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Interactive Comment

Interactive comment on "Global distributions of overlapping gravity waves in HIRDLS data" *by* C. J. Wright et al.

Anonymous Referee #1

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General Comment

The paper by Wright et al. provides a lot of useful information about limitations of HIRDLS limb sounding observations of gravity waves. One of the effects addressed are signal oscillations due to the HIRDLS field of view blockage.

The number of wave findings is investigated, depending on horizontal and vertical wavenumbers (k_h and k_z). Four subgroups Ll, Ss, Ls, Ss, are defined according to their location in the spectral domain of k_h and k_z. Variations of the number of wave findings with latitude, altitude and season are discussed. Possible consequences for the interpretation of limb observations of atmospheric gravity waves are pointed out.

The paper is well written and contains valuable information for the readership of ACP. However, there are two major concerns and a number of inaccuracies that should be addressed before publication in ACP.

Major Concerns

(A) The effect of noise on the number of wave findings is not addressed. In particular, an enhanced number of low amplitude findings at high k_h, increasing with altitude, may be due to measurement noise.
Specific Comments (15), (18), and (27).

(B) During part of the discussion, variations in the number of wave findings at high k_h / low k_z are attributed to the QBO in the tropics. These variations, however, are probably not caused by the QBO. Instead, they will mainly arise from seasonal variations of high gravity wave activity in the subtropics.
Specific Comments (22) and (29).

Specific Comments

(1) p. 4335, l. 2: Please explain the expression "pseudo-instantaneous satellite data"

(2) p. 4335, l. 10–11: Please include the reference Kim et al. (2003)

Kim, Y.-J., Eckermann, S. D., and Chun, H.-Y.: An overview of the past, present and future of gravity-wave drag parameterization for numerical climate and weather prediction models – Survey article, Atmos. Ocean, 41, 65—98, 2003.

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(3) p. 4338, l. 21–22: "using Fourier transform methods" — Please be more specific! In Fetzer and Gille (1994) sequential estimation by Kalman filter was used, but I suppose you are using a different method.

(4) p. 4339, l. 1: This overage of a priori data and the zero-padding could lead to an underestimation of gravity wave variances, as soon as the wavelet of the S-transform extends into these ranges of zero gravity wave signal. Potentially, this will low-bias gravity wave variances at altitudes below 30 km.

(5) p. 4340, l. 15–19: It should be clarified that multiple soundings of the same wave are only a problem if the properties of a "wave packet" shall be determined, multiple soundings are no problem if fixed-size regions are considered!
Of course, it is difficult to determine the horizontal wavelength of the "average wave packet", because the size of the wave packets is not known, and there may be multiple soundings of the same wave packet. Talking of averages for (sufficiently large) regions of given extent, however, this is not a problem if we assume that these regions are more or less uniformly sampled. According to their larger area, larger wave packets should contribute more to the average horizontal wavelength, momentum flux,... than smaller ones. Assuming uniform sampling, these larger wave packets will be sampled more often, thereby being weighted with their area, as required.

(6) p. 4341, l. 14: The reference Trinh et al. (2014) should be added.
In this paper the effect of the observational filter of limb viewing satellites on observed gravity waves is discussed in more detail.

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. Discuss., 7, 10771–10827, doi:10.5194/amtd-7-10771-2014, 2014.

(7) p. 4344, l. 13 onward: The definition of parameters is unclear!

In Eq. (1) $\Delta X_{i,i+1}$ is the "profile separation". Here, on p. 4344, starting in l. 13, it is used as traveled distance during a single scan. It is also unclear what $\Delta r_{i,i+1}$ stands for. Is $\Delta r_{i,i+1}$ the distance between two different soundings, depending on altitude? Is this different from (which?) $\Delta X_{i,i+1}$?

Please clarify!

(8) p. 4345–4346, Sect. 4.5: Are these S-transform limitations valid only for the HIRDLS measurement configuration (orbit parameters, scan durations,...), or are these limitations applicable to all other limb scanning instruments? Please state more clearly!

(9) p. 4346, l. 14: "in any conditions where we do not detect a full wave packet" — This statement is too general!

