Review of *Long-Lived High-Frequency Gravity Waves over the Southern Ocean and the East Antarctic coastline and Their Influence on Cloud Properties* by Wenyue Wang et al.

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This article documents the recurrent presence of high-frequency gravity waves (HFGWs) over the Southern Ocean during a shipborne campaign, primarily using a Doppler cloud radar. These waves exhibit small-amplitude vertical velocity fluctuations (of around $0.02\,\mathrm{m\ s^{-1}}$) and are often long-lived. A detailed analysis of one case-study is provided, and then expanded into a statistical analysis covering the entire campaign period. Possible implications for wave-cloud interactions are discussed and future research directions are outlined.

These small-scale, small-amplitude HFGWs have rarely been sampled and are poorly, if not at all, represented in models, with potential impacts on the representation of cloud properties. A better understanding and characterization of these waves and their effects on clouds are therefore of primary importance. The present study contributes to this effort and, as such, it might be considered for publication in ACP. The methodology appears proper and is generally well detailed. The use of high-resolution cloud radar observations, along with complementary measurements (e.g., radiosoundings), is appreciated. In our opinion, the observed ubiquity of HFGWs and their characterization, as summarized by Figure 11, represent the main contributions of the manuscript.

That being said, we believe that the manuscript in its present form requires some further work before it is ready for publication. The scientific motivation could be outlined more clearly and some statements or results appear poorly justified or supported. We believe that the manuscript would greatly benefit from a more detailed discussion of multiple aspects of the case study analysis and of the broader implications of the results. Based on these considerations, we recommend a major revision for this submission.

Major comments

1. The manuscript presents three case studies, but only one of them is thoroughly characterized and utilized. We appreciate that the authors make use of a rich suite of instruments and observational approaches to describe and/or interpret the cases (including radiosoundings, total sky images, radar-lidar target classifications, back-trajectories). However, the derivation of wave parameters, which is of particular interest to the community given the general lack of measurements of HFGWs, appears very brief (lines 244-255) and a bit disproportionate compared to the amount of data introduced. The manuscript would greatly benefit from further and more explicit details on how the wave parameters are calculated and on their relative uncertainties, as well as from the derivation of additional wave parameters.

   The authors could independently determine the wave characteristics to validate their current estimates by only using radiosoundings, following the methodology of Guest et al. (2000). In particular, the vertical profiles of zonal (u) and meridional (v) velocity could be used to make a hodograph, which is expected to reveal an elliptical polarization (at least in the altitude range of most intense wave activity). The aspect ratio of the ellipse should allow the determination of the wave frequency, the direction of its major axis would indicate the orientation of the horizontal wave vector, while the anticyclonic (cyclonic) rotation of the hodograph with altitude wuold inform on the upward (downward) energy propagation. Finally, the vertical wavelength can be determined by fitting a polynomial to the vertical profiles of u and v with the least-squares method, as outlined in Guest et al. (2000).

Using wind profiles from radiosoundings, the authors could also compute the wave intrinsic frequency and intrinsic phase speed, which account for the Doppler shift induced by the background wind (ruled out as unimportant, hence neglected in the present version of the manuscript, cf. lines 356-357). If radiosoundings were performed regularly throughout the campaign, it would be very interesting to see what Figure 11d looks like when plotting the intrinsic period instead of the ground-relative one, e.g., would the data collapse?

2. Echoing the first point raised above, the availability of radiosoundings would also allow for a deeper investigation of the large-scale environmental conditions than currently presented. In the conclusions, the authors claim that these HFGWs can persist "for up to 48 hours under favorable conditions" (line 383), but a systematic analysis is missing in this respect. For instance, the presence of wave ducting at least on the day of the Mawson case study should be more rigorously assessed using the Scorer parameter (e.g., Stephan, 2020), computed directly from radiosoundings. The Scorer parameter quantifies the relative roles of thermal stratification and wind shear in creating wave ducts and would provide a solid physical validation for the wave trapping mechanism, rather than just qualitatively invoking it, as done at lines 357-358 in the present version of the manuscript. It might well be that wind shear plays a role in defining the boundaries of a possible duct, since strongly sheared layers appear to be present at several levels (e.g., at 1.5 km and 9 km in the first two soundings in Figure 5b,e). The authors need to confirm their argument on the thermal duct (lines 354-355 and 357-358) with a more quantitative approach and, if measurements are available, determine to which extent the environment can generally sustain wave ducting.

