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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.

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We are grateful for the reviewer's positive review, valuable comments and suggestions, which helped us to improve the quality of the paper. Following are our replies to all of your comments:

Main comments (1) The time intervals of gravity wave analysis (2007-2010) and of the analysis of annual cycles in reanalysis data (1979-2013) are very different, and there is considerable interannual variability at high northern latitudes during autumn and winter. For consistency, it should therefore be checked whether the anomalies of the annual

cycles are also found for the shorter time interval 2007-2010.

As the referee suggested we have analyzed the reanalysis data also for the shorter time interval 2007-2010 (see the supplement to this response). The features are similar and maybe even more distinct than for the interval 1979-2013.

We added a paragraph starting at the page 18298, line 23. Also in connection with the short comment from Jan Laštovička we added two sentences to reference the findings of Kozubek et al. (2015) regarding interannual variability in this region:

Now: Finally, it should be noted that the time interval of the analysis of the MERRA dataset is much longer than the interval of the following IGW analysis. For consistency, we computed annual cycle amplitude and mean seasonal averages also for the period 2007-2010 (not shown here). The results show that the above-described features are similar for this short period comparing to the original one. Kozubek et al. (2015) studied the NH stratospheric winds longitudinal distribution and long-term trends identifying a dominant effect of the Aleutian high (our region of interest is located at its western border in the stratosphere). They found that the trends of meridional winds connected with Aleutian height are independent of SSW or QBO. They observed intensification of the winds in the period of ozone depletion deepening (1970–1995) and weakening of the winds in the period of ozone recovery (1996–2012). However, there is an indirect dependence of the winds on QBO, as the solar cycle influence is pronounced mainly for the west phase of QBO.

With corresponding reference added: Kozubek, M., Krizan, P., and Lastovicka, J.: Northern Hemisphere stratospheric winds in higher midlatitudes: longitudinal distribution and long-term trends, Atmos. Chem. Phys., 15, 2203-2213, doi:10.5194/acp-15-2203-2015, 2015.

(2) Effects of the observational filter on the wave amplitudes should also be mentioned when discussing the possibility of below-threshold IGW breaking. For details see the specific comments below.

We thank the referee very much for this comment. We agree that an effect of the observational filter should have been discussed before. For changes in text please see our reply to the specific comments.

Specific comments: (1) p18289, l.9-13 shorter horizontal and shorter vertical wavelength with same amplitude as in other regions... This is not necessarily an effect of higher buoyancy freï£ijquency. It should be mentioned that this effect could also be related to IGW sources or background winds.

We thank the Referee very much for this comment and change the text accordingly, page 18289, lines 9-13: Old: This might suggest higher buoyancy frequency (stronger stratification) values in the area of interest. Now: This might suggest higher buoyancy frequency (stronger stratification) values in the area of interest, or this could be an effect of different IGW sources or different background wind structure in this region.

(2) p18289, l.14-19: Having a look at Faber et al. (2013), I had the impression that the findings mentioned in your manuscript are supported for horizontal and vertical wavelength, as well as for momentum flux. For Epot, however, this is not so clear. During summer Epot seems to be about average, and during DJF2006/7 Epot over the region of interest is lower than over the Asian continent. Suggest to just omit Epot in p18289, l.18.

We agree with the Referee and omit Ep in page 18289, line 18. Also, we would like to apologize for a not completely accurate citation of Faber et al. (2013).

(3) p18296, l.11-13: About instability criteria... The probability of observing IGW at the exact time of breaking is not the only effect why IGW breaking could occur although criteria are below threshold. It should also be mentioned that the IGW wave amplitude of GPS RO will usually be low-biased due to the observational filter of the instrument. Therefore it should be self-understood that IGW breaking should happen, even if IGW amplitudes observed by GPS are below threshold. A sensitivity function for squared amplitudes, applicable to limb observing geometries like GPS RO, is given, for exam-

ple, in Trinh et al. (2015), their Fig.7b. (Trinh, Q. T., Kalisch, S., Preusse, P., Chun, H.-Y., Eckermann, S. D., Ern, M., and Riese, M.: A comprehensive observational filter for satellite infrared limb sounding of gravity waves, Atmos. Meas. Tech., 8, 1491–1517, doi:10.5194/amt-8-1491-2015, 2015.)

We are very grateful for this comment, because taking the observational filter into account makes clear that it is almost impossible to observe perturbations with amplitudes above the threshold. We change the text accordingly, page 18296, lines 11-13: Old: It is also natural to expect the values of those characteristics below their threshold because the probability that the occultation takes place at the exact time of IGW breaking is low. Now: In fact, it is natural to expect the values of those characteristics below their threshold because of the effect of the observational filter (Lange and Jacobi, 2003; Trinh et al. 2015), and also the probability that the occultation takes place at the exact time of IGW breaking is low. With correspondent reference added: Trinh, Q. T., Kalisch, S., Preusse, P., Chun, H.-Y., Eckermann, S. D., Ern, M., and Riese, M.: A comprehensive observational filter for satellite infrared limb sounding of gravity waves, Atmos. Meas. Tech., 8, 1491–1517, doi:10.5194/amt-8-1491-2015, 2015.

(4) p18298, end of Sect.3.1: For consistency, it should be mentioned whether this climatological pattern (determined from the years 1979-2013) is also found for the four years (2007-2010) used for IGW analysis in Sect.3.2.

We agree. See our response to the main comment (1).

(5) p18306, around l.23: waves over the Himalayas / over the Andes This discussion addresses the question whether Epot is a good measure of the average IGW wave energy. This uncertainty, however, is not explicitly mentioned there, reference should be made to the previous discussion related to Eq.(2) in Sect.2.

We thank the referee for notification and change the text as follows: Old: The advantage of this method could be seen in the results of (e.g. Wright and Gille, 2013), where the enhancement of significance of wave activity around the Himalayas could be ow-

ing to the better representation of lee wave activity with smaller slope of the phase lines (higher ratio between kinetic and potential energy). Now: The advantage of this method could be seen in the results of (e.g. Wright and Gille, 2013), where the enhancement of significance of wave activity around the Himalayas could be owing to the better representation of lee wave activity with smaller slope of the phase lines (higher ratio between kinetic and potential energy), whose activity would be underestimated using Ep.

Also in connection with the previous referee comments, we feel that a sentence should be added at the end of this paragraph: Page 18306, Line 29: In this discussion, to show our point, we avoid for simplicity to discuss the effect of observational geometry with respect to the wave orientation, which could otherwise be a leading source of differences between observed IGW activity especially when contrasting Himalayas and Andes.

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