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.2,
Thank you very much for your suggestions and comments.
Please find below the detailed response to your comments:

The primary concern with the manuscript is that it fails to appropriately credit previous work, and gives a misleading impression that the approach here is new in ways that are not new.
AC:
This work is new in a way of making use of all available information in the rather

coarse dataset without wasting information by over-determination. Also new is the geometrical argumentation of the derivation of the horizontal wavelength, where the inner angles of the 3-point grouping plays an important role. We feel, that these points are significantly new to the research community and deserve to be published.

Please forgive the current "Poor" rating for category "2) Scientific Quality". This rating is due to the poor representation of what is new here relevant to previously published related work. If this were corrected as suggested below, I would give the paper a higher rating.
AC:
In the revised paper, we will make it more clear, what is new in comparison to the Wang and Alexander (2010) paper, which is not only a minor change but in the spacing algorithm but also an optimization of the approach of Wang and Alexander (2010), since this 3-point method makes use of all available information without over-determination within the derivation of the horizontal wavelength.

Reviewer No.1 has stated what is new about this work is that "the 3-point method has the potential to provide more accurate horizontal wavelength estimates." The title, abstract, and body of the paper all give the impression that this is a "new approach". Instead, this 3-point method was already published and described in detail using GPS data in Wang and Alexander (2010). It seems this previous work was not properly cited, and hence gives the wrong impression to readers like Reviewer No.1 that this is new. Equation (4) summarizes the 3-point method, and it matches Wang and Alexander (2010) (their equation (6)), but this paper is not referenced anywhere in this section, so it should be referenced there.
AC:
Thank you for that comment. Yes, Wang and Alexander (2010) also used this equation, and a citation is added now, but the calculation method varies. They used a least

square fitting algorithm to all profiles within a $15°$ lat/lon area. We always use three measurements, which often do not exhaust the $15°$ lat/lon spacing, thus avoiding an over determination of the problem and so optimizing the available limited information, given the considerably sparse data density. For the mean lat/lon point of each 3-point grouping, the horizontal wavelength is derived. The calculation is different from the least square fitting. Therefore the following is added in the text after Eq.4:

This equation is also the basis of the least square fitting method which Wang and Alexander (2010) used to determine the horizontal wavelength. Here, derivation of the horizontal wavelength is derived using alway only three measurements, thus avoiding an over determination of the problem and so optimizing the available limited information, given the considerably sparse data density.

The methods used in the present manuscript to derive horizontal wavelength, vertical wavelength, potential energy, and momentum flux of gravity waves using GPS radio occultation data are very nearly identical to those in Wang and Alexander (2010). Differences in the new method are very minor, and summarized in one paragraph on page 2911. The criterion for grouping profiles in order to determine horizontal wavelength is slightly different here. This is a minor change, and hence the title of the paper is misleading. It is not a "new approach". The title should be changed. Suggestion: "On the determination of gravity wave momentum flux from GPS radio occultation data".
AC:
The change in the calculation of the horizontal wavelength, which leads to a higher resolution is the results is not only a minor change to the method of Wang and Alexander (2010). The changes in the temporal spacing however are only minor changes to the Wang and Alexander (2010) paper. The derivation of parameters like the potential energy and vertical wavelength are similar to the steps of Wang and Alexander (2010) which is reasonable. The title will be changed.

<div align="center">C2000</div>

The abstract says that the "method follows Ern et al. (2004)". While this was an important work that suggested the use of wave phase variations between adjacent profiles to estimate horizontal wavenumber, this older work did not estimate true horizontal wavenumber, but only apparent horizontal wavenumber along one line between two adjacent profiles. The current work instead follows Wang and Alexander (2010), utilizing the variable sampling of GPS to advantage, and estimating the true wavenumber from multiple profiles. The only substantial difference is the use of a different criterion to select which profiles to group for the wavenumber calculation. The abstract should be changed. Instead of referencing Ern et al. (2004), Wang and Alexander (2010) should be referenced here.
AC:
The equation for the determination of the horizontal wavelength is first presented by Ern et al. 2004. Wang and Alexander (2010) split this equation into the horizontal parameters of the horizontal wave vector. The basis is still the equation of Ern et al. 2004. In addition, the computation of the horizontal wavelength of this work dose not agree with the least square fitting algorithm applied by Wang and Alexander (2010′). Since this work can also be seen as an optimization of the work by Wang and Alexander (2010), the abstract will be changed.

Wang and Alexander (2010) also discussed different criteria for grouping profiles (their paragraph [19]), and reported the same result that is shown in the present work in Figure 6. This earlier result should be referenced where Fig. 6 is introduced on page 2917, near line 10.
AC:
Yes, the same comparison has been applied to the computation method by Wang and Alexander (2010). But it should be noticed that the absolute values do not agree. Only the change in the absolute values when decreasing the search spacing agrees for both methods. Also the results by Wang and Alexander (2010) only tell the change in the absolute value of the momentum flux, not about the horizontal wavelength itself. A

direct comparison is therefore not reasonable.

The present manuscript uses a wavelet method and cross-spectrum between profiles very similar to Wang and Alexander (2010) to find the dominant vertical wavelength and amplitude. The Ern et al. (2004) method instead looked for a match in the peaks of two independent spectra derived from two adjacent profiles, and they discard profiles that do not match. The present method is much more similar to Wang and Alexander (2010) than Ern et al. (2004). Suggest changing the reference on p. 2914, near line 5.
AC:
This reference relates to the limitation of the vertical wavelength when comparing close-by profiles with respect to the same wave origin. There is not such limitation in the Wang and Alexander (2010) paper. Therefore I cannot change the reference.

Geller et al. (2013, J. Climate, early online release) has reported on some important differences in the Ern et al. (2004) method for matching waves in adjacent profiles from the wavelet cross-spectrum method similar to the one used in the present work. The Geller manuscript also discusses how selection criteria that reduce the number of measurements gives higher momentum flux values in a map of mean values. This Geller et al. (2013) work should be referenced near Fig. 9c and d (page 2919-20).
AC:
Thank you for that suggestion. The Geller et al. 2013 paper will be referenced in the revised version.

The Wang and Alexander (2010) paper concluded that the GPS data sampling was rather too coarse for accurate determination of gravity wave parameters. Reviewer No.1 apparently agrees. I agree with Reviewer No.1 on this point, and the Wang and Alexander (2010) conclusions should be referenced in the revised manuscript.
AC:

Yes, the data density is rather coarse to determine horizontal wavelengths which are outstanding results. However the change in the computation algorithm to the work on Wang and Alexander (2010) improves the resolution to $5x5°$ instead of $15x15°$ from Wang and Alexander (2010). This work is therefore more a optimization to the suggested method my Wang and Alexander (2010). A reference of the conclusion of Wang and Alexander (2010) is given in the Conclusion section of the revised paper.

All the editorial comments and the reference are included in the new version.
Thank you very much again.
Best regards,

Antonia Faber

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/6/C1998/2013/amtd-6-C1998-2013-supplement.pdf
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