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Interactive Comment

Interactive comment on “Polar mesosphere summer echoes (PMSE): review of observations and current understanding” *by* M. Rapp and F.-J. Lübken

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We greatly appreciate the reviewers' constructive comments and their very positive judgement on our manuscript. We have taken the reviewers' suggestions into account when preparing the revised version of our manuscript.

In the following we address the three reviewers' comments point by point.

1. Reply to Referee 1

This referee has made a couple of very good suggestions that will help to make our manuscript more easily accessible for the non-experts in the field. In particular, the

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suggestion to show the power spectrum of a tracer for turbulent motion already at the time that turbulence and related scales are mentioned is very good and we have added such a Figure (i.e., Figure 3 in the revised manuscript) to the manuscript. In addition, we have added a panel to Figure 1 showing a temperature profile and the frost point temperature of water vapor. This will make it much easier for the un-experienced reader to understand why ice particles form in the narrow altitude range between 80 and 90 km.

In addition, also as a reaction to the comments by referee 3, several of our statements have been rephrased and clarified. When doing so, we have tried to be as didactic as possible.

Finally, we have also addressed all the individual points indicated by this referee:

- Brief explanations of Thomson and Fresnel scatter have been added as suggested.
- Here, the referee has asked an important question for the understanding of the physics underlying PMSE: Certainly, there **must** be an initial gradient in the tracer. Otherwise, mixing can of course not lead to small scale structures. We have now clarified this important point in our manuscript when discussing the schematic power spectrum of a tracer for turbulent motion (new Figure 3).
- As indicated above, we have added an extra Figure (Nr. 3) showing a typical turbulent power spectrum and we discuss in more detail the relevant scales and subranges.
- We have added an extra panel as suggested.
- We have rephrased this sentence and hope that it is clearer now.

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- Done as suggested.
- We have added a reference to the paper by Driscoll and Kennedy (1985).
- In equation 6, the amplitude of the Gaussian electron number density disturbance is designated n_{e0}. We realize that we did not explicitely mention this. This has now been changed. The diffusion coefficients D_1^0 and D_2^0 both depend on Λ. This has now been explicitely stated in the text.
- Figure 18 is indeed a snapshot for a given time, and the seen longitudinal structure is a direct consequence of tidal effects. However, we realized that this Figure would require more detailed explanations than what we consider appropriate in the scope of our review of PMSE-physics. We have hence clarified the text describing the work of Berger and von Zahn, but have also decided to delete this Figure from the manuscript since it would only distract from our main topic.
- The altitudes have been specified as requested.
- All the other minor points were corrected as requested.

2. Reply to Referee 2

This referee is completely correct to point out that our statement regarding volume scatter versus specular reflection was a little bit too strong. We have modified the relevant sentence in section 2.4.3 to:

‘.. These results were later confirmed by several independent investigators (Inhester et al., 1990; Luebken et al., 1993; Blix et al., 2003) and hence showed that PMSE are consistent with the assumption of volume scatter from electron number density irregularities and can not be due to specular reflections from single steep gradients

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as they appear for example at the edges of electron biteouts (Hocking and Roettger, 1997). Still, it must be noted that the aspect sensitivity of PMSE shows that the scattering structures can not be purely isotropic. See section 2.3.3 for a more detailed discussion of this particular point.'

Then, as already pointed out in our response to referee 3, we have modified section 2.3.3 and now discuss the possibility that the turbulent velocity field itself might be anisotropic (as pointed out, e.g., in the papers by Petterson Reif and Andreassen, 2003 and Werne and Fritts, 2001.) These references have been added to the manuscript. In addition, we now also mention the possibility that a horizontal layering of small scale structures could lead to strong backscattering and large aspect sensitivity. This is a known mechanism in the HF wavelength range (Thrane et al., 1981) and has recently also been discussed in the context of PMSE in the VHF range (Thrane et al., 2004).

In addition, we have tried to improve the quality of Figures 1, 15 and 16, as requested.

3. Reply to Referee 3

First of all we would like to thank this referee for his thorough reading of our manuscript and pointing out several small inaccuracies and also giving useful hints to parts of the literature that we had overseen.

- As suggested, we have added that the quoted Bragg condition is only valid for monostatic radars. In addition, we have changed the expression '..inner scale of the turbulent velocity field..' to '..inner scale of the neutral gas turbulent velocity field'.

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- At PMSE altitudes and below, i.e., below 90 km, the temperatures of neutrals, electrons and positive ions are identical due to the prevailing high collision frequencies. In order to emphasize that PMSE observations provide information on the neutral background atmosphere, we have changed the phrase to '..like neutral temperatures..'.

