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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #3

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This is a most relevant and timely review paper. It is interesting and attractive to read. It summarizes well the state of the knowledge and updates the reader on the major developments, which happened in the past years since the last reviews by Cho, Kelley and Röttger.

The general aspect and the weight of this manuscript is a little biased to the research work done by the two authors, albeit they anyway did most of it. Some of the sections, particularly towards the end are strongly correlated directly with recent publications of the authors. This unbalance should be reduced.

The detailed report on suggested partial revisions follows here:

page 1, column 1 Note that this often cited 3 m Bragg condition holds only for monostatic radars (which are so far applied in all the MST radar applications). . . .inner scale of the neutral turbulent velocity field. . .

p.1, c.2 . . .like temperatures (neutral, ion, electron?). Say something here about the required water vapor. Czechowsky et al. reported only summer-winter differences o mid-latitude mesospheric echoes; they did not observe in polar regions and did not quote or propose any new mechanism such as Ecklund and Balsley did. This should be said here.

p.2, c.1 Define here what you mean with "charge balance"? Over which volume? Where do you know from that the ice particles are "advected", and what is "turbulent advection"? Say something here about the scales. As originally defined, it is the "radio" refractive index (not "radar"), and it is not only directly proportional to the electron number density but on its fluctuations as well and on other parameters.

p.2, c.2 ". . .solely determined by electron number density" is not correct. 3 m Bragg condition, see comment on p.1. c.1.

p.3, c.1 Here you have to define epsilon in a little more detail. Also, what does ALOMAR stand for?

p.3, c.2 The occurrence rate depends directly on the radar system parameters and is not a good indicator of latitudinal variations. You have to say something about the difference of the system parameters of the radars used for comparison.

p.4, c.1 Resolute Bay: Even noting the apparently painstaking checks of the authors of the cited paper, it has to be questioned why there is a 50% difference, which is unlikely when one compares with more recent PMSE observations at 78 degree N. A soft word of caution on these Resolute Bay observations would not harm.

p.4, c.2 Say why the different measurements were not performed of the radar reflectivity. Use "Spectrum shape" instead of "Spectrum width". ". . .comparison of the spectra

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of incoherent scatter signals. . .” What is meant with ‘different physics’??

p.5, c.1 Please use Brunt-Väisälä frequency, and briefly define it here.

p.6, c.1 add: “. . .other way (see also section 3), but this was soon contested and debated in view of the newer observations at even smaller wave numbers with the EISCAT radars”.

p.6, c.2 That turbulence was observed mainly in the upper portion of PMSE layers was also observed in EISCAT radar measurements and reported. Be a bit more clear what you mean with: “. . .nothing argues against turbulence as the ‘generation’ mechanism of PMSE”. Bite-outs were also observed with radar (EISCAT 933 MHz), please refer to this as well.

p.7, c.1 “. . .that the electron density bite-outs. . .”. Include latitude of Jicamarca. There is a paper by Kopp et al. (1984/85 or so in ESA-PAC Proceedings) reporting about massive ions in the polar summer mesopause, observed by rockets. This should be cited as first observations. Instead of “electron number density” better say “the number density of free electrons” as compared to the electrons which are tied to the ice particles etc.

p.8, c.1 Use MKS system: i.e. electrons/m^3. Define “local charge neutrality”.

p.8, c.2 It is not clear here which role a (DC-)electric field should play. Can that be said? Does the distinction/classification between these three types of PMSE/NLC have a physical meaning? If so, it should be described. For instance: The classification of type 1, 2, and 3 E-region irregularities introduced by Farley et al. has a proper physical meaning. The same should hold, if one does such a classification for PMSE/NLC.

p.9, c.1 It’s not a Heating “radar”!

p.9, c.2 There is a paper by Chilson et al. (JGR, 102, D20, 23819–23828, 1997) and a later one of Klostermeyer (2000?), clearly investigating and proving the tidal relation of PMSE. These should be cited and discussed.

