Dear Reviewers,

Thank you very much for your helpful suggestions for the revision. We have below addressed each comment, reciting the comment together with our responses. We have addressed all editorial-related corrections by the reviewers given to us in the annotated pdf during our revision of the manuscript and do not recite them below.

Line 10: Commonly, temporal integration is applied to increase the SNR of radar signals. Meteor trails drift slow enough in the atmosphere and allow for temporal integration while meteor head echo targets move too fast.
COMMENT: Suggest this [second sentence] is moved to before previous sentence.
REP: We have restructured the two sentences to make it more clear, they are now: "Meteor trails drift slow enough in the atmosphere and allow for temporal integration while meteor head echo targets move too fast. Temporal integration is a common method to increase the SNR of radar signals."

Line 58: Meteor radiants can also not be determined if the location is ambiguous.
COMMENT: Not strictly true: the ambiguous DOA candidates can be used for radiant determination, and if one DOA candidate is from a shower is will contribute to the radiant PDF.
REP: To avoid being drawn into a lengthy additional explanation of different applications we removed that sentence and clarified the surrounding text.

Line 98: The five-antenna receiving array is arranged as an interferometer, and phase differences in the signals arriving at each of the antennas of the
interferometer are used to determine a theoretically unambiguous angle of arrival. **COMMENT:** Suggest you say "Jones interferometer" or something similar so it is obvious it is the Jones config - as this is not explicitly stated elsewhere.
REP: Good catch! We changed the sentence to: "The five-antenna receiving array is arranged as a Jones 2.5λ interferometer, and phase differences in the signals arriving at each of the antennas of the interferometer are used to determine a theoretically unambiguous angle of arrival."

Line 128: The second method is temporal integration of the spatial correlation matrix, described in detail in Sect. 3.2. This
second method is easily implemented in data analysis pipelines and requires less computations then regular DOA determination.
COMMENT: I'm surprised by this comment. But it's not clear what you mean by "regular DOC determination".
REP: We think the manuscript gremlins have been at work here and old and new sentences have been mixed and therefore does not make much sense. It a lot faster in the sense that if a DOA determination takes T-time for N pulses, temporal integrates only takes T/N time as only one set of integrated pulses is analysed. We changed the sentence to: "This second method is easily implemented in data analysis pipelines as it can rely on previous DOA determination implementations and is only analysing a single integrated pulse rather then all pulses individually.".

Line 130: [Not recited here]
COMMENT: These sentences disrupt the flow of the paragraph and would be better placed somewhere earlier in the paper (e.g. line 52).
REP: We agree! These have been moved.

Line 157:Also, the system used in the study is located in a rather underpopulated region of northern Finland where interference in the used radio frequency band does not seem to be a concern.
COMMENT: This comment appears to be contradicted by line 292 which refers to "ionosonde interference".
REP: The ionosonde interference is "bursty" in nature and we know the transmission setup, it is therefore easily filtered out as transient events that are not trails and will hence not act as constant background interference. We have changed the sentence to clarify this is what we mean: "Also, the system used in the study is located in a rather underpopulated region of northern Finland where interference in the used radio frequency band does not

seem to be a concern (except the transient ionosonde interference which is easily filtered out, See section 4)."

Figure 1:
COMMENT: Were radial velocity effects simulated? If so what value was used?
REP: Radial velocities were not simulated as this was only a showcasing example.
COMMENT: This appears to contradict line 178 which says "Azimuth measures the angle counterclockwise from East." If this is the case the red circle of azimuth 0 should be along the x-axis (Ky = 0).
REP: Good catch, more manuscript gremlins. Line 178 was never updated after we changed the plotting conventions. This has now been fixed!

Figure 2:
COMMENT: I couldn't see the grey lines overlapping the yellow and blue lines in the left plots until I'd look at them about five times. I suggest you make the grey lines clearer. Also, is the width of the yellow/blue lines significant (e.g. uncertainty).
REP: The widths are not significant, it was simply for the clarity of the plot. This has been added to the figure description. We also made the grey lines darker for clarity.
COMMENT: Also, the echo shown in the top-middle plot looks like an overdense echo, and that the end time is incorrectly calculated. This is not a problem for the DOA estimation demonstration but the resulting radial velocity estimate would not provide useful wind information.
REP: We simply picked the first ambiguous event in the list as generated by the parallelized automated analysis and is hence a random pick (we did this to remove selection bias from our side), the start and end points marked are calculated by the SKiYMET analysis.

Figure 3:
COMMENT: It seems odd that the probabilities are either zero or one. I would have expected that the two candidates produced by the SKiyMet processing would have had comparable probabilities (say around 40%).
REP: This is what is so interesting about Bayesian inference! It is a way of formalizing what one can call "eyeballing" data. By just looking at the distributions in panels labeled input 2 and 5 we see that input 2 generates samples in all places where we measured samples, while input 5 does not match the measurements as well. While eyeballing would stop us there at "one matches better", Bayesian inference can give the quantitative values for this. For example, for input 5, there is practically no chance of generating samples in the ambiguity left of input 2 and high chance of generating samples in the top-most ambiguity. As these two locations sample probabilities are so different between input 2 and 5, the measurements basically tell us that "drawing samples from the probability distribution of input 5, it is extremely unlikely the get a configuration close to our measurements". And furthermore forcing the Bayesian inference to only consider these inputs as the possible candidates (i.e. they must sum to 100%), its easy to see why its 0.00...something probability at input 5.

