

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 #2

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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.

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.

Reviewer #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

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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 #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.

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”.

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.

Wang and Alexander 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.

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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.

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).

The Wang and Alexander (2010) paper concluded that the GPS data sampling was rather too coarse for accurate determination of gravity wave parameters. Reviewer #1 apparently agrees. I agree with Reviewer #1 on this point, and the Wang and Alexander (2010) conclusions should be referenced in the revised manuscript.

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

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