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Interactive Comment

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.

Anonymous Referee #2

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This study undertakes the task of identifying the differences in the capabilities of a variety of instruments to observe atmospheric gravity waves in the specific geographic region at the tip of South America known to produce some of the largest amplitude orographic waves in the Earth's atmosphere. The motivation for this study is sound and some interesting results have been presented, however I feel it has not reached it's full potential.

The comparison of SAAMER radar to the other instruments is treated very casually. The justification is not clear for assuming that the wind variance, which is a measure of kinetic energy, is linearly proportional to potential energy. The approximation that the ratio of kinetic to potential energy is constant can be reached through the assumption of a model universal spectrum as in VanZandt, (1985). This model is largely empirical relying on many assumptions, the validity of which should be considered for the present study. If the authors rely on an alternate justification this should be detailed. In general the relationship between kinetic and potential energy is frequency dependent (e.g. Geller and Gong, (2010)), so given that the various instruments are observing different bands of the gravity wave spectrum this should be considered.

Given that the purpose of the present study is to provide a quantitative comparison of observations, simply assuming the generalised results of previous studies without discussion does not seem appropriate. The comparison of GPS RO gravity wave potential energy to radar kinetic energy has been undertaken in the past by Tsuda et al., (2000) and Nastrom et al., (2000) who discussed this issue in detail. I don't think that these studies alone can be used as justification for the assumption made here given the differences between the regions considered, altitudes considered (stratosphere vs mesosphere-lower thermosphere), and instrumentation compared (GPS RO and Doppler wind profiling radar vs IR limb sounders and meteor radar). I think the comparison of the meteor radar energy estimates with those of the other instruments should be treated much more carefully or left out of this part of the study (no doubt the momentum flux comparison in part 2 would be very interesting).

The authors note that the observed exponentially increasing GWPE profiles in figure 11(a) imply minimal dissipation of gravity wave energy and equivalently free amplitude growth for non-dissipating waves. This would be an important conclusion as it is not consistent with previous momentum flux observations which indicate strong gravity wave dissipation over the altitude ranges considered here e.g. Geller et al. (2013). However I do not think that it is all that meaningful to draw conclusions about wave

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dissipation from profiles of the annual median GWPE given the vastly different middle atmosphere wind regimes that characterise the different seasons in this region.

In general too much emphasis is placed on the annual medians when comparison of the seasonal median profiles can provide more insight into instrumental differences over a range of conditions, particularly given the implications of the wind profiles for seasonal variations in the gravity wave spectrum. Rather than focusing on the seasonal differences of each instrument in the plots, figures 11(b)-(h) could be replaced with four plots, one for each season, in which all instruments are compared in the same manner as in figure 11(a).

Analysis of the seasonal profiles is more appropriate however the authors conclude in section 8.3 that there is little evidence of any dissipation below 85km in seasons other than spring. To my eye this is not the case at all, for example, the summer and winter profiles for HIRDLS obviously diverge and then converge with increasing altitude, and other deviations from the exponential scale exist in the other instruments.

Discussion of wave dissipation is complicated, as the authors note, by the possibility of Doppler shifting of waves into and out of the various instruments' observational filters due to the strong wind shears which are present. Further complicating this, for a non-dissipating wave which is Doppler shifted by the background wind shear, conservation of wave action implies that a change in intrinsic frequency results in a relative change in wave energy. So conclusions regarding wave dissipation should be drawn carefully.

Given that the stated aim of the study is to provide a quantitative understanding of the key differences between the observational filters of the various instruments, the study is quite light on conclusions relating to this, section 8.2. The first point in this section is a point laboured a bit too hard in the manuscript in general given how well established it is. This is not so much a conclusion of the present study as a motivation for it. There is some discussion in various parts of the manuscript the possibilities of substituting one dataset for another or using one as an overall proxy for wave activity but there is

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no mention of how the different observations could be used together.

The analysis method used here preferentially selects large amplitude, long vertical wavelength waves in a region where such waves are preferentially excited, so it is not surprising that the satellite instruments, which are inherently most sensitive to such waves, show such good agreement. While this is good for cross validation of certain aspects of these instruments it does not seem to be the optimal means of identifying the key differences between their observational filters.

