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Atmospheric Measurement Techniques Discussions

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7, C3662–C3670, 2014

Interactive Comment

Interactive comment on "Intercomparison of stratospheric gravity wave observations with AIRS and IASI" *by* L. Hoffmann et al.

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We thank the referees for the time and effort spent on the manuscript. We carefully considered all comments and hope that our revision properly addresses all concerns and issues raised. Below please find the point-by-point replies. A revised manuscript with 'tracked changes' is made available as an electronic supplement.

Anonymous Referee #1

This paper compares the stratospheric gravity waves in two nadir viewing instruments, AIRS and IASI. Although the AIRS gravity waves have been extensively investigated by many scientists, this is the first paper introducing stratospheric gravity waves observed by IASI. Careful characterizations of the two instruments are performed by the authors. The discussion of the impact of noise upon the observation is satisfactory. Several case studies of mountain waves and convective gravity waves are presented. The morphology of these gravity waves are in well agreement between AIRS and IASI. Furthermore, statistical studies are carried out to examine the diurnal, seasonal and spatial variations of stratospheric gravity waves in both AIRS and IASI. The "hot spots" of mountain waves and convectively generated gravity waves agree with previous findings.

This is a well-written paper and the analysis of the gravity waves in IASI is significant. Indeed, it is promising in the near future to combine both AIRS and IASI measurements and broaden the spatial and temporal coverage of stratospheric gravity waves in the global scale. I recommend the paper to be published at AMT with a minor revision.

Major comments:

1. The non-LTE effect is important for the daytime CO2 4.3 um brightness temperature measurement. Please elucidate its effect with one paragraph. Does the non-LTE effect both AIRS and IASI in the same way?

Reply: The non-LTE effect is indeed important for the 4.3 micron brightness temperature measurements. In Sect. 2.2 of the paper we pointed out (p8426, l18-22): "Daytime scene temperatures are up to 10 K higher than nighttime values as the CO2 molecules experience solar excitation and get into the state of non-local thermodynamic equilibrium (non-LTE) (e. g., de Souza-Machado et al., 2006)." The paper of de Souza-Machado referenced here provides a detailed description of the non-LTE effect on the AIRS 4.3 micron brightness temperature measurements. A more detailed discussion of the non-LTE effect is also presented in Sect. 3.4 of our paper. We slightly revised on p8434, l5-8: "Comparing day- and nighttime data (not shown), we found that the daytime background temperatures were biased high by up to 10 K within +/-60 deg latitude. The bias diminishes towards the poles. It is attributed to the non-LTE effect (Sect. 2.2). Note that the non-LTE bias of the daytime background temperatures is rather constant

AMTD
7, C3662–C3670, 2014

in space and time and is effectively removed by the detrending procedure." However, the difference in background temperatures is still important regarding the measurement noise, which depends on the scene temperature. In Sect. 3.4 we pointed out (p8434, l19-21): "Day- and nighttime differences in background temperatures due to non-LTE map into noise as well, with nighttime noise variances typically being 2-5 times larger than daytime noise variances at low and mid latitudes." Our analysis indicates that the non-LTE effects on the AIRS and IASI measurements are rather similar.

2. Can the authors comment on the possible CO2 15 um measurement of gravity waves by AIRS and IASI?

Reply: A comparison of 4.3 and 15 micron measurements would be very interesting, but we feel that is beyond the scope of our current study. Nevertheless, we agree that this aspect should be mentioned in the paper and we added the following statement in the introduction (p8418, l9): "The 15 um waveband of CO2 offers another choice for gravity wave studies. As the 4.3 and 15 um measurements usually have different characteristics in terms of vertical coverage, sensitivity, and noise they can be considered complimentary to each other. Our study focuses on the 4.3 um measurements because these have not been studied extensively before." We would like to point out that we performed a qualitative comparison with the gravity climatology of Gong et al. (2012), which is based on AIRS 15 um measurements. This is discussed in Sect. 4 of the paper (p8439, l12-24).

Minor comments:

1. Abstract: suggest to remove the actual numbers of 45% and 30%. Without their definitions (relative to which value?), these numbers are confusing.

Reply: We followed the suggestion and removed the numbers from the abstract.

2. page 5, line 6: are these equator crossing times? if so, please mention it explicitly.

Reply: We rephrased "For gravity wave research it is promising that AIRS and IASI

AMTD

7, C3662–C3670, 2014

Interactive Comment

take measurements at different local time, with Equator crossing times around 01:30 and 09:30, respectively." More details on the local times of the AIRS and IASI measurements are presented in Fig. 1 of the paper.

3. page 9, line 5-7: please explain this exception. Seems all the tropospheric emissions are not important in the last sentence (line 3-5).