In the real atmosphere, a wave packet could be rapidly decaying from its center altitude to both higher and lower altitudes, without displaying one or several full wave cycles. Still, this would be called a "wave packet".

In addition, in the real atmosphere, the vertical wavelength of an observed wave could change with altitude.

Suggestion:

where we do not detect a full wave packet → where we do not detect one or more full wave cycles of the same wave

(10) p. 4347, l. 19 onward: It should be stated more clearly that this truncation is introduced because shorter vertical wavelength waves are detected, but not considered reliable detections. It should also be stated that the reduced resolution is due to the retrieval, and not due to the FOV of the instrument.

(11) p. 4348, Eq. (4): Definition of parameters is unclear!

Eq. (4) is not entirely clear. From my understanding, ε_{LOS} is a parameter of

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geophysical meaning. It should be the angle between the line connecting tangent point and satellite location and the local tangent plane at the satellite location (the plane perpendicular to the line between center of Earth and satellite location). In other words, ε_{LOS} is the elevation angle by which the instrument has to view downward to view the desired tangent altitude. The scan angle, say ε, of the scanning mirror could be different from this, depending on the optical system. Please clarify!

However, assuming these definitions, I do not understand the factor 2 in Eq. (4)! Eq. (4) should read instead:

$$R_E + H_t = R_S \cos(\varepsilon_{LOS})$$

I have the impression that the expressions LOS angle and scan angle are interchanged in Eq. (4) and the related discussion.

Please check and clarify the definition of the different parameters!

(12) p. 4348, l. 25 and p. 4349, l. 15–21: Here it is stated that a vertical wavelength of 9 km corresponds to an elevation scan angle range of 0.9°. However, in Gille et al., 2008, Fig. 5 the period of the blockage oscillations is only 0.1°.

Nevertheless, this corresponds to around 9 km vertical wavelength, having a look at Gille et al., 2008, Fig. 4.

Please check!

(13) p. 4350, l. 5: Please state more clearly that the contribution of the anomalous peaks is estimated by relating their area to the total area below the respective curves in Fig. 1a.

(14) p. 4351, l. 23: Please explain why you expect that vertical wavelengths longer than 16 km should be strongly affected by the blocking oscillations.

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From Fig. 1, it seems that the relative importance of these oscillations decreases toward longer wavelengths, while atmospheric signals dominate more and more.

(15) p. 4352, l. 19: Possible effects of measurement noise have to be mentioned!
Another effect that could contribute to the increased number of wave findings by the method of Wright and Gille (2013) could be an increased number of low amplitude events that might be influenced by measurement noise. In Fig. 8b, the **average** amplitude of waves at high k_h is 0.5 K, close to or even below the HIRDLS noise level in the stratosphere. Many wave events will fall below this level!
As expected by Ern et al. (2004), the contribution of random fluctuations would peak at horizontal scales of four times the horizontal sampling distance. This is at about the same location as the spectral peak at high k_h in Fig. 3a. With increasing altitude, the effect of measurement noise will increase. This is also seen in Fig. 3a. The contribution of such short scales increases with altitude, as would be expected from a contribution by noise. Even for HIRDLS–pk (Fig. 3c) at 70 km altitude, an enhancement at high k_h is seen that may be caused by increased measurement noise at high altitudes.

I am not saying that the whole additional contribution at short scales is due to noise. However, the potential effects of measurement noise mentioned above should be included in the discussion!

(16) p. 4353, Sect. 6.1: It should be stated more clearly that the global average spectral distribution is calculated just as a reference. The global spectrum is an average over gravity waves from different sources with different spectral characteristics, and also an average over different atmospheric background states in terms of background wind and stability.

(17) p. 4355, l. 5–19: It should be mentioned already in the beginning of this discussion that in Ern and Preusse (2012) momentum flux spectra are shown. Direct comparison with occurrence rate spectra is therefore not possible.

Momentum flux spectra do not only depend on the occurrence rate, but also on wave amplitudes, as well as horizontal and vertical wavenumbers. Further, in Ern and Preusse (2012) a different method is used for identifying waves and selecting pairs of profiles.

