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Interactive comment on "Turbulence associated with mountain waves over Northern Scandinavia – a case study using the ESRAD VHF radar and the WRF mesoscale model" *by* S. Kirkwood et al.

S. Kirkwood et al.

sheila.kirkwood@irf.se

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Authors response

Response to first reviewer

1) The reviewers main recommendation is to consider the possibility of estimating convective mixing from the radar measurements rather than the model. This is unfortunately difficult to do in a reliable way. In the absence of humidity, conditions of convective instability in principle lead to a zero scattered signal. In practice, because of

contributions from humidity fluctuations in the lower troposphere, and the limited range resolution of the radar, such zeros will not be observed. So although the radar can effectively measure turbulence, it cannot measure the very low buoyancy frequencies which would lead to convective instability. We suggest adding the following sentence at the end of the Discussion section:

"The radar measurements can be used to examine the occurrence rates of various levels of turbulence (V_{RMS}^2) but uncertain assumptions on the corresponding values of ω_B would be needed to estimate t_K."

2) The errors in figure references (e.g. 5 instead of 5b) do not appear in the author's own version of the formatted text - this is clearly a problem of the way the LaTex source document is processed and will need to be carefully checked in the final proofs.

3) The reviewer asks for more information on the non-turbulent process contributing to small V_τ. We propose to add a sentence and a reference:

"Short-lived quasi-specular reflections from sharp temperature gradients are also a source of low values of V_τ, particularly in the upper troposphere and lower stratosphere (Kirkwood et al., 2010)

4) While it is true that the model profiles look smoother than might be expected with 150 m resolution, the model still succeeds in simulating very narrow layers of static instability ($\sim$ 300m thick in Figure 12). So it seems the model internally can produce features at the resolution prescribed, however the boundary conditions are likely a strong constraint. We suggest adding the following sentence in the 2nd paragraph of section 5

" Although the nominal vertical resolution of the model is 150 m, the model profiles are quite smooth even at this scale. This is likely the result of a lack of vertical resolution in the boundary conditions (i.e. the NCEP analyses). "

5) The reviewer suggests that the agreement between model and observations is not good for 24th January. Good and not good are of course rather subjective terms. We suggest changing the sentence (3rd sentence of the 3rd paragraph of section 5) from "On 23 and 24 January there is good agreement between model and observations in vertical wavelength, amplitude and direction of the vertical wind" to

"On 23 January there is good agreement between model and observations in vertical wavelength, amplitude and direction of the vertical wind. On 24 January the model amplitudes are somewhat larger and the agreement is less good."

6) The reviewer suggests that 'low' and 'high' are reversed. It is perhaps not clear exactly which height ranges are occupied by the air masses being refereed to as 'low stability' in the text. We suggest adding the sentence

"Note that the low-stability airmass is much deeper on 24 January (5000m at the ESRAD latitude of 68 $^{\circ}$) than on 25 January (2000 m at ESRAD)."

7) The reviewer questions this statement in view of the poor agreement of Fig. 9 and in view of discussion earlier in the text : "However, there is reasonable agreement in height and time between model predictions of reasonably low Richardson number ($R_i < 2$) and the observed occurrence of turbulence."

This sentence is intended to refer primarily to the overall occurrence rate profiles as shown in Fig. 13, rather than the (poor) simultaneity shown by figure 9. We propose

to reformulate as : "On a case-by-case basis there is poor agreement between model predictions of low Richardson number and observation of turbulence by the radar. However, there is reasonable agreement in the height profiles of overall occurrence rates of low Richardson number ($R_i < 2$) and observed turbulence. The onset of turbulence around midday on 23 January and its continuation for a further 24 h coincide with the onset and persistence of conditions of low Richardson number in the model. Subsequently, however, the agreement in time is poor. "

8) We agree to replacing "show" by "support the view" regarding wind shears as the cause of turbulence. However we feel that "reasonably well" should be kept as a description of the agreement between height profiles of occurrence rates for low Richardson number and for turbulence.

Response to second reviewer

1) The reviewer requests further discussion of other numerical studies of mountain waves and comparisons with radar and other data, particularly to indicate that a resolution of $\sim$ 1 km for the model can be expected to be appropriate. In preparing our study, we tried to find the earlier work which was most relevant - numerical modelling of mountains in high-latitude locations, using real topography with high spatial resolution, using real synoptic meteorological conditions at the boundaries, for mountain ranges with similar extents and heights to northern Scandinavia, and addressing turbulence associated with the waves. Those we found are described in the introduction. There are also a number of mountain-wave studies, not filling all of these criteria but including comparison with VHF radar or radiosondes, for the relatively low-level mountains in England and Wales. Perhaps these are the studies referred to by the reviewer. We propose to add the following paragraph in the introduction :

"A number of studies using 3-D modelling for more typical conditions have been made for the mountains in England and Wales, the most relevant in this context being by Vosper and Worthington, 2002. In that study, the authors found good agreement between vertical wind amplitudes measured by a VHF radar in Wales and modelled mountain-waves. Horizontal and vertical model resolutions of 1 km and 500 m, respectively, were used. The authors comment that the radar measurements showed evidence for turbulence both in the upper troposphere and below 3000m height, although the reasons for the turbulence were not evident in the model simulations. This is an indication that model resolutions of order 1 km horizontally and 500 m vertically, while being adequate for some aspects of mountain-wave modelling, may not be good enough to model the conditions leading to turbulence."

