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Interactive comment on "Meteor echo height ceiling effect and the mesospheric temperature estimation from meteor radar observation" *by* Changsup Lee et al.

Anonymous Referee #2

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This paper clearly presents an evaluation of a method for estimating atmospheric temperature near the mesopause using the heights of meteor radar detections. As such, the content is of scientific interest and worthy of publication. The writing is clear with a few small grammatical errors that will be easy to correct. The figures are clearly presented and are integrated well with the text.

There are, however, some problems with missing references and poorly described processes that are not fully justified in the text. It is my recommendation that the paper be published following minor revisions.

Overall, it should be noted that while the authors are inferring an estimate of MLT temperature from the width of the meteor radar detection zone, the most directly related parameter is the density scale height. A discussion of the role of scale height on the vertical extent of meteor trails is curiously absent from the manuscript. This was first discussed by Eshleman, 1957 and was investigated in detail in Younger's publicly available 2011 PhD thesis, which the lead author is familiar with.

General: The authors neglect the significant effect that meteoroid velocities have on determining the FWHM of the meteor height distribution. Faster meteors will have a smaller FWHM and are more susceptible to high-altitude cutoff. Furthermore, the relative numbers of different velocity meteoroids changes with time of day and season for a fixed observation location. Thus, the authors should calculate FWHM for a number of velocity bins and construct a fitted value for a single representative velocity, say, 30-35 km/s.

General: The asymmetry of the meteor detection height distribution is due primarily to the high-altitude cutoff. What is the effect of using the standard deviation of heights calculated separately above and below MPH?

Page 1, line 18-19: Here and throughout the paper, the authors state that they are measuring the mesopause temperature, but what is actually being estimated is a temperature near the mesopause. The height of the mesopause varies substantially more than the meteor peak height for which the authors state that their estimates are representative of.

Page 1, line 18-25: The authors should include some references to general meteor radar operation, such as McKinley, 1961, Ceplecha et all, 1998, or Holdsworth et al., 2004 (Radio Science). Furthermore, a discussion of meteor radar temperatures is incomplete without reference to Tsutsumi et al., 1994 (Radio Science) and Hocking, 1999.

Page 1, line 28-30: The authors fail to acknowledge the theoretical foundation of Eshleman, 1957, which provides the basis for their link between the height range of detected meteors and density scale height, and thus approximate temperature.

Page 2, line 5-6: The authors should cite a paper describing the meteor radar response function, such as Cervera and Reid, 2004 or just the review paper of Ceplecha et al., 1998.

Page 2, line 16-22: For a description of what is now a standard design for meteor radars, the authors should include a reference to Jones et al., 1998 for basic concept and Holdsworth et al., 2004 (Radio Science) for the detection and analysis software used by the King Sejong MR.

Page 2, line 29: When the authors say that they limit phase error to less than six degrees, do they mean for each of the receiver channels, individual antenna pair combinations, or the array mean?

Page 3, line 27: It should be noted that atmospheric density is the determining factor in meteoroid ablation. Pressure is really only relevant in a discussion of diffusion of the meteor trail after formation.

Page 3, line 25-29: A discussion of meteoroid ablation should include a relevant reference, such as Love and Brownlee, 1999 or Rogers et al., 2005.

Page 4, line 1-10: It should be noted that this formulation is only valid for an isothermal atmosphere. This is implied later via the use of <T>, but it should be stated in the derivation. I would like to see how the FWHM compares with the density scale height, which includes a temperature gradient term.

Page 4, general: The authors' derivation and method depends on meteor detections starting and ending at two well defined pressures, P1 and P2, but they do not state why this assumption is valid. Furthermore, they provide no concrete values for P1 and P2 as used in this study and do not provide information on where they obtained theses values, although perhaps the reader is meant to infer that SABER values were used? At the very least, the authors should supply the values and uncertainties.

Page 5, line 9-12: It is worth noting that 92 km is around (and sometimes past) the upper limit of reliable measurements by the MLS instrument. As such, the vertical resolution is less important than the accuracy of values extrapolated from MLS data.

Page 5, line 17: The authors are comparing a “theoretical” prediction based on C in equation 3, but C itself is derived from experimental observations for the individual radar system. This seems like circular reasoning.

Page 5, line 26: The authors need to provide more detail than “seems plausible”. It would be helpful to compare <T> obtained from their method with an average of SABER values, weighted by the distribution of meteor detections. Given the asymmetry of the meteor height distribution, would this result in a value of <T> corresponding to the lower than MPH maximum correlation height in figure 3?

Page 6, line 11: Needs reference.

Page 6, line 13-14: This statement should, at the very least, cite Jones, 1995.

Page 6, line 16-17: The destructive interference of backscatter from off-axis portions of the trail is described in detail in Younger, 2008.

Page 6, line 26: It is not just the reduced electron volume density responsible for reduced backscatter from trails with large initial radii. Backscatter from cylindrically symmetric distributions experiences significant destructive interference past the first maximum of the Bessel function in the backscatter amplitude integral (see e.g. McKinley, 1961 eq. 8-22 or Younger, 2008 figure 2).

Line 32-33: The precision of the FWHM is a purely statistical quantity determined primarily by the height accuracy of the radar and number of meteors detected. While attenuation terms do determine the behaviour of the high-altitude cutoff in detectability, it does not make sense to invoke attenuation terms in a discussion of the precision of the FWHM term.

Page 8, line 2-4: I fail to see how a demonstration of established meteor radar attenuation theory validates the authors' temperature estimation technique. The method is validated by correlation with independent measurement techniques. An assessment of attenuation coefficients is valuable for describing the shape of the meteor detection height distribution, but does not validate the method.

Figure 2: Label text in the plot area is too small to be legible.

Figure 4: This figure would be improved if the authors also showed the cumulative attenuation coefficient (product of all 3).

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Discussion paper