Atmos. Meas. Tech. Discuss., 6, C2063–C2067, 2013
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Atmospheric Measurement Techniques Discussions

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

Received and published: 15 August 2013

General Comment

I agree with Reviewer 2 that the manuscript is much improved now. In particular, the newly introduced Figure 6 shows that improved horizontal wavelengths can be obtained by applying the 3-point method.

There are however still major concerns about details of this comparison.

(1) For the 2-point method also a phase difference limit has to be introduced, like for the 3-point method

(2) Results in section 5 are for 10deg gridding, the statement derived there does not

generally hold for the 15deg gridding used later

There are also several important points that are still not addressed in the revised manuscript that was provided as a supplement in the Interactive Discussion.

These concerns have to be addressed before publication in AMT. For details see below.

Detailed Comments

Numbers refer to my previous "specific comments".

about SC4: *factor of 2 missing in equation 3*

I disagree with your reply! I think that indeed a factor of two is missing in your equation 3!

In Wang and Alexander (2010) T' is NOT the temperature fluctuation. In this paper T' is the temperature amplitude, see the first sentence in their section 3.2. Equation 9 in Wang and Alexander and equation 7 in Ern et al are basically the same. As far as I understand, T' in your paper is the temperature fluctuation

$$T'(z) = T(z) - \overline{T}(z)$$

as defined by Schmidt et al., GRL, 2008, equation 2. The average over T'^2 is the temperature variance.

Because in your paper T' is the temperature fluctuation, and not the temperature amplitude, a factor of 2 has to be added in your equation 3.

about SC7: *How many data points are typically falling into one of the 10x15 deg data fields?*

In your reply you state that "The number of data points is not large..."

Please give numbers for the 1-day gridding at midlatitudes and at the equator.

about SC10: *Fig.1d: Please provide colorbar with units!*

In the caption of Fig.1 you state that Fig.1d shows "the amplitude at each altitude ... for the dominant vertical wavelength". The colorbar that is given in the revised manuscript however gives the phase shift. Please provide a colorbar for amplitudes!

about SC14: *Phase progression caused by the wave frequency is an additional error source for the horizontal wavelength because the soundings are at different times. Therefore dt should be much shorter than the wave period.*

This is still not mentioned in the revised manuscript! Please include!

about SC18: *results of a 300-km 2-point-method should be added*

I think it is a good idea to introduce another section (section 5) dedicated to this comparison. This section should however be better embedded in the manuscript.

For example, please provide the following information:
Are horizontal wavelength and momentum flux in Fig.6 for the SH summer season DJF (corresponding to Figures 8–10, right side)?
Are horizontal wavelengths in Fig.6b the same as in Fig.7a?
If so, please state this clearly in both text and figure caption.

The reference McDonald, JGR, 2012 should be included in this section!

Suggestion: add the following sentence in l.402, latest version of the manuscript

"... maximum value of 300km (Fig.6c). The 300km-limit has been selected because the probability of observing the same wave event increases for spacings shorter than this (McDonald, 2012). Using this 300km-limit, the determined projected..."

Citation: McDonald, A. J. (2012), Gravity wave occurrence statistics derived from paired COSMIC/FORMOSAT3 observations, J. Geophys. Res., 117, D15106, doi:10.1029/2011JD016715.

There are two major concerns about the 2-point/3-point comparison in the new section 5.

(1) For the 3-point-method soundings with small phase differences of $<$0.5 rad are discarded. This has obviously not been done for the 2-point-method. If no such limit is introduced for the 2-point method the two methods cannot be directly compared!

Therefore please use the same 0.5 rad limit for Figs.6a and 6d. For Figs.6c and 6f this limit could be reduced to 0.15 rad because of the shorter horizontal spacing between the soundings.

(2) In new section 5 the 2-point method is compared to the 3-point-method using 10x10 deg intervals. This indeed shows that this **10 deg** gridding can provide shorter horizontal wavelengths than the 2-point method.
Your conclusion however is that the 3-point-method with the **15 deg** spacing is generally more realistic than the 300 km 2-point method. Two concerns:

(a) I think that this general statement does not hold!

At mid and high latitudes average values of horizontal wavelengths for 300 km-2-point and 15 deg-3-point are quite similar (about 2300km). Only at latitudes $<$20...30 deg the 15 deg-3-point wavelengths are shorter on average.

(b) At this point 15 deg-3-point results have not yet been shown

about SC20: *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 8c, revised manuscript).*

I noticed that the discussion of horizontal wavelengths in section 6 (revised manuscript) has been left unchanged.

I am really concerned that even details of the global distribution are discussed as if there were physical reasons for these details (land-sea distribution).

Particularly the statement that the horizontal wavelength in the winter hemisphere over land is shorter than over ocean and also shorter than in the summer hemisphere is not well supported.
This statement comes mainly from the zonal structures in Fig.10a at latitudes between equator and 40S. Very similar structures are however found in Fig.8c in the average horizontal distances. These variations of the average distances will influence the horizontal wavelengths in Fig.10a. Also from Fig.7 it can be seen that the horizontal wavelength strongly depends on the average distance.

Therefore it cannot be excluded that the structures seen in Fig.10a are not real.

During the DJF season with the more homogeneous distance distribution (Fig.8d) similar structures are not seen (Fig.10b).

I suggest to drop this point from the discussion or state clearly that this could be an effect caused by variations of the average horizontal sampling distance.

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