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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Gravity wave variances and propagation derived from AIRS radiances" *by* J. Gong et al.

Anonymous Referee #1

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The paper presents the first climatological analysis of AIRS data for GWs. It presents new and interesting results on the propagation direction of GWs. The paper therefore merits publication in ACP. The paper also aims at giving new evidence on the predominant horizontal wavelengths for different sources. This touches an open issue which is frequently discussed in a speculative way but for which experimental evidence is extremly limited. New evidence would mean a real advance in GW research. However, the paper only mentions that 3pt analyses, which may held the answer, were carried out but never shows it. Precisely because this topic is very important you really need to show this evidence and offer quantiative interpretation. In addition, some specific minor comments with respect to language are given below, but I recommend a general check.

Major comment:

It is frequently debated which part of the horizontal wavelength spectrum is carrying most of the momentum into the middle atmosphere. There is evidence for different parts of the spectrum, but so far insufficient experimental evidence to conclude on the whole and different models give different answers. This problem has been reviewed in some depth by Preusse et al. (2008). Sub-limb data (as the outer tracks of AIRS, cf. below) can provide new evidence on this problem a) in itself and b) if combined with other data. My major point is to be more precise and quantitative in the discussions and to use all data and all available climatologies.

Regarding a), more evidence from AIRS data: A previous case study analysis of AIRS (Alexander and Barnet, 2007) identifies dominant wavelengths slightly longer than 100km, but AIRS should be able to see shorter waves as well. Do they find this dominant wavelength, because they considered one special case, because they focused more on a true nadir view or because of a true general feature of the excited mountain waves? You generally use 7pt data with a peak sensitivity at horizontal wavelengths somewhat longer than 100km. 3pt variances are not shown (except F10) and compared in the text only in a non-quantitative way. Since for 3pt variances the sensitivity peaks at wavelengths substantially shorter than 100km, the quantitative comparison can give new insight whether the predominant wavelengths are around 100$\sim$km and longer or substantially shorter. Both 3pt and 7pt variances should be shown in the paper for the zonal mean cross-sections and for the maps. Methods to consider also the across-track (swath) direction should be considered (e.g. using variances in a square) should be considered for a follow-up study.

Regarding b), comparison with other climatologies: You compare only with the AURA-MLS measurements. As both instruments have strong observational filters, you compare the relative strengths of the maxima caused by convective and orographic waves (Did I got this right? It wasn't very clear from the text). However, since the observational filter of saturated-limb/sub-limb is a narrow diagonal stripe in the lz/lx projection and a horizontal cut-off is used for the GW analysis this implies also a narrow vertical-wavelength filter for the backward viewing MLS instrument. Considering as well the published climatologies of other instruments should help to disentangle horizontal and vertical wavelength distributions in a more clear way. For infrared limb sounders you should consider both, data for variances (wide range from mid to long horizontal wavelengths) and momentum flux (peaking at a few 100 km, unfortunately true climatologies still missing). In addition, there is evidence for MWs at steep ridges (e.g. the Andes, Norwegian Alps) as well as above wide regions (e.g. Siberia) (e.g. Eckermann and Preusse, 1999; Jiang et al. 2004). It maybe worth to consider ridges and wide regions separately as they also may exhibit different wave characteristics. You should give the ratios explicitly in the text or summarize them in a table to make the comparison quantitative.

Such a comparison requires the detailed knowledge of the observational filter. Please include an additional figure showing the observational filter in swath (across-track) direction.

References for climatologies (probably incomplete): GPS Fröhlich et al., 2007 Limb-Variances Preusse et al., 2009 Limb momentum Ern et al., 2004, Alexander et al., 2008, Wright et al., 2009

General comments:

Please consider that correcting language takes time. For some corrections you need an expert from the field. However, some simple corrections, e.g. where to use an article (a, the), where to use plural/singular etc. could be done by any native speaker. On the other hand, any proof reader will give you only a certain amount of corrections. If the proof reader is busy with more trivial errors, this distracts from the content. Thus, you should search some native speaker who reads the text before actually sending it out to your native speaking coauthor. And keep in mind that also the reviewer is annoyed by having to do frequent language corrections.

