

## ***Interactive comment on “The southern stratospheric gravity-wave hot spot: individual waves and their momentum fluxes measured by COSMIC GPS-RO” by N. P. Hindley et al.***

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The authors are very grateful to the anonymous reviewer for all of their helpful comments and suggestions. The comments are fair, reasonable and contribute to the improvement of the paper.

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### **1 Major Comments**

- **(MC1) Reviewer’s Comment:** More discussion is needed regarding the vertical wavelength limitations of the analysis method. Otherwise it cannot be decided whether features of the gravity wave distribution are caused by the analysis technique, or whether they are an effect of gravity wave propagation. This concern is addressed in more detail in the specific comments, particularly in specific comment 7.

**Authors’ Response:** Agreed. We have provided more discussion on the vertical wavelength sensitivity of our analysis method in Section 2.2 and elsewhere. See our response to specific comment 7.

- **(MC2) Reviewer’s Comment:** Horizontal separations of altitude profiles as short as 10-20km are used for determining horizontal wavelengths. This is quite short and will lead to random effects. This may explain some of the differences between HIRDLS and COSMIC in Fig. 10. Fig. 10 should be revised by using longer horizontal separations. See also specific comments 16, 17 and 21.

**Authors’ Response:** Agreed. We have included Appendix A to investigate the precise effect of these short horizontal separations on our estimation of  $\lambda_H$ . It is not possible to revise Figure 10 using only longer horizontal separations. There are too few COSMIC profile-pairs with which to draw useful statistics if we exclude pairs with short horizontal separations (see Figure 11). We present results from the largest dataset we have available and discuss biases that may be introduced as a result. See also responses to specific comments 16, 17 and 21.

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## 2 Specific Comments

1. **p.3175, I.4: Reviewer's Comment:** For completeness, the earlier reference Eckermann and Preusse (1999) should be included.

**Authors' Response:** Reference to Eckermann and Preusse (1999) included as requested.

2. **p.3179, I.26: Reviewer's Comment:** It should be pointed out more clearly that at this stage of calculating  $E_p$  the blue dashed curve in Fig. 6 applies, and a certain contribution of  $\lambda_z > 10$  km is still contained in  $T'$ . This is important because later, when discussing Fig. 3, it is claimed that meridional propagation of gravity waves would be seen. Assuming a sharp cutoff at  $\lambda_z=10$ km, mountain waves would become invisible if the background wind  $U$  parallel to the wave vector exceeds  $\sim 30$  m/s. For mountain waves:  $\lambda_z \approx 2\pi U/N$  (Eckermann and Preusse, 1999, Eq. 1)

**Authors' Response:** We have revised this section to include a better description of the vertical scales to which our analysis is sensitive. There is in fact no sharp cut-off at  $\lambda_z = 10$  km. Indeed, as the reviewer suggests, the analysis method is predominantly sensitive to waves with  $\sim 3 < \lambda_z < 14$  km, though wavelengths greater than  $\sim 10$  km will be suppressed by some factor.

3. **p.3180, I.55: Reviewer's Comment:** It is unclear why the gravity wave distribution consisting of more or less randomly distributed temperature fluctuations should be affected by removing coherent planetary scale waves with  $s=1$  or  $s=2$ . Please explain!

**Authors' Response:** Sentence deleted.

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4. **p.3180, I.23-24: Reviewer's Comment:** Why should a 10km window further reduce contributions of planetary waves? If  $T'$  at a fixed altitude is affected by planetary waves, this planetary wave contribution will enter  $E_p$ , independent of the size of the window. Further reduction of planetary waves can only be achieved by separately removing the offset in every 10km window. This is however not mentioned. Please either explain or delete this sentence.

**Authors' Response:** Agreed, sentence deleted. We use a single 10 km vertical averaging window at a single point, not a sliding 10 km window along the whole profile.

5. **p.3181, I.28: Reviewer's Comment:** The reduced sensitivity for gravity waves directly over the mountain ridges is not only an effect of the GPS-RO observation technique. The data analysis technique plays also an important role! Depending on the strength of the background wind, vertical wavelengths of mountain waves can be quite long. These long  $\lambda_z$  waves should be contained in the GPS-RO temperature altitude profiles and can therefore be detected. Also other limb observations with similar observational filter show maximum gravity wave variances over the mountains (for example, Yan et al., 2010). Possibly, the vertical wavelength limitation in your study to only short  $\lambda_z$  reduces the sensitivity to mountain waves, and may in some regions favor waves from sources other than orography. It should therefore be mentioned that increased  $E_p$  over the Southern Ocean could be just an effect of the analysis technique that is limited to short  $\lambda_z$  waves.

**Authors' Response:** We have updated this section to reflect the reviewer's comments. Our filtering method does not preclude the detection of longer  $\lambda_z$  waves directly over the mountains, only underestimates their amplitude. We do acknowledge that this is likely the reason for the differing distributions from other studies

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such as Yan et al. (2010), and we have updated the article to reflect this.

