We appreciate the very detailed and insightful advices from this reviewer. We adapt most of the suggestions in our new manuscript. Some of the suggestions require too much work to do, or require adding figures and discussions which we think are not so relevant to the main topic of this paper. Besides, the paper length is already long. Hence, we decide to keep the majority unchanged. One-by-one response can be found below.

**Anonymous Referee #1**

Received and published: 5 June 2011

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 extremely 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.

The reviewer may misunderstand some of our points. Our method cannot derive horizontal wavelength of GWs from different sources. 3-pt and 7-pt analyses have different filter shape and size, so AIRS would be sensitive to slightly different part of the GW spectrum. But we cannot by any means infer GW horizontal wavelength from the filter.

We compute 3-pt and 15-pt analyses in order to test if our findings are robust or sensitive to window size. Only results that are robust are shown here. Due to the limitation of the paper length, we decide not to include parallel results from 3-pt and 15-pt analyses. One figure of 3-pt result is shown in the Appendix though. The authors have some conference presentations that include some of the 3-pt results. If the reviewer is interested in, one example is:
http://www.google.com/url?sa=t&rct=j&q=&esrc=s&source=web&cd=3&ved=0CDQQFjAC&url=http%3A%2F%2Fyly-mac.gps.caltech.edu%2Fz_temp%2Fgong%2520AIRS%2520waves%2Fgong%2520waves%25202010%2520Yuk.Yung.seminar2.pdf&ei=s6wET-f6A8Lm0QHbs5iDAg&usg=AFQjCNGF3Qw7munR4CHEMKKH1UqHM59VNg

We thank the reviewer for pointing out typos and grammar errors in the original manuscript. We have the new one proofread by a native speaker, and we try our best to eliminate English problems.

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 com- pared 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~km and longer or substantially shorter. Both 3pt and 7pt variances should be shown in the pa- per 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.

Per the request of the reviewer, we add a 3-pt latitude-height figure analogous to Fig. 3 in the Appendix for better comparison. 3-pt GW variance is smaller than 7-pt variance, suggesting GWs that are captured by 7-pt filter have larger power in the spectrum than those captured by 3-pt window.

7-pt filter has the best sensitivity of horizontal wavelength of ~ 105 km. Endorsed by Alexander and Barnet (2007) case study, 7-pt may be the best to capture mountain GWs.

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

We not only compare our results with Aura-MLS, but with other instruments, e.g., Aqua-AMSUA, CRISTA, radiosonde, GPS. We mentioned comparison with Aura-MLS more frequently in the paper is mainly because that analogous methodology is used as well to derive GW climatology from Aura-MLS (Wu and Eckermann, 2008), so a direct comparison is very easy to made. Secondly, some GW information derived from other satellite instrument (e.g., SABER, CRISTA) using quite different methodology (Ern et al., 2004; Preusse et al., 2009), where they calculate GW energy and momentum flux after making some assumptions. GW variance cannot be directly compared with results. Thirdly, this paper is not intended to make thorough comparisons among different satellite observations. Similar works have been done in other literatures, e.g., Wu et al. (2006), Preusse et al. (2009). Hence we don't want to spend too long pages on comparisons.

Following the reviewer's suggestion, we add a few more sentences to review previous literatures on satellite observations. We add a table (new Table 1) as well for better clarification.

We also include some discussions of mountain ranges versus broad elevated regions in section 4.1.

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.

We thank the reviewer for pointing out typos and grammar errors in the original manuscript, and we apologize for the trouble caused in the previous draft. We have the new one proofread by a native speaker, and we try our best to eliminate English problems.

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.
Agreed. We don't intend to depreciate other techniques. We modify some of our wording to try to eliminate the discomfort that the reader may experience.

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.

We follow the definition in Wu et al. (2006). The reviewer's consideration is now briefly mentioned in the section 1 to make the statement more rigorous.

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 domi- nated by mid-frequency waves.

Agreed. There's no clear boundaries. We mentioned our separation way at the end of the introduction section.

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).
We modify accordingly. We feel like the finding of AIRS being able to detect shallow GWs is the shining point of this paper and has a big potential to illuminate future research, so we keep the last paragraph, but modify it according to the reviewer's suggestion. Now it's "AIRS geometry makes its out-tracks capable of detecting GWs with vertical wavelengths substantially shorter than the thickness of instrument weighting functions. The novel…"

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 ...

Advice adopted. Thanks.