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p.10, c.1 Again: ". . .electron density bite-outs. . .".

section 3.3.2 Fig. 22: It is "Thomson", not "Thompson" scatter (also called "incoherent scatter"). What is meant here with "fluctuation in the electron gas". In the case of "incoherent scatter" the fluctuations in the electron gas, i.e. the random motions of free electrons. cause scattering. The authors, though, do not mean this here rather than the small scale fluctuations resulting in some way from turbulence. This should be clarified. Define what is meant with "Gaussian disturbance in the electron number density"; which scale, which strength, which gradients at the boundaries. What is the "heating pulse"? How can the electrons thermalize? Iit is likely meant here, that the electrons attend the neutral gas temperature, which would be a better description.

section 3.4 It is not seen in "the previous sections" what the "physics of the formation and growth of mesospheric ice particles" is. The authors have not described this process rather the interaction of these with the electron gas. The introduction of this proxy is a good idea. Fig. 25: declare that the "backscatter ratio" is for lidar observations.

section 3.5 is more or less a repetition of another published paper by the authors and should be shortened. The conclusion appears reasonable.

section 3.6 summarizes briefly other published theories. This section appears a little short compared to the presentations of the authors' own theories.

section 4 and following: p.15, c.1 The turbulence statistics section is a little weak: "TOR" should be more clearly defined. How can one determine how long a turbulent event is lasting? It depends on the scale size, of course. It also depends on whether the turbulence is developing, mature or decaying. The "turbulence event" is advected through the volumes sensed by radar and rocket, which affects the "life (Observing) time". It is admitted that such a definition is a difficult, almost impossible task. I feel that the resulting relation of POR and TOR is, thus, somewhat vague, also because of the following further reason: The PMSE Occurrence Rate POR depends on the radar system sensitivity. Since that is different by about an order of magnitude when

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comparing results from such radars like Poker Flat and ALOMAR or Resolute Bay, an absolute POR is a really meaningless.

p.15, c.2 This also brings me to pinpoint that in many graphs the signal-to-noise (SNR) is shown. This is strongly dependent on radar system parameters, which seldom has been considered in publications. Certain publications also state SNR but really show the signal plus noise power in arbitrary logarithmic units. This needs to be discussed, and, where possible, the graphs should be converted to signal power, or much better radar reflectivity. The latter is the only proper term which can be used for an acceptable comparisons. How can one guess that "ice particles are always there"? Define "PMSE decay time". Add: ". . .together with the . . . discussed above, but one has to discriminate between active and fossil turbulence." What is the Schmidt number 'idea'?

p.16, c.1 Section 4.3: Preferably use "Spectrum shape and width" instead of spectral width. It's the other way around: The width of the Doppler spectrum is directly related to the velocity variance. Little mistake: In this section 4.3 reference is made to section 4.3.

p.16, c.2 How do 'sedimentation effects' could explain anisotropy? ". . .active turbulence has stopped." How can this be observed or defined?

p.17, c.1 Again: Define how one can state that "neutral air turbulence has stopped". Refer here also to earliest spectrum shape observations with EISCAT published in the early 1990s.

p.17, c.2 What do you mean with: "mesopause altitudes can be observed independent of tropospheric weather conditions"? How are temperature measurements at the appropriate scales being done by satellites? "wave motions are usually excited in the troposphere". Really and only?

p.18, c.1 What is the meaning of "turbulent advection". Define more clearly. It is doubted that a narrow beam MF radar can provide simultaneous and continuous ob-

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servations of mesospheric turbulence strength. The reason is that at MF the effects of ionization irregularities, which could partially result from ionospheric effects, add up to the neutral turbulence effects.

p.18, c.2 Had there any relation been reported or investigated to meteor showers? Again refer to Kopp et al. (1985?) on local chemistry. “permanently”: add: in summer at high latitudes. Add a section on the most recently exploited potential of electron heating by high power HF waves. What else could be done to get a higher resolution information on the thermal structure?

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