  We have changed the phrase to: 'These extremely low temperatures marginally allow ice particles to form and grow at altitudes between ∼80 and 90 km, in spite of the minute abundance of water vapor with typical volume mixing ratios of only some ppm.'

  Regarding the reference to Czechowsky et al., the referee is certainly right. We have deleted it from this first point where we mentioned it. At the second point, i.e., in section 2.1., we further clarify that observations by Czechowsky et al. were made at mid latitudes.

- The statement about the charge balance is only confusing here and has been removed.

  'Turbulent advection' has been replaced by 'transport due to the turbulent velocity field'. That this type of transport happens was shown by Batchelor (1959) and was verified in the laboratory (Gibson and Schwartz, 1963). In addition, strong evidence for the fact that particles are advected by the turbulent velocity field is presented for example in Figure 34 (33 in the revised manuscript). Please note, however, that in this part of the paper, our intention is to give a summary in order to ease the reading of this review. I.e., we have included this paragraph out of didactic reasons (see also recommendation by referee 1). We feel that this part of the manuscript should not be 'blown up' by adding many references or cross-references to other parts of the paper. These are certainly provided later in the paper.

  'radar refractive index' has been changed to 'radio refractive index' as requested.

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However, as detailed for example in the Handbook for the Middle Atmosphere Program (volume 30, edited by S. Fukao, page 23-26, 1989), the radio refractive index, n (appropriate in the VHF range), is given by

$$n = 1 + \frac{0.375e}{T^2} + \frac{7.76 \times 10^{-5} P}{T} - \frac{N_e}{2N_c} \quad (1)$$

where e is the partial pressure of water vapor in mbar, T is the temperature, P is the total pressure in mbar, N_e is the electron number density (not a fluctuation!), and N_c is the critical electron number density (i.e., the number density of electrons at which the frequency of the radar wave equals the plasma frequency so that total reflection occurs). At an altitude of 80 km, the contributions from water vapor, pressure, and electron number density are (assuming a typical pressure of 0.01 mbar, 3 ppm water vapor mixing ratio, a temperature of 150 K, an electron number density of 3000 cm^{-3}, and a radar frequency of 50 MHz): 5×10^{-13}, 5.2×10^{-9}, and 1×10^{-4}. In order to avoid misunderstandings, we have changed our wording to '...which is effectively determined by the electron number density at these altitudes.' That scattering only occurs when the refractive index has gradients is an other story and is already stated in our manuscript.

- We have changed 'solely' to 'effectively'. We have clarified that it is the 'Bragg condition for monostatic radars.'

- We have now explained that epsilon, the turbulent energy dissipation rate, is the rate at which turbulent kinetic energy is dissipated into heat, and is hence a measure of the turbulence strength. An explanation of the abbreviation ALOMAR has been added.

- The referee is absolutely correct. Actually, the next paragraph already contained a cautionary note regarding the dependence on system parameters. Guided by the referee's suggestion we have expanded this paragraph and now also state

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the different peak transmitted power-values of the different radars (i.e., ranging from 60 kW in the case of the SOUSY-radar down to only 12 kW for the radar at Resolut Bay.)

- See above.
- Unfortunately, measurements of absolute reflectivities are rare because this requires an absolute calibration of the radars that is often difficult to achieve. We have added an appropriate statement to the text. In addition, we have stated that in future experiments, every effort should be undertaken to obtain calibrated signal strengths.

  As requested we have replaced ‘spectral width’ by ‘spectral shape’.

  ‘different physics’ has been replaced by ‘different physical processes’.
- We have changed the term ‘Brunt frequency’ to ‘Brunt-Väisälä-frequency’ and we have added a short definition as requested.
- Done.
- Statements (including references) regarding the radar estimates of turbulence and biteouts were added. In addition, we have deleted the confusing and unnecessary statement that ‘..nothing argues against neutral air turbulence as the generation mechanism of PMSE.’
- The latitude of Jicamarca has been included.

  Regarding the mass spectrometer measurements of Kopp and co-workers we don’t agree with the referee: The measurements described in the mentioned proceedings paper (ESA-PAC conference in Loen, 1985) show evidence for proton clusters up to $H^+(H_2O)_{12}$. However, these are by far not massive enough to be

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considered as macroscopic particles which should have masses of several thousand amu. In order to avoid misunderstandings, we have now clarified what we mean with the term particles (in contrast to cluster ions).

We have changed the term to ‘number density of free electrons’ as requested.

