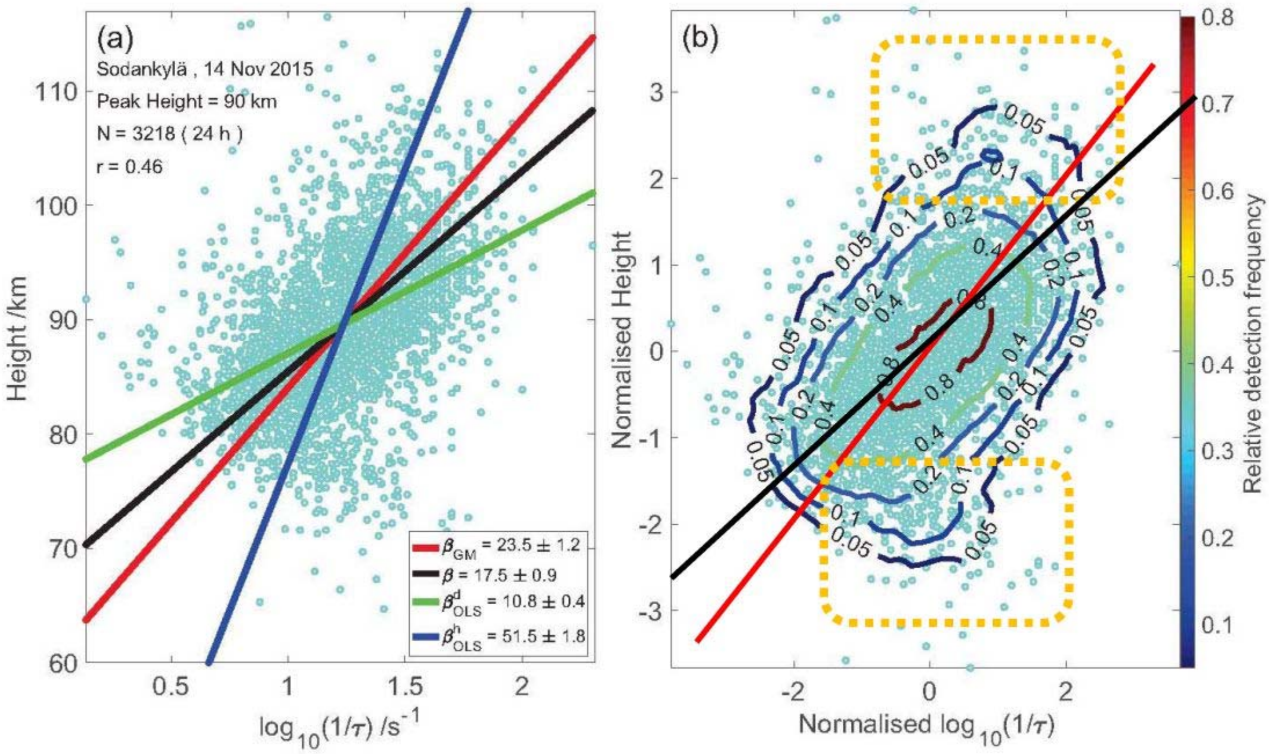
The manuscript “Improved method of estimating temperature at meteor peak heights” by Sarkar et al. deals with a long-debated issue of an accurate temperature estimation method using meteor echoes. They have statistically scrutinized the issue by taking into account the nature of detected echoes, and shown for the first time a method which could estimate the ‘log(decay-time) vs height’ slope without additional information other than the meteor echoes themselves. Although I think that the method is still crude and has a lot to be improved, their trial can be much appreciated. I therefore would recommend the publication of the manuscript once the following remarks are addressed accordingly.

General Comments

I should say that the manuscript is not very easy to follow mostly because of the use of lots of variables, concepts, and definitions. This might be indispensable due to the nature of the manuscript full of mathematical treatment, and also due to my limited skill in mathematics. My most concern regarding the present work is, however, the description in the appendix. It is the most important part of the manuscript dealing with the key correction term, μ'i or Sμ'. I recommend that the authors include it to the main body of the manuscript if no word limit exists. The derivation of the first equation A1 is not clear to me. Eq31 does not imply A1 to me maybe because of my mathematical skill. The mathematical and physical meaning of the final result A8 needs to be more explained. This is the key part of the manuscript. I should admit that I don’t fully understand the meaning of A8 although I can half guess the meaning from its simple relation and the similarity with a GM formula of Eq28. A schematic explanation using a plot like Figure 1 would be beneficial for readers.

Further, I am not sure if the final results of λ=1.76 and β=0.75 can be regarded as “close to 1” (line 520), which is a necessary condition to derive the analytic solution of Sμ' in the appendix. Since the GM solution needs to be a good approximate of the final slope and the correction should be small enough, an attempt to reduce unwanted error sources, which are referred to as “equation error” in the manuscript, would be necessary and justified although the authors avoid such rejection as “arbitrary”. The effect by such equation error sources may not be so severe that the GM approximation fails. The proposed method apparently works well, at least for the meteor data used here. But it is only justified when another temperature data such as lidar data to compare is available. Please note that I don’t mean to criticize the authors’ effort. Since the idea of introducing a correction term is superb, I would be pleased if I could contribute to the proposed method. In the following paragraphs I will show my idea on “natural variances”/”equation error” in meteor echo observations, because of which I think that an adequate data selection/rejection would be justified prior to applying the proposed technique.