Reply: We expanded this paragraph to explain the exception: "Tropospheric emissions from interfering species like water vapor, from clouds, or from the surface are therefore not expected to influence the 4.3 um measurements in most cases. The only exception we found are daytime scenes with very large contrasts between land- and sea-surface temperatures. These scenes occur near the coasts of desert areas and may have surface temperature differences up to 40 K between neighbouring pixels. For these scenes we found minor biases (up to 0.1 K) between the pixels in the 4.3 um measurements. This specific case needs to be treated carefully or should be excluded from gravity wave analyses." A physical reason for the bias in the 4.3 um measurements has not been identified. It may be related to instrument effects.

4. Figure 5, It reads odd to have "sensitivity" as x-axis in the left-hand panel and as y-axis in the right-hand panel. Please make these two panels consistent.

Reply: We changed Fig. 5 so that both plots show wavelength on the x-axis and sensitivity on the y-axis.

Anonymous Referee #2

In this study, L. Hoffmann et al. have compared the retrieval of stratospheric gravity waves with two infrared hyperspectral limb-sounding instruments: AIRS and IASI. While AIRS data have already been extensively used to provide characteristics of gravity waves in the stratosphere, it is the first time that the IASI instrument, which shares many similarities with AIRS, is used in this respect. The approach used by the authors to derive gravity-wave disturbances in brightness temperatures from the raw spectrally-

AMTD

7, C3662–C3670, 2014

resolved radiances is described in a very rigorous way, as is the detailed comparisons between the instrument performances and sensitivities. The authors then illustrate their results with 3 case studies of orographic and convective gravity waves that enable the reader to get a feeling of the similarities and differences between AIRS and IASI gravity-wave retrievals. A first global climatology of gravity-wave brightness temperature variances is provided at the end of the article. The article is well written, and the authors' method and results are clearly stated and discussed. It provides solid foundations for the future use of IASI observations to extend our knowledge of gravity waves in the stratosphere. The article perfectly fits with AMT, and I thus recommend to publish it with only minor revisions.

Minor comments

p8422, l26: you have chosen here to convolve the IASI spectra with a Gaussian window to mimic AIRS spectral resolution. Although such treatment is perfectly justified in this comparison, I wonder how it might degrade gravity-wave retrievals from IASI raw measurements? Can you tell us how you expect the IASI results would be modified if such treatment were not performed?

Reply: Although the convolution of the IASI spectra with spectral response functions representing AIRS may facilitate a more direct comparison of the radiance measurements, we realized that this approach may cause some confusion. In our analysis we are only comparing spectral mean radiances in the spectral ranges indicated in Fig. 3. For this comparison it does not matter much whether the IASI spectra are convoluted with the AIRS spectral response functions or not. We decided to replace the IASI plot in Fig. 3 to show spectra at the full nominal resolution of the Level-1C data instead. We removed any statements regarding the convolution of the AIRS data and added (p8423, l3): "Note that IASI has much higher spectral resolution than AIRS at 4.3 um and that the instrument is capable to resolve individual CO2 lines." On the one hand the new IASI plot in Fig. 3 still allows to identify the temperature differences of the spectra due to the gravity wave. On the other hand it illustrates the potential of the

IASI 4.3 micron measurements to provide information on different altitude ranges due to the different optical depths of the individual spectral grid points. The new IASI plots also fits better to the plot of the IASI temperature kernel functions in Fig. 4, which are calculated for IASI spectra at full nominal resolution and not for the convoluted data.

p8424, l8-9 and 21-24: I have the impression that the detrending procedure you are using to subtract the variance associated with large scale processes might remove signals due to gravity waves if the gravity-wave packet phase lines are (nearly) aligned with the cross-track scan direction (i.e. if the wave propagates along the satellite track). Is this correct? If yes, the sensitivity to horizontal wavelengths shown in Figure 5 represents actually a maximum sensitivity, and this sensitivity might be degraded depending on the wave direction of propagation. I encourage the authors to discuss this point, and to indicate how this directional sensitivity might influence the gravity-wave climatology provided at the end of the paper.

Reply: The reviewer has described this issue correctly. It was also identified in earlier studies (e.g., Hoffmann and Alexander, 2010). To clarify, we rewrote in Sect. 2.2: "Note that this detrending procedure will suppress wave components with wave fronts parallel to the across-track direction. Along-track smoothing of the background may help in this case, but we found that it can introduce problems in regions where there are strong latitudinal gradients in the temperature field, e.g., at the polar vortex edge, and we did not consider it here." We also clarified in the caption of Fig. 5: "The plot shows the maximum horizontal sensitivity, which is obtained for gravity waves propagating in the across-track direction." We think that this effect may explain some of the differences between the day- and nighttime data seen in AIRS and IASI. We added in the discussion (p8439, l12): "Part of the difference may also be due to the different sensitivities of AIRS and IASI to gravity waves propagating in the along- or across-track direction. In particular, this may play a role if the gravity waves are observed from either ascending or descending sections of the satellite orbits."

p8425, l9-11: This point should at least be clearly stated in the caption of Figure 5,

AMTD
7, C3662–C3670, 2014

or even better, Figure 5 (right) should be redone with this finite resolution effect (and the x-axis going down to 0). Right now, this figure gives the erroneous feeling that the AIRS and IASI sensitivities are perfect down to infinitely small wavelengths.

