We thank both the reviewers for the very useful reviews! We have revised the paper accordingly. We have added a small comment on how we have addressed each specific issues raised in the review according to the summary below.

RC #1

1) The "temporally integrated sample spatial correlation" used by the authors is identical (sans normalisation) to the zero-lag cross-correlation commonly used to calculate DOA for meteor radar observations (e.g. Holdsworth et al, 2004, as referenced by the authors). The reference to "using temporal integration of the signal spatial correlation" in the abstract leads the reader to believe that the authors are proposing and applying a novel technique but this is not the case.

> We have rewritten the abstract and the rest of the manuscript to emphasise that the concept per se is not new and carefully related it to previous work.

2) The paper refers to the use of the "temporally integrated sample spatial correlation matrix." This is actually a "sample spatial covariance matrix". It is a "sample" matrix as the actual spatial covariance is a priori unknown - i.e. we can only make a sample estimate of it. And it is a covariance matrix as the result is not divided by the covariance of each channel. The authors should use the correct terminology throughout.

> We also intentionally dropped "sample" in our terminology (as also seems to be the case in H2004) as this was considered implicit, but for clarity we will use it in the revised version of the manuscript and mention when it is dropped! In most modern statistical texts that we have examined there are three distinct definitions: Cross-correlation (second order statistical moment), Cross-covariance (central second order statistical moment), and the Pearson correlation coefficient. According to the definition of these, $XX^\dagger$ is an estimator of the first, Cross-correlation, where the expected value is taken over the pulse sampling. Hence we have not changed this naming convention.

3) Only 27 meteor echoes are used to test the analysis. While it is encouraging that the 26 of these echoes yield the same results as Bayesian inference, I'd prefer the technique was applied to a much larger data set: e.g. thousands of echoes. While the Bayesian inference technique appears to work well, this was described in a previous paper. Since the focus of the paper is on the comparison/combination of the temporally integrated sample spatial covariance matrix (which as discussed in 2) is not new) and the Bayesian inference technique (which was described in a previous paper), I feel that the authors need to do considerable work to revise the paper to emphasize the novel contribution of the work. The benefits of "temporal integration" are well known, and while it is interesting to see these benefits illustrated it's not clear to me what is novel about the work. I have included an annotated version of the manuscript with some proposed corrections. This also includes some of the comments below which I have included for the benefit of other readers of the paper.

> We have applied the technique on 2,222 events and revised the paper to emphasize novel contributions in the work.

1) Line 74) "linear regression is more sensitive to outliers and noise then temporal integration of complex amplitudes". This somewhat misleading as the cross-correlation is identical to the "temporal integration" of the spatial covariance function used in this paper. And the reason the linear regression is used is to avoid use of the zero-lag cross-correlation/covariance used in this paper which is sensitive to noise correlated between received channels.

> We have rewritten the text and tried to remove misleading parts.

2) Line 79: "However, a more effective coherent integration would be to apply a matched filter and coherently integrate prior to calculating the cross correlation." It's not clear to me what the authors mean by match filter here. Do they essentially mean correcting for the trial drift. Please clarify.

> We have implemented a matched filter which correct the individual channels for the trial drift prior to calculating the cross correlations. We have discussed how this differs from regular temporal integration and compared

the results. A detailed discussion/comparison is given in appendix a.

3) Line 134: Given you are talking about the Rx/interferometer antennas the reader may assume g(k) is the Rx antenna patter. It is however the combined Tx and Rx pattern. Please calrify this.

> What we had considered is that: for volume filling targets the Tx pattern matters for the Rx sensor response since the illumination of the volume filling target is given by the Tx pattern (for point targets or small targets, only Rx pattern matters for interferometry). As such, we had assumed that the Tx pattern is smooth and that illumination of the relatively small meteor trail target (in terms of steradians) is close to uniform since the Tx is a single Yagi. We added a sentence describing this.

4) Line 140: "any spatially correlated noise from galactic sources". Galactic noise is not the only source of correlated noise. Other sources include users instrumental effects (e.g. Tx and Rx phase noise) and other users on the same frequency channel.

