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Interactive Comment

Interactive comment on "Short vertical-wavelength inertia-gravity waves generated by a jet–front system at Arctic latitudes – VHF radar, radiosondes and numerical modelling" *by* A. Réchou et al.

Anonymous Referee #2

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GENERAL COMMENTS

This paper provides a case study of inertia-gravity waves (IGWs) observed at a high-latitude location. It combines radar and radiosonde observations with model simulations. This approach provides a useful perspective from which to understand the observations. The scientific significance is good. Although the scientific quality in the latter parts of the paper is generally good, I do not think that some of the assumptions made in the early parts of the paper are well justified. Some of the arguments would be clearer if material was presented in a different order. Also, some of the important figures are far too small to be useful - notably the right-hand panels of Figs 5 and 6. The presentation quality could be significantly improved.

SPECIFIC COMMENTS

I am generally happy with the idea that the radar power is giving a measure of N^2. A reference should be given for this technique. It is overstating the case to say (line 14, page 31258) "the agreement between the radar-derived and and the sonde-derived N^2, even as regards small fluctuations in the height profiles, is very good." I would only describe the agreement as being "generally good". The first panel of Fig. 1 should be combined with Fig. 2 and with the same scaling. The comparison between the two is fundamental to the rest of the paper. It would also be better to show the radar/sonde comparisons (lower panels of Fig. 1) with the same altitude extent and scaling as the height-time plots of Figs. 1 and 2.

The authors should give a more-detailed and more-relevant description of the WRF model in section 2.3. For example, they should state whether the IGWs arise spontaneously from the model or whether they arise from a particular component of the model - c.f. Rechou et al. (2013). It would be useful to include a reference to an existing study of inertia-gravity waves using this model. My biggest concern is that from an early stage the authors appear to assume that all structure in the N^2 field is the result of IGW activity - e.g. they state on line 27 of page 31258 that "the morphology of the short-vertical-wavelength wave-fronts seen in Fig. 1, is re-produced well by the model (Fig. 2), although the amplitudes in the model are clearly smaller." (as an aside, the final point is not that clear from this style of plot). Variations in N^2 could also arise from mountain wave activity, as shown by Rechou et al. (2013). In fact, the period of time considered in the present paper immediately follows that considered by Rechou et al. (2013) - I don't think this was mentioned. It is therefore clear that there is mountain wave activity above the radar at least at the beginning of the period covered by the present paper. I will return to this point below. The authors do not state whether they

ACPD

13, C12293–C12296, 2014

run the WRF model with or without orography. They merely state (on line 12 on page 31260) that "This means that any waves generated in the troposphere (e.g. orographic waves) would be blocked by the wind reversal, and would not propagate upwards to the stratosphere." They could check this in the model and in the radar and sonde data.

The derivation of IGW characteristics is based on the assumption that they remain similar over the altitude interval 10 - 14 km. However, Fig. 2 seems to suggest a distinct difference above 12 km (where there appears to be little structure on the sub 1 km vertical scale) and below 12 km (where there appears to be much structure at this scale). There is also a suggestion of this in Figs. 1 and 9 (upper panels). The bottom panels of Fig. 9 - showing the hodographs - are quite hard to interpret. It is not possible to see which way the curves are rotating with increasing height and, particularly in the case of the 2 right-most panels, there is too much overlapping detail to see anything useful. Making these panels physically larger, changing the scaling (to cover smaller perturbation velocities) for the final two panels, and/or reducing the height range covered would probably help. Are the authors able to produce similar hodographs using model data? The vertical wavelengths of less than 1 km reported in this paper are significantly shorter than are reported elsewhere in the literature. It is not yet clear to me whether these smaller values are reliable.

Why have the authors chosen a height of 6 km in Fig. 3? Using a height of 9 km - c.f. Lane et al. (2004) - would show up the jet stream location much more clearly. This figure would also be clearer if the panels were made larger.

TECHNICAL CORRECTIONS

The authors sometimes use the term "precision" where the term "accuracy" would be more appropriate: line 24 page 31255, line 12 page 31257.

The authors sometimes use the term "resolution" where the term "vertical interval" would be more appropriate: line 25 page 31257, line 23 page 31258.

ACPD

13, C12293–C12296, 2014

On line 22 of page 31255, "short-vertical-resolution measurements" should either be "high-vertical-resolution" or "short-vertical-interval".

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 31251, 2013.