Reply: We agree and added a statement in the caption of Fig. 5: "Note that the sensitivity to horizontal wavelengths shorter than 100 km was not analyzed here. In this case the sensitivity is limited by the satellite footprint size (see text for details)."

p8429, l20-24 and Figure 8: I have found these lines quite confusing. Especially, I am not sure of what is meant by "wave amplitudes" here. Is this brightness temperature amplitudes, or sensible temperature amplitudes? Similarly, the color bar of Figure 8 left panel refers to "4.3 BT perturbation (K)" (which is perfectly clear), while that of the middle panel refers to "amplitude (K)" (which is confusing)? It would furthermore be very helpful if only one wording were used (either perturbation or amplitude). Last, I wonder whether the "horizontal wavelength" on Figure 8 right panel actually refers to the horizontal wavelength along the cross-track direction. Is this right or are you using a 2D S-transform?

Reply: We tried to clarify this by rewording parts of the paragraph in the following way: "We performed a 2-D spectral analysis for the two satellite overpasses using the S-Transform (Stockwell et al., 1996), and the method described by Alexander and Teitelbaum (2007). This approach has the distinct advantage of providing spectrally-resolved information on wave amplitude and phase on a variety of local scales. Results are presented in Fig. 8. The patterns found in the wave amplitudes provided by the spectral analysis clearly coincide with the wave patterns in the observed brightness temperature perturbations." We here used the term "amplitude" intentionally to refer to the outcomes of the spectral analysis, which is directly based on the observed 4.3 um brightness temperature perturbations. To confirm that "horizontal wavelength" indeed refers to the combined wavelength in along- and across-track direction we replaced "spectral analysis" by "2-D spectral analysis" in this paragraph. (Please note that the method of Alexander and Teitelbaum (2007) is based on a 1-D S-Transform in the across-track direction. It than uses the phase information to extend the analysis in the along-track direction. More details are provided in the reference.)

p8431, l12-13 and Figure 9: It might be worth stating how the local variances are obtained in the caption of Figure 9.

Reply: We added the following statement: "The local variances are calculated based on the perturbations of footprints within circles with 100 km radius."

p8432, l23: "westerly"? Summer stratospheric winds are easterly winds...

Reply: We replaced "westerly" by "easterly".

p8434, l5-11: I did not succeed to follow this discussion. "IASI daytime temperatures are warm biased by up to 10K"... "AIRS and IASI background temperatures compare very well" (night-time temperatures then?)... "Daytime AIRS temperature are 2-3K larger than IASI" (so they are even more biased?)

Reply: We revised the first part of this paragraph regarding the day- and nighttime differences of the background temperatures according to the major comment 1 by reviewer #1. (It is argued that both AIRS and IASI have a $\sim$10K high bias at daytime due to the non-LTE effect.) We also revisited the analysis of the differences between the AIRS and IASI background temperatures and decided to simplify the second part of this paragraph: "Comparing AIRS and IASI (not shown), we found excellent agreement in the background temperatures with differences being less than +/-2 K at all latitudes at day- and nighttime."

p8435, l2-3 and l7-8: On the one hand, you seem to suggest that the high-latitude gravity-wave cycle is mainly due to source effects, while on the other hand you argue that it is mainly due to wind-induced satellite visibility. My feeling is that the latter definitely makes sense given your sensitivity analysis and Figure 11 (ECMWF winds), while you do not have strong arguments here to support the former.

Reply: We reworded this paragraph to stress that the observations are indeed most

AMTD
7, C3662–C3670, 2014

closely related to the wind-induced visibility effect. In particular, we removed the statements pointing to the source effects.

p8436, l17: Is it "K" here or "Kˆ2"? Your correlation coefficients apply to gravity-wave variances, and Figure 13 and 14 also display variances.

Reply: This number refers to variances. We replaced "K" by "Kˆ2".

Figure 11-13: I would encourage the authors to be more specific on the axis/colorbar title: "4.3 um BT variances" rather than simply "variances".

Reply: The term "4.3 um BT variances" might be more specific, but we are afraid it may also be misleading because what is shown in the plots is variance due to gravity wave signals only and not the full variance of the 4.3 micron brightness temperatures (which would include background signals and noise). Therefore, we would like to suggest to leave the term "GW variance" unchanged in Fig. 11, 13, and 14. In Fig. 12 we used "variance" only, because the curves in the plots show both the variances due to the gravity wave signals and the variances due to noise. It may become clear from the captions of the figures that all results are referring to 4.3 micron brightness temperature data.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/7/C3662/2014/amtd-7-C3662-2014-supplement.pdf

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