Comments on "The climatology of Brewer-Dobson circulation and the contribution of gravity waves" by Sato and Hirano

This study presents seasonal variations of Brewer-Dobson circulation in terms of stream function and upward mass flux for 30 years (1986-2015), using four recent reanalysis data sets (MERRA, MERRA2, ERA-Interim, and JRA55). Special emphasis is given to the contribution of gravity waves on the stream function and upward mass flux in the whole stratosphere, and their seasonal and height dependency. Compared with the original manuscript, the current version is improved by including additional discussions on the logistics of the authors' approach and several new figures. Nevertheless, some major questions raised from the reviewer still remain in the current manuscript and some statements, especially related to the limitations in GWD parameterization in GCMs and their conjunction to the assimilation increment, are highly problematic, which could be misleading readers. Therefore, the review would like for the authors to address following comments properly before the current manuscript to be accepted to ACP.

Major Comments:

1. Logistics in Ψ_{GW} calculation

In the present study, Ψ_{GW} is calculated using Eq. (11). As the reviewer understand, first the stream function Ψ_{dir} is calculated using Eqs. (3)-(5), which is conventionally called as a direct stream function. Then, based on the downward-control principle, stream function by planetary waves (Ψ_{DC_RW}) and zonal-mean zonal wind tendency ($\Psi_{DC\_du/dt}$) is calculated. Then, Ψ_{GW} is estimated as a difference between Ψ_{dir} and sum of Ψ_{DC_RW} and $\Psi_{DC\_du/dt}$. The reviewer still cannot understand why this rather odd approach is needed, which may include some additional uncertainties.

1) Although the stream function Ψ_{dir} and Ψ_{DC} should be equal theoretically, it is not exactly the same, as shown from many previous studies, likely because the governing equations used and physical processes in the GCM of each reanalysis data set are somehow different from rather simple TEM equation.

2) If we agree with that Ψ_{dir} and Ψ_{DC} are exactly the same, as the authors assumed, then Ψ_{GW} estimated based on Eq. (11) is the same as Ψ_{DC_GWF} using Eq. (8), if $\overline{GWF} + \overline{X}$ is considered as GW forcing. However, in Line 10-11 of Page 7 of the manuscript, the authors mention that "Ψ_{GW} *cannot be directly calculated because of unknown* $\overline{GWF}$." This statement makes the reviewer be confused. Note that when

Ψ_{GW} is estimated using Eq. (11), TEM equation is no need for being used. This is not a matter of whether GW forcing is represented by either parameterized GWF provided from the reanalysis data or the residual of the first four terms in the TEM equation ($\overline{GWF} + \overline{X}$). The Ψ_{GW} is better to be calculated using the TEM equation to assure the momentum balance.

2. Sources of $\overline{X}$

1) One of main assumptions of the current study is that the grid-resolved planetary waves and zonal-mean zonal wind tendency are accurate by assimilation process (Abstract: Line 11-12), and $\overline{X}$ represents assimilation increment due to the limitation in GW parameterization. This is too optimistic, given that there are many factors for the resolved meteorological variables to be biased from the observed variables at each time step, which is represented by assimilation increment from the reanalysis data. As shown in the current results, there are quite significant differences in the planetary wave forcing and resultant stream function among the reanalyses used. It may be almost impossible to directly separate out the resolved part and parameterization part of the assimilation increment.

2) Accordingly, some statements regarding this issue should be modified.

A. Line 12-13, Page 16: "*The difference between the upper and lower panels suggests the deficiency of the GW parameterizations*"
B. Line 13-15, Page 16: "*It is encouraging that similarity among the four reanalyses is higher for Ψ_{GW} than for Ψ_{pGW}. … This suggests that current assimilation schemes act to make the GW contribution to the stream function realistic*". The relatively similar Ψ_{GW} among the reanalyses than Ψ_{pGW} is due to the assimilation increment in general, to make the model results to be better compared with the observation, not necessarily for fix the parameterized GWD.
C. Line 26-27, Page 16: "*This result suggests that net non-orographic GW forcing is more strongly eastward in the real atmosphere than given by parameterizations*"

3) There is one paper that may need to be included in the current manuscript, which is similar objective, although using a single reanalysis data set (MERRA), which calculated stream function by EPD, $\overline{GWF}$, and $\overline{X}$ separately (Kim et al. 2014).

