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Interactive Comment

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

**Anonymous Referee #1**

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1. The main conclusion of this investigation is that there is a causal relationship between PMWE and plasma turbulence driven by unstable atmospheric gravity waves. The main argument is rocket measurements of ion density fluctuations that have a spatial 1-D spectrum that apparently has a power law with exponent -5/3. The ion density fluctuations are co-located with PMWE as measured by a 53.5 MHz radar. A calculation of the turbulence energy dissipation rate is not possible because instrument noise dominates at a scale length that is smaller than the turbulence inner scale length. However, a lower bound of the dissipation rate has been calculated by assuming that the inner scale coincides with the scale at which the noise level begins to dominate. The dissipation rates found have values that are typical of mesospheric turbulence, about 10 mW/kg, or less. Then, the inference is made that it is plausible that the turbu-

lent inertial convective subrange extends beyond the noise scale sufficiently to cover the scale length of the radar. Thus, PMWE is driven ultimately by atmospheric turbulence without the need to invoke a viscous convective subrange driven by enhanced Schmidt number, which is possible only in summer. In my judgement, this is the main conclusion of this investigation.

2. I suggest that the authors have mistaken wave length for scale length. Although they quote repeatedly “scale length", the numbers they utilize are wave lengths. Since wavelength is a factor of $2\pi$ larger than scale length, the errors can be very large, as I will show. See Kelley, M. C., and J. C. Ulwick (1988), Large- and small-scale organization of electrons in the high-latitude mesosphere: Implications of the STATE data, J. Geophys. Res., 93(D6), 7001–7008; La Hoz et al. (2006), Observations and theories of Polar mesospheric Summer Echoes at a Bragg wavelength of 16 cm, J. Geophys. Res., 111(D04203) doi:10.1029/2005JD006044. The first shows that the choice is not so simple as it may appear; while the second describes the theoretical underpinnings.

3. I will employ here a heuristic argument. Angular frequency ($\omega$) in the time domain corresponds to wave number ($k$) in the space domain. The frequency spectrum can be described as a function of angular frequency, $\omega$. Time constants (equivalent to scale lengths) are the inverses of angular frequency, not the inverses of regular frequency ($f$), which, in fact, are periods. The Kolmogorov turbulence spectrum is described as a function of wave number, $k$, which is not the inverse of wavelength, but the inverse of wavelength times $2\pi$. Thus, the inverse of wave number $k$ is the scale length (not the inverse of wavelength, $\lambda$).

4. On Page 7104, lines 4, 5 and 6, the authors mention “. . . The $k^{-5/3}$ slope extends from $k$ 10$^{-2}$ m$^{-1}$ (corresponding to scale sizes of roughly 400–600 m) and up to $k$ 1 m$^{-1}$ (scale sizes of roughly 6–10 m), where the signal drops below the electronics noise." The correct scale lengths corresponding to the quoted values of wave number should be 100 m (instead of 400–600) and 1 m (instead of 6–10 m) respectively. Thus,

“the signal drops below the electronics noise" at a scale length of 1 m, not 6m (or 10m) On the same page, line 9, the authors mention “. . . the Bragg scale (2.8 m) of the ALWIN radar. . . ". The correct value of the scale length for the ALWIN radar should be 0.446 m, not 2.8 m.

5. I have re-calculated some of the values quoted in table 2 using correct scale lengths. For the low value of viscosity of 0.032 $m^2$/s and for ALWIN’s scale length of 0.446 m, the dissipation rate is 8 W/kg; while for the high value of viscosity of 0.24 $m^2$/s, the dissipation rate is 3350 W/kg. It is out of the question that these values of dissipation rate cannot occur in the mesosphere. Thus, the conclusion that neutral turbulence can drive PMWE cannot be correct.

6. I have other comments that are less weighty, but still important, to which I will come back in more detail after the authors have answered my main objection described here. Briefly, among these comments, are the decision to use straight lines on the spectral plots “. . . drawn to guide the eye, they have not been fitted." They should be fitted.



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