30: as noted above, the vertical anti-correlation of the interannual circulation diagnosed here is mainly attributable to the QBO vertical structure (linked to tropical wave dynamics and mean flow interactions). This important aspect should be incorporated into the interpretation and summary discussions of vertical structure.