Response to comments from Reviewer #2:

General comments:
(1) Gravity wave observational satellite instruments are categorized into limb, sub-limb and nadir sounders. The authors sometimes compare AIRS results with other instruments. However, the explanation about "observational filter" is insufficient. Gravity wave distributions observed by AIRS are not necessarily corresponding to those observed by other instruments. I understand the authors know this point, but more careful explanation and discussion about "observational filter" are needed. For example, observable spectral ranges for each instrument are explained and/or some references are added.
(2) In section 3.2, the authors showed monthly mean geographical maps of gravity wave variances and discuss two major gravity wave sources of mountains and convections. However, there are other gravity wave sources, which the authors did not mention.
Line 8 of page 11702: "They are highly variable along the longitudes, and not necessarily related with jet"
I partially agree with this opinion, but I think variances of gravity waves generated by the jet are also included in Figs. 5 and 6. But, it seems that the authors decided prematurely that these variances are due to mountain induced gravity waves. More careful explanation is needed here.

Jet-generated GWs have wide range of their frequencies, so we agree with the reviewer that we cannot completely rule out jet sources (jet imbalance source and/or shear source). Therefore, we modified words accordingly to include this possibility in (LXXX at page XXX).

However, it is likely NOT true that the GW enhancements at 10 hPa mentioned in the context and Fig. 5 and 6 are related with jets. Firstly, we make the continent lines denser as the reviewer suggested, so you would see more clear that the GW enhancement at 10hPa in Fig. 5 extends from Alaska to the entire Canada, which is neither at the exit region (jet imbalance production) nor flank (shear production) of the jet. It is similar for the Antarctic region in Fig. 6. Secondly, if these GWs are generated from the jet, they should have positive phase speeds at the source level, which should be able to get revealed on the east – west difference map. However, we don't see any indication of this possibility on the 80 hPa map. Hence, we keep our original idea on Line 8 of page 11702, but add the above two reasons. Now the sentence is: " They are highly variable along the longitudes, and not necessarily related with the jets as they do not locate at the exist region of the jet stream, nor do they show preferred propagation directions".

Line 12-18 of page 11702: "In the subtropics and tropics, large GW activities are found in the upper stratosphere over the deep convective regions. The deep convective regions are identified from the ice water content (IWC) from Aura MLS (Wu and Eckermann, 2008). In particular, they are Western Pacific warm pool region, Amazon rainforest region, and Central Africa rainforest region for NH

winters, and Southeastern US monsoon region and India-South China monsoon region for SH winters”.
I agree that gravity waves in the NH winters are generated by deep convections, but I am wondering whether gravity waves over monsoon region are generated by deep convection. Property of convection would be different between tropics and mid- latitudes. Monsoon includes several small-scale disturbances, and jets vary associated with monsoon activity. Do 2.5 hPa gravity wave variances over the Southeastern US monsoon and India-South China monsoon certainly correspond to distribution of “deep convection”?

To answer this question, we include Fig. R1 and R2 here using methods from Choi et al. (2012, submitted to JGR, under revision), where they compare GWs in the GCM filtered by the AIRS “observational window” directly with AIRS observations.

In their CGWD scheme, a spectrum of GW rays is lunched at the top of convections with parameters proportional to the convection strength and depth (for details, please refer to their paper and Song and Chun [2008, JAS]). Despite their scheme’s imperfectness, you can find in Fig. R1 and R2 that the GW spectra do not significantly differ between Southern Hemisphere deep convective regions during January and Northern Hemisphere monsoon regions during July.

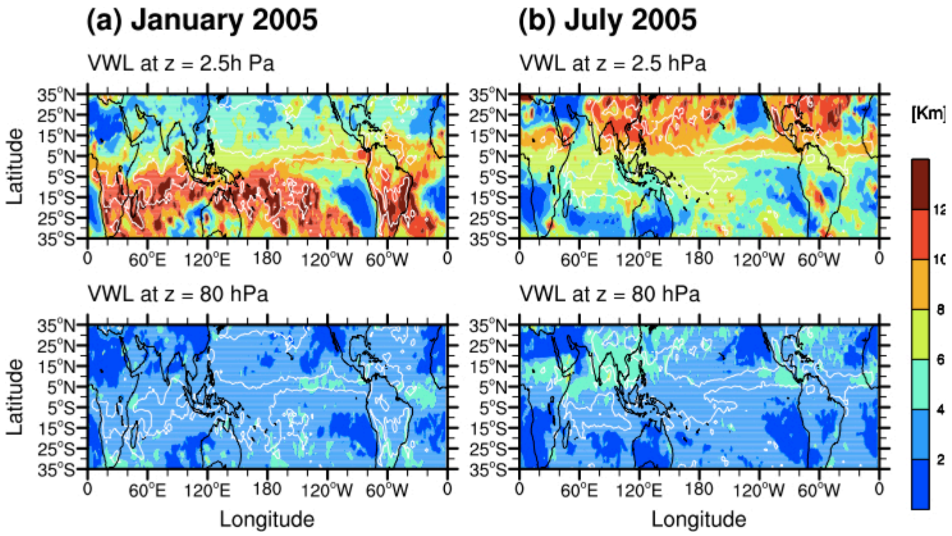

Figure R1: Vertical wavelength (km) of GW rays at the launch level (80 hPa) and at 2.5 hPa using CGWD scheme in Choi et al. (2012, submitted to JGR, in revision). White contour line is NCEP deep convective heating (DCH) rate. Courtesy to Hyun-Joo Choi.