Some mention could be made in the introduction of previous intercomparison studies such as Preusse et al., 2000 and Wu et al., 2006 and again in the discussion.

p6898 l14 - What is the "edge of the lower thermosphere"? The mesopause?

p6801 l1-2 - Geller et al. 2013 addressed this question explicitly, including gravity wave analysis of HIRDLS and SABER data using exactly the same technique (as well as an alternative method of analysis for HIRDLS).

p6803 l7 - I'm not sure but I think this data rate should be 50 Hz not 50 MHz.

p6807 l3 - 50 presumably refers to 50 degrees south.

p6810 l21 - "MDVW of 2" should be "MDVW of 2 km"

p6816 l26-28 - It is not clear what the process of quantisation of S-Transform output is or why it has such a dramatic effect on observable vertical wavelengths.

p6819 l14 - Should across-track be along-track?

p6820 l23 - GWMF should probably be GWPE, although this point obviously applies to both, the present discussion topic is GWPE.

p6824 l11 - The maximum observable vertical wavelengths imposed by the analysis is quoted here as being 30 km for limb sounders and 5 km for radiosondes, however in the analysis descriptions in section 4 these were stated as being 18 km and 3 km in

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practise due to the quantisation of S-Transform output. These latter values should be used in this discussion and in figure 9.

p6824 l23-24 - Radiosonde intrinsic frequency coverage overlaps all instruments except AIRS and MLS.

p6826 l7-9 - The methods applied by Moffat-Griffin et al. (2013) are not the same methods applied to radiosondes in the present study. Moffat-Griffin et al. (2013) use a single polynomial fit to detrend each profile rather than a Savitsky-Golay filter. They then use the mean square of the residual temperature profile to calculate a single value of GWPE for each profile rather than applying an S-Transform to identify the temperature perturbation amplitude and hence GWPE at each height. GWPE values in the centre of the stratospheric profile (the most reliable as stated on p6828) are at least a factor of two larger than those determined by Moffat-Griffin et al. (2013), which seems broadly consistent with the differences in the methods applied.

p6826 l24-27 - The tropospheric radiosonde GWPE profile does not appear to tail off at 10 km, rather the opposite.

p6829 l12 - Remove one of the references to SABER.

p6830 l14 - "in stronger" should be "is stronger".

p6835 l18-20 - The annual distributions at 25-55 km are distinctly skewed. Is this due to seasonal variations (i.e. are the seasonal distributions Gaussian but with different means) or does this reflect the bias in the analysis towards larger events leading to a longer tail in the distributions?

p6835 l20-23 - Reference should be made here (and potentially elsewhere) to Baumgaertner and Mcdonald, (2007) who analysed gravity wave potential energy in CHAMP data in the Antarctic region, finding a lognormal distribution.

p6936 l12 - seasonality should be seasonal.

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p6936 l21-23 - It should be very easy to check this by looking at the actual seasonal histograms. Rather than note that this is "suggested", check the distributions and state it definitively (or otherwise if it is not the case).

p6836-7 l27-2 - It is not clear what is being referred to as the anomalous double-trough, are we looking at panel (i)? Which dataset is "this dataset"?

p6841 l12-14 - On what grounds would these three months be excluded? This is a clear difference between HIRDLS and the other instruments and given the objective of the paper should be investigated not dismissed. COSMIC at 40 km does not seem to exhibit a semiannual cycle either. Any insight into the reasons for these differences in wavelength seasonality across the satellite instruments above 25 km would be worth sharing.

Figure 1 - c), d), e) Axis labels are missing.

Figure 5, 6 - Instrument colour/symbol legend should appear in figure 5.

Figure 8 - The caption describes data as being smoothed by 7 days while in the main text on page 6814 they are described twice (l3 and l13) as being smoothed by 14 days.

Figure 13 - The start and end times of the various time series data here do not correspond to the plots of temporal availability in figure 6(b)-(h).

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