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 momen- tum to produce the required wind tendencies in GCMs.
Advice adopted. Thanks.

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

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

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

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 res- olution ($\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.
Advice adopted. Thanks.

Temperature versus radiances is an old argument. If the temperature retrieval is performed adequately and if it does not induce too 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.

We agree with the reviewer that method proposed by Ern and Preusse give a way to infer GW energy and momentum flux. However, assumptions always need to be made before we do retrieval, which may be violated in some cases. Also, the 2D retrieval error in the stratosphere is ~ 1K, which is way larger than the instrument noise level (Table 1). Therefore, we keep this item.

P95 L18 Please be more specific on the type of spectrometer.
We don't understand the meaning and purpose of this comment.

P96 L3 These are not waves: at the maxima and minima of
Advice adopted. Thanks.

P96 L4 ... most stable ...
Advice adopted. Thanks.

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.
Agreed.

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.
Reference link to a figure of AQUA orbits is given.

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

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.
We include a 3-pt result in the Appendix for comparison.

P00 L7 What exactly is comparable to what? Please be more precise and give numbers. ... at 10 hPa ...
Compared with GW variance calculated from other satellite measurements, e.g., Aura-MLS, Aqua-AMSUA.

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

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

As mentioned before, retrieval has many problems as well. Here the low activity is compared with the mean from the same measurement technique, not inter-satellite comparison.

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 propagate 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 probability 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.
Advice adopted. Thanks.

P00 L20 Please quote also Alexander at al., 2002
Advice adopted. Thanks.

P00 L26 omit "indirectly"
Advice adopted. Thanks.

P01 L3 Orography is one of the ...
Advice adopted. Thanks.

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 wave-lengths and thus become visible to AIRS.
Advice adopted. Thanks.

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.
The last sentence has been re-written as: "Notice that the overall pattern in Fig.~\ref{all01}c does not strictly follow the mean zonal wind contours, as meridional component is missing here."

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."
Advice adopted. Thanks.

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!
3-pt latitude-height figure is added in the appendix per the reviewer's request. We don't include a 3-pt map because of the color-scale (small values of variance are all not colored so cannot be compared).

We agree with the reviewer that our logic is not strict. The paragraph is hence re-written as: "Very similar features can also be obtained with the 3-pt filter, though the amplitudes are smaller (Appendix B), confirming that the result is robust."

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.
Many thanks. A paragraph is indeed accidentally deleted!

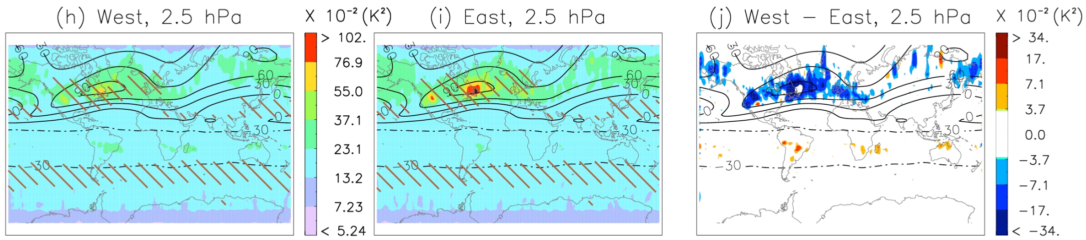
As for the amplitude, since we cannot directly compare variance with energy, we can't say AIRS shows stronger MWs than, say, CRISTA. But we have compared with other literatures that use variance as the proxy of the wave energy, and AIRS indeed has the strongest wave activity. We have listed several references for comparison. In the two literatures listed here, they study MW cases instead of climatology, so a direct comparison cannot be made.