> We have extended the paragraph. On the existence of other users on the same frequency, this is essentially handled by using the MUSIC algorithm: as the signal subspaces are determined (all of them) and the noise subspace is defined as the complement space of only the strongest signal, any such other signal will automatically be filtered away as long as its signal is weaker than the trail echo. 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.

5) Line 145: "This phase calibration data should include the effects of mutual coupling." This depends upon whether the calibration solution incorporates the antennas: e.g. using a near or far field source. The authors should describe how their phase calibration is performed to justify that the calibration solution incorporates the antennas.

> We have removed this particular sentence and rephrased the paragraph as it was misleading. We do not perform phase calibration but use phase calibration values stored as part of the standard data products.

6) Line 169: First, the bar above the R indicates a mean is used - this is not the case. Second, this equation is identical to the zero lag cross-covariance as used by Holdsworth et al, 2004, as mentioned above.

> For clarity we added a normalization with samples to make it into a mean value. Also added additional reference to Holdsworth et al, 2004.

7) Figure 1 a) caption: I'm confused by this as based on your definition of azimuth stated earlier (counterclockwise from East, line 162) I'd expect to see a population of AOAs at (0, 1/sqrt(2)) but there is nothing there. Also, it would help if you indicated the theoretical AOA on the plot - perhaps an unfilled red circle?

> There was a bug in the plotting script where x,y had been switched. This should now be fixed. A red circle has been added for clarity.

Please also note the supplement to this comment: https://amt.copernicus.org/preprints/amt-2020-220/amt-2020-220-RC1- supplement.pdf

> Unfortunately, we do not find any annotations in the provided supplement.

RC #2

Overall, this is an excellently written manuscript that clearly lays out a new technique for determining the direction of weak meteor radar echoes. The structure is logical and easy to follow and the writing clear and nearly grammatically faultless. Of particular note is the authors characterization of directional ambiguity and confidence using statistical methods. I recommend that the manuscript be published after considering some minor suggestions.

Line 19-44: A reference to Herlofson (1947), McKinley (1961), or other similar early work on meteor radar would be appropriate.

> Done!

Line 28: ". . .phenomena occur lies between. . ."

> Thanks, done.

Line 107: The variable "ambig" is written here in all caps, but in an alternate font online 49. Change for consistency.

> Done!

Line 128: It also assumes that the antennas are electrically independent, i.e. no coupling.

> We have added this to the sentence.

Line 144-145: A comment on coupling errors (no change needed): phase calibration methods assign constant phase biases between antenna pairs, but this cannot account for coupling, as coupling is a function of DOA. The authors do however, explicitly state that they do not account for coupling.

> As that sentence did not add anything relevant to the discussion, we removed it and instead only stated directly what correction we applied to avoid confusion.

Figure 2: There are several issues with figure 2. The red and especially green lines are quite difficult to see, requiring a substantial zoom in and examination. Furthermore, green/red is the most common form of color blindness, so this choice of marking scheme may provide accessibility challenges for some readers. Also, in the top right panel, why is the signal discontinuous? There are large portions of white space between line segments that I assume are connecting sample points. Perhaps this is a plotting issue, or something going wrong in format conversion?

> We have improved the appearance of figure 2 following the recommendations. The colors have been chosen from a color blind friendly palette. The discontinuous signal in the previous version was a plotting issue due to the line width unintentionally being too thin for the resolution down-conversion that occurred when compiling the pdf version of the manuscript.

Figure 3: Similar to figure 2, the red crosses and circles are hard to see without zooming in quite close. Changing the line thickness and increasing the symbol size would help with this.

> We have improved figure 3 according to the recommendations!

Line 268-269: I am curious as to the author's decision to include events that could not be unambiguously classified as meteor echoes. I understand that the purpose of the exercise is to push DOA analysis to low SNRs, but it seems that any confirmation of the technique relies on the example echoes being genuine meteor detections.

> We have included events that were classified as unambiguous and had nominal signal and confirmed that our DOA analysis in 100% of these cases found the same DOA as the original analysis. For ambiguous events, we have investigated whether the signal was nominal or anomalous and divided the results thereafter. As the reviewer points out, we will obviously not confirm the technique by applying it on events that are not genuine meteor detections.