3. The discussion introduces several interesting directions of research, but still lacks some focus. In our opinion, the discussion around potential gravity wave sources currently feels a bit general and it is difficult to determine which mechanism is most likely responsible for the waves observed in the analyzed case. It would be highly beneficial to link the proposed mechanisms more directly to the specific case study, rather than referring the reader to previous literature, as often done here. The identification of a direction of propagation from the hodograph, as suggested in point 1 above, could help for this purpose.

   With regards to cloud-wave interactions, the methodology to obtain cloud geometrical properties appears quite concise (line 322). We are not entirely sure whether the manuscript demonstrates a direct influence of waves on clouds (thereby indicating a causality), as also suggested by the manuscript's title, or just a correlation between wave activity and cloud geometrical properties. How significant is this correlation? Could the authors speculate on the physical processes responsible for the differences in cloud geometrical properties with and without HFGWs? Moreover, a discussion on the limits of the present approach would be useful, e.g., how challenging is it for the Doppler radar to detect thin clouds and the signature of wave activity within them?

   Finally, the periodic modulation of cloud properties by the waves is a very interesting aspect. The discussion here currently feels somewhat descriptive. Commenting on the physical processes that can contribute to this modulation would definitely add value.

Specific comments

Title, Abstract Based on point 3 above, we believe that the expression "influence on cloud properties" in both the title and the abstract (line 3) should be removed or carefully re-evaluated, unless new analysis is provided to substantiate it.

Lines 75, 273 The authors refer to cyclone-generated gravity waves and cite Nolan and Zhang (2017). Since that study focuses on tropical cyclones, the authors need to clarify to what extent its findings are applicable to the extra-tropical cyclones considered here, or alternatively to provide references more directly relevant to the generation of waves by extra-tropical cyclones.

Lines 149-154 The reliability of the chosen ERA5 variables should be discussed. In particular, the vertical velocity in reanalyses (presented in Figure 8b) has been questioned a lot, at different scales (Uma et al., 2021).

Line 182 The authors select the dominant spectral peak within a pre-determined frequency interval and then conclude that the waves satisfy the condition $\omega < N$, with N lying outside (above) the interval (lines 248-251). The condition would be mathematically satisfied by default. Please clarify how this can be considered meaningful.

Line 208, Figure 4 This figure is hard to interpret in its present version. The cloud stripes are not really visible, so we would not claim they are ”evident”, as done at line 208. Could you make the figure bigger or increase the contrast? The cardinal direction are not readable. Maybe adding the synoptic scale wind direction at the cloud altitude could be interesting.

Lines 211 The ubiquitous nature of HFGWs cannot be stated from three snapshots. We suggest leaving ”ubiquitous” for later when the statistical analysis is presented.

Lines 212-213 Could you please rephrase? Assuming that the favourable conditions refer to the presence of clouds, you could write something like ”the persistence of clouds allowed a continuous 48-hour monitoring of gravity waves...”.

Figures 2a,c,e and A1 In addition to the gravity wave signal, marked vertical gradients in Z and w are apparent. Are these due to ice accretion processes? Could you briefly comment on them (even in the appendix if you prefer)? It might not be obvious to the reader.

Lines 221-234 The structure of this paragraph should be made clearer and more precise. At lines 222-223, the authors state that ”the radiosondes launched at 05:30 UTC and 11:30 UTC passed through or close to cloud layers”, based on the fact that ”the dew-point temperature is close to the dry-bulb temperature” (lines 226-227). Radiosoundings indeed measure thermodynamic state variables and in these cases they identify near-saturated conditions. The authors should therefore consider removing the claim of ”evidence of clouds” at line 228. They could then discuss the vertical stability structure, and we would recommend replacing the expression ”step-like temperature variations” (line 224) with ”regions of enhanced stability” (or similar) here and wherever applicable throughout the manuscript.

Line 235 Could the authors clarify what is meant exactly by ”convective generating cells” here?

Lines 235-239 Is this statement based on the target classification mask? Could you verify the presence or absence of liquid water with MPL backscatter or depolarization or is the lidar signal fully attenuated?

Line 250 Here, what is the hypothesis you make to analyze your snapshots? Do you consider the observation as stationary? Does it remain valid when looking at the moving ship (avg. 10 km is of the same order of magnitude as the phase speed)? Could be solved through major point 1.