In order to see the nature of the observed meteor echo distribution I over-plotted the GM and final slopes in Figure 1b (a careful replica of Fig1a on Power Point). A close inspection will tell us that the GM solution (red) clearly overestimates a slope which the core distribution inside the 0.4 and 0.8 contour lines indicates while the final slope (black) follows in a better way the core distribution (this is good of course), especially the 0.8 contour area. This implies that the use of core distribution could be a good way to get a better GM solution although the GM solution does not explicitly show up in the equations (35) and (37). The values of final λ and β would be closer to 1 than the proposed use-every-thing method. This will be because the most annoying regions, the areas surrounded by the orange dotted lines, can be tactfully avoided. The usefulness of this approach should be tested experimentally.



The authors think that the distribution in these orange areas are affected by natural variations and should not be rejected in an arbitrary way. It will be a sincere attitude, but I believe through my long experience in meteor echo study that such distribution is mostly a result of the limitation of observation techniques. For the upper orange distribution, any magnetic field effect may exist, but observation made with a long radio wavelength (2.4 MHz) indicates that ambipolar diffusion shows an exponential increase, at least, up to 110 km without restricted as seen in Figure 7 of Tsutsumi and Aso (2005). The apparent clip seen at around 95 km in the upper part of Figure 1 of the present study is a manifestation of the limited sampling speed and the height ceiling effect (Lee et al., 2018) for a VHF system. Such effect is also seen in the 2.4 MHz observation, but at around 105-110 km. On the other hand the lower orange region is somewhat complicated. Although a chemical effect may exist, the signal-to-noise ratio of echoes in the region can be responsible, at least partly. The SNR in this region is obviously lower than that in the higher region (please check this with your data) leading to noisier estimate of ambipolar diffusion and mostly apparent offset toward larger diffusion. Because of these reasons an adequate rejection will be justified for a better first estimate of GM solution. There would be no need to pay respect to system-dependent and non-natural error sources. Asymmetric error sources could be handled with Sμ, but a better GM estimate should be tried for a real independent slope estimate without any external temperature information.

Because of the above mentioned height dependent error sources (and more perhaps) the assumption made in the following part of the manuscript is weak as the authors have already realized.

Lines 140-142
Lines 229-230 and Eq23
Line 520
Line 530

After applying an adequate rejection criterion the assumption can be more acceptable and the proposed fitting method will be more applicable and yield a reliable slope estimate.

Individual comments

Figures 2, 3 and 6 X-axis: The dates at the tick marks seem shifted by 15 days.

Line 110: The daily echo numbers around 2000-4000 seem somewhat smaller than expected as a SKiYMET system although I know the number is reduced around spring equinox because of the tilted earth's axis. Some tuning on the radar system may improve the number; antenna tuning, impedance matching.... This is just a comment not necessary to be addressed in the revision.

Figure 3 (a): The figure and caption do not correspond with each other. Figure 3 (a) shows estimated temperatures including those by the lidar (black), and the caption indicates what are plotted is "temperature offset".

Lines 276-277 "the standard errors in these temperatures, which is on average 19 K":
Is this a value estimated using one season of the CORAL lidar data?

Lines 287-289: The use of high contour density area seems worth trying. Or the use of high SNR echoes and/or small zenith angle echoes can be another choice for a better slope estimate because of their less height and decay time estimation errors.

Line 299: Some words are fallen out?

Line 301 "only": Is this necessary?

Lines 317-318: Since it is the decay time that is mostly affected in the lower and upper distribution rather than height as seen in Fig 1, it seems natural to use a correction term to decrease the effective variances of di instead of to increase that of hi. Is such an approach possible? I presume it will give an equivalent result.

Lines 331-332: A strict mathematical treatment of μi is beyond my understanding. But is such a practical approach that a constant value Sμ' represents the whole equation error mathematically acceptable? Or simply practical? Maybe a meaningless question...

Line 342: Two of the four ν redundant?

Line 353: What are the bars over h and d?

Line 358: Could you explain more about "a priori knowledge"? The knowledge of λ being larger or smaller than 1 as well as closer to or far from 1 seems important to decide what model to be used. If so, is a certain amount of shift from 1 (always positive or negative) necessary to apply a model to the data? Always approach from a fixed side to the final solution? This may be another key point of the present method (or I totally misunderstand it).

Line 366-267: Does this mean that the resampling was made 20000 times for every 24 hr?

Line 415: "can estimated" > "can be estimated"

Appendix: More detailed description and mathematical explanation/insight of A8 are wanted as mentioned in the general comments since this is the most important part of the self-consistent slope estimation.