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Atmospheric Measurement Techniques Discussions

Interactive comment on "Multi-instrument gravity-wave measurements over Tierra del Fuego and the Drake Passage – Part 1: Potential energies and vertical wavelengths from AIRS, COSMIC, HIRDLS, MLS-Aura, SAAMER, SABER and radiosondes" *by* C. J. Wright et al.

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This paper is very ambitious in its goal of "... understanding and interpreting gravity wave measurements in their complete multi-dataset context, rather than as individual spotlights on particular parts of the wave spectrum." It tries to advance this goal through analyzing a number of datasets in the geographic region over Tierra del Fuego and the Drake Passage. The authors have been very careful in using rather similar analysis

methods on very different types of data, and interesting results have been obtained. I was struck by the authors making the rather obvious point in their abstract that, "no one dataset can represent the whole spectrum of gravity waves in the terrestrial atmosphere. This point has been made many times before (e. g., in Alexander et al., 2010).

The authors refer to Geller et al. (2013) quite a bit, but they never pick up on an important point in this paper. This is that the satellite-derived gravity wave momentum fluxes seem to fall off with altitude faster than they do in models, both in models with gravity wave parameterizations and also in models that seek to resolve a good part of the gravity wave spectrum. I don't know how the authors would resolve this with their result that they see little attenuation of the gravity wave potential energy with altitude in their observational analysis. In the Introduction they imply there was significant discussion of gravity wave horizontal wavelengths in Geller et al. (2013). There was not. Also, the paper uses some "loaded" words, such as "large discrepancies" and "large dissimilarities" between observational and model datasets. In fact, Geller et al. (2013) noted that there were significant similarities between results from models and results from observations, and several differences were also noted between gravity wave momentum fluxes in models and observations. In the authors' paper, there was the implication that SABER and HIRDLS differences in their derived gravity wave momentum fluxes were not discussed, but we a lot of attention was paid to this point in Geller et al. (2013), and there was significant discussion indicating that the data selection criteria were different in the two analyses of HIRDLS data, and SABER analysis was similar to one of the HIRDLS analyses.

On page 6811, it is said that the altitude resolution of the meteor radar is due to the physical processes by which meteor trails are generated. I think this is rather due to the observational method, which sees the Fresnel reflection and also the measurement technique for altitude resolution.

The discussion on page 6825 says that "visual inspection" suggests that AIRS and

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SABER are seeing the same wave. This is not obvious to me. Perhaps, the physical processes are generating several waves?

I am really confused about figure 11 showing that the waves are not undergoing dissipation with increasing altitude. Are the authors suggesting that no critical levels are present, and there is no wave breaking? I doubt that is the case. The authors mention waves being Doppler shifted into and out of the observational windows several times in this paper. Are the Doppler shifting and other effects making this appear so. Wang and Geller (2003) found a great difference between gravity wave energy in the troposphere and stratosphere. Also, they found a good correspondence between 200 hPa wind and gravity wave energy in the gravity wave energy in the lower stratosphere, derived from radiosonde observations. This paper should probably be referenced in the discussion on page 6834.

In section 8.3, the paper says "little evidence of wave dissipation below $\sim$ 85 km in seen in any dataset. This is very odd, and seems be at odds with other evidence that there is a zonally averaged mesospheric meridional flow at these altitudes that can only be maintained by gravity wave drag there. Also, what about the evidence that HIRDLS and SABER gravity wave momentum fluxes falls off so fast (in Geller et al. 2013)?

Wang and Geller (2003) have previously indicated that the tropospheric gravity wave energy is virtually uncorrelated with the gravity wave energy in the lower stratosphere. That being the case, the lack of correlation with surface winds is not surprising. What would be surprising is no fall off in gravity wave energies and such a lack of correlation.

I find many of the figure captions to be inadequate. For instance, in figure 5, the different symbols are not explained in the caption. They are explained in the text, but they should be explained in both.

Looking at figure 8, what is being plotted apparently is momentum flux per unit mass, if the units on the ordinate are correct. That should be clarified.

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Figure 9g suggests that radiosondes can measure all horizontal wavelengths. How can this be so?

If the same wave is being seen by AIRS and SABER, this should be apparent from the derived wavelengths. Perhaps, these should be shown.

The annual variations strongly suggest that wind filtering is playing an important role. How is this consistent with the stated lack of gravity wave energy attenuation with increasing altitude?

I apologize for the negative points in this review, but it seems that the authors have wondered off a bit from their original objective to see what different observational methods of the same gravity waves show. Their discussions of the results and some of their interpretations need to be toned down or deleted, in my opinion.

âĂČ References

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