6. **p.3182, I.9: Reviewer's Comment:** Please omit "significant "; up to 10% is not a large fraction.

**Authors' Response:** Omitted as suggested.

7. **p.3183, II.13ff / discussion of Fig. 3: Reviewer's Comment:**

The background wind in the southern polar jet can be quite strong. It is up to 80 m/s in Fig. 3, and it changes a lot from 40S/22km and 20 m/s to 55S/40km and 70 m/s. Therefore it could be doubted that mountain waves are captured by your analysis over the whole range of altitudes and latitudes considered. A discussion of observational effects related to the vertical wavelength range of your analysis should therefore be included in the manuscript. In addition, previous work that supports your findings should be mentioned. Please find below a suggested roadmap for this additional discussion:

- $\lambda_z \approx 2\pi U/N$  (Eckermann and Preusse, 1999, Eq. 1): therefore it might be doubted that mountain waves are captured by your analysis for background winds stronger than 40-50 m/s, taking into account the limited vertical wavelength coverage.
- Still, your analysis will capture mountain waves for even stronger background winds because the background wind vector and the wave vector of the gravity waves will not be exactly parallel (for example, Alexander and Teitelbaum, 2011)
- This is further supported by vertical wavelength observations in the southern polar jet from analyses with larger vertical wavelength coverage. On zonal

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average, these estimates are in the range 10–13 km (for example, Yan et al., 2010; Ern et al., 2011).

- Therefore the slanted vertical column of enhanced Ep in Fig. 3 could be due to mountain waves and could indicate meridional propagation.
- Similar effects have been observed before in other regions (for example Jiang et al., 2004; Ern et al., 2013)

**Authors' Response:** We are grateful to the reviewer for their comments and suggested roadmap for revision. We have included a paragraph in the paper discussing the points raised around Figure 3.

8. **p.3184, II.11-14: Reviewer's Comment:** Which latitudes are considered for the zonal cross-section? The range is given in the figure caption, but should also be given here.

**Authors' Response:** Latitude range included as suggested.

9. **p.3187, II.18-19: Reviewer's Comment:** p.3187, II.18/19: "at least 60% of the root-sum-squared energy of the profile" Is this correct? Shouldn't it read "at least 60% of the spectral amplitude..."? Please check!

**Authors' Response:** We have revised Section 3.1 to provide an improved description of the properties involved with the Wave-ID methodology. Instead of fraction RSS energy, the output of the CWT can be roughly interpreted as a map of pseudo-correlation coefficients between the profile and the analysing wavelet, provided both have RSS energy equal to one and both are zero-averaged. This means we are able to move away from the rather confusing description fractions of RSS energy as described in the discussion paper. The methodology, figures

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and results remain unchanged, only the description of the parameters involved has been improved. To answer the reviewer's specific question, we did mean "at least 60% of the root-sum-square energy of the profile".

10. **p.3187, II.17ff: *Reviewer's Comment:*** Does a squared spectral amplitude threshold of 0.36 / spectral amplitude threshold of 0.6 imply that at a given altitude usually only one wave is selected? Or are multiple selections possible? This information should be included in the manuscript.

***Authors' Response:*** We currently limit this methodology to detecting one (the dominant) wave per profile. This information has been included at the end of Section 3.1.

11. **p.3187/8, *Reviewer's Comment:*** The following should be mentioned: It is assumed that the vertical wavelength does not change much with altitude, which may no longer hold for gravity waves in the real atmosphere if the width of the wavelet gets too large. In particular, long vertical wavelength waves will therefore be selected with lower probability. This may explain the slight mismatch between the histogram in Fig. 6 and the "permitted" range of vertical wavelengths given by the black curve.

***Authors' Response:*** We have included this suggestion and the implicit limitation of the method at the end of Section 3.1.

12. **p.3189, I.23 and elsewhere: *Reviewer's Comment:*** The statement "profiles that did not contain a wave" is too strong, given the limitations of the analysis method. Perhaps replace with "profiles with no wave detected".

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***Authors' Response:*** Replaced as suggested.

13. **p.3190, I.15: *Reviewer's Comment:*** Sector C has quite large longitudinal extent. It would be good to add a sector C',  $\sim 1/3$  of the longitude extent of C, thereby focusing more on the vicinity of the Drake Passage where "primary mountain waves" should cause even higher intermittency.

***Authors' Response:*** Adding another small sector to focus on the Drake Passage region would indeed be of interest, however a number of difficulties exist with regard to including it in the figure, mainly relating to the change requiring an otherwise-unnecessary normalisation of the data. We have run the analysis on a small sector from 80W-60W in the latitude band. The resulting histogram is very similar to Sector C but as the reviewer suggests, a very slightly bias towards larger amplitude waves is evident, possibly consistent with intermittent mountain wave activity. This effect is quite small however.

14. **Figure 9 *Reviewer's Comment:*** In this figure both the number of waves and the number of profiles are indicated below the histograms. Calculating the ratio of these numbers, the fraction of profiles containing an identified wave is around 80%. This is in discrepancy with the number of 25-40% mentioned on p.3188, I.5. Please check and clarify!