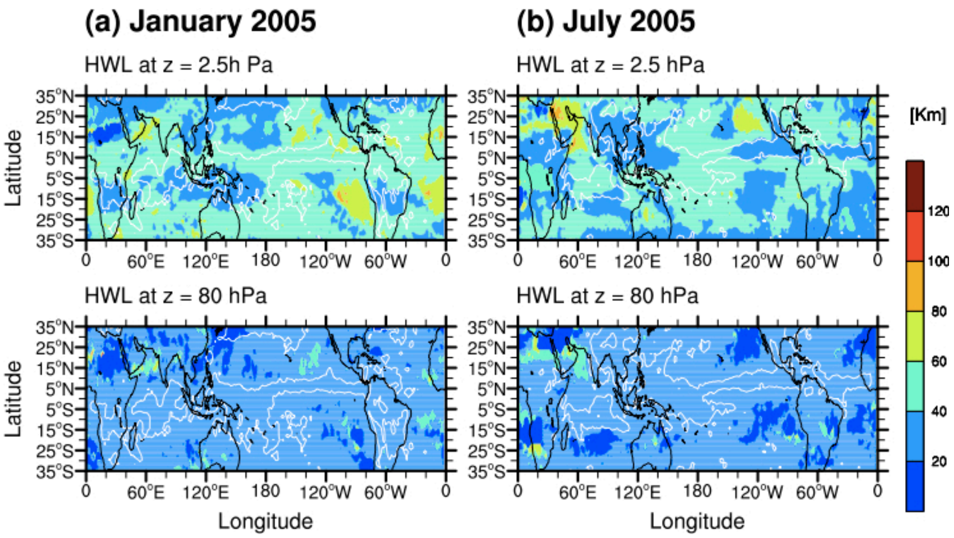

Figure R2: Same with Fig. R1, except for horizontal wavelength (km). Courtesy to Hyun-Joo Choi.

(3) In section 4.3, the authors discussed gravity wave variation associated with the QBO. The authors found that AIRS could observe gravity waves with vertical wavelength smaller than the thickness of the waiting functions. However AIRS generally observes high-speed internal gravity waves with vertical wavelength larger than 12 km.

The maximum speeds of the easterly and westerly associated with the QBO are -35 m/s and 20 m/s. Pure internal gravity waves with zonal phase velocity Cx=20 and Cx=35 m/s have vertical wavelength of ~5.6 km and ~10km under square of buoyancy frequency N^2 of 5.0E-4 s^-2. Most gravity waves observable by AIRS are not affected much by the QBO.

Thus, it is not surprising for me that there are some discrepancies between AIRS gravity wave variances and the QBO phase seen in Fig. 9, and the sentence below is not adequate, I think.

Line 27-29 of page 11709: “It suggests that the GWs observed by AIRS may play a more important role for the descent of QBO westerly phase than that of the easterly phase as more AIRS GWs are removed and hence deposit their momentum fluxes in the QBO westerly phase”

On the other hand, the authors mentioned from line 2 of page 11710 as follows: “the GW variance is only slightly modified by the QBO rather than playing a dominant role on the formation/propagation of QBO phases. This is expected since GWs seen by AIRS are mostly high frequency waves that 5 are usually with fast vertical group velocity”

I recommend the story written in section 4.2 should be revised majorly.

We agree with the reviewer’s opinion that section 4.2 has some ambiguous arguments. The 3rd and 4th paragraphs of section 4.2 have been hence altered significantly to clarify our major points, and 5th paragraph is largely deleted.

First of all, we do see QBO signals in both the GW variance and the east-west difference, as shown in Fig. 9, which indicate interactions between AIRS observed GWs with QBO winds. The GW variance is strongly suppressed not only along the zero-wind line of the westerly shear, but also when westerly phase is fully presented (i.e., strong westerlies, weak wind shear between 20 and 80 hPa), while it is the opposite at the easterly phase. This means that the AIRS observed GWs filtered more by QBO westerlies than QBO easterlies, which reversely contribute to the downward propagation of QBO winds. We admit, however, that it is just a preliminary finding as the record length is too short and we haven't investigated the cause yet.

Secondly, it is suggested by the small amplitude of QBO signals that AIRS observed GWs do not contribute much on the downward propagation of QBO winds, majorly because that AIRS GWs are relatively high frequency GWs with long vertical wavelength. I think the reviewer agrees with this point.

As pointed out by the reviewer, GWs with vertical wavelength ranging between 5.6 and 10 km can impact QBO and QBO downward propagation. AIRS does have a sensitivity to these GWs in the lower stratosphere, as explained in section 4.3. Therefore, the first and second points do not conflict with each other. We thank the reviewer for pointing out this range, and we include it in the revised manuscript (section 4.3, paragraph 4).

Minor comments
(1) Costal lines in Figs. 5, 6, 8 should be denser.
Done. Thanks for your suggestion.

(2) line 27 of page 11710: Wakatani should be changed to Kawatani
Done. Sorry for the mistake.