Atmospheric
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Discussions

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

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Dear Reviewer No.1,

Thank you very much for the helpful comments and suggestions.

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 $T0(z) = T(z)$ ôĂĂ $T(z)$ 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.
AC:
Thank you again for this comment. We now recalculated the potential energy distribution with the temperature amplitude instead of the temperature fluctuation. Therefore also the momentum flux is calculated using the temperature amplitudes as in the paper of Wang and Alexander 2010. The graphics and descriptions are changed accordingly.

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.
AC:
For the mid-latitudes there are around 30 profiles within one day in a box 10x15°, for the equatorial region, there are about 16 profiles within one box for example for day 01/29/2007. For 01/02/2007, where there are less occultations, the no. decreases to 20 and 10 profiles at mid and low latitudes, respectively. For the mean number of 2000 profiles daily, a mean number of 10 to 20 profiles per bin in the equatorial region were located.

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!
AC: We are very sorry, that we did not see that error before, the color bar is correct, only the caption within the figure was wrong. The phase shift is displayed in that figure.

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!
AC:
We included this statement in the revised version.:

Another reason for small time windows is, that the phase progression caused by wave frequency is a additional error source for the horizontal wavelength determination. Therefore the time window should be much smaller than the wave period.

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.
AC:
Thank you for that important comment. Additional information about the graphics will be included in the revised version.
The horizontal wavelength in Fig. 6b is the same as in 7a but with different scalings and color bars to provide a better comparison to the horizontal wavelengths derived from the 2-point method. Therefore the corresponding results of the momentum flux are related to the $10°$ spacing as in Fig. 7a but not to the once of Fig. 8-10. These information will be included in the new Section.
Also the suggested sentence and the reference to the McDonald, 2012 paper are

included.

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.*
AC:
For the new version the 0.5rad phase difference minimum is also applied to the 2-point method. Therefore the results of the 2-point method differ slightly from the one of the last version. The text is changed accordingly.

(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.
AC:
Thanks for that comment. We changed this sentence to the following to weaken the statement.
:
The comparison to the horizontal wavelength derived from the 2-point method (Fig. 6) states clearly, that even though the spacing of $15°$ is rather big, the results can match those from the 2-point method or sometimes also provide a detection method for shorter wavelengths than the

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2-point method.
(b) At this point 15 deg-3-point results have not yet been shown.
AC:
The horizontal wavelength for the 15deg spacing were already shown at the end of the Sensitivity Section. The graphic of the new Section was accidently included in the wrong order.

about SC20: It makes no sense to discuss details of the lh distribution, because lh is too strongly high-biased! For instance, the mentioned regions of short lh 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.
AC:
Thanks again, we will include the hint of the influence of the sampling in the revised version.:
Since there is also a small correlation between the mean distance distribution of the three point groupings and the derived horizontal wavelengths, the discussed variations might be influenced by the sampling method.

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Thank you very much again.
Best regards,

Antonia Faber

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