Interestingly, the stream function by $\overline{X}$ is larger than that by $\overline{GWF}$, but the mass flux (Fig. 6 of Kim et al. 2014) that is calculated using the stream function at turn-around latitude is smaller than that by $\overline{GWF}$.

3. Contribution of GWs to the mass flux

In the present study, the contribution of GWs to the mass flux is up to 40%, although it is different from each reanalysis data set, as mentioned at Line 22-24, Page 14: "*the GWs to the mass flux is ~20% at 70 hPa for MERRA and MERRA-2 at the most, while it is ~35–40% for ERA-Interim and JRA-55*". It should be noted that most GCMs already overestimate parameterized GWF through the tuning process, in order to compensate extremely underestimated planetary wave forcing in the model (Geller et al. 2013; Kim and Chun 2015; Kang et al. 2018). Therefore, contribution of GWs to the mass flux, which is calculated using $\overline{GWF} + \overline{X}$ in the present study might be strongly overestimated.

4. Limitation in GW parameterization

The authors estimate GW forcing by $\overline{GWF} + \overline{X}$ in the TEM equation, although their calculation method is not directly from TEM, as mentioned in the comment #1. As the reviewer understand, the major reason for not using $\overline{GWF}$, which could be provided from reanalysis data sets, as the GW forcing is likely due to that the authors consider that there is a significant limitation in GW parameterization used in GCMs. The limitation of GW parameterization is likely based on some previous studies using satellite data, such as Geller et al. (2013), where GW momentum flux estimated from HIRDLS is compared with GW parameterization from GCMs (and some resolved from relatively high-resolution GCMs). This comparison, however, is not very meaningful, because satellite observed GWs with horizontal wavelengths about 500-1000 km (Kalish et al. 2016) and parameterized subgrid-scale GWs with horizontal wavelength mostly less than 100 km are in significantly different scales. This has been discussed in depth in recent work by Kang et al. (2018). It is true that some non-orographic GW parameterizations, especially for those not physically formulated and source-dependent, have high degree of uncertainties in the tuning process, mostly with too strong source magnitude in order to compensate highly underestimated planetary wave forcing (Kim and Chun 2015). The real problem in the GW parameterization is in fact that it is strongly overestimated, rather than underestimated. Therefore, the stream function and resultant mass flux in the present study that are based on $\overline{GWF} + \overline{X}$ are even more overestimated compared with real GW contribution.

5. Sensitivity of mass flux to the turn-around latitude

In the mass flux calculation, the results should be very sensitive to the choice of turn-around latitude, which is different for the stream function derived from different wave forcings (Figs. 3-5), although the turn-around latitude from Ψ_{dir} is used for the calculation shown in Figs. (10-12). The sensitivity of the mass flux calculation to the turn-around altitude should be included.

Minor comments:

1) Line 1-2, Page 16: GWs in the convective region has a clear annual variation, which is shown recently by Kang et al. (2017, JAS). Accordingly, the statement should be modified.
2) Line 26-27, Page 17 (Fig. 15): The way to calculate Ψ_{INC} needs to be explained.
3) Line 30-31, Page 17: "*These features are consistent with the difference between Ψ_{pGW} and Ψ_{GW}, and likely indicate a shortage of eastward GW forcing at the low latitudes*". The first part of the statement is correct, because your Ψ_{GW} is simply the sum of Ψ_{pGW} and Ψ_{INC} (by $\overline{GWF} + \overline{X}$), but the last part of the statement is not correct, again, given that the assimilation increment is not solely by GW parameterization but include all part of model deficiency.
4) Line 3-4, Page 17: "*The maximum around 60°S in Ψ_{GW} is consistent with observations and GW-resolving GCM simulations (Sato et al., 2009; Geller et al, 2013).*" The GWs considered in the TEM equation is the small-scale GWs that are not resolved from GCMs but are parameterized. The GWs estimated from satellite observations have horizontal wavelengths longer than 500 km, and those calculated from high resolution GCMs with horizontal grid spacing of 0.25^{o} x 0.25^{o} are longer than 200 km. If these relatively long GWs observed from satellite and resolved from GCMs are matched with the Ψ_{GW}, this is more likely related to Ψ_{INC} causes by grid-scale GWs rather than small-scale parameterized GWs.
5) Figure 10: The reason not to calculate JJ in JRA-55 is better to be included in the figure caption.

References

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