Therefore some speculation about differences between different approaches may make sense, however the statements from p. 4355, l. 5 onward should be treated with some caution, and this should be stated already at the beginning of this discussion.

(18) p. 4356, l. 5: It should be stated that larger horizontal wavenumbers at higher altitudes could be an effect of instrument noise, and are not necessarily a true feature of the global distribution of atmospheric gravity waves.

(19) p. 4356, l. 18 onward:
The polar orbit of the satellite may be one of the effects contributing to lower horizontal wavenumbers near the equator. It should however also be mentioned that there are physical reasons why gravity waves can attain lower horizontal wavenumbers at low latitudes. The Coriolis parameter limits the possible range of gravity wave intrinsic frequencies, and thereby also the permitted range of horizontal wavenumbers. At low latitudes this permitted range maximizes, allowing also for very low horizontal wavenumbers.
For a discussion of this effect see Preusse et al., JASTP, 2006.

Preusse, P., et al.: Tropopause to mesopause gravity waves in August: Measurement and modeling, J. Atmos. Sol. Terr. Phys., 68, 1730–1751, doi:10.1016/j.jastp.2005.10.019, 2006.

(20) p. 4356, l. 22–24: It should be mentioned that the finding of smaller k_z at high latitudes and higher k_z at low latitudes is in agreement with previous studies, for example, Alexander et al. (2008), Yan et al. (2010), Ern et al. (2011).

(21) p. 4357, l. 18–19: Latitude dependent k_h variations are not only instrument-induced, there are additional effects from gravity wave physics. This should also be stated. See comment (19).

(22) p. 4357, l.22 until p. 4358, l. 6: two comments

(a) It is not correct that the QBO is mainly driven by low k_z / high k_h waves!

The QBO should be driven mainly by waves with intrinsic phase speeds of the order of typical QBO wind speeds. These wind speeds rarely exceed 40 m/s. In the tropics, this corresponds to vertical wavelengths shorter than about 12 km ($k_z > 10^{-4.1}$ m^{-1}). This includes only minor part of the regions of low k_z / high k_h mentioned here that display strong variations.

A recent discussion of this topic can be found in Ern et al. (2014).

Ern, M., Ploeger, F., Preusse, P., Gille, J. C., Gray, L. J., Kalisch, S., Mlynczak, M. G., Russell, J. M., and Riese, M.: Interaction of gravity waves with the QBO: A satellite perspective, J. Geophys. Res. Atmos., 119, 2329–2355, doi:10.1002/2013JD020731, 2014.

Further, the choice of regions is not adequate for making statements about the QBO, because latitudes of 0–30deg are always combined in one region. The QBO, however, is mainly driven in the latitude range 10S–10N, and thus only a small fraction of the regions as defined.

I suggest to either drop the discussion related to the QBO, or to introduce a new region from 10S to 10N, more suitable for QBO research. Due to very low gravity wave amplitudes near the equator, however, I am afraid that detection of multiple waves will be more strongly affected by measurement noise than other regions.

(b) Variations at low k_z / high k_h at latitudes equatorward of 30deg will be dominated by seasonal variations of convectively active regions in the subtropics, mainly over the continents and Indonesia.

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It is known that in the subtropics there is a characteristic pattern of high gravity wave activity, related to deep convection over the continents and Indonesia (for example Wright and Gille, 2011). This pattern is found only in the summer hemisphere, leading to strong seasonal variations considering a fixed location or region. It has been shown that low k_z / high k_h gravity waves contribute strongly to this enhanced gravity wave activity (Ern and Preusse, 2012).

This obviously explains the seasonal variations seen mainly in the regions equatorward of 30deg over the continents and Indonesia in Figs. 6 and 7. Even the strong variations found in the 30–60deg latitude region over North America coincides with the northward extension of the corresponding region of enhanced subtropic gravity wave activity that is usually observed in this region.