- We have considered changing all units to MKS as requested - but unfortunately, this turns out to be an impossible task for this review paper. This is mainly because many of the Figures that are just reproduced from other publications use for example cm^{-3} for number densities instead of m^{-3}. We hence apologize for this omission but in order to be as internally consistent as possible in this review we have chosen to stay with the initial units used (i.e., it would be really confusing for the reader if we used different units in the text than the ones shown in the Figures).

- We agree that it is not clear which role a DC-electric field could play. The intention of this paragraph, however, is only to report the new measurements which showed that if there is any DC-electric field, it is minute. We consider this an important experimental fact since it will avoid speculation about PMSE generation mechanisms involving large DC-electric fields (which are, for example, important for E-region plasma instabilities).

- The classification of NLC/PMSE as described in the paper by von Zahn and Bremer is purely empirical. This has now been clarified in the text.

- ‘Heating radar’ has been changed to ‘Heater’.

- In this part of the paper we describe our current capabilities to model mesospheric ice particles. The mentioned model by Berger and von Zahn is the only available model so far that treats these ice particles in a three-dimensional general circulation model. This is the reason why we mention it here and we have now clarified this in the text. In addition, we have taken out the corresponding

Figure since we realized that an appropriate dicussion of the shown brightness features is beyond the scope of the current review (and is actually not needed for our discussion of the physics of PMSE).

Note further, that tidal effects were already discussed in section 2.2.3 where a reference to Klostermeyer (1999) was already included. The mentioned paper by Chilson et al. does not deal with tides but inertia-gravity waves. Anyway, we have added this reference to section 2.2.3 where gravity wave influences on PMSE are discussed.

- Done.

- 'Thompson' was changed to 'Thomson' (thanks!).

  We have clarified that '$N_e \cdot \epsilon_e$ is proportional to the absolute magnitude of the electron number density fluctuations *due to turbulence* and determines the absolute level of the calculated reflectivity.'

  'Gaussian disturbance' was rephrased as 'disturbance of Gaussian shape' and we have added that the other necessary details can be found in the Figure caption.

  'during the heating pulse' was changed to 'during the time the heater was switched on'.

  We have clarified that 'thermalize' indeed means that the electrons acquire the neutral gas temperature.

- We did describe the physics of the formation and growth of mesospheric ice particles in section 3.1. We have changed the first sentence of section 3.4 to 'As demonstrated in section 3.1. ...'.

  Furthermore, we have indicated that the backscatter ratio is the observable of a lidar.

- We agree that section 3.5 was too detailed and we have followed the referee's advice to significantly shorten it. In particular, we have deleted several unnecessary equations and corresponding explanations. In addition, we have deleted the former Figure 26, because it did not really add information that could not easily (and briefly) be included in the text.

- As pointed out in the first paragraph of section 3.6. we did not want to repeat the excellent discussion (and rejection) of some of the alternative theories in the previous review article by Cho and Roettger (1997). On the other hand, our description of the two other remaining theories (dressed aerosol scatter and particle growth feedback) indeed goes into many details and should be sufficient. Note again, that we have significantly shortened the description of our own theory in section 3.5.

- We agree with the referee that section 4.1 is a strong idealization of the actual processes in the atmosphere and we have now clearly mentioned this in the beginning of this section and several times in the course of the discussion. Nevertheless, these simple estimates show that using the measured turbulence occurrence rate (deduced from the available sounding rocket measurements), reasonable ice particle parameters as described in section 3.1, the multipolar diffusion theory described in section 3.5, and a typical duration of a turbulent event, predicts a mean PMSE occurrence rate that compares reasonably with observations. Most importantly, these estimates show that the PMSE occurrence rate can be much larger than the turbulence occurrence rate such that the measured absence of turbulence at PMSE altitudes does not exclude the possibility that the small scale electron number density fluctuations at which the radar waves are scattered originated from turbulence some time ago (hence the term 'fossil turbulence').

  Still, the referee is correct in pointing out that this model is idealized, i.e, in the sense that it only applies to a Lagrangian observation whereas radar observations are certainly made from a fixed point on the ground. This has now been explicitely mentioned.

However, assuming Lagrangian observation geometry, the length of a turbulent event can certainly be defined, for example as the time that an intertial subrange exists in the power spectrum of the velocity fluctutaions. Certainly, direct numerical model simulations can provide us with information on this time scale (e.g., the cited paper by Andreassen et al., 1994) and show that several Brunt-Väisälä-periods, i.e., $\sim$ 15 min, is a reasonable estimate for this lifetime. This is the value that we used.

