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Atmospheric Measurement Techniques
Discussions
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Interactive comment on "A two-dimensional Stockwell Transform for gravity wave analysis of AIRS measurements" *by* N. P. Hindley et al.

Anonymous Referee #1

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General Comment

In their paper the authors introduce the 2D S-transform as a new method for deriving gravity wave amplitudes, horizontal wavelengths and propagation directions from brightness temperature distributions of the nadir-viewing Atmospheric Infrared Sounder (AIRS) instrument. The method is tested using simulated data, and optimized by selecting an elliptical window instead of the traditionally used Gaussian window. Advantages and disadvantages of the method are thoroughly discussed, and the method is applied to three granules of AIRS data over the Southern Andes, the Drake Passage/Antarctic Peninsula, and the isolated mountainous island of South Georgia.

Overall, this is a very interesting study and an important step forward in the estima-

tion of gravity wave properties and momentum fluxes. These parameters are needed to improve the representation of gravity waves in global models, which is one of the major uncertainties in global modeling. Only minor revisions are required before the manuscript is recommended for publication in AMT.

Main comments are:

(A) momentum fluxes without attenuation correction should also be shown

(B) a rough comparison with momentum fluxes from limb sounders should be included

Please find the Detailed Comments below.

Detailed Comments

(1) p.2, l.31:
for completeness, the reference Jiang et al., 2002 should be included:
Jiang, J. H., D. L. Wu, and S. D. Eckermann, Upper Atmosphere Research Satellite (UARS) MLS observation of mountain waves over the Andes, J. Geophys. Res., 107(D20), 8273, doi:10.1029/2002JD002091, 2002.

(2) p.3:
Sect. 1.1 is quite short and somehow out of place, in the introduction following the organization and overview of the paper. I would suggest to introduce a new section 2: "Data and method", shift Sect. 1.1 to 2.1, and change Sect. 2 to Sect. 2.2.

(3) Suggestion: briefly introduce the expression "voice" by stating that this denotes a specific wavenumber or frequency out of a discrete set.

(4) p.8, l.12: Please mention, if this is correct:
Similar to real-world waves, spread of frequencies would be expected also for your simulated waves because they form some kind of "wave packets" with the wave amplitude damped with increasing distance from the center of the packet. This will introduce a spread of spectral power that is not fully captured by just focusing on the dominant waves.

(5) p.8, l.23, and elsewhere:
It seems that in your paper wavenumbers are generally defined as 1/wavelength, rather than 2π/wavelength.
This should be clarified and stated in the manuscript;
please check equations for consistency whether this definition of wavenumbers has effect on the different scaling factors that are required.

(6) Question about Fig.2:
If integrated over the whole domain, could it happen that the temperature variance contained in Fig.2b or Fig.2c could be higher than in Fig.2a? This information should also be included in the manuscript.

(7) Fig.3:
You should mention that the windowing functions are normalized with max. value of 1. This is different from the use in Eq.(7) where the integral over the windowing function should be unity.

(8) General comment:
Sometimes the windowing functions are given with scaling factors that make sure that the integral over this function should be unity. (Eq.(2)–(4)).
Sometimes it rather looks like the maximum of this function is scaled to unity (Eq.(5), (6), (8)–(11)). Is this correct?
Please comment/check for consistency.

(9) p.10/11, discussion of Fig.4:
As a cross check, please calculate the temperature variance over the whole AIRS granule for the raw data (Fig.4a) and compare with the variances for the reconstructed T' in Figs.4b, 4c, and 4d. Two reasons:
(1) It almost looks like the variance of the distribution in Fig.4(d) would overestimate the raw data in Fig.4(a).
(2) Calculating these variances provides a rough measure which method performs best in reproducing the total variance of the wave field.

(10) p.13, l.3-5:
Please mention that the resolution of this regularly-spaced grid closely matches the spatial resolution of AIRS in the center of an AIRS swath, and not much information is lost. In the off-center regions of an AIRS swath the regularly-spaced grid is even better than the spatial resolution of AIRS, however the grid will not exactly match the location of the AIRS footprints.

(11) Fig.5/Fig.6:
Also momentum flux values without attenuation correction should be shown because this correction is quite substantial, possibly up to a factor of 100, and probably this correction is strongly susceptible to errors in the vertical wavelength. This is particularly important because vertical wavelengths are derived assuming that a mountain wave is observed, and based on ECMWF winds. At high altitudes ECMWF winds can easily be biased by about 10m/s. There is some error discussion later in the manuscript, however these limitations should be more clearly mentioned already in the discussion of Figs.5 and 6.
It should also be mentioned whether or not for Fig.5/Fig.6 an upper limit was used for this correction.

(12) p.15, after l.9:
Overall, the agreement between Fig.5a and Fig.5b is very good.

However, to me it looks like the amplitude of the mentioned small-scale event in Fig.5b would be somewhat overestimated compared to Fig.5a. Further, in the reconstructed wave field the strongest positive wave crest of this small-scale event is somewhat shifted to the west, into the negative wave phase of the larger scale event. This is not seen in the original AIRS brightness temperatures.
Another difference is seen in Figs.5a and 5b at 63S, 28W where in the original brightness temperatures only a larger scale bow shaped positive anomaly is seen. In the reconstructed T'-field for the dominant spectral features, however, there is a smaller amplitude shorter scale feature.

Therefore you should mention that, apart from the really good agreement between Fig.5a and Fig.5b, there are still some remaining uncertainties.

(13) p.15, after l.14:
At least a rough comparison with gravity wave momentum fluxes from limb sounders should be included. To me it looks like typical values for limb sounders over the Antarctic Peninsula during July are somewhere in the range 1–10mPa (Geller et al., 2013; Ern et al., 2011, their Figs.3c and 9d). These values do not include attenuation corrections, still your AIRS values seem to be much higher, which is an important information.

Reference:
Ern, M., P. Preusse, J. C. Gille, C. L. Hepplewhite, M. G. Mlynczak, J. M. Russell III, and M. Riese (2011), Implications for atmospheric dynamics derived from global observations of gravity wave momentum flux in stratosphere and mesosphere, J. Geophys. Res., 116, D19107, doi:10.1029/2011JD015821.

Technical Comments

- p.1, l.7:
  and directions in both directions simultaneously. $\rightarrow$ in both directions simultaneously, and thereby the propagation direction of the waves.
- p.3, l.20: fluxes easier $\rightarrow$ fluxes are easier
- p.3, l.20: pertubrations $\rightarrow$ perturbations
- p.6, l.22: descirbes $\rightarrow$ describes
- Fig.2c:
  In Fig.2c spectral amplitudes are shown. In the manuscript, this parameter is denoted ξ, while the parameter given at the colorbar in Fig.2c is denoted $|T'|$. This is somehow misleading because temperature fluctuations were denoted T' before.
- p.8, l.23:
  probably, this should read $(k_x^2 + k_y^2)^{-0.5}$
  (lowercase k, and power of -0.5, instead of -2)
- p.12, l.6: furture $\rightarrow$ future

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