***Authors' Response:*** The 25-40% value on p.3188, I.5 is a global, all-year average. During June-August, in the latitude band 40S-65S, the fraction of profiles in which wave-like features were identified is significantly increased, as the reviewer calculates up to  $\sim 80\%$ . We agree that the sentence on p.3188 is not clear enough. We have replaced it with "we find that on global year-long average around 25-40% of profiles contain an indentifiable gravity wave signal. In some regions and seasons, as will be seen later, this fraction can be as high as  $\sim 80\%$ ."

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15. **p.3193, I.8: Reviewer's Comment:** p.3193, I.8: Please mention that  $k_h$  has previously been determined by McDonald (2012) and by Faber et al. (2013) using pairs of COSMIC radio occultations.

**Authors' Response:** References added as suggested.

16. **p.3193, I.22/33ff: Reviewer's Comment:** Horizontal separations as short as 10-20 km are probably too short to reliably determine  $k_h$ . Estimated horizontal wavelengths of 1000 km for gravity waves are quite common in satellite data, as seen in your Fig. 10. For 10-20 km horizontal separation the phase difference between two profiles would be  $360^\circ/(50\dots100)\approx 4^\circ\dots 7^\circ$ . I doubt that the determination of vertical phases is that accurate! The use of small separations will therefore introduce a strong random component, resulting in too large phase differences on average and, hence, underestimation of horizontal wavelengths. This may also explain some of the differences between COSMIC and HIRDLS horizontal wavelengths in Fig. 10. Therefore I recommend to revise Fig. 10 using only longer horizontal separations for COSMIC. I leave it to the authors which range of horizontal separations is suitable, keeping in mind that separations should not be too long, and sufficient statistics is needed for the horizontal distributions in Fig. 10. Possible consequences of too short separations should be briefly mentioned already on p.3193. The discussion on pp.3196/7 should be adapted accordingly.

**Authors' Response:** We agree with the reviewer that short horizontal separations of profile-pairs introduces a large source of error in the determination of  $k_h$ . We have added an Appendix A (with two new figures) to specifically address the validity, possible errors and subsequent bias of the methodology.

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17. **p.3193, II.22/23ff: Reviewer's Comment:** p.3193, II.22/23ff: Horizontal separations as short as 10–20km may result in unphysically short horizontal wavelengths. Are these values used for Fig. 10, or are they filtered out before?

**Authors' Response:** We use horizontal wavelengths between 100-5000 km for the maps in Figure 10. We have included this information in Section 4.1, and justified the lower limit in Appendix A.

18. **p.3195, I.4: Reviewer's Comment:** An illustration of this geometry can be found in Preusse et al. (2009).

**Authors' Response:** Reference included as suggested.

19. **p.3195, II.11-13: Reviewer's Comment:** It is not clear that this estimate of momentum flux is necessarily a lower bound. Your analysis technique focuses on strong wave events, causing an increase of  $E_p$  by a factor of 3-5 in Fig. 7. This might overcompensate the low-bias in  $k_h$  and the restriction to low  $\lambda_z$  values introduced by the analysis technique. Suggestion: omit the final sentence in Sect. 4.1.

**Authors' Response:** Agreed, sentence omitted.

20. **p.3195, II.20/21: Reviewer's Comment:** Theoretically, COSMIC and HIRDLS should be sensitive to about the same part of the gravity wave spectrum (Preusse et al., 2008, Sect. 5).

**Authors' Response:** There are subtle differences in the useful stratospheric vertical height windows, but generally agreed. Changed "different but overlapping" to "overlapping".

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21. p.3196, ll.16ff: **Reviewer's Comment:** p.3196, ll.16ff: Possibly, differences in the geographical distribution of  $\lambda_H$  are caused by the short horizontal separations used for COSMIC. This should be checked. See also specific comments 16 and 17.

**Authors' Response:** This was checked during the analysis, and has been checked again. We could find no link between the geographical distribution of COSMIC  $\lambda_H$  and horizontal separation,  $\Delta r$  despite a link being present in absolute terms as discussed in Appendix A. It is likely therefore that the error in  $\lambda_H$  estimation exceeds any geographical structure.

22. p.3197, l.8: **Reviewer's Comment:** p.3197, l.8: "slightly lower" to "considerably lower" (the horizontal wavelength scale differs by about a factor of two!)

**Authors' Response:** Agreed, changed.

23. p.3197, l.25: **Reviewer's Comment:** This is not an effect inherent in the HIRDLS observations. HIRDLS and GPS-RO observational filters should be similar. Suggestion: "since HIRDLS generally resolves" to "since our HIRDLS analysis generally resolves".

**Authors' Response:** Agreed. Changed as suggested.

We additionally would like to thank the reviewer for their detection of typographical errors.

In addition to the changes requested by the reviewers, we have also made some small changes to the structure of the abstract, introduction and conclusions, with the aim of providing a better scientific context for the work undertaken.

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 3173, 2015.

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