Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-235-RC2, 2017

Atmospheric Measurement Techniques Discussions

Interactive comment on "Intercomparison of AIRS and HIRDLS stratospheric gravity wave observations" *by* Catrin I. Meyer et al.

Anonymous Referee #2

Received and published: 9 September 2017

Overall comments:

The manuscript presents some interesting and new results on how well gravity wave results from HIRDLS and AIRS high-resolution retrievals agree with each other in statistical averages, and in some individual cases. It also presents informative results that extend and confirm previous conjectures on the complementarity of nadir and limb measurements, without, however, acknowledging some of that previous work sufficiently. The comparison of AIRS and HIRDLS observational filters is very nice, as are the comparisons of the two data sets for orographic and non-orographic waves, and the comparisons of seasonal patterns of variance. Although a minor point of the paper, the comparison of the gravity wave calculations based on AIRS operational and high-resolution data shows why the latter are needed.

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Discussion paper

However, the description of the instruments and data used is sometimes unclear, and occasionally wrong or misleading. Similarly, the description of the filtering is also occasionally unclear. The advantages of the filtering they have used, and the differences from alternative methods, is not spelled out.

The wording is sometimes poor or awkward.

Specific Comments:

Sec. 2.1 needs to be revised. The beginning is quite stilted. It could be noted that the 3x3 pattern of AIRS footprints fit within the footprint of the microwave instrument, which is used in the cloud-clearing approach. The discussion of the high- resolution data is needed, but should be made clearer. The source of the pressure mentioned on p. 4, l. 23 is not clear. Any additional references for the systematic errors and retrieval diagnostics would be useful if they exist. Do ll 35-36 mean that only nighttime data are used in this study? This seems to be the case, but t is not clearly stated. The range of the high-resolution retrieval is stated to be 10 to 70 km, with 5-6 degrees of freedom- does this mean that the vertical resolution is 10-12 km?

In the discussion of HIRDLS, it could be noted that HIRDLS was damaged during launch, precluding its planned ability to scan in azimuth, which would have given it 3D capabilities [Gille et al., 2003]. The damage resulted in its single view direction of -47° relative to the orbit plane. This also required extensive corrections to the processing algorithms [Gille et al., 2008, 2011]. Measurements of thermal emission with 1 km vertical resolution are made in 4 channels on the long-wave side of the 15 ïĄ■m bands, from which the temperature is retrieved as a function of pressure Khosravi et al. [2009a,b]. The Field of View of the instrument is always 1 km; the resolution of the retrieval varies with altitude.

Sec. 2.2: The “background removal” for AIRS is local, within one cross-track scan, ∼25°. It is noted that this strongly suppresses wave fronts parallel to the cross-track direction which cover large fractions of each scan. Why isn’t this an important problem?

This seems much different from the method described for HIRDLS. Why couldn't this approach have been applied to comparable data from the overlapping 31 day time windows of HIRDLS data? It would be interesting to see how different those results would be from those used by Fetzer and Gille [1996], Alexander et al. [2008] and Wright et al. [2011, 2013], who used departures from 6 or 7 planetary scale waves that varied smoothly in time. Note that the HIRDLS V6 data also have a gridded product (using a Kalman Filter approach described in Gille et al., 2011). Please clarify the last sentence of the first paragraph on p. 6. It appears that all small-scale perturbations that get through the filtering are assumed to be GW. Is this correct? Is there evidence for this assumption? Please comment on last sentence of second paragraph: it is surprising that NH variance in winter is > SH variance in winter, given the large zonal winds and the Andes and Antarctic peninsula as such a large source.

Sec. 2.3: HIRDLS empirically estimated precisions for V6 appear to be underestimated. The values for V7 are much closer to the predicted precisions up to $\sim$ 0.5 hPa, above which they are smaller [Gille et al., 2012]. The last sentence of this section is unclear.

Sec. 2.4: First paragraph- the treatment of wave phases is not clear. These sensitivity functions, and related discussion, are close to those of Wright et al. ACP 15, 8459-8477, 2015 [2015], which should be referenced and included in the discussion.

Sec. 4, toward end, could note that some combination of limb and nadir observations was done by Wright et al., GRL 43, 894, 2016.

Figures: Figure 8: Suggest second sentence change to . . .presence of high clouds associated with a storm system. . .

Technical comments:

p.1, l. 3: presumably vertical and horizontal resolution l. 12, also l. 22- better word than conform needed l. 18- better word than "fit" needed p.2, l 17 give overviews, l. 18

comparisons p. 3, l. 1 Suggest “Zonal average differences tend to be . . . l. 31 scan covers 1780 km p. 5, ll1,2: combine the first 2 sentences. l. 20: measurement typically consists p. 6, l. 28: data are p. 7, l. 16: perturbations, l. 21: The sensitivity function of the current generation of limb sounders. . .

p. 9, l. 31: Does the sentence beginning in this line refer to Figure 6? Text not clear.

p. 12, l. 28: . . .current limb measurements.

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