Tuning of a convective gravity wave source scheme based on HIRDLS observations

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This paper describes an application of the Song and Chun, 2005 convective gravity wave source parameterization using the GROGRAT ray-tracing model, an observational filter consistent with limb sounders (e.g. HIRDLS) with a focus on January and July 2006 (using MERRA Reanalysis data). The papers aim is to better understand the confounding influence of convective sources (and their concomitant wave spectra) and the filtering of the background winds on the climatology of observed small scale gravity waves. The study represents a bold attempt in using a combination of source parameterizations and (ray-tracing) modelling (including dissipation mechanisms) to gain additional useful information in light of the current paucity of suitable (satellite) observations.

The paper has about the right number of suitable figures for a paper of this type and restates briefly all the main conclusions in the abstract. It is generally well written although would benefit from further proof reading for grammar from the native english speaking co-authors.

I suggest that some of the analysis be redone, or further supplementary material added, to examine for the influence of the unique meteorological conditions occurring during the NH winter stratosphere during January 2006. Other similar studies have also made similar use of data, without mention of anomalous filtering effects during this time. Following an adequate addressing of the main points I recommend publication.

Comments

- There does not seem to be a mention of filtering effects associated with the SSW occurring mid to late January 2006. This event would highlight differences between the NH and SW winters shown. In this respect, why was 2006 chosen? Should some of the conclusions be revised in the light of this? Perhaps another NH HIRDLS-observed winter should be examined for a comparison. Or if the authors are planning to show SSW filtering effects in another paper, they should examine another January year in the present study (e.g. Jan, 2007). GW filtering (as observed by HIRDLS) of this event was reported in Wright et al., 2010, 10.1029/2009JD011858
- A comparison of observations of GWs and GW parameterization employed in GCMs showed a marked difference in the vertical attenuation of GWMF (figure 3 of Geller et al, 2013, 10.1175/JCLI-D-12-00545.1). These differences were in part attributed to the observational window effects apparent in the satellite derived data (resulting in a faster drop-off in GWMF in observations as compared to models). It would be good to show if this effect is captured after applying the observational filter used in the present paper.

Other Comments :
(34300, L19) eq 2 does not describe a Gaussian, although it is derived from one in Song and Chun, 2005.
(34331, L18) "larger"
(34336, L17) “ray-tracing calculations”
(34336, L21) The simulated ‘momentum flux’ is shown in figures 3a and 4a, but how is this diagnosed. As previously mentioned, saturation mechanisms including e.g. radiative damping are used to limit the amplitude of rays in GROGRAT, but how is this calculated to produce the absolute GWMF shown? An equation or reference would be good in support of this.
(34337, L3) Not sure I see stronger reductions in GWMF at and below the tropopause. Also, ‘stronger’ than what in particular?
(34337, L10) Suggest “…better match at low latitudes. At mid and high latitudes HIRDLS observations indicate an enhancement, likely due to other sources.”

(34337, L20) The authors suggest the tropical differences seen between HIRDLS and the filtered GWMF are due to other GW sources. I am not convinced by this. Can the authors describe how the existing convective GW source parameterization formulation could lead to an underestimate in tropical GWMF at low altitudes between 3b,c and 4b,c?
(34338, L21) I am not sure of this attribution statement: vertical gradients in wind are located with regions of div_F. The ray-tracing scheme also allows for horizontal propagation and so horizontal and vertical gradients in zonal wind may be associated with div_F. Furthermore, features in the upper tropical stratosphere (e.g. 4d) cannot be explained by a statement like, "positive drag is found in regions of positive wind shear", as in that example the opposite would appear true. Perhaps the authors might like to mention the combination of horizontal propagation as well as dissipative processes not including critical layer filtering, which could better account for the sizeable drag features in the tropical mid to upper stratosphere.
(34339, L9) suggest moving the word 'only', "The vertical gradient Q only considers the dissipation caused…". The authors might like to consider further describing the differences expected between P and Q. Superficially, the difference comes down to the non-commutativity between the observational filter and the (vertical contribution to the) divergence operator, but the reader will most probably not know what this difference ought to be. Another sentence or two highlighting these differences would be a good idea.
(34340, L1) Suggest "As mentioned in Sect. 2.1, further tuning is achieved by reducing the launch amplitude by a factor of $1/\sqrt{\alpha}$ and simultaneously multiplying the number of launched rays by a factor of α. In this study α is chosen to be 5."
(34340, L5) Presumably changing α can shift the relative height of saturation higher or lower, depending on how much wave energy is shared within a given wavenumber band. This was mentioned earlier, so I suggest the following; "…can affect GWMF aloft, shifting the saturation level to different altitudes."
(figures 3-8) Please mention MERRA data in captions.
(figure 3 caption) presumably the y-axis shows -1/rho*...to be consistent with equations 5-7.