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Please find the list of corrections of the manuscript entitled "Short vertical-wavelength inertia-gravity waves generated by a jet–front system at Arctic latitudes – VHF radar, radiosondes and numerical modelling
" by Anne Réchou, Sheila Kirkwood, Joel Arnault and Peter Dalin "

Replies to reviewer 2 comments/suggestions

At the outset, we would like to thank the reviewer for his constructive suggestions and comments, which we feel improved the manuscript significantly.

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 wouldPbe 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.

Reply: Two references are given (p 31527 line 9) "e.g. Kirkwood et al., 2010a; Arnault and Kirkwood, 2012 "

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".

Reply : we will change 'very good' to '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.

Reply : we don't combine into the same plot Fig 1 and Fig 2 , since the first figure presents observations while the second figure presents model data, so it's two complementary figures. But , height-time plots of Figs 1 and 2 do have the same scaling.

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.

Reply: It's not meaningful to show radar data above 15 km - it's just noise, and below 5 km the radar isn't expected to measure N^2 because the contribution of humidity becomes significant.

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. Réchou et al. (2013).*

Reply : see below.

It would be useful to include a reference to an existing study of inertia-gravity waves using this model

Reply : see below

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).

Reply : We agree that it becomes clear only later in the paper that these are IGW's, so we could here instead of « the morphology of the short-vertical-wavelength wave-fronts » write « the morphology of what appear to be short-vertical-wavelength wave-fronts »

Variations in N^2 could also arise from mountain wave activity, as shown by Réchou 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 run the WRF model with or without orography.

Reply : Indeed , the WRF-modelling reported in Réchou et al 2013 was continued up to the end of the period reported here, although only the first part (waves of convective origin) was reported in that paper.
We can add a second paragraph to section 2.3 as follows :

" In an earlier paper (Réchou et al. 2013) we have reported observations and WRF-modelling of the period 18 -20 February, 2007. In that study, several models runs were made both with and without orography, and with and without clouds. The modelling was continued up to the end 22 February, although only the first part (waves of convective origin) was reported in Réchou et al.,2013 The waves we focus on in the present study appeared in model runs with or without mountains and with or without clouds, so their cause lies in the larger-scale wind, pressure and temperature fields (from ECMWF) which drive the WRF model. For the present study, the area of the model domain was extended to cover all of Scandinavia, and the orography was included. It can also be mentioned that the WRF model has been previously used to study spontaneous generation of inertia-gravity waves in idealized jet/front conditions, e.g. by Plougonven and Snyder (2007) so it is likely suitable for the task. "

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.

Reply : see paragraph above – we have checked with the model that the waves we focus on here are not a mountain waves (i.e. they appear even with no orography). This comment is just an explanation why, even if mountain waves are generated, in this case they do not complicate the picture.

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).

Reply :Refering to the plots in fig 1 and fig 2, we can focuse on the structures where they are clearest, between 10 to 12 kms to realize the hodograms (new fig 9). The ground based frequency was already checked at 12 km (Fig. 8) . Nevertheless, since the vertical wavelengh is between 500 m and 800m, it is more accurate to work between 10 to 14 kms (Fig. 7 was slightly improve, I put nw=2 in in [Pxx,w] = pmtm(x,nw)).

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.

Reply :The hodographs have been redrawn now from 10 to 12 kms(Fig 9a), since the waves are clearest in this interval. The lowest height (10 km) is marked by a red circle, the highest (12 km) by a green diamond. In this representation, we can see an upward propagation of the waves (clockwise rotation) in the lower stratosphere.

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.

Reply : Fig 9 b presents now the hodographs obtained from the model :again showing an upward propagation of the waves.

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..

Reply : Fig 3 is now done at 8.5 km to see the jet stream location in agreement to Lane et al.(2004) and to be in the upper part of the troposphere (see fig 1 and 2 , where 9 km is sometimes in the lower stratosphere).

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.

Reply : it is corrected, thanks !

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

Reply : it's corrected, thanks !

OTHER CORRECTIONS

Typing errors in equations 6, 8 and 9 have been pointed out to us by Dr. Gubenko. These should read

$$a_e = [\, 2\, (1-f^2/\omega_\iota^2)^{0.5}\,] / [1+(1-f^2/\omega_\iota^2)^{0.5}\,] \qquad (6)$$

$$|\, u'\, | = (2 - a_e)\, \lambda_z N / 2\, \pi \qquad (8)$$

$$|\, v'\, | = (1 - a_e)^{0.5}\, \lambda_z N / \pi \qquad (9)$$