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Comment on egusphere-2022-474

Anonymous Referee #2

Referee comment on "On the impact of Himalaya-induced gravity waves on the polar vortex, Rossby wave activity and ozone in CMAM30-SD" by Ales Kuchar et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-474-RC2, 2022

This study investigates flow changes in the boreal winter stratosphere following parameterized orographic gravity wave drag (OGWD) events over the Himalayas at 70 hPa, using a 30-year simulation of a chemistry climate model where temperature and wind have been nudged to ERA-Interim fields. Based on composites of different values around OGWD events, it is claimed that these OGWD events alter the propagation of planetary waves in the lower stratosphere and the morphology of vortex, preconditioning the vortex for a subsequent breakdown. It is also claimed that the alteration in the planetary waves have impacts on planetary wave breaking and large-scale stirring in the lower stratosphere, affecting the concentration of high latitude ozone.

I find that the figures shown do not allow a proper analysis of the problem, as it is explained in my comments.

Major comments:

1) The methodology of compositing around OGWD events over the Himalayas does not necessarily imply that these events are the cause of the circulation alterations shown on a global scale. The argument given by the authors is that the potential connection between increased OGWD over the Himalayas and vortex alterations is mediated by changes in planetary wave propagation and dissipation. Figure 4 is supposed to prove this point, but there are several reasons why this is not achieved: 1) the EP flux vectors are not well scaled and they are dominated by the horizontal component, so it is not possible to analyze the anomalies properly. Please see Edmon et al (1980) and/or Jucker (2020) to scale the vectors correctly. 2) Only positive lags are shown. This is an important point, since it would be perfectly possible that the parameterized GW events are the result of large-scale changes of the background flow and/or planetary waves. So claiming that EP flux anomalies at lag 0 are the result of simultaneous OGWD events over the Himalayas, without any further analysis, is unconvincing.

2) About the impact of OGWD events on vortex geometry and variability.

Figure 1. The evolution of the vortex geometry parameters are not compared to their mean seasonal evolution, and hence we cannot tell if the evolution is statistically different from the mean seasonal evolution. Showing the 95% confidence intervals does not give any useful information in this respect unless the composites show deseasonalized anomalies instead of the global value.

In lines 179-192 there are strong claims about the vortex being "robustly preconditioned for a breakdown", and "indicating a polar vortex split". This should be easy to quantify: How many of these parameterized OGWD events are followed by SSWs? How many SSWs are preceded by OGWD events? Also, the time evolutions shown in Figs 1-3 stop at lag +10 days; I strongly suggest to show more positive lags, so as to be able to analyze if significant alterations to the vortex indeed take place.

3) About the impacts on ozone.

I do not follow very well the arguments trying to explain the connection between the TCO anomalies and the wave activity anomalies at 70hPa and effective diffusivity at 450K, in turn being "caused" by the parameterized OGWD events. The first thing is that the spatial patterns of TCO and wave activity are said to be "perfectly correlated", and are clearly not. Aside from that, the argument is that strong finite-amplitude wave activity may be signaling planetary wave breaking, and this stirs and mixes trace gasses. And this is why effective diffusivity is shown, as a metric for irreversible mixing. However, significant anomalies of all these fields are shown at lag 0, so it is difficult to argue that at lag 0 the planetary waves have already had enough time to alter their propagation and breaking, and produce an impact on TCO. In lines 172-174, the authors already acknowledge that "it is reasonable to expect such an indirect effect to manifest with some time delay, presumably of a few days", when talking about the proposed mechanism for the OGWD impacting on the planetary waves.

Figure 7 - effective diffusivity at 450K. The isentropic level of 450K is located in the lowermost stratosphere, and I believe it is not very useful to understand variations in the TCO. At this level, there is more ozone concentration at high than at low latitudes, so increased ozone mixing would smooth the latitudinal gradients by reducing otherwise high concentrations over high latitudes, and increase otherwise low ozone concentrations over low latitudes. These effects on O3 mixing rations are captured in zonal mean ozone anomalies around 100-150 hPa (Fig. S3 in the supplementary material).

4) The organization of the results is not intuitive: The first subsection presents the evolution of different metrics of vortex geometry and variability from 10 days before to 10 days after OGWD events over the Himalayas. The second subsection shows the evolution of EP fluxes after the events, which is supposed to explain the dynamical mechanism behind the vortex evolution shown in the previous subsection. The third subsection

presents the evolution of total column ozone after the events, and this evolution is tried to be explained by changes in quasi-isentropic mixing. And the last subsection goes back to the dynamical mechanism behind the connection between OGWD events over the Himalayas and the propagation of planetary waves.

A more logical structure would start by showing how OGWD over the Himalayas may change the propagation of planetary waves in the stratosphere. If this is demonstrated, then the changes in the vortex structure and variability, and potential impacts on ozone concentrations can be shown.

Other comments:

- Figure 1. The evolution of the vortex geometry parameters are not compared to the mean seasonal evolution, and hence we cannot tell if the evolution is statistically different from the mean seasonal evolution. Showing the 95% confidence intervals does not give any useful information in this respect unless the composites show deseasonalized anomalies instead of the global value.

- The interpretation of positive anomalies of EPFD as a “wave source” is not correct. When there is a source of planetary waves, there will be divergence of the EP flux (the total field, not the anomaly). There can be a positive anomaly of EPFD but with a total negative divergence. So there is not enough information in Fig. 4 to conclude that there is an anomalous wave source (i.e. it could be weaker convergence).

Besides, a positive EPFD in a total sense in daily data does not directly imply a wave source. Indeed, a propagating wave will induce a flux convergence when arriving at a specific region, and a flux divergence when leaving. So it is important to analyze the time sequence of EP fluxes and divergence to correctly interpret the results.

- Please motivate/justify the use of a specified-dynamics climate model simulation. Although the evolution of the nudged variables should be similar to the reanalysis, it is known that other dynamical features such as the residual circulation are not well constrained by the nudging process (Chrysanthou et al. 2019). All reanalyses provide parameterized gravity wave drag output, and ERA5 resolves a good part of the GW spectrum, so I see no reason for going to nudged runs instead of reanalysis.

On the other hand, one of the strengths of using a climate model is the possibility of working with large sample sizes as compared to reanalyses, improving the statistics. But this possibility cannot be exploited using nudged runs.

- Please indicate the number of GWD events identified in the model run.

References

Chrysanthou, A., Maycock, A. C., Chipperfield, M. P., Dhomse, S., Garny, H., Kinnison, D., … Yamashita, Y. (2019). The effect of atmospheric nudging on the stratospheric residual circulation in chemistry–climate models. *Atmospheric Chemistry and Physics*, *19*(17), 11559–11586. https://doi.org/10.5194/acp-19-11559-2019

Edmon, H. J., Hoskins, B. J., & McIntyre, M. E. (1980). Eliassen-Palm Cross Sections for the Troposphere. *Journal of the Atmospheric Sciences*, *37*(12), 2600–2616.

Jucker, M. (2021). Scaling of Eliassenâ□□Palm flux vectors. *Atmospheric Science Letters*, *22*(4). https://doi.org/10.1002/asl.1020