

## ***Interactive comment on “The latitude dependence and probability distribution of polar mesospheric turbulence” by M. Rapp et al.***

### **Anonymous Referee #2**

Received and published: 5 December 2006

#### General comments

I recommend publication only after a significant revision. The goals of the paper are most commendable and, had the underlying dataset and treatment of errors been adequate to the task, the conclusions would have been of considerable interest and value to the scientific community.

1) There appear to be too few Svalbard data to justify the assertions; similarly there is a wealth of Andenes / Esrange soundings yet apparently only a subset is employed here, although the last author has a huge database at his fingertips!

2) There is no treatment of errors, i.e. there is not a single standard deviation indicated on any of the figures. Without adequate treatment of errors any assertion that the

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distributions are, for example, log-normal can't be justified, and neither can any latitude difference in Figure 2 be attributed a significance.

3) The title is very impressive and promises information workers in this field are eager to obtain. Unfortunately what we are served is the departure of a somewhat limited  $79^{\circ}$   $12^{\circ}$ E dataset from a well established  $69^{\circ}$ N  $16^{\circ}$ E dataset. Probability distributions at a variety of altitudes could indeed be derived from the Andenes data, and usefully so; incorporating all altitudes in order to obtain sufficient values to build a histogram for Svalbard doesn't seem so useful. "Latitude dependence" summons up a picture of a plot of average values of  $\epsilon$  versus latitude and perhaps for several altitudes, and furthermore with a standard deviation for each point.

A solution might be to:

- a) tone down the title to reduce the readers' expectations,
- b) use as much  $69^{\circ}$  July data as possible employing Lübken's published material ,
- c) add standard deviations to the presentation of the data, and
- d) fit analytic distributions to the histograms including uncertainties in the fits

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#### Specific comments

- 1) Is there quantitative evidence that the distributions are log-normal?
- 2) If there is no turbulence one might expect to see spectra lacking a buoyancy sub-range. One has to identify the spectrum as being non-turbulent - not discard a spectrum you can't fit to (which is the impression the manuscript gives the reader) and say there's no energy dissipation.
- 3) J. Höffner actually published the lidar results in Hall et al. J. Geophys. Res. doi: 10.1029/2005JD006794, 2006.

- 4) Instead of using the Andenes soundings listed in Table 1 and then producing a plot like Figure 1, why on earth have the authors not just used as many summer (July) soundings as possible, and, for that matter simply used the results from one of Lübken's excellent earlier (incontestable) papers?
- 5) Dind't Lübken and Müllemann find that the 78°N mesopause was higher up than for 69°N so that the saturation level should be accordingly higher too?
- 6) For the model results (e.g. in Figure 6), where is the quantitative evidence these distributions are also log-normal?
- 7) Since the in situ results are from 69°N and 79°N, why ever choose to compare with model results from 60°N (Figures 5 and 6) - why not use the same latitude(s)?
- 8) Fig. 5 might more usefully be employed to show the variances for different timescales - and again, why not at the latitude(s) of the observations.

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#### Technical corrections

Figures 2 and 6 could employ colour making it easier to read the annotation.

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#### Checklist

- 1) Does the paper address relevant scientific questions within the scope of ACP? YES
- 2) Does the paper present novel concepts, ideas, tools, or data? YES
- 3) Are substantial conclusions reached? YES
- 4) Are the scientific methods and assumptions valid and clearly outlined? YES
- 5) Are the results sufficient to support the interpretations and conclusions? NO
- 6) Is the description of experiments and calculations sufficiently complete and precise

to allow their reproduction by fellow scientists? See specific point 2.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES

8) Does the title clearly reflect the contents of the paper? YES

9) Does the abstract provide a concise and complete summary? PARTLY

10) Is the overall presentation well structured and clear? YES

11) Is the language fluent and precise? YES

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? YES

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? YES

14) Are the number and quality of references appropriate? YES

15) Is the amount and quality of supplementary material appropriate? N/A

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 12199, 2006.

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