

Interactive comment on “Gravity waves in the winter stratosphere over the Southern Ocean: high-resolution satellite observations and 3-D spectral analysis” by N. P. Hindley et al.

Anonymous Referee #1

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In this paper, an analysis of gravity waves in the Southern Hemisphere Winter stratosphere is presented. More precisely, the authors apply a 3-D Stockwell transform (3DST) analysis method to 3D AIRS temperature measurements from Hoffman and Alexander (2009), which enables them to derive different wave characteristics (such as amplitude, wavelength and propagation direction). A key result of the paper is to report a region of horizontal convergence of gravity wave momentum flux near 60° S during southern winter, thus confirming earlier findings. Horizontal propagation is, together with intermittency, one of the main topical questions regarding gravity wave parameterizations in climate models. Quantifying horizontal propagation in observations, as attempted in this paper, is hence highly valuable.

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Overall, the paper is well presented and easy to read. However, I think some of the analyses are not sufficiently detailed. This partly comes from the fact that the authors attempt to accomplish two tasks, namely: 1) presenting their method and 2) describing the scientific results they obtain by applying it. Both aspects would be worth deeper investigation, but for this study I would recommend to investigate more thoroughly the performance of the method when applied to the AIRS data. Hence, although the large-scale picture of momentum flux convergence at 60°S is, as said above, valuable to the community, I would like the authors to carefully address my comments and suggestions below before the paper can be considered for final publication.

Main comments

1) The authors offer a thorough evaluation of the performance of their analysis method, and make an effort to address the amplitude reduction problem inherent to the 3DST. However, they disregard the impact of noise and observational filter in AIRS data on their gravity wave retrieval. This could be addressed in a similar manner as done in Sect. 3.5, but by adding typical noise from AIRS observations and applying the observational filter to their synthetic waves. I believe this is necessary for a rigorous description of the capabilities of the method.

2) The authors motivate their study (e.g. in the abstract lines 2 to 5) using the “cold-pole” bias, i.e. the too cold southern polar vortex and delayed vortex breakdown and stratospheric final warming which are simulated in climate models (MacLandress et al., 2012). However, in the stratosphere, the “cold-pole” bias is mainly an early spring-time feature (see, e.g., Fig. 2 of Garcia et al., 2017), while the analysis presented here spans the months of June, July and August 2010. Since the authors believe that daytime measurements are as valuable as nighttime ones, I think using the months of September and October would be more appropriate to address their motivations.

3) Although they have access to a very valuable dataset, the authors do not come to any strong conclusion regarding the missing ingredient in parameterization which

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should be introduced to solve the “cold pole” problem. This is particularly evident in the discussion (6.1 and 6.2), where the authors remain cautious and mainly point to the literature (e.g. expressions like “could be significant”). In particular, regarding the role of small islands, the authors could reproduce Fig. 11 of Jewtoukoff et al., 2015 (contribution to the zonal mean momentum flux of GW momentum flux above the islands and the ocean). I understand that the limited range of wavelengths accessible within the data hinders any definitive conclusion, but I believe that the authors could speculate using their observations.

Specific comments:

p 3 l 1-2: “unresolved in GCMs due to their size”: unresolved and not accounted for in parameterizations

p3 line 10: Can secondary waves alone explain the lack of drag at 60°S, or do they require to be combined with one of the explanations above?

p5 line 1-2: What is this noise level?

p5 line 14: “interpolated”: what interpolation is used?

p5 line 22: “a tapering function . . . is applied”: Is this smoothing really necessary? How does it affect the retrieved vertical wavelength? (in relation to main comment 1)

p7 line 11-13: Could they also be orographic waves originating from South America or Africa and propagating horizontally?

p7 line 25: Could you briefly explain how the noise analysis of Hoffman and Alexander (2009) works?

p10 Eq. 1: The authors sometimes use absolute frequency (equation 1) and sometimes angular frequency (e.g. appendix A). They should be consistent for better readability. Similarly, they sometimes use j (appendix 1) for the Euler number, sometimes i . This is a matter of taste, but I would also recommend using the scalar product notation

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instead of matrix product with a transpose.

p 11 line 7: This approach of deriving the analytical signal is not strictly consistent with the definition of the 3DST given in Eq. 1. This does not affect the results, but the target quantity should be more clearly defined. Similarly, I believe there is a phase shift of $2\pi\tau.f = \tau.k$ between Eq. 1 and the method description p11 or Eq. A7 in the appendix. Please check and correct if needed.

p11 line 11: “FFT-1Ha(alpha) * Wn (...)”

p 13 section 3.4.2: It seems to me that you are converging towards the analytical signal for your amplitude retrieval. Why not use the full analytical signal (rather than a filtered version)?

p 16 and Figure 3: How does it change when noise and observational filter effects are included?

P17 l 33: How is it “windowed”? P20 line 30: “it could also result from the reduction in vertical resolution with altitude”: an evaluation of the impact of the observational filter would help clarify. See also Main comment 1

p21 : The two case studies help illuminate the potential and limits of the AIRS measurements and the analysis method. Again, this point would be clearer if an analysis of the impact of the observational filter were included.

p 23 l 15 and 17-18: Motivating your observations by the cold-pole problem is not consistent with using June, July, August (see also main comment 3).

p24-25, Sect. 5.1: You mainly compare your results to other AIRS observations, do you know of any reference with other instruments (HIRDLS, SABER)? I understand that the waves observable by different platforms correspond to different parts of the GW spectrum, but this would be of interest to the reader.

p27 lines 11-13: Interestingly, the authors hypothesize a change in the dominant gravity

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wave source between June and August (from orographic to non-orographic). Any idea why this would be the case?

p 27 lines 16-18: comparison with Ern et al.: you explain part of the discrepancy with their study (25

p 27-28: How significant are the differences in vertical and horizontal wavelengths?

p28 lines 17-18: "Generally ..." Longer vertical wavelengths also seem to correlate with shorter horizontal ones, correct? I find that the color scale makes it difficult to use the figure quantitatively.

p28 lines 22-28: This could be considered more carefully in Sect. 2 (see main comment 1).

p30 Table 2: It would also be interesting to separate the regions in the Southern Ocean with and without island. Having the mean in each region besides the zonal mean would also be helpful.

p 32 Sect. 5.4: Intermittency: Besides the Gini coefficient and the 90 % quantile, it is common to estimate intermittency using PDFs of momentum fluxes (Plougonven et al., 2012; Jewtoukoff et al., 2015, Holt et al., 2017). It would be interesting to show such pdfs for the different months and regions.

p33: A number of previous studies have suggested that intermittency is generally larger over orography than over the ocean. An exception in this dataset are orographic waves above the Kerguelen, which have lower intermittency and do not stand out as more intermittent relative to the ocean. Any idea why?

p34 line 12: "could occur in reality" → "occurs for the waves observed in AIRS". I think the authors can be more specific and affirmative in their formulation.

p34 sect. 6.2: The authors mainly report references here. They could be more quantitative regarding the role of small islands by estimating their relative contribution in Table

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2.

p36 line 23: "given the size, intensity, spatial extent": Could the authors be quantitative here and refer again to the figure? Why do your observations point to secondary waves and not primary ones? I do not doubt the importance of secondary waves, but I do not see how you can infer that from your observations. If this is pure speculation, you should state it clearly; otherwise, clarify your arguments.

p37 line 2: "under-represent": or under parameterize?

Typos:

p4 line 19: "sensitive to gravity waves"

p14 line 9: "as generalised" → "as general" ?

p18 l19: "explained in section 6" → section 6.3

p28 line 29: "it may be possible" → "we will attempt"

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