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Atmospheric Measurement Techniques Discussions

Interactive comment on "Resolving ambiguous direction of arrival of weak meteor radar trail echoes" *by* Daniel Kastinen et al.

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This paper propose a method for interferometric meteor data analysis using temporally integrated sample spatial covariance matrix to resolve DOA ambiguities. The method is validated by a combination of Monte Carlo simulations and application on 174 meteors obtained using the Sodankylä Geophysical Observatory all-sky interferometric meteor radar. The authors applied a Bayesian method to determine the true location of meteors with ambiguous DOAs in the data set, and found using the phase of the temporally integrated sample spatial covariance matrix,gave the correct output DOA as determined by Bayesian inference. In 26 out of 27 ambiguous cases, the temporally integrated sample spatial covariance matrix temporally integrated spatial correlation gave the correct output DOA as determined by Bayesian inference.

I have three main issues with this paper:

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.

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.

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.

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.

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.

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.

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.

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.

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.

7) Figure 1 a) caption: I'm confused by this as based on your definition of azimuth stated earlier (counter-clockwise 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?

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

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