

Interactive comment on “Rocket measurements of positive ions during polar mesosphere winter echo conditions” by A. Brattli et al.

Anonymous Referee #2

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The paper demonstrates that the regions of enhanced radar echoes during a PMWE are associated with enhanced turbulence that extends to scales smaller than 100 m, indicative of turbulent dissipation of breaking gravity waves. Estimates of the lower limit for the turbulent energy dissipation rates are consistent with this interpretation.

The question as to whether PMWE, like their PMSE counterparts, are caused by charged aerosols or enhanced turbulence has vexed the community since their discovery. The present work presents the same data that were first analysed by Lübkin et al (2006). While extending the wavelet analysis of that paper to all the observed PMWE layers and a second rocket flight, the conclusions are, to a large extent, the same as those in the earlier work. However, the additional evidence gives further support to the primary conclusion of both works: that aerosols, though not excluded from

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the observations, are not required to explain the presence of PMWE. In that sense, the additional evidence and more detailed analysis presented here merit publication.

In general, the paper is very well presented, clear and concise. However, given that the coincidence between the enhanced spectral densities and the PMWE layers is key to the results, there are several points that should be clarified or noted before publication in ACP.

First, the time and location of the ALWIN radar measurements relative to the rockets should be stated in the paper. Related to this, there is no mention made between the location of the enhanced spectra shown in Figures 3-5 and the radar location of the PMWE. From the figure, some of the enhanced spectra appear above the radar observed layers and some below. An explanation of these differences should be given.

Also, no explicit criteria as to how to classify radar echoes as PMWE are mentioned. In the case of the RWMM-02 flight, the identification of PMWE in the radar signal would appear to be rather subjective below 65 km. As identifying a radar echo as a PMWE based upon the enhanced spectral density would, given the conclusions reached in this particular paper, be circular reasoning, such criteria should be explicitly stated.

Finally, it is not apparent from Figure 3 that the spectra above 82 km on RWMM-02 cut off above the 10 m scales, particularly on the upleg. This is perhaps a result of the spin modulation masking this behaviour in the lower levels. Thus, it would be very useful if the spectra from the 82-85 km region on the upleg could be plotted as a separate panel in Figure 5 to clearly demonstrate that they do not extend into the 10 m scales.

In addition, there are some minor points and suggestions that the authors should consider.

On page 7097, the authors mention that the wavelet technique "overcomes many of the time-frequency localization problems" of the Fourier transform method. I would suggest that if the authors briefly detail these problems, it would be beneficial to the

community involved in such measurements. In section three, the authors give a brief description of the Positive Ion Probe (PIP). Does there exist a more detailed instrument description to which the reader might be referred?

In the conclusions the phrase: "the observational data suggest that PMWE is caused by turbulence", might be better as "the observational data suggest that PMWE are caused by turbulence". This is assuming that the term PMWE is plural.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 7093, 2006.

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