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Interactive Comment

Interactive comment on "A new approach to global gravity wave momentum flux determination from GPS radio occultation data" *by* A. Faber et al.

Anonymous Referee #1

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General Comment

In their paper the authors introduce a new method to derive horizontal wavelengths of gravity waves. By combining three satellite soundings, this 3-point method has the potential to provide more accurate horizontal wavelength estimates. The 3-point method is applied to radio occultations of the COSMIC constellation of GPS satellite receivers. Global distributions of gravity wave amplitudes, horizontal wavelengths and momentum fluxes are derived. The problems that may arise in the different steps of the data processing chain are described in detail in the introduction and section 2.

Therefore the paper is potentially publishable in AMT. There are however two major

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concerns regarding the quality of the results that can be obtained with current data, and how this quality could be judged. These major concerns have to be addressed before publication in AMT.

Major Comments

(MC1) The horizontal spacing of 10-15deg is chosen way too large! Therefore the shown distributions of horizontal wavelengths and momentum fluxes are not reliable!

Of course, it makes sense to show these results as a demonstration of the newly introduced method. It should however be stated more clearly that the statistics of the currently available data is still too sparse to produce more reliable results.

(MC2) No measure or reference is given to judge whether the 3-point method works satisfactorily, especially regarding MC1. As a reference, in Figs.6–9 the authors should also provide results of a standard 2-point method for the same data set, but using small horizontal spacings of $<$300km between the 2 points.

The 2-point data set can be used as a reference, because 2-point methods will systematically overestimate the horizontal wavelength. If successfully applied, the 3-point method should provide always shorter horizontal wavelengths than the 2-point method.

If no such reference is given, the reader is left alone and is unable to decide whether, for a given data set, applying the 3-point method is an improvement over the 2-point methods currently used.

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Specific Comments

1. p.2, l.14 and further on: The expression “triad” might be somewhat misleading, because in atmospheric physics it is also used for cases of resonant wave-wave interactions (Wuest and Bittner, JASTP, 2006). Maybe, this expression should be replaced throughout the paper, for example by “3-point method”.

2. p.3, l.14/15: “footprint” is not a good expression here
   Suggestion: “the sampling of GPS is more irregular in space and time than other techniques, such as limb or nadir scanning of atmospheric emissions from satellite.”

3. p.4, l.24: λ_h is not explained!
   Suggestion: ...λ_h, the true horizontal wavelength of a gravity wave, at least...

4. p.5, eq.3: It looks like a factor 2 is missing in this equation!
   Also the momentum fluxes in Fig.9 could be too low by this factor of 2! Please check!

   Ep as defined before in eq.1 contains T’, the temperature fluctuation, while the equation for momentum fluxes requires temperature amplitudes, therefore:

$$|\text{MF}| = \mathbf{2} \times \rho \frac{\lambda_z}{\lambda_h} E_{pot}$$

5. p.6, l.5: Please add the reference Preusse et al., AMT, 2009. In this paper the geometry of gravity wave detection from satellite is illustrated in more detail.

6. p.6, l.23: What is the advantage of a CTW over performing simply a FFT to determine zonal wavenumbers 0-6 from the gridded data?

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7. p.6, l.23: How many data points are typically falling into one of the 10x15 deg fields?
If these are too few, a clear separation of planetary and gravity waves might no longer be possible. I suppose there is enough statistics outside the tropics. However in the tropics the COSMIC data coverage is strongly reduced, and the quality of the finally obtained gravity wave variances might be worse.
At least a cautionary note should be added in the text!

8. p.7, l.16: See also Major Comment MC1
The horizontal spacing of 15deg (1500km at the equator!) is much too coarse! Most gravity waves will be severely under-resolved (aliasing). Horizontal wavelength distributions derived by using such large spacings will be almost meaningless!
Several studies suggest spacings $<$300km. For example, in McDonald, JGR, 2012, Fig.6, the gravity wave occurrence frequency in COSMIC observations strongly increases at horizontal spacings $<$300km.
Because of its relevance, please add this reference to your paper!

9. p.8, l.4: Why is the vertical wavelength difference chosen so low?
I suppose this must have something to do with the range of vertical wavelengths that is covered in your study. This range is however never mentioned.
Please state clearly which interval of vertical wavelengths is investigated!

10. Fig.1d: Please provide a color bar with units!
In the caption of Fig.1: horizontal $\rightarrow$ vertical
and one "(d)" should be removed in the caption

11. p.9, l.3: The phase shifts are also known, no information on xi,yi and dx is needed for that. Suggest to rewrite as follows: "... are known, and also the phase shifts..."