Overall, this means that differences in Doppler shifting may play a certain role, but the main effect is likely just a seasonal variation of gravity wave sources. Doppler shifting would affect all k_h values, however seasonal variations are mainly seen at low k_z / high k_h that should be most strongly influenced by well-known seasonal variations in convective sources.

The discussion should be revised accordingly.

(23) p. 4359, l. 10–11: once orbital geometry effects are removed. $\rightarrow$ once k_h variations due to orbital geometry or due to limitations by the Coriolis parameter are removed.

(24) p. 4359, l. 21: Does the number wpp refer to the whole profile, or to a given profile at a given altitude?
Please clarify!

(25) p. 4360, l. 26: It should be mentioned that the importance of Ls waves in the monsoon regions is in agreement with the findings by Jiang et al. (2004) and Ern and Preusse (2012).

Jiang, J. H., Wang, B., Goya, K., Hocke, K., Eckermann, S. D., J. Ma, J., Wu, D. L., and Read, W. J.: Geographical distribution and interseasonal variability of tropical deep convection: UARS MLS observations and analyses, J. Geophys. Res., 109, D03111, doi:10.1029/2003JD003756, 2004.

(26) p. 4362, l. 3–11: Here it is discussed that close to the critical level, the vertical wavelength would be shorter. This should be illustrated more clearly in Fig. 11. In Fig. 11, wave crests of the second wave packet should be plotted using narrower distances.

(27) p. 4363, l. 13: Again, possible effects of measurement noise should be stated!
The increase in horizontal wavenumber with increasing altitude may be an effect of measurement noise.

(28) p. 4363, l. 14–15: Again, possible physical reasons should be mentioned.
Proposed rewording:
arising most probably due to the polar-orbiting scan pattern of the instrument.
$\rightarrow$ arising from the polar-orbiting scan pattern of the instrument and from the fact that gravity waves can attain longer horizontal wavelengths at low latitudes because the Coriolis parameter is reduced (Preusse et al., 2006).

(29) p. 4363, l. 27: It has not been shown that Ls waves are important for the QBO. This statement therefore has to be revised accordingly!
The low-latitude regions that were investigated cover latitudes 0°–30° and will therefore be dominated by variations of the high amplitude Ls waves in the subtropics that even cover a larger area. See also comment (22) and major comment (B).

(30) In the references, the page range of quite a number of papers starts with page number "1". Please check!

(31) Figs. 6–10: It should be stated clearly in both text and figure captions which altitude these figures are representing. I suppose 32 km, but this information is missing.

Other Comments

(1) p. 4334, l. 5–6: small vertical and large horizontal wavenumbers → large vertical and small horizontal wavenumbers

(2) p. 4339, l. 23: km^{-1} → m^{-1}

(3) p. 4340, l. 22: largest-amplitude peak → largest covarying amplitude peak ??? (pairs rather than single profiles?)

(4) p. 4343, l. 1–3: much smaller → somewhat smaller ????

(5) p. 4344, l. 3–4: Aura will have travelled → the HIRDLS measurement track will advance by

(6) p. 4348, l. 24–25: elevation scan angle → elevation scan angle range

(7) Fig. 2b: Vertical dashed lines of Aura pitch-up maneuvers are missing or not visible in my pdf viewer. Please check!

(8) p. 4351, l. 5: at which would be expected to occur → at which one of the peaks would be expected to occur

(9) p. 4359, l. 12: suggestion
to the global MF distribution → to variations of the global MF distribution

(10) p. 4359, l. 20: Figures → Figure

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(11) p. 4360, l. 18: tropical → tropical and subtropical

(12) p. 4364, l. 2: of low wave intermittency → of strong wave intermittency

(13) p. 4379, Fig. 8: In the figure caption the description of panels does not match the figure panels. “(b)” and “(c)” are interchanged in the caption.

(14) p. 4379, Fig. 8: To avoid confusion, for (b) and (d) the figure caption should be reworded as follows:

(b): temperature perturbations → temperature perturbation per wave event

(d): momentum flux → momentum flux per wave event

(15) Fig.11: The distance between the different wave crests should be narrowed for the wave closer to the critical level.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 4333, 2015.