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Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #3

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The paper compares the gravity wave detection capabilities of the AIRS nadir sounder and the HIRDLS limb sounder. Reviewer 1 has already described the science area in some detail, so I will not repeat this except to say that the area is of significant current interest and the study is eminently suitable for AMT.

Reviewer 2 has already addressed several important technical details, and I agree with him/her that these should be addressed. In particular, I strongly agree with his/her comments that the large differences in background removal method are important, and will discuss this further in my comments below.

I also agree with both other reviewers that the language needs some work, although it is generally clear throughout and to a certain extent can be handled in copy-editing. Aside from this minor issue, the paper is well-structured and clear, and I suggest only

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Discussion paper

moderate additional revisions beyond those suggested by Reviewer 1 and 2.

=======Major comments=======

(specific references are listed at the bottom of the page)

1. I feel that the time difference between the two datasets could do with more consideration. This takes two main forms:

1a. in figures 4,5,6 and 9, the waves appear to be in almost exactly the same phase to the eye. For the mountain wave case, this is quite plausible; however, for the non-orographic case I'd like to see more evidence to confirm why this is so. In particular, since the full three-dimensional wavenumber vector can be inferred from the available data, it should be possible to infer the phase and group velocity of the wave (e.g. Fritts and Alexander 2003; Wright et al 2017), and hence confirm if the change between the two measurement times is indeed so small.

1b. in the global time series of variance, presumably there is a not-insignificant time-of-day difference between the two datasets. There's not much that can be done about this, but a little more discussion of how it may affect the results would be useful. This is likely to be most significant in the tropics, where convection has a diurnal cycle: while Aura and Aqua cross the equator in formation, the high viewing angle HIRDLS uses presumably means the scan track will cross at quite a different time than AIRS' nadir sensor.

2a. (also discussed by reviewer 2). The background-removal analysis is inconsistent between the two datasets. I'm not sure why this needs to be so: since global data is available for both AIRS and HIRDLS, presumably a common background removal method could be implemented, presumably more similar to the HIRDLS method used in the paper.

2b. also, why in particular is a fourth-order polynomial specifically used for the background removal? I realise this is in common with previous studies, but my under-

standing was that this was to remove solar glint from the AIRS radiances, which is presumably removed in the 3D temperature retrieval. [I am happy to be corrected on this!]

3. P10L21 onwards: You refer in passing to a double-peak in HIRDLS GW variance at 44N in winter 2007, with no attribution, but then explain in detail a similar features in the AIRS data as being due to an SSW. I definitely believe the AIRS feature - for example, the AIRS time series look extremely similar to figure 3 of Wright et al (2010) and it may be useful to say this - but it seems odd to focus in the text on this relatively small feature of the AIRS time series but not on the (to my eye) much larger change in the HIRDLS series in early 2007. Do you have any idea why the HIRDLS double-peak in early 2007 occurs?

4. The idea of combining limb and nadir sounders has been used previously, for example by Wright et al (GRL, 2016) [and references therein]. It would be useful to mention this in your conclusions, where you suggest that combining limb and nadir datasets for better coverage would be useful. [I realise there are important differences in the two approaches!]

=======Figures=======

5. The colourbars on figures 1, 10 and 11 are extremely difficult to read, with most of the range condensed into a small region on the left and the rest used solely to indicate the extrema in the data. They need to be modified significantly to be useful; saturation in some regions should be an acceptable tradeoff for clarity over most of the globe.

6. Related, most graphs makes heavy use of both red and green; this is difficult for our colourblind colleagues, and should be modified if possible by e.g. changing line styles as well as colours.

7. Figure 7 has the upper panel is labelled in km, and the lower panels in hPa. While the conversions are given in the text, this still makes it hard to read. I'd suggest either

adding a pressure axis to the upper panel or changing the titles of the lower two panels.

8. I'd also suggest putting a box on the maps on figure 7 showing the region covered by figure 6.

9. The black circles on figures 4 and 6 are quite hard to see on my screen; I'd suggest either strengthening or enlarging them.

10. I'd suggest rearranging figures 7 and 8 to not be between figures 6 and 9, as I had to scroll a lot to match up the common features in figures 6 and 9.

11. You refer to both the predicted and directly-estimated precision for both HIRDLS and AIRS for figure 2, but only show one for each. Is there a reason?

12. Use of 'boreal winter 20XX' in several places is ambiguous - is this: December 20XX - February (20XX+1), or December (20XX-1) - February 20XX? It would be clearer to specify it as, e.g. DJF XX/(XX+1), to remove the potential ambiguity.

=======Minor Comments=======

13. I don't understand P05L13 - please rephrase.

14. HIRDLS version 6 is now fairly old, and was supplanted several years ago. Is there a particular reason this was used?

15. P09L30: what height is the 8.1um channel, approximately?

Fritts and Alexander (Rev. Geophys, 2003), doi:10.1029/2001RG000106

Wright et al (Atmos. Chem. Phys., 2017), doi:10.5194/acp-17-8553-2017

Wright et al (Geophys. Res. Lett, 2016), doi:10.1002/2015GL067233

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