12. p.10, l.2ff: This formulation confuses me, please rewrite!

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Suggestion (is this more correct?):
"When displacing the black sinusoidal along the line connecting the brown and blue points into the same direction as before, we would obtain the grey sinusoidal line. The resulting slope..."

13. General comment regarding section 3:
    Although everything may be correct, the problems arising from the evaluation of phase differences for different groupings/arrangements of points is written down in a very complicated way.
    I do not know whether I got the key point right, but if so, please add a statement like the following:
    "When evaluating the phase differences, inconsistencies may arise from wraparound effects. Depending on the reference point, due to the periodicity of the problem, the same phase difference could be regarded, for example, as either very small, or close to 2π. This effect is accounted for by evaluating all possible combinations of a considered group of three points, and sorting out the inconsistent combinations."

14. p.10, l.17: To make sure that the same wave is observed, probably one should be more worried about the dx criterion than about the dt criterion (see also McDonald, 2012).
    The main limitation of the dt criterion is the phase progression caused by the wave frequency within this time difference. This effect could easily bias the phase differences between two soundings of the same wave! Please change the text accordingly.

15. p.10, l.24ff: Why are very long values of λ_h sorted out before forming the groups of three points? This could be a valid solution!
    It could easily happen that two of the three points are aligned along a line of constant phase of a real wave. The "true" horizontal wavelength would then be

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available from the information added by the third point.

16. p.11, l.4ff: Why are so small (5x5 deg) grid boxes used for the global maps, given the facts that:
    (a) the number of points is very small
    (b) the horizontal resolution inherent in the method is much worse: For defining groups of 3 points regions of 10x10 or 15x15 deg were used!
    Possibly, using larger 10x10 deg grid boxes would solve many problems. Anyway, many of the horizontal patterns are larger than 5 deg, probably because of the large regions used for grouping 3 points.

17. p.11, l.9ff, about Fig.6: A statement commenting the sensitivity of the horizontal wavelength on the size of the lon/lat regions is missing and should be added! See also Major Comment MC1.

    Suggestion:
    "The fact that the horizontal wavelength is strongly dependent on the maximum distance limit shows that the global distribution of gravity wave horizontal wavelengths cannot be reliably determined with the large distances required to obtain sufficient statistics."

18. p.11 and in the following: As a reference, please always show corresponding results of a 300km 2-point method in Figs.6–9. See also Major Comment MC2!

19. p.13, l.5–7: How does Ep compare with the distribution shown in John and Kumar, GRL, 2013, accepted?
    In John and Kumar (2013) there is no maximum of Ep at the equator if planetary waves are removed from COSMIC by a similar horizontal fitting procedure. Instead, maximum variances during NH summer are observed at around 15N. This discrepancy suggests that there is still some uncertainty in removing planetary waves, at least in the tropics. This should be mentioned in the revised manuscript.

20. p.13, l.8–19: It makes no sense to discuss details of the λ_h distribution, because λ_h is too strongly high-biased!
For instance, the mentioned regions of short λ_h over land coincide with regions of shorter 3-point distances dx on average (see Figure 7c). Obviously variations in the distribution of λ_h rather reflect variations in dx than variations of λ_h in the real atmosphere!

    Because most of the features seen in the λ_h distributions are probably not robust, this again shows the requirement of a kind of reference! Please include according figures for a short-dx 2-point method and discuss the differences.
    See also Major Comment MC2.

21. p.14, l.14: The first sentence of the Conclusions section is somewhat out of place. Momentum fluxes have been derived before from GPS RO data (Froehlich et al., 2007; Wang and Alexander, 2010).

22. Figure 7a: In this figure the "number of triads" in the tropics is usually very low. But at (0,0)deg lon/lat there is a spot of very high values. This really looks strange! Please check whether this is an artifact. If this is not the case, please add a short explanation why there is an enhanced number of RO.

Technical Comments:

1. p.5, l.11: spacial → spatial
2. p.6, l.9: pos-processed → post-processed
3. p.8, l.18: omit comma after "2b"
4. p.10, l.4: is not confirm with → is different from

5. p.11, l.23: then → than
6. p.12, l.6; to → too
7. p.12, l.8: presented for → attributed to
8. p.12, l.18: then → than
9. p.13, l.10: fads in other → requires different
10. p.13, l.19, 24: spacial → spatial
11. p.20, caption of Fig.2: od → of

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 2907, 2013.

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