Figure 4:
COMMENT: I strongly recommend this Figure is made larger int he final paper. It is very cluttered around the radiant curve which makes it hard to evaluate.
REP: If the Journal agrees, we will make it text-width.

Line 291: In total 2,222 events were automatically analysed. We did not include events with undetermined τ (these may be ground echoes or long-lived non-specular echoes) , τ less than 0.001 s (these may be ionosonde interference) or radial velocity exceeding 100 m/s (these may be due to Farley-Buneman instabilities).
COMMENT: What is tau? This has not been introuduced previously.
REP: τ is the time for which the amplitude of the backscattered signal from a meteor trail falls to one half of its maximum value. We have added this description to the text in the following preceding sentence: "To select authentic specular trail echoes we used the time for which the amplitude of the backscattered signal falls to one half of its maximum value τ."

COMMENT: They could also be overdense echoes. As an aside, it is interesting there is no reference to underdense/overdense echoes in this paper.
REP: For DOA it does not matter whether the trail is over- or under-dense as far in both cases the backscatter is

specular. To clarify this sentence we have added a reference for underdense/overdense echoes: Bronshten, V. A. (1983), Physics of Meteoric Phenomena, 356 pp., Kluwer, Dordrecht, Holland. We also added the following reference for ground echos: Kozlovsky, A., Shalimov, S., Oyama, S., Hosokawa, K., Lester, M., Ogawa, Y., and Hall, C. (2019). Ground echoes observed by the meteor radar and high-speed auroral observations in the substorm growth phase. Journal of Geophysical Research: Space Physics, 124, 9278– 9292. https://doi.org/10.1029/2019JA026829. Finally we also added a reference for long-lived non-specular echoes (where also overdense trails are mentioned): Kozlovsky, A., Lukianova, R., and Lester, M. (2020). Occurrence and altitude of the long-lived nonspecular meteor trails during meteor showers at high latitudes. Journal of Geophysical Research: Space Physics, 125, e2019JA027746. https://doi.org/10.1029/2019JA027746.
COMMENT: Please add a reference to FB instabilities here as it may not be clear to the reader what these are.
REP: We added a reference to: Kelley, M. C. (2009), The Earth's Ionosphere: Plasma Physics and Electrodynamics, 2nd ed., 556 pp., Elsevier, New York.

Line 333: Fig. 5
COMMENT: This comes before Figure 4 and hence should be Figure 4.
REP: Fixed

Line 334: This further validates the robustness of the implementation as their difference typically is less than the reported expected angular measurement accuracy of the system ($\approx 1^\circ$, Jones et al. (1998)).
COMMENT: Please quote the RMS errors and perhaps indicated them by a vertical line in the histograms of Figure 5.
REP: Calculated and added the following to the figure text: "The root mean square differences are 0.68° for the temporal integration results and 0.79° for the matched filter results."

Line 346: The remaining events that did not concur, all appeared to be noise or too weak to be identified by manual inspection.
COMMENT: Do you mean "noise" or "interference" - since you have used the interference earlier you should make this clearer - i.e. I would expect ionosonde interference to be "impulsive" which could be detected as a meteor (i.e. sharp rise, short liftetime) and thus more likely to be detected than a random noise spike or burst (given the other sources of noise you mention will be less "transient" or "impulsive"
REP: Yes the sentence was unfortunately not very clear. We mean noise as in we cannot see any discernible transient event at all by visual inspection, i.e. it looks like regular background signal. We changed the sentence to: "For the remaining events that did not concur, upon manual inspection no events could be identified in these cases and the signal appeared to be only background noise. Hence they were not examined further."

"Author contributions."
COMMENT: Is that Lester's only contribution? If so, is this enough justification for co-authorship?
REP: Good catch, we had forgotten to add Mark as a data provider. That part now reads as: "Alexander Kozlovsky and Mark Lester provided the data set analysed."

COMMENT: The manuscript contains an empty acknowledgements section and a "TEXT" placeholder under disclaimers.
REP: We removed the "disclaimers" section as this is optional. We also removed the "acknowledgements" as all involved parties in this study are authors.

COMMENT:
The Herlofson 1951 reference is not appropriate. It should be the sub-paper as contained in: Lovell, A. C. B., J. P. M. Prentice, J. G. Porter, R. W. B. Pearse, and N. Herlofson, Meteors, Comets and Meteoric Ionization. Nature 160, 76–78 (1947). https://doi.org/10.1038/160076a0

REP: Thanks for catching that! However it was also suggested by the editor to use:
Lovell, A. C. B., J. P. M. Prentice, J. G. Porter, R. W. B. Pearse, and N. Herlofson (1947), Meteors, comets and meteoric ionization., Reports Prog. Phys., 11(1), 389–454, doi: 10.1088/0034-4885/11/1/313

As these seem to come from two different entries (https://ui.adsabs.harvard.edu/abs/1947Natur.160...76L/abstract versus https://ui.adsabs.harvard.edu/abs/1947RPPh...11..389L/abstract) we have chosen to use the one suggested by the editor.