Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-371-RC2, 2019

Atmospheric Chemistry and Physics
Discussions

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 #2

Received and published: 22 July 2019

The authors present a significant extension of the 3D-Stockwell transform (3DST) and then apply the 3DST to Southern hemisphere AIRS observations to determine gravity wave parameters like amplitudes, wavelengths, momentum fluxes and intermittency. With these parameters resolved in 3d, the authors discuss the problem of "missing gravity wave drag" at 60°S as introduced by McLandress et al. (2012).

In all, this is a very comprehensive manuscript that will ultimately make an important contribution to the scientific literature on the role of gravity wave dynamics in Southern hemisphere climate. I recommend publication in ACP once the following mostly minor comments are properly addressed:

1) When introducing satellite observations of gravity waves (e.g., page 3, lines 15-22) the corresponding observational filters and the accessible spatial scales should be mentioned.

2) Please specifically show the AIRS observational filter in Section 2 where the AIRS measurements are introduced. On page 4, line 20 reference is made to Ern et al. (2017) but no numbers are actually mentioned. I consider it critical for this paper to point out what these numbers are. Also statements like "by measuring these waves in AIRS observations we can provide constraints on a large part of the momentum budget" must be quantified! How large is this part? Is this statement based on hard facts? If yes, please present them with suitable references.

3) Page 5, line 8: Don't you know how many observations are made during day- and nighttime? Why is it only likely?

4) Page 7, line 31: Why not explicitly mention the mean/median noise error value? Since all these numbers are available why not compute it exactly?

5) Page 9, Section 3.1: I was initially irritated that the authors introduce this section as an extension of the 2D-Stockwell transform in Hindley et al (2016) and not of their first attempt of a 3D-Stockwell transform as described in Wright et al., ACP 2017. The latter is mentioned at the end of the section, but should be mentioned at the beginning! Why is another extension needed? What is new here? That should be pointed out from the start.

6) Also, I am wondering why the authors bother to develop the 3D Stockwell transform when there is such an overwhelmingly lare body of literature on the application of wavelets to geophysical data sets. Of course, the authors are free in their choice of a suitable method, but a few words on why not using wavelets would be appreciated.

7) Page 15, Figure 3 and related text: it is a very good approach to test the newly developed 3DST on synthetic data. However, I am wondering how realistic the synthetic

data set is. For example, the different wave packets all appear to be well separated in physical space. Is this what we expect to find in the atmosphere? Wouldn't it be more appropriate to do more sensitivity tests to find out how well the 3DST is performing?

8) The assumption of exponential altitude growth with altitude (page 18, line 1 and below) is certainly not generally applicable and that should be stated here. For example the paper by Kruse and Smith (JAS 2016; DOI: 10.1175/JAS-D-16-0173.1) demonstrates a very different behaviour in the vicinity of a wind minimum over New Zealand. It really depends on the local wind conditions and also on the initial wave forcing as found by Kaifler et al. GRL 2015 and Fritts et al., BAMS 2016 whether or not the waves propagate without breaking or not.

9) When discussing the possibility of upward versus downward propagating gravity wave reference could also be made to the paper by Kaifler et al. JASTP 2017 (http://dx.doi.org/10.1016/j.jastp.2017.03.003) which shows strong indications of downward propagating waves in lidar observations.

10) Page 20, line 29/30: Please indicate whether or not the increase of vertical wavelength is consistent with wind profiles from reanalysis data. Thanks!

11) Page 23, line 4/5: is this observed propagation pattern physically plausible?

12) Page 20-22: The authors here hint to a potential bias of their observation sbut never really discuss whether or not that strongly impacts their results. Some more discussion would be appreciated.

13) Page 34, line 10-12: These are very interesting results! Can this analysis be repeated at other altitudes below to see whether the momentum flux can actually be traced to where it is initiated at lower altitudes? That would be extremely valuabe to support the "refraction into the polar vortex"-hypothesis.

14) Page 37, line 24-26: Isn't it just the other way round (see table 2)? Sorry, if I am confused.

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