You try to distinguish your own results from previous work by depreciating other techniques. That is neither good style nor helpful. In combining the different techniques we may receive the full picture we can unfortunately not gain from a single instrument.

The limits between limb, sub-limb and nadir are of course fuzzy and we are missing a clear definition. So we could call sub-limb any view which looks below the horizon (in which case MSX is sub-limb but MLS is not) or we can call sub-limb any view which saturates in the downview at any angle notably different from nadir. Both definitions would make the outer tracks of AIRS sub-limb.

The limits between high, mid and low frequency GWs are fuzzy as well. There is a common understanding, however, what mid-frequency means, i.e. waves that have intrinsic frequencies \hat{omega} sufficiently far from either N or f in order to use the most simple form of the dispersion relation in good approximation. In this definition limb view sees both mid and low frequency waves (actually the short-horizontal-wavelength sensitivty limit for limb almost coincides with the maximum sensitivity for microwave sub-limb of e.g. MLS) and momentum flux distributions from limb sounders are dominated by mid-frequency waves.

Specific comments:

Abstract:

Drop the last paragraph; these are motherhood statements. Instead include the main point in the previous paragraph:

suggestion: ... from orographic and convective sources). For this sub-limb geometry GWs with vertical wavelengths substantially shorter than for true nadir can be detected. The annual cycle ... Indication for a weak two-year variation in the tropics is found. This variation is presumably related to the Quasi-Biennial-Oscillation (QBO).

All comments below indicate the original text in the paper they refer to by pagenumber and line. The page numbers below are only the last two digits, e.g. page 11693 will be indicated as P93.

P93, L2 ... from the lower to the upper ...

P94, L2

It is actually not the disperion relation which gives the relation between the wave parameters and the momentum flux but higher level equations whithout a specific name, which are based on both dispersion and polarization relations. Suggestion:

Gravity waves of higher intrinsic frequency and shorter horizontal wavelengths potentially can carry greater momentum flux than lower frequency GWs and could therefore excert significant wave drag (suitable reference). Currently we are missing global information in particular on these shorter horizontal scale waves. This presents an important information gap for any effort to constrain GW drag in climate models by measurements.

Just as a further comment: For the longer wavelengths we have at least the limb sounder climatologies of absolute values of GW momentum flux, though with large error ranges, for shorter waves we have next to nothing. Anyway, note that even with horizontal wavelengths of 500-1000km you could convey more than sufficient momentum to produce the required wind tendencies in GCMs.

P94L12 In addition, ... therefore ... Either it's a new aspect or it continues the current argument: decide!

P94L15 ... with lz/lh ratios of less than 10-20, i.e. mid and low frequency GWs.

P94L19 Please quote also the reference paper by Alexander et al., QJRMS, 2010.

P94L20 Firstly, this is blatant praize. Secondly, these are not four separate features.

Nadir and sub-limb sounders such as AIRS provide unique information on the short horizontal wavelength part of the spectrum: Firstly, AIRS has a high horizontal resolution ($\sim$13 km at nadir). Therefore AIRS is very sensitive to high-frequency

GWs, which are badly constrained in their global distributions by both observations and models. Secondly, for the outer tracks the sensitivity depends on the tilt of the wave fronts. This allows us to estimate preferrential GW propagation directions from the viewing-dependent variance difference between the two outmost off-nadir views.

Temperature versus radiances is an old argument. If the temperature retrieval is performed adequately and if it does not induce to much additional noise it compensates for different sensitivities in different background atmospheric conditions. It also provides the access to real physical quantities such as GW energy and momentum flux. Please omit the sentence listing radiances-only as an advantage. Also, there is no need to disapprove of other measurement techniques.

P95 L18 Please be more specific on the type of spectrometer.

P96 L3 These are not waves: at the maxima and minima of

P96 L4 ... most stable ...