Line 253 In Figure 2f, the slope of the iso-phase (iso-reflectivity) lines is of about $4\,\mathrm{km\,h^{-1}}$, which gives a vertical wavelength of 0.5 km for the given period of 7.28 min.

Lines 292, 321 Given that HFGWs can propagate over long horizontal distances from their source under favorable conditions (e.g, ducting), topographic forcing cannot entirely be ruled out. Maybe it would be better to replace ”cannot” with ”might not”?

Lines 296-302 The expression ”could no longer” suggests a permanent malfunctioning after 2 December 2017. Could you please clarify this aspect or provide a clear timeline of the data interval used? Furthermore, it is claimed that Figure 2 *demonstrates* that the measurements during stationary periods are reliable. Why is this plot a demonstration that the data are reliable?

Line 364, Figure 15 A dominant periodic signal can be seen, but more at periods close to 30 minutes. How does the FFT of reflectivity depend on time bounds? Maybe a third panel could show again for comparison the FFT of Doppler for the same time bounds?

Discussion The very weak amplitudes of the observed waves is a finding that could be of interest. Could the authors elaborate a bit more on this, maybe comparing these amplitudes with those found by some previous studies?

Technical corrections

Line 1 Wouldn't it be more precise to say that the Southern ocean *contains* sources?

Line 24 Could the authors further elaborate on the "favourable propagation conditions"?

Lines 24-25 "the conditions over the Southern Ocean [...] are considered one of the most active regions for GWs globally". Could you please rephrase this sentence, which seems grammatically inconsistent?

Line 34 Would it be possible to add further references regarding the detection and characteristics of HFGWs?

Line 37 A strong dependence on → are associated with?

Line 38 Could you provide a definition of waveguide or a reference? It might not be obvious to the reader.

Line 39 Jia et al. (2019) do not analyze wave-cloud interactions, so we think the reference should be moved to the end of the previous sentence (i.e., after "predominantly transport energy horizontally").

Line 48 Could you expand a bit on which type(s) of satellite products you are referring to?

Lines 90 jumps → jump.

Figure 1 Would it be possible to add to the caption or the colorbar the starting day of the campaign for reference?

Line 153 Please put citation in brackets.

Lines 165-169 Could the authors add a reference for the critical value of the gradient Richardson number?

Lines 171 "original irregularly sampled measurements": is there a contradiction with the statement at line 125 ("measurements acquired every 5 s")?

Line 200 *Strong* vertical coherence is not always clearly visible from Figure 2.

Line 208 On 03:18 → at? Could you also specify that the image corresponds to the case study of Figure 2a,b?

Line 214 Can we confidently say it is only one event?

Line 219 Why cannot we identify a vertical wavelength for Casey?

Figure 3 This figure is not used much except for stating that the clouds are almost entirely composed of ice. Can it be moved to an appendix?

Figure 4 2027 → 2017?

Figure 6 You could consider adding this figure as a panel to Figure 5, perhaps also for the other soundings? Maybe you could also perform some vertical smoothing of the vertical profiles of temperature and horizontal wind, which would make the message more evident.

Line 282 Does Grazioli et al. (2017) talk about gravity waves?

Figure 11 Would it be possible to show the dates of the three case studies as vertical bars in panels a,d,e? Would it be possible to also add a panel on the right of panel d with the KDE of gravity wave periods for Davis, Casey and Mawson?

Line 341 On which day?

Line 352 Could you add the corresponding period?

Line 356 Where is the local buoyancy period calculated?

Figure 14 Maybe some vertical smoothing could make the stratification profile clearer?

Line 393 MAwson → Mawson

References

Guest, F. M., Reeder, M. J., Marks, C. J., and Karoly, D. J. (2000). Inertia–Gravity Waves Observed in the Lower Stratosphere over Macquarie Island. *Journal of the Atmospheric Sciences*, 57(5):737 – 752.

Stephan, C. C. (2020). Seasonal Modulation of Trapped Gravity Waves and Their Imprints on Trade Wind Clouds. *Journal of the Atmospheric Sciences*, 77(9):2993 – 3009.

Uma, K. N., Das, S. S., Ratnam, M. V., and Suneeth, K. V. (2021). Assessment of vertical air motion among reanalyses and qualitative comparison with very-high-frequency radar measurements over two tropical stations. *Atmospheric Chemistry and Physics*, 21(3):2083–2103.