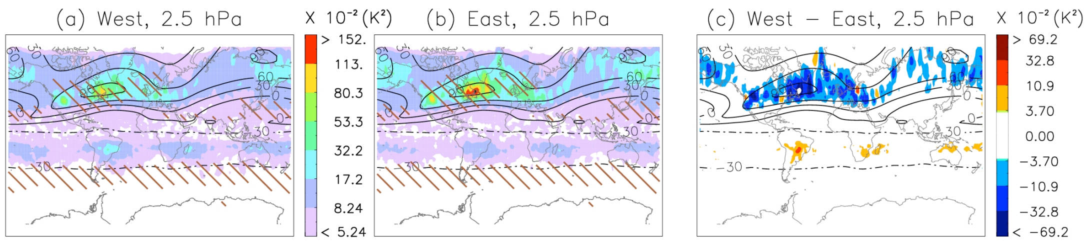
P01 L25 Please use 'mountain waves' or abreviate MWs.
Advice adopted. Thanks.
P02 L01 I cannot follow you with regard to the poleward propagation: At low altitudes (80 hPa) I see maxima of GW activity at very high latitudes (e.g. Greenland, the Antarc- tic peninsula) which vanish at higher altitudes (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.
You are right. The original sentence has been deleted. We add several sentences now: "In both hemispheres, orographic GW maxima are at high-latitudes in the lower stratosphere, and the peaks within 50 -60 deg become more and more identified in the middle and upper stratosphere. This feature can be attributed to vertical wind structures, as the strong wind near the jet center Doppler-shifts the Gws toward longer vertical wavelength, and Gws near the core of the jet hence have better visibility to AIRS."

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.
Advice adopted. Thanks.

P02 L19 The spectra shown by Beres et al. 2005 tend to peak around 20~ms$^{- 1}$ but extend further then 50~ms$^{-1}$. Background winds are of the order of 20-30~ms$^{-1}$. 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; Pi- ani 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.
Since the paper is too long and we certainly cannot put everything in it, the 3-pt and 7-pt maps @ 2.5 hPa @ January, 2005 are attached here (top panel is 3-pt, and bottom panel is 7-pt result).




7-pt result undoubtedly shows larger convective GW activities than 3-pt. A paper by Choi et al. (submitted to JGR, in revision) compares GCM convective GWs with our results, and in their model, parameterized convective GWs have two branches in the spectrum: one is very short vertical wavelength and horizontal wavelength GWs, and the other is very short vertical wavelength and long horizontal wavelength GWs.

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

P03 L7 Include IR limb sounders and give references
Advice adopted. Thanks.

P03 L10 argue that the
Advice adopted. Thanks.

P03 L12 noise
Advice adopted. Thanks.

P03 L14 ... the low wave activity in the NH ...
Advice adopted. Thanks.

Advice adopted. Thanks.

P03 L17 ... but GWs hardly propagate upward ...
Advice adopted. Thanks.

P03 L19 ... mountain waves ...

Advice adopted. Thanks.

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 ..."
Advice adopted. Thanks.

P04 L2 ... are averaged ...
Advice adopted. Thanks.

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

P04 L9 And background winds at observation altitude!
We are talking about background wind at the source level would affect the source spectrum. Sorry about the confusion.

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
Thanks for the suggestion. We think this phrase is not too long to get people lost. So we decide to keep the original format.

P04 L18 vertical structure
Advice adopted. Thanks.

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.
What Hertzog et al. (2008) discussed in their paper is GWs over the ocean, we believe. Here our argument is that, if it's MWs, the wind parallel to the mountain range (or coastline here) is a non-favorable condition to excites MWs.

P04 LL26 Could you please explain the mechanism and give a reference.
We can't explain the mechanism at this moment. We currently have a little project working on the details of MWs (like those spikes shown in section 4.1) that may yield a better explanation.

P05 L5 The polar ...
Advice adopted. Thanks.

P05 L14 specify difference
West-east 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.
The entire paragraph is trying to describe patterns at the subtropical stratosphere, and explain why we see the pattern. At the subtropics, deep convective GWs are very active during summer season. But these waves are not pronounced at 80 hPa. We explain the opposite annual cycle of 80 hPa compared with 2.5 hPa by the density stratification, that was proposed in Eckermann (1995).

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

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.
The main idea of this paper that we want to convey to the readers is a GW climatology from AIRS observations (section 3), and some uniqueness of AIRS GW that are different from previous findings or are never been reported before (section 4). Section 4.1 is some preliminary results, and we would like to explore them in the near future.
To answer specific questions: these peaks come from one month average. The sharpness of the peak is very interesting, which could be important and illuminative on, say, improving GWD parameterization schemes in GCMs.

We changed some sentences in this section, and hopefully it's improved now.

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 wave- lengths 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?
Section 4.2 has been significantly re-written now as suggested by the other reviewer. We include the possibility of inertial GWs discussed in section 4.3.

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.
Agreed. We don't see any conflict between this argument and the reviewer's comment.

P08 L24 IR limb sounders: Krebsbach and Preusse (2006)
Advice adopted. Thanks.

P09 L1 In F7 there is at 10hPa and 2.5hPa an only one color-step annual cycle in the tropics!
We have adjusted color-scales in Fig. 7 now. Thanks for the advice.