P96 LL5 Please check the material of the filter. In addition, thermal expansion can be a reason for shifted calibration and these variations are slow. Thus, since you checked the mean values for orbit-synchronized variations, the 7pt variances, which act on a short time-scale are reliable.

P96 LL13 Since AIRS scans perpendicularly to ... for most of the time except at latitudes beyond +/-80°, which therefore are excluded from the analysis. - If you really want to explain the scan pattern you need an additional figure.

P98 L24 It would appear more logical to me to say: convolving a wave with the WF.

P99 LL5 Alexander and Barnet discuss a case study, you do a climatology. The fact that Alexander and Barnet do not find shorter waves in their example does not mean they do not exist in general. Thus, as said in the general comment, show both 3pt and 7pt analyzes.

P00 L7 What exactly is comparable to what? Please be more precise and give numbers. ... at 10 hPa ...

P00 L11 "Very low GW activity is observed by AIRS in the summer polar regions." deduction -> subtraction of the instrument noise ;

P00 L13 All satellites indicate low activity. A quantitative comparison is almost impossible whithout retrieved temperatures.

P00 L14 This discussion must involve the observational filter. The vertical wavelength is given by $\lambda_z= 2\pi \hat{c} / N$ with $\hat{c}$ the intrinsic phase speed and N the buoyancy frequency; 10 ms^{-1} phase speed roughly correspond to 3∼km vertical wavelengths. As your sensitivity starts roughly at 12∼km vertical wavelength, you need 40∼ms^{-1} phase speed. This will occur much more frequently for waves propagating versus a strong background. Therefore in particular for sensitivity to longer vertical wavelengths only the observational filter could account for a large part of the observed global structure (Alexander, 1998). Even limb sounders with much better vertical resolution are compatible to merely observational filtering, if only altitudes up to 40∼km are considered (Ern et al., 1995) and only at higher altitudes significant evidence for a quiet summer hemisphere is found (Preusse et al., 2009).

Suggestion for reformulation of the paragraph:

Very low GW activity is observed by AIRS in the summer polar regions. Low phase-speed GWs cannot propagte into these regions because of the wind reversal between troposphere and stratosphere. In addition, the low wind velocities in the summer high-latitude stratosphere allow AIRS only to observe GWs of high ground-based phase speeds (larger than $\sim$40∼ms^{-1}; cf. discussion of observational filter by e.g. Alexander, 1998). However, such fast waves should be emitted from sources such as fronts and are currently parameterized in models to reproduce the wind reversal around the mesopause (Charron and Manzini, 2002; Richter et al., 2010). The very low values in AIRS indicate rather low propability of such waves to occur and support

observational evidence that GWs from subtropical convection propagating polewards are a major source of momentum for the high latitude summer mesosphere (Preusse et al., 2009; Ern et al., 2011).

Combining different instruments one could really give suggestions to the modelers.

P00 L20 Please quote also Alexander at al., 2002

P00 L26 omit "indirectly"

P01 L3 Orography is one of the ...

P01 L4/5 Only in strong westerly winds mountain waves, which have zero ground-based phase speeds, can gain high intrinsic phase-speeds and long vertical wavelengths and thus become visible to AIRS.

P01 L6 You should consider that most of the wave-filtering occurs at the tropopause level. In particular fast waves best visible for AIRS may not directly correspond to the wind systems. The other way around: GWs are only one type of waves forcing the winds. This paragraph is unclear and should be reformulated.

P01 L9 Start a new paragraph. ; "between the two outmost views both at the 10 hPa winter-pole maximum and in the tropical lower stratosphere, which indicates the same amount of eastward propagating and westward propagating GWs."

P01 L15 The global distributions also could have only weak dependence on the horizontal wavelengths, i.e. be largely the same independent what horizontal wavelength you are considering. This is not unlikely as the phase speed (i.e. the vertical wavelength) determines the wind filtering and the baseline for many non-orographic GW parameterizations. You need a quantitative comparison for this conclusion!

