

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.

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This is an excellent paper, and certainly acceptable as it stands for publication in ACP. It is very well written; the presentation is clear and concise. In addition to some very minor corrections (described below) I make only one suggestion for improvement, which the authors may wish to consider. That is, it seems to me that the authors put too much faith in the model and rather modestly, too little in their observations. They note that the model is unable to reproduce the radar observations of turbulence (page 20789, lines 26-27), and that it comes close to predicting the observed occurrence rates only when (model-calculated) Richardson numbers as great as 2 are allowed. Moreover, areas

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where the model calculates buoyancy frequency < 0 and Richardson number < 2 do not seem to be related in any obvious way (Figure 14). Therefore the 5-10% chance of convective mixing they estimate in Figure 15 does seem low, and I am surprised that they dismiss the possibility of estimating this from the radar observations (page 20795, lines 10-12). If this can be done, I think it would be much more interesting than the rather doubtful model estimate they present.

Minor points:

On pages 20783, 20784 and 20786 (3rd line), references to Figure 5 should be to Figure 5a. On pages 20786 (line 25) and 20787, references to Figure 5 should be to Figure 5b. It would be helpful if these figures were displayed side-by-side (and it would also avoid the pedantic issue that Figure 6 is referenced before Figure 5b in the text).

Page 20785, lines 16-17: “...it cannot always be assumed...”: Since other processes (examples?) will contribute to a small V_t in the absence of turbulence?

Bottom of page 20787 and following: the discussion of the model profiles steps around an issue that is evident from Figure 4: although the model’s nominal vertical resolution may be 150m, in fact the profiles look much too smooth, suggesting that there is some sort of smoothing in the vertical (imposed correlations, damping of some sort, reliance on climatology in the absence of data?). Might this not bear on the model’s inability to reproduce the radar observations of turbulence?

Page 20788, line 13: although the figures in my printed copy are quite small, it seems to me that the agreement for January 23rd is pretty good, but not for January 24th.

Page 20791, lines 2-6: I believe “low” and “high” have been reversed; that is, it is the airmass with low static stability that is on top.

Page 20793, lines 17-19: I find this statement a bit strong, given the rather poor agreement of Figure 9, and the authors’ own remark on page 20789 (lines 26-27).

Page 20795, line 20: “show” seems too strong. This was argued primarily from theory

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and on prior knowledge. Perhaps “support the view”? Line 25: “reasonably well” — perhaps “moderately well”?

My sincere apologies to the authors for the delay in completing my review.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 20775, 2009.

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