P09 L4 Replace Apparent by Obvious ; At the equator: Please give the latitude range you have averaged.
Exactly at 0N.

P09 LL08 Do the years 2008-2010 fit? You could test your hypothesis by a correlation analysis allowing a time-lag.
We did run a linear-correlation with QBO index (30 hPa) and Nino3.4 index, and the previous one is statistically significant, while the latter is not. We didn't run a time-lag correlation though.

P10 L6 Fritts and Alexander
Advice adopted. Thanks.

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".
First, we'd like to clarify a misunderstanding here. 2003-2004 behaves differently than the rest of the timeseries, and the reason is unknown so far. We give one possible candidate, ENSO, without any further exploration of evidences. We do not claim that ENSO can cause close-to-two-year variation.
Secondly, Torre et al. (2006) see a pronounced QBO signal in CHAMP GPS, where they claim it's Kelvin wave. It is also true that various analyses of radiosonde GWs using traditional method can neither exclude Kelvin waves. So strong QBO signals in GPS and radiosonde measurements can be, at least partly, attributed to Kelvin waves. We list Kelvin wave as part of the reason why AIRS looks different than others, and we also attribute another reason that to different observational windows of various techniques.

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.
This paragraph has been largely re-written.

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 wave- length 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.
Thanks for pointing out the caveats and rewriting the paragrph. We adopt some of the sentences in the modified manuscript, and take good care of the jumping between 80 and 10 hPa. Now 10hPa enhancement is only mentioned at the end of the entire explanation, and we focus on the 80hPa tropical enhancement.

This section is very hard to understand for readers that lack background knowledge of satellite measurements or waves. If we shrink the section as suggested by the reviewer, and put most of the details in the appendix, the reader easily gets lost. Besides, most of the mathematics have already been moved to the appendix. So we still think that it is better to move step-by-step from the very beginning of introducing the WF shape, before people get lost.

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).

Sensitivity experiment has been carried out already in terms of varying horizontal wavelength, vertical wavelength, and wave amplitude. We certainly cannot test all combinations, as they are numerous possibilities, but we tried out best.

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).
We had one explanation in our manuscript, that the slanwise propagating inertial GWs would meet f boundary in terms of frequency limit when the propagate to higher latitude. But the reviewer provides other posibilities. We thank the reviewer, and include these points in.
Meanwhile, we recommend Choi et al. (submitted to JGR, in revision) to the reviewer, as their GCM experiment strongly supports our original explanation.

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.
Agreed. We now shorten the discussion at 10 hPa, as it's still mysterious.

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.
No. We don't talk about vortex in this paper. We briefly mentioned jet source at section 2, but don't think it's a major source for waves we saw from AIRS.

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.

Advice adopted. Thanks. We significantly shorten the discussion session, but keep the three major findings. It is designed for readers who have no time to go over the entire paper, but can only glance at the conclusions.

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.
We modify the manuscript carefully following the reviewer's insightful and detailed suggestions. Hopefully it is satisfying now and publishable.

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!
The method is listed in the methodology part (section 2). We don't feel this is a major point to make, so we decide to keep the current format.

P15 L23 most sensitive
Advice adopted. Thanks.

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.
Agreed.

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

P10, C1, L-1 Let's wait for the new evidence.
We can't find the line the reviewer specifically pointed out.

P10, C2, L2 I do not think that you have sufficient information in the paper to infer preferential propagation directions.
We can't find the line the reviewer specifically pointed out.

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 QBO paragraph is shorten now.

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.
Thanks for the advice. The conclusion part has been rewritten and shorten. We try to show AIRS uniqueness while not disrespect other measurements.

Figures:
F2: Since you do not use 15pt results you could plot only horizontal wavelengths smaller than 500km.
We modified the figure and set the x-range to 1000 km, as 15-pt window still has a noticeable response at $\lumbda_h = 1000 km$.

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.
Advice adopted. Thanks.

F8 recommendation: discuss in different paper.
The spikes shown in Fig. 8 and section 4.1 are first-time findings. Hence, we feel like that they deserve to be pointed out in this paper as the theme of this paper is to present a climatology and uniqueness of AIRS GWs.

We indeed have ongoing works on the topic mentioned in Fig. 8 and section 4.1. We may want to write a separate paper in the near future to discuss the reasoning of the spikes.

F8: linear fits
Advice adopted. Thanks.

F11 "imported"?
Changed to “input”. Thanks.