P01 L22 Is a paragraph missing here? You first need to describe the spatial collocation of orography and enhanced GW activity before you can continue with "Such orographic ...". Some more references for MWs: Eckermann and Preusse (1999), Jiang et al.

(2004). Is it really true that AIRS shows the strongest wave activity among all these observations? Please quantify this and give numbers.

P01 L25 Please use 'mountain waves' or abreviate MWs.

P02 L01 I cannot follow you with regard to the poleward propagation: At low altitudes (80 hPa) I see maxima of GW activity at very high latitudes (e.g. Greenland, the Antarctic peninsula) which vanish at higher altitudes (2.5 hPa). Instead, maxima between 50 and 60 deg latitude become more enhanced (Eastern Canada, south tip of South America). This does not look like oblique propagation though, but rather that upward propagating GWs attain longer vertical wavelength and hence better visibility in the core of the jets.

P02 L5 In general, GW amplitudes are much smaller at low altitudes (80 hPa) than at higher levels. At high latitudes the GW activity decreases with altitude independendent on longitude and not directly related to the wind velocities. This causes the high-latitude maxima around 10 hPa in Figures 3 and 4.

P02 L19 The spectra shown by Beres et al. 2005 tend to peak around 20∼ms$ˆ{-1}$ but extend further then 50∼ms$ˆ{-1}$. Background winds are of the order of 20-30∼ms$ˆ{-1}$. The waves propagating opposite to the winds should have vertical wavelengths of 12-24∼km accordingly, well inside your observational filter. Estimates of the phase speeds from CRISTA indicate even higher phase speeds (Preusse et al., 2001). An alternative explanation might be that the wavelength at high latitude might be so long that MLS sensitivity already decreases. As long as you base only on relative strengths you can only speculate. Also from the physical point this point merits more discussion. There is a debate in the community which waves convection is exciting. Large scale-modeling points to rather long horizontal wavelengths of a few 100km, but does not converge when resolution is increased (Kim and Chun, 2009). Observations from aircraft also show 100-200km wavelengths and was related to the build-up and decay of a convective system (e.g. Pfister et al., 1993). On the other hand simulations

of a single convective cell point to the excitation of rather short horizontal wavelength (about 50km or less) GWs via the mechanical oscillator effect (Fovell et al., 1992; Piani et al., 2000; Lane et al., 2001; Grimsdell et al., 2010) but also do not converge (Lane and Knievel, 2005). Observational evidence is from airglow (e.g. Taylor et al., 1987, Suzuki et al., 2007) and sub-limb (Dewan et al., 1998). This makes the horizontal wavelength above convection a very interesting and debated question and new evidence is in urgent need. Again, you should try to learn more from the 3pt variances.

P02 L25 Convection is known ... top of the deep convection ... The majority ... are filtered by ...

P03 L7 Include IR limb sounders and give references

P03 L10 argue that the

P03 L12 noise

P03 L14 ... the low wave activity in the NH ...

P03 L17 ... but GWs hardly propagate upward ...

P03 L19 ... mountain waves ...

P03 L26 "Apparent" is problematic since it used both for something which is very clear and something which only appears to be clear. I recommend not to use it. "A pronounced annual cycle ..."

P04 L2 ... are averaged ...

P04 L4 ... are observed, for instance, by ..., omit etc. ; IR limb sounder: Preusse et al., 2009

P04 L9 And background winds at observation altitude!

P04 L13 ... when the jet ... differences between the two outmost viewpoints. (important in the first sentence!) Maybe you give a name to this differences like outer-track differences

P04 L18 vertical structure

P04 L21 This is an interesting result and worth showing. Other experiments (for example the Vorcore balloon measurements; Hertzog et al., 2008) find enhanced GWs primarily where the coastline is not orinted along a latitude circle.

P04 LL26 Could you please explain the mechanism and give a reference.

P05 L5 The polar ...

P05 L14 specify difference

P05 L18 the NH winter maximum ??? Give evidence for the stratification hypothesis. The two paragraphs starting at lines 12 and 21 are somewhat at odds.

