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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mesospheric gravity waves and their sources at the South Pole" *by* Dhvanit Mehta et al.

Anonymous Referee #1

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The paper presents a case study of mesospheric gravity waves detected in airglow emission above the South Pole using data from three austral winter months in 2003 and 2004. The authors identify likely wave source regions based on backward ray-traces using the GORGRAT ray-tracing model. Notably, Mehta et al. find evidence for gravity wave sources in the lower mesosphere.

While I enjoyed reading the paper, I feel that limitations and uncertainties associated with backward ray-tracing are not satisfactorily discussed. There are two major sources of error which contribute to uncertainties in the computed trajectories: 1.) uncertainties in the initial wave parameters (horizontal wavelength, direction of propagation, observed period) which are derived from airglow observations in this paper, and 2.) uncertainties in the background wind and temperature fields. Depending on the state of the atmosphere, small changes in the direction of propagation or in the horizontal

wavelength may cause the wave's ray path to terminate at vastly different locations. The problem becomes more severe when the polar vortex is displaced and rays propagate though strong shear flows. As Mehta et al. interpret the termination point of their ray paths as potential gravity wave source regions, uncertainties in the backward trajectories may lead to a large volume with potential sources instead of single source regions. This is my major concern with this case study. The authors compare ray paths which result from using different atmospheric background fields (climatologies and ECMWF analyses). I suggest that the authors also investigate the sensitivity of the wave's ray path to variations in the initial wave parameters. It would be helpful if the authors could provide estimates of the accuracy of their derived wave parameters. For example, Figure 1 looks rather noisy and I find it difficult to motivate a propagation direction of precisely 207° (page 3, line 20). The same concerns apply to the derivation of the horizontal wavelength and observed period. I recommend the paper for publication provided the issues mentioned above are adequately addressed.

Minor comments:

Page 2, line 24: What is NJIT? Please spell out.

Page 4, line 24: The authors use ECMWF data below 50 km altitude and NRLMSISE-00 an HWM-93 above. How were the data sets stitched together? I assume there are significant differences between a climatological model and ECMWF analyses. The two data sets need to be joined somehow in order to obtain smooth background fields suitable for ray-tracing. I suggest the authors investigate how this "transition zone" affects the computed ray paths (e.g. transition at different altitudes).

Page 5, lines 2-5: "The polar vortex is displayed away from its normal configuration centered close to SPA and tilted in the region where the wave is determined to originate. This can be seen more clearly in the 3-dimensional projection shown in Figure 5." The contour lines are difficult to relate to the coordinate system in the 3D projection. I suggest a 2D plot like Figure 4.

Figure 5: The kink in the wave's ray path at ∼43 km looks suspicious to me. The authors combine climatological winds with ECMWF analyses. I expect significant differences in the wind fields, especially when the vortex is displaced. This may introduce artificial wind shears and thus refraction of gravity waves where the two data sets are joined.

Page5, lines 5-14: I assume ECMWF data were used as background fields in the lower atmosphere (no "climatological" runs). Please clarify.

Page 6, line 28: "Low" winds at the pole during winter may help to reduce the error in estimates of intrinsic wave parameters, but even small wind speeds can cause gravity waves to be significantly refracted if the waves encounter strong shear flows. This may happen when the vortex is displaced.

Page 6, line 29: The authors mention that meteor radar winds are available at South Pole. I suggest that the authors use these data instead of the HWM-93 climatology as background winds for ray tracing or at least compare the climatology to observations (meteor radar data) in order to estimate potential errors in ray tracing.

Page 7, line 2: It is not clear to me what the authors mean by "we have presented a compelling case for a previously unidentified source of small-scale gravity waves in the polar MLT". The backward ray traces presented in this paper terminate at different altitudes in the troposphere, stratosphere and lower mesosphere.

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