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Interactive comment on "The Climatology of Brewer-Dobson Circulation and the Contribution of Gravity Waves" *by* Kaoru Sato and Soichiro Hirano

P. Šácha

o.o.d.sachta@seznam.cz

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Dear authors,

Thank you very much for an interesting study. This comment is mainly motivated by the relationship of your analysis with the compensation mechanism (not only Cohen et al. 2013, but also Cohen et al., 2014 and Sigmond and Shepherd, 2014).

Your argument against the compensation mechanism in reanalyses (P2L30) appears to be speculative and it does not take into account various processes that can stand behind the compensation. For example, Haynes et al. (1991) noted that the DC prin-

ciple applies to the zonally symmetric forcing, as the longitude-dependent force could set-up a Rossby wave field. This was demonstrated in a modeling study by Šácha et al. (2016) together with the effect of different zonal distribution of forcing on the residual circulation. There is an inherent zonal asymmetry in the gravity wave drag distribution (concentration into hotspots -e.g. Hoffmann et al., 2013; Šácha et al., 2015), which is reflected also in the parameterizations (at least orographic GW parameterizations, Šácha et al., 2018).

The compensation hardly makes it possible to clearly separate the effects of resolved and unresolved waves. This is an important point and we think that it has to be properly discussed in your paper.

We also report on a typo (P15L16), where you probably wanted to relate the boundary condition (wstar = 0) to the turn-around latitudes.

Besides that, we have general doubts regarding the conclusions of Appendix A, as we can see similar inequality between the wstar and vstar based method for residual mean streamfunction computation in a model reaching up to 150 km (i.e. including the wave forcing in the mesosphere, not published yet). We would recommend checking if the net tropical upwelling across a particular level inferred from wstar integration nears zero.

Best regards,

Petr Šácha (sacha@uvigo.es) and Petr Pišoft.

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