P06 L5 Orographic GWs (or Mountain waves) ... of the orographic GWD (no s!)

Section 4.1: Have you used your data to a full extent to understand these plots? Are these peaks coming from single overpasses or do they occur in the same way in several overpasses? How look the 'images' of the individual overpasses? What is the influence of the 7pt analysis? How are the regions chosen with respect to the source? The paper discusses climatological results and this case study does not really fit in. It is premature for publication, as your concluding sentence shows. My recommendation is to omit 4.1 from this paper and reserve it for a new publication on e.g. intermittency.

It might be advisable to switch 4.2 and 4.3. In 4.3 you conclude that the 'belt' of tropical variance is likely caused by GWs of relatively long horizontal and short vertical wavelengths while the patterns above deep convection are caused by GWs of relatively short horizontal and long vertical wavelength. Are the long-term biennial (?) changes connected with the belt or with the convective forcing regions? Could you use a region without particular forcing to decide this question?

P08 L22 More important than the evidence from the models is the evidence from the

measurements as they hold the truth: Dunkerton (1997), Ern and Preusse (2009) and a recent paper by Joan Alexander.

P08 L24 IR limb sounders: Krebsbach and Preusse (2006)

P09 L1 In F7 there is at 10hPa and 2.5hPa an only one color-step annual cycle in the tropics!

P09 L4 Replace Apparent by Obvious ; At the equator: Please give the latitude range you have averaged.

P09 LL08 Do the years 2008-2010 fit? You could test your hypothesis by a correlation analysis allowing a time-lag.

P10 L6 Fritts and Alexander

P10 LL8 I do not agree with your discussion of the previous results. First, Kelvin waves are believed to contribute only 25% to 50% of the momentum driving the QBO. Second, Tsuda and others have argued that the latitudinal distribution of GWs observed by GPS is not the distribution of Kelvin waves. On the other hand the QBO signal in the GPS is very strong. Third, Ern et al. (2008) show also GW distributions of IR limb sounders with Kelvin waves removed (Krebsbach and Preusse also removed slow Kelvin waves, but the 'detrending' in Ern et al is better). Thus we may assume that the previous results are real and that differences are really due to the different characteristics of the observed GWs. Therefore it would be particularly interesting to know whether the belt or the convection centers cause the long-term variations.

Your point that variations in the forcing e.g. by ENSO could also cause a close to two year variation is very interesting. This merits further investigation but is perhaps outside the scope of this paper. You should definitely make "forcing" an explanation as likely as "QBO".

P11 L9 Not exactly what you mean and again overdone. Suggestion: The AIRS data thereby provide unique evidence on the change of propagation direction in the different

QBO phases.

Section 4.3: In the discussion you jump between the 80hPa tropical maximum and the 10hPa maximum in the vertical. This is confusing. Discuss the tropical maximum first. Be more specific what you mean by turbulence. Large-scale 2D mixing processes or localized 3D turbulence? If you start to discuss this point in a full paragraph it needs to be self-explaining from the paper. The discussion of the sensitivity function is not very clear and likely parts of it can be moved to the appendix. The message you need to transport in the main part of the paper is (text suggestion):

Assuming a constant wave amplitude only waves with vertical wavelengths longer than about 12km are visible in the nadir viewing center tracks. If, however, the wave amplitude varies with altitude, this can result in an effective narrowing of the weighting function. We carried out sensitivity tests assuming a modulation of the temperature amplitude by changes in the buoyancy frequency. This yields to non-negligible sensitivity also to short vertical wavelength waves. Figure 12a shows the radiance response for a 5K wave of 700km horizontal wavelength. The best match to the observed variations shown in Figure 10b is found for 5km vertical wavelength. The difference between the nadir and the outmost view is shown in Figure 12b. The value for 5km vertical wavelength matches the difference between center and outmost views observed in Figure 10b. Varying the input parameters of the simulated wave we find that the best match of both the absolute value and the variation with the FOV is reached for a wave of 5K amplitude, 700km horizontal and 5km vertical wavelength.

