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Comment on acp-2021-190

Anonymous Referee #1

Referee comment on "The semiannual oscillation (SAO) in the tropical middle atmosphere and its gravity wave driving in reanalyses and satellite observations" by Manfred Ern et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-190-RC1, 2021

Review of manuscript: “The semiannual oscillation (SAO) in the tropical middle atmosphere and its gravity wave driving in reanalyses and satellite observations”, by Manfred Ern, Mohamadou Diallo, Peter Preusse, Martin G. Mlynczak, Michael J. Schwartz, Qian Wu 4 , and Martin Riese.

This article investigates the gravity wave (GW) driving of the semiannual oscillation in the equatorial middle atmosphere. Different estimations of both the SAO and GW drag from reanalysis and satellite observations are presented. The results (particularly those derived from satellite data) show the relation between the wind oscillation and the GW driving, and even give a quantification of this wave driving (lines 875-879).

The paper is clearly written and the results are discussed in depth. However, I have some major concerns about the way the GW drag is estimated that would need to be addressed before recommending publication.

Comments:

1) There are a lot of figures, all of them with multiple panels. This makes it difficult to compare panels from different figures. Since a good part of the panels are barely discussed in the text, it seems unnecessary to show all those time evolutions.

I suggest to combine the mean annual cycle of the different variables of Figs. 3-16 into single figures. For example, the multiyear mean SAO extracted from the reanalysis and SPARC (Figs. 3-6) could be combined into a single figure. This would not require to substantially modify the text, and would definitely ease the reanalysis intercomparison.

A similar thing could be done with the figures showing the estimations of GW drag from the reanalyses (Figs. 7-10), the different approaches to estimate the SAO from satellite observations (Figs. 10-11), and the estimations of GW absolute momentum flux, its vertical derivative and the wind shear (Figs. 12-16).

If necessary, the time .evolution of all these estimations could be included as supplemental material.

I would also suggest to rearrange the sections (and figures) so the different estimations of the SAO-realated cycle are presented first (reanalysis and observations), and then the GW drag. In my opinion, this would avoid going back and forth with the results and the comparisons among different data sets.

2) GW drag from reanalyses.

The authors combine the EP flux divergence from resolved waves with high horizontal wavenumber (k>20), the parameterized GW drag and the residual term from the zonal-mean momentum equation to provide an estimation of the GW drag from the reanalyses (eq. 3). There is no question about the first two terms (resolved and parameterized waves), beyond the somewhat subjective choice of k > 20 for waves to be considered as GWs. However, there is a fundamental problem with the third term (the residual) that the authors already identify in the text (lines 261-265): the validity of interpreting the residual of the zonal-mean momentum balance as a GW drag is necessarily tied to having realistic analyzed winds. If this is the case, the residual should mainly arise from the assimilation of observations, and one can argue that this is due to the effects of unresolved GWs that parameterizations fail to reproduce. But if the winds are not entirely realistic, as it would seem from Figs. 3-6, the residual term is not (mainly) representing the correctional force by the observations on the forecast fields, and I believe its interpretation as a "missing" GW drag is not justified.

It would be useful to show the mean annual cycle of the different terms of the GW drag calculations (i.e. resolved, parameterized and the residual), to better understand what the resolved and parametrized GWs are doing in each reanalysis. But given the differences between the SAO signal in reanalyses and the SPARC climatology, it seems difficult to believe the "total" GW drag estimations.

3) Absolute momentum flux and GW drag derived from satellite data.

In section 6.1, it would be useful to discuss in more detail the limitations of the momentum flux calculations from SABER. For example, if horizontal wavelengths are mainly directed zonally as it it is argued, then the trajectory of the satellite is basically perpendicular to the wavenumber vector and the errors in the estimation of the momentum fluxes are considerable.

If I understand correctly, the GW drag is a force per unit mass, or a torque per unit mass. It is therefore a vector (calculated as the divergence of the momentum flux vector), and the absolute GW drag should be the absolute value of that vector. This is not what the authors call "absolute GW drag" in the manuscript, but the vertical derivative of the absolute momentum flux. While I understand that this quantity may approximate the value of the absolute drag under certain conditions (which should be more directly discussed in the text), I do not consider it appropriate to systematically call it "absolute GW drag". Among other reasons, because it gives the impression that GW drag can be derived directly from these observations.

Minor comments:

- lines 578-585. Could there be an effect of GWs with zonal momentum flux being very few at these high altitudes due to critical level filtering by the QBO and the SSAO?

- lines 595. Normalized SABER GW drag is the same as GW drag anomalies in Fig. 14?

- Fig. 17, line 598. Why not showing the correlation over the whole period of study, instead of the correlation of the multiyear mean annual cycle? What would be the difference between the two, and its interpretation?

- Lines 734-743. What is the explanation of a high correlation between the so-called (SABER) absolute GW drag (see main comment #3) and the zonal wind speed, if saturation of GWs due to decrease in density is the proposed mechanism?

In the same spirit, how can ERA-5 have a realistic GW driving of the SAO if the SAO in ERA-5 is not realistic (section 8.4)?

- Lines 771-773. I do not understand what the authors mean here. Since MERRA-2 assimilates MLS observations, the driving of the SAO in MERRA-2 at 45-70km is likely the result of this process. But this does not mean that the GW driving of the SAO in MERRA-2 is realistic.

Technical comments:

- line 99: Agency ☐ Administration (NASA)

- line 429: effictively ☐ effectively

- line 489 (and elsewhere): gradients ☐ divergence (or derivative)?

- line 558: 100 ms-1 ☐ ms-1d-1 (?)