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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Enhanced internal gravity wave activity and breaking over the Northeastern Pacific/Eastern Asian region" *by* P. Sacha et al.

P. Sacha et al.

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We would like to thank Referee #2 for the comments. Although we did not fully agree with all of them, they helped us to improve our paper.

1) First, authors need to justify that the observed density perturbations are gravity-wave (GW) perturbations. Since authors are discussing GW potential energiy (Ep) and GW breaking simultaneously, it is not easy for this reviewer to clearly understand whether the Ep presented is indeed due to gravity waves or due to turbulent motions. It seems necessary to separate stably stratified cases and convectively (or dynamically)

unstable cases from the GPS-RO basic-state profiles and then to re-compute Ep using the density perturbations that are believed to be GW perturbations in a stably stratified (or dynamically stable) environment.

We do not agree with this comment since we see the turbulence as acting rather to lower wave amplitudes and so, although physically present during each occultation, its contribution to the computed potential energy of disturbances is mainly indirect. And also, as we are in stratosphere, it seems absolutely unnecessary to separate the stable or unstable cases, because the background stratification in this altitude region will be in all cases stable. We can easily verify this, but we think that it is not necessary to show this in the paper. The possible instability detected in the profiles is only local, caused by the IGW existing in a stably stratified background. For a better illustration we will add to the supplement a new figure showing a typical profile of the density perturbations.

2) Second, authors claim that using non-linear color scaling in their plots is a key to find out the unprecedented GW activity in the East Asian region, but it is hard to believe. Authors need to show comparison between their original plots and some results plotted with linearly scaled colors after appropriately separating GW perturbations from the GPS-RO density profiles as mentioned above.

We would like to thank the Referee #2 for this comment. After comparing all the original plots with new figures plotted with linearly and also quartile scaled colors (all created using IDL), we must confess that an influence of the color scaling on the visibility of the" region of interest" is minimal. Our previous assertion stemmed from differentials between graphics produced by two different software tools (IDL, Panoply). Therefore we delete the following paragraph:

Page18304, Lines 1-7: The presented results also illustrate an important role of a proper visualization approach. As described above, we use the color-scale derived from the relative frequency of the detected values. Employing e.g. a linear scale would significantly reduce the visibility of the area of interest and without previous knowledge it could be almost unidentifi able. This is also one of the unique aspects of our analysis that we were purposefully looking for anomalous wave activity in the region of interest, which was hypothesized due to anomalies found in the zonal wind, temperature and ozone fields. We left only one sentence: One of the unique aspects of our analysis is also that we were purposefully looking for anomalous wave activity in the region of interest, which was hypothesized due to anomalies found in the zonal wind, temperature and ozone fields.

3) Third, authors claim that mountain waves are primary waves in the unprecedented GW activities revealed through the GPS-RO observations in the Eastern Asia. However, it is unclear to reviewer that mountain waves are able to extend far eastward to regions where there is no strong horizontal wind. Note that mean horizontal wind is not so strong in the "region of interest" as authors have shown in their manuscript.

We are not aware that we need the mountain waves to extend far eastward in our study. In most cases the enhanced IGW activity is observed directly above or in close vicinity of significant topography. The reviewer may be suggesting that we should investigate if the mountain waves can freely propagate from the topography in our region of interest employing e.g. linear Scorer parameter. Such an analysis could be an interesting idea for future research, but it would need to be done as a case study. Moreover, the results of de la Torre and Alexander (2005) suggest that such an analysis results in rather negligible restrictions on the horizontal wavelength of propagating mountain waves. The aim of this paper is rather to provide the scientific community with information about existence of this hotspot and to encourage future research in this region. Nevertheless, as discussed under the next comment, we agree that orographic waves not necessarily need to be the prime source of IGW activity in this region and we are giving more credit to the spontaneous adjustment processes now.

de la Torre, A., and P. Alexander (2005), Gravity waves above Andes detected from GPS radio occultation temperature profiles: Mountain forcing?, Geophys. Res. Lett., 32, L17815, doi:10.1029/2005GL022959.

4)Finally, in terms of possible source mechanisms, reviewer recommends that authors should discuss more the possibility of spontaneous adjustment process. References mentioned about the spontaneous wave generation are too out-dated. There are some active scientists such as Fuqing Zhang and Riwal Plougonven who have researched for a long time on the spontaneous generation of gravity waves around the tropospheric jet axis. As long as mountain waves are not easy to be believed to be major gravity waves, spontaneous generation mechanism is certainly worth being described.

We would like to thank the Referee #2 for this recommendation. We agree that the spontaneous generation of gravity waves around the tropospheric jet axis should have been mentioned and emphasized, together with a citation of the review of Plougonven and Zhang (2014). Two sentences are significantly revised and changed:

Old: Page18303, Lines 2-13: Considering the wind field in the region of interest, the Doppler shifting plays a role in amplifying wave amplitudes while propagating upwards and it also may be accounted for one of the possible sources of enhanced wave activity in this region. Finally, in connection with the in situ wave generation in the upper troposphere/lover stratosphere of the region of interest, there is likely a contribution to the IGW spectra from geostrophic adjustment processes connected with the jet stream location there. According to Mohri (1953), during the colder season the subtropical jet stream reaches the maximum intensity south of Japan, while north of the Tibetan Plateau the polar front jet is located. Moreover, these jets sometimes merge (mainly in winter) and create extreme thick frontal layers (Mohri, 1953). Such episodes can become a very interesting and unique source of IGW in this area.