In order to give a better feeling for the sensitivity of this test it would be interesting to show an additional figure: Reproduce 10b and simulate the response at various angles according to your radiative transport simulations. Use different input parameters (e.g. a 500km and a 1000km horizontal wavelength wave with adapted vertical wavelength and amplitude).

Why does the tropical maximum weaken with height? This is not yet conclusively

answered. Here are some explanations:

1. Low vertical group-speed waves might dissipate 2. If they 'speed-up' by refraction the visibility may lessen 3. At higher altitudes the waves may indeed leave the tropical confinement by f. This happens when they are Doppler shifted by favorable winds to higher intrinsic frequencies (cf. e.g. Figure 2i in Preusse et al., 2009).

The 10hPa maximum: This is certainly a noteworthy feature. However, your interpretation is less conclusive than for the equatorial maximum. The reason for the structure may not well be captured in the zonal mean, but refraction by wind is a more likely cause of changing wavelengths and amplitude than buoyancy frequency. IR limb sounders show a plateau of about equal amplitudes in the mid stratosphere, but not a maximum. I think you should note the 10hPa maximum, but shorten the discussion.

I missed the line in writing the review, but I recall that you referred to evidence for the favorable propagation direction of waves at the vortex edge. There is a paper by Jonathan Jiang; I think it's Jiang and Wu, GRL, 2001.

P15 LL8 This is a summary before the summary. If you switch the order and discuss the tropical maximum first and the QBO afterwards it makes sense to do a short summary, though. Suggestion for reformulation:

In summary, evidence is found for short horizontal wavelength internal GWs (of the order of 100km or less (?)) forming dedicated geographical maxima closely related to localized sources such as orography and deep convection and a generally smaller background of longer horizontal wavelength GWs forming band-like structures for instance in the lowermost stratosphere tropics. It is remarkable that AIRS can detect these inertio GWs even though there vertical wavelength is smaller than the width of the AIRS weighting function, if the wave amplitude changes notably over the range of detection.

The sentence: "This interpretation ..." is cryptic to me, the last two sentences highly speculative and should therefore not conclude a (sub)section.

Conclusions:

I hope you will look in more depth into the 3pt and 7pt variances, so you may need to rewrite part of the conclusions. As a general comment you should not overpraise your results.

P15 L18 'by carefully removing instrumental noises at various pressure levels' The removal of noise is an important side aspect but not the method. Give the method here!

P15 L23 most sensitive

P16 L26 Phase speed is omega/k or lh/tau, i.e. if the wavelength is long the frequency does not need to be exceedingly high in order that the wave has a large phase speed.

P17 L8 Observations of the propagation direction by AIRS may help to constrain this parameter.

P10, C1, L-1 Let's wait for the new evidence.

P10, C2, L2 I do not think that you have sufficient information in the paper to infer preferential propagation directions.

P17 LL17 Please consider the simulations by Girogetta et al., 2006. They have a pretty nice QBO simulation. There are still some deviations, but your statement is too general. A question may be whether they get the right answer for the right reasons. Still I would doubt that the problem are high-frequency GWs.

Anyway, do you really observe QBO or rather ENSO modulation? I don't think that the QBO should receive too much attention in the summary.

The last two paragraphs should be reconsidered.

In general I think the structure should go:

- AIRS can see high-frequency waves in sub-limb geometry. Propagation direction can be inferred. Indication for inertio GWs with relatively small vertical wavelength is found under special conditions.

- Results for propagation direction.

- Results for horizontal wavelength distribution.

All written concise and without speculation. You have really nice results and you hopefully can further improve on this, so the results can stand for themself.

Figures:

F2: Since you do not use 15pt results you could plot only horizontal wavelengths smaller than 500km.

F5 The maps are too small (use all the space you have), the continents hardly visible. Change to: ... absolute values smaller than the minimum detectable difference sqrt(2)sigma are whitened.

F8 recommendation: discuss in different paper. Anyway: linear fits

F11 "imported"?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 11691, 2011.