2) The reviewer suggests more discussion (in terms of the Scorer parameter) of the possible limitations of the model due to wave trapping. We suggest adding the following paragraph after the 4th paragraph in section 5:

"Additionally, any numerical model has inherent limitations. In this context it is of interest to consider whether the conditions are such as to lead to trapped waves which can be particularly difficult to model. Conditions for wave trapping are usually estimated using the Scorer parameter, $l^2 = \omega_B^2/U^2 - (\delta^2 U/\delta z^2)/U$, where U is the horizontal wind, and the equation refers to conditions upstream of the mountains. When this parameter decreases substantially with increasing height, waves with wavenumber $> l$ at the upper heights, will be trapped below. For our study period, l is relatively high in the lower troposphere but decreases in the upper troposphere more or less at the times and heights corresponding to the wind jets in Fig. 5. At these times, waves with horizontal wavelength less than $\sim$20 km will tend to be trapped below the wind jets and accurate modelling may be difficult. At other times, specifically 12 - 20 UT on 23 January and 12 - 20 UT on 24 January, the Scorer parameter varies little with height and waves should be free to propagate upwards."

Also, at the end of the following paragraph, describing the comparison of model and measured vertical wind, we propose to add

"We can also note that the agreement between the model and the measurements is not particularly better or worse during the periods when there should be no wave trapping (12 - 20 UT on 23 January and 12 - 20 UT on 24 January) than at other times."

3) We agree to add shaded contours of wind speed in Fig 2a and Fig 2b - see enclosed updated versions

4) We have tried manipulating the contrast and zooming the cloud image but unfortunately we cannot find any way to make it clearer.

Further papers to be cited

Vosper, S. B. and Worthington, R. M., VHF radar measurements and model simulations of mountain waves over Wales, Q. J. Roy. Met. Soc.,128,185-204, doi = 10.1256/00359000260498851, 2002

Kirkwood, S. , Belova, E. , Satheesan, K. , Rao, T. N. , Satheesh, K.S. and Prasad, T. R., Fresnel scatter revisited - Comparison of radar and radiosonde data from the Arctic, the tropics and Antarctica, Ann. Geophys.,submitted 2010,

Updated citations

This paper is now published Valkonen, T. , Vihma, T., Kirkwood, S. and Johansson, M.,

Fine-scale model simulation of gravity waves generated by Basen nunatak in Antarctica, Tellus A, doi = 10.1111/j.1600-0870.2010.00443.x, 2010

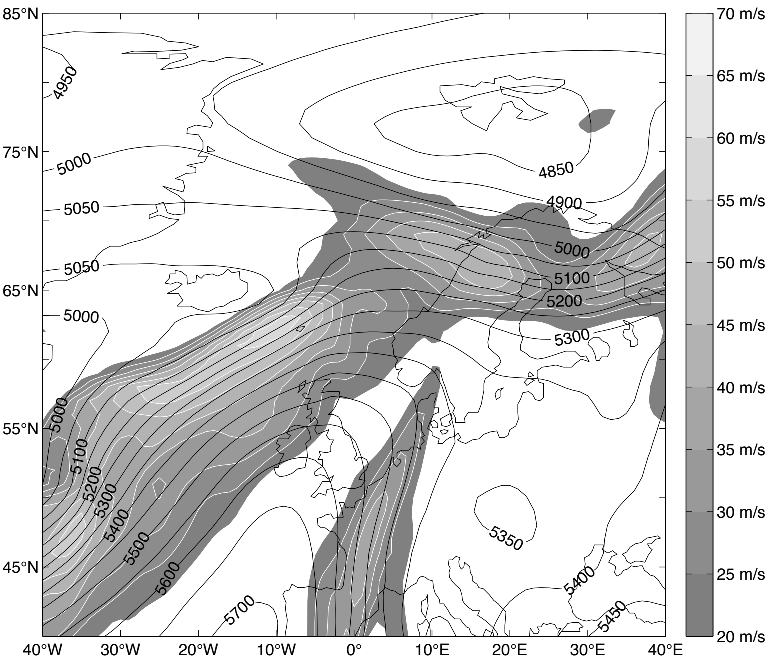
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Fig. 1.

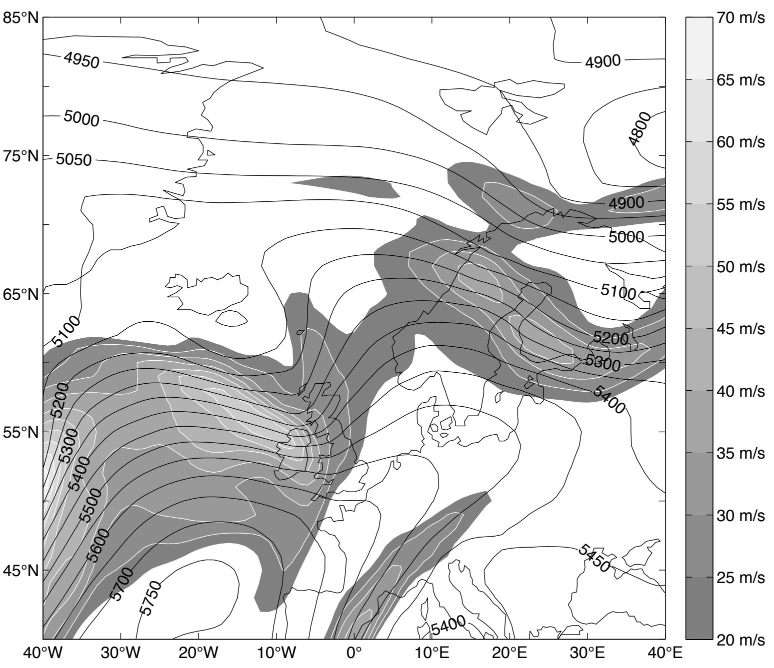
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Fig. 2.