Now: Considering the wind field in the region of interest (seasonally dependent location of the subtropical westerly jet and the polar front jet in the upper troposphere/lower stratosphere), the Doppler shifting must play a role in amplifying wave amplitudes while propagating upwards and it also may be accounted for one of the possible reasons of the enhanced wave activity in this region. Finally, in connection with the jet location above the region of interest, we can expect a strong contribution to the IGW spectra from spontaneous emission processes (Plougonven and Zhang, 2014). Evidence for this claim can be found e.g. in Hirota and Niki (1986) and Sato (1994) who analysed Middle and Upper atmospheric radar data (located at Shigaraki, Japan, falling into our region of interest) and who found inertia-IGW propagating upward and downward from the jetstream. Orographic waves were also identified. As a curiosity, according to Mohri (1953), during the colder season the subtropical jet stream reaches the maximum intensity south of Japan, while north of the Tibetan Plateau the polar front jet is located. Moreover, these jets sometimes merge (mainly in winter) and create extremely thick frontal layers. Such episodes can become a very interesting and unique source of IGW in this area.

References added: Plougonven, R., and F. Zhang (2014), Internal gravity waves from atmospheric jets and fronts, Rev. Geophys., 52, doi:10.1002/2012RG000419.

Hirota, I., and T. Niki (1985), A statistical study of inertia- gravity waves in the middle atmosphere, J. Meteor. Soc. Jpn., 63, 1055–1065.

Sato, K. (1994), A statistical study of the structure, saturation and sources of inertio-gravity waves in the lower stratosphere observed with the MU radar, J. Atmos. Terr. Phys., 56 (6), 755–774.

Specific comments:

At line 19, page 18287: Author need to clearly show the region of interest in their plot rather than vaguely mentioning like "a tilted ellipse".

In the next version the tilted ellipse will be plotted over all figures.

At line 13, page 18288: There has been a number of -> There have been a number of
At line 25, page 18288: Kuroshiro -> Kuroshio

Thank you very much, we will implement your suggestions.

From line 27, page 18292: Authors justifies the use of density profiles instead of us-

ing temperature mentioning density profile includes non-hydrostatic waves. However, in page 18293, authors claim that their wave modes may possibly have vertical wavelengths of 2-5 km. Discussion about non-hydrostatic waves seem unnecessary and confusing.

On page 18293, line 17, we write that the most energetic modes of the IGW spectrum in the lower stratosphere are likely to have vertical wavelengths from 2 to 5 km, referencing Fritts and Alexander (2003). But as the potential energy of disturbances (Ep) is computed from the whole spectrum we cannot rule out the possibility that even the nonhydrostatic modes can influence the spatial distribution of Ep. The advantage of using density profiles is then obvious and does not need further justification. It simply bears more information than the dry temperature data. Nevertheless, we have found a little inconsistency in our paper: On page 18293, lines 9-10, we justify the absence of the lower boundary of the vertical wavelength cutoff by assuming the noise to be almost independent of geographical location. But as shown by Marquardt and Healy (2005), the noise can be variable even in the zonal direction probably due to the small-scale plasma irregularities.

Therefore we change the statement (page 18293, lines 9-10) to: (We assume the noise to be almost independent of the geographical location, which is, however, generally not true (Marquardt and Healy, 2005) and so our calculated distributions of IGW activity can be partly affected by the spatio-temporal distribution of noise.)

Nevertheless, we still use no vertical wavelength lower boundary cutoff, because as shown by Wu (2006), even at small vertical wavelengths there are imprints of important wave effects.We are currently preparing a study that should investigate the differences between IGW activity and power spectral density from GPS RO density and temperature data (amplitude and modal filtration due to the use of hydrostatic balance and an influence of this filtration on the noise levels).

Marquardt, C. and Healy, S.: Measurement Noise and Stratospheric Gravity Wave

Characteristics Obtained from GPS Occultation Data, doi:10.2151/jmsj.83.417, 2005.

Dong L. Wu, Small-scale fluctuations and scintillations in high-resolution GPS/CHAMP SNR and phase data, Journal of Atmospheric and Solar-Terrestrial Physics, Volume 68, Issue 9, June 2006, Pages 999-1017, ISSN 1364-6826, http://dx.doi.org/10.1016/j.jastp.2006.01.006.

At line 7, page 18294: VanZandt (1985) did not mention about a theory about the partitioning of kinetic and potential energy of gravity waves. The partitioning is quite empirical rather than theoretical.

We do not directly refer to VanZandt (1985). In the paper we state that Tsuda et al. (2000) referenced VanZandt (1985) for a theoretical evidence that the ratio between kinetic and potential energy is approximately constant. Further in our paper we show, that by disregarding the crucial assumption of saturated IGW spectrum, the potential energy of disturbances is an imperfect proxy for wave activity.

At line 1, page 18296: Description about the maximum growth rate of Rayleigh-Taylor convective instability is confusing. How gravity waves drive the fluid to be overturning when the value of sigma is real? In fact, authors described instability due to gravity waves using negative values of the sigma in their figure 7.

We thank the referee very much for this notification, because in section 3.3 we forgot to mention that we are plotting and analysing sigma squared (which indicates instability in case it has positive values).Corrections: Page 18301, Lines 3, 6, 9, 14, 17 and the legend of Fig. 7: sigma changed to sigma squared

Description about figure 7 is pretty confusing. There is no secondary maxima shown in figure 7, but authors are explaining a lot about the secondary maxima without describing anything about 8-th or 12-th maxima shown in figure 7.

The confusion stems probably from our naming convention when we termed the 2nd, 3rd, 4th - 12th highest values secondary maxima. To clarify this we will adopt the

suggestion from Referee #1: caption of Fig.7 and S13: not only secondary maxima are shown, suggested rewording: selected (secondary) sigma maxima -> primary and selected secondary (i.e., higher order) sigma maxima

Also, we add a new figure showing the evolution from the first to the 12th maxima to the supplement.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 18285, 2015.