Regarding the problem of defining an absolute PMSE occurrence frequency due to its dependence on the radar sensitivity, the referee is principally right. However, we did not claim to make a detailed comparison with observations but only discuss qualitative features (see arguments above). Again, we consider it a worthwhile finding that our simple model does reproduce the main features of observed PMSE properties very well!

- Again, we do agree with the general statement of the referee that the best way to present the radar data would be to show absolute volume reflectivities. We will be happy to do so if possible in our future research papers presenting new measurements, however, it is unfortunately not possible to do this in the scope of a review paper that summarizes published work.

  We hope the following new sentence is less confusing: 'Considering, however, the strong idealization underlying this result (i.e., no horizontal transport, assumed permanent existence of ice particles in the polar summer mesopause region, etc.), this minor discrepancy is not surprising.'

  We have clarified that the PMSE decay time is defined by the diffusional decay time by 10 dB according to equation 2 of our revised manuscript.

  We have changed the sentence to '..and shows that either active or fossil neutral

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air turbulence (together with the details of charged particle- and electron diffusivity discussed above) can account for all observed PMSE'.

We have changed the phrase '..on the basis of neutral air turbulence and the large Schmidt number idea..' to '..on the basis of neutral air turbulence in combination with a large Schmidt number of the electrons'.

- We had already changed the title of section 2.3.2 to 'Spectral shape' as requested. In that section (2.3.2) we discussed the matter of spectra that did not reveal the usually assumed Gaussian shape. Here, however, we only discuss the broadening of the spectrum and not the shape. In order to avoid misunderstandings we have changed the section title to 'Doppler broadening and aspect sensitivity'.

  Regarding the relation between velocity variance and spectral width the referee is certainly right. We have changed our sentence accordingly.

  The correct reference is to section 3.5.

- Our statement about sedimentation effects causing anisotropy was pretty speculative at this point. We have deleted this statement from the manuscript and refer instead to the possibility that the turbulent velocity field itself might have been anisotropic from the very beginning (as pointed out, e.g., in the papers by Petterson Reif and Andreassen, 2003 and Werne and Fritts, 2001.) Appropriate references have been added to the manuscript. In addition, guided by the comment of referee 2, we now also mention the possibility that a horizontal layering of small scale structures could lead to strong backscattering and large aspect sensitivity. This is a known mechanism in the HF wavelength range (Thrane et al., 1981) and has recently also been discussed in the context of PMSE in the VHF range (Thrane et al., 2004).

- The existence of turbulence can be defined by the requirement of the existence of an intertial subrange in the power spectrum of the velocity field or a suitable tracer. This can be observed with high resolution in situ techniques like the ones discussed in section 2.4.2. Since we discuss the shape of observed power spectra here, a reference to EISCAT measurements would only be confusing for the non-radar expert. The EISCAT observations that the referee refers too, have already been mentioned and discussed in detail in section 2.3.2 (see also Figure 5).

We have clarified our sentence to 'Through their close relationship to ice particles, PMSE are a direct indicator for extremely low temperatures (i.e., less than $\sim$150 K) at mesopause altitudes. Importantly, radar observations of PMSE are independent from tropospheric weather conditions in contrast to optical soundings from the ground for example with lidars.'

Temperature measurements from satellites are usually being done spectrometrically, i.e, by observing the temperature-dependent line width of absorbing species, e.g., in the 15 μm-band of CO_2. As an example, we have added a short statement and a reference to recent attempts from the TIMED satellite (i.e., Mertens et al., 2004) to the text.

The largest percentage of gravity waves is excited in the troposphere and lower stratosphere. We have added a reference to the review paper by Fritts and Alexander (2003) to the text.

- We have changed the term 'turbulent advection' to 'transport by the turbulent velocity field'.

  Hocking et al. (1983) and recently also Latteck et al. (2004) have described methods to distinguish the turbulent and the ionospheric contributions from the measured spectra. We have added these references to the text.

- From our best physical understanding of the physics of meteoric smoke particles (in particular the paper by Hunten et al., 1980), it is clear that the time constant

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for the formation of these particles is on the order of several days to more than a week. Hence, a relation to meteor showers is not expected.

We have added the clarification ‘polar summer mesopause region’ as requested.

The suggestion to mention the large potential of future heating experiments is a very good one. We have added a short paragraph to the ‘outlook’-section of our manuscript.

Finally, we feel that answering the referee’s final question in this concluding part of the manuscript would be rather confusing and distract from the main focus of the paper, i.e, our physical understanding of PMSE and how it can be further exploited. Anyway, techniques for the high resolution measurement of mesospheric temperatures are already available on sounding rockets and have been discussed in some detail in section 2.4.1 (see also Figure 7).

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 4777, 2004.

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