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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #4

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Review of "The latitude dependence and probability distribution of polar mesospheric turbulence" by M. Rapp et al.

Recommendation: reject in its present form.

This paper is concerned with the possibility of a decrease in mesospheric turbulence toward higher latitudes. The data used to produce this suggestion comes from thirteen rocket flights launched near 70°N during summer months over an eleven year period from 1991 to 2002, and three rockets launched near 80°N in 2003. The authors find that the energy dissipation is reduced at the higher latitude, suggesting a trend toward lower values turbulence at higher latitudes during polar summer. With this extremely limited data-base, they postulate that the data follows a log-normal distribution, and

use updated models to attempt an explanation of the active processes. In order to demonstrate this property, the data is averaged over all altitudes for each flight, but is heavily weighted to regions near 85 km, where the dissipation is higher by 2-3 orders of magnitude. Most of the paper is dedicated to explanation of the effect with a model, assuming the log-normal distribution is correct.

The result of the rocket data comparisons is at best, a suggestion, and hardly substantiated by data taken at two latitudes. Also, the three measurements at 80°N were made at ˜ 800 UT, whereas the lower latitude measurements were all close to midnight UT. Furthermore, what are the effects uniform for the different months during summer? Is it safe to assume that different years are repetitive? For example, recent data from rockets and satellites have shown some summers to be warmer than average in the mesopause region.

The use of the KMCM model to analyze the rocket data is interesting, but hardly unique considering the uncertainty of the inputs provided by the variability and poor statistics of the available data. The authors note that their simulated results provide a narrower log-normal distribution than that “observed”, and also produce no cases of zero turbulence, which dominates the observed data. It’s true that small scale wave structure and unconsidered winds may be responsible for these differences. I wonder what might happen if the log-normal distribution, which is feasible but not a unique possibility, were modified. Other tests are also required to make the model study more realistic.

The idea is certainly worth pursuing and should be offered to the scientific community. A solution would be to make the title more suggestive and modify the text accordingly. The findings have certainly not been proven in this analysis.

Some minor comments:

There seems to be an inconsistency on page 12202, line 12 and lines 23/24. If this period was a “typical” summer night, how come “(blue) crosses represent the first in-situ measurement in the polar night, where not a single altitude binĚ.revealed any turbulent

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activity." This doesn't sound "typical" to me.

Figure 4. The data curve doesn't agree in magnitude or height with the model curves. Yet we expected to believe that the model results are reasonable. An explanation of these discrepancies is required.

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