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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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Dear David Holdsworth,

Thank you very much for the useful review! We will revise the paper to emphasize the novel contribution of the work. Tentatively, we plan to address the specific issues raised in the review according to the summary below. You would be very welcome to let us know what you think of the revision plans as part of the open discussion.

We believe that the main points of the paper should be:

- It was shown (partly theoretically) how DOA ambiguity depend on SNR. The distributions were predicted and also simulated: Do the simulations/predictions match reality? [Yes]
- We now know the SNR limits from the theoretical study: Can we apply standard techniques (temporal integration) to resolve already published ambiguous meteor radar data (SKYiMET) and remove ambiguities? [Yes]
- If one cannot apply temporal integration: Does the proposed Bayesian method actually work? [Yes, with modification]
- Since we can both integrate and apply the Bayesian method: Do they cross-validate? [Yes]

This data-set of trail echoes, that is published with ambiguities, is a good way to test the simulations and methods from the other theoretical study because: a) we can do both temporal integration and perform the Bayesian method, b) we have a simple system that is extensively used, and c) we have a well-used analysis to cross-check results with. Regarding e.g. meteor head echo data we cannot easily and efficiently perform coherent integration due to a fast moving scattering centre, effects of fragmentation, etc. It is harder to validate the merit of the Bayesian method using head echo data. In addition, using a more complex radar system that has not been extensively used for meteor observations before (which would be novel) introduces an uncertainty when it comes to "comparison with real data" since the prior behaviour is less well-known and the analysis procedures new. Even if the current study is perhaps a bit "dry", we still believe it addresses useful questions.

We have realized that we really should have applied a matched filter (compensating for Doppler shift across radar-pulses) to achieve optimal SNR-gain as another comparison. To improve the comparisons and the manuscript, we have now implemented a matched filter analysis and are in the process of analysing the selected events using that method. We will add those results to the manuscript in the revision.

Below are some general comments/responses on the numbered issues raised:

1.
First of all, it was not our intention to make it seem like coherent integration is something novel. If the text was badly worded so that it conveyed such a message, we apologize. We will rewrite it during revision. We realize that the main focus of this study was not properly conveyed in the current version of the manuscript and we will revise it to make sure that the focus of the study is better described in the abstract and the rest of the text in the revised version!

On the topic of zero-lag cross-correlations: We must have first misunderstood the method described in the reference Holdsworth et al, 2004 [hereafter H2004]. Without going into detail on what our miss-understanding was, after re-reading the paper a few more times, our new understanding is that: if we let $x_{i,j,k}$ be a complex voltage sample where i=pulse, j=pulse sample and k=channel (antenna), then first a set of "CCFs" is produced as

$$CCF(i, k_1, k_2, n) = \sum_{j \in code} x_{i,j,k_1} x^*_{i+n,j,k_2}. \tag{1}$$

So the "lag" is in pulses, not in pulse samples (where the first lag of the auto-correlation, now lag in pulse samples, is used to determine $j \in code$, i.e. the range). Then linear fits are done on $\{\angle CCF(k_1, k_2, n)\},\ n \in \{-2, -1, 1, 2\}$. The slope is then an estimate of the phase shift rate due to radial drift rate ω, so this is applied as a correction and something like

$$\sum_{i \in trail} \sum_{j \in code} x_{i,j,k_1} x^*_{i+n,j,k_2} e^{-i\omega t_i}, \tag{2}$$

is used with the original Jones DOA determination method so only a single DOA is calculated for each trail? And if $n = 0$, it is assumed that $\omega = 0$? If we now understand the method correctly, then the zero-lag cross-correlations, after the summing step in "3.8. Phase Difference Re-Estimation and Receiver Recombining", is the same as the first row of $\bar{R}$! Could you please confirm if this was indeed the correct interpretation? If this is so, we will of course revise the manuscript to reflect that we have applied the H2004 method.

2.
In [Schmidt 1986] $(XX^\dagger)^*$ is defined as the covariance matrix of the measurement vector. However, in most modern statistical texts that we have examined there are three distinct definitions (where * is complex conjugation):

Cross-correlation (second order statistical moment)

$$\mathrm{corr}_{XY}(t_1, t_2) = E\left[X(t_1)Y(t_2)^*\right]. \tag{3}$$

Cross-covariance (central second order statistical moment)

$$\mathrm{cov}_{XY}(t_1, t_2) = E\left[(X(t_1) - E[X(t_1)])(Y(t_2) - E[Y(t_2)])^*\right]. \tag{4}$$

The Pearson correlation coefficient

$$\rho_{XY}(t_1, t_2) = \frac{\mathrm{cov}_{XY}(t_1, t_2)}{\mathrm{std}(X(t_1))\mathrm{std}(Y(t_2))} \tag{5}$$

One can show that $XX^\dagger$ is an estimator of the first, where the expected value is taken over the pulse sampling. If there is a better naming convention (with reference), or we are mistaken somehow, we will be happy to change the naming.

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.

3.
Unfortunately, the SKiYMET radar system used in the study is a commercially provided system where we do not have full access to the "raw" data or the code that produces it. Our intention with the current study is to validate and demonstrate the possible application the of methods in [amt-2020-157] on measurements, not to implement a new independent analysis pipeline. Some parts of the data analysis was done manually. These parts unfortunately do not scale practically to use on thousands of events. We believe that if we would perform a large statistical study, and thereby essentially create a new second pipeline for the system, this would take focus away from the scope and current goals and probably be an inefficient use of time. Many of the settings of the system are currently frozen and not optimal (e.g. no alternating coded sequences on transmission which means there are range ambiguities in addition to the angular ambiguities). In the future, we plan to communicate with the radar system provider (Genesis software) about their possible interest to update the pipeline, whereby it would become relevant to perform a much larger study.

Line 74: We agree that the sentence was badly worded, linear regression is indeed a good insensitive estimator under certain conditions (e.g. Gaussian noise on all included data points and no outliers from other distributions). In e.g. [Vierinen, 2016 (https://amt.copernicus.org/articles/9/829/2016/amt-9-829-2016.pdf)], what we call a matched-filter approach (therein coherent deconvolution), was used to solve for the state parameters. Even with such an approach, in Sect. 3.4 Bursty interference, outliers were addressed. We will revise the sentence to better highlight what our intention was: that outliers usually need to be treated (or at least monitored) for fitting

methods to be effective.

Line 79: In Appendix A we describe why a matched filter approach is more efficient. What we mean by a matched filter is basically the following (see also the reference in the previous comment): Calculating a theoretical complex signal as a function of time and space, where the form of that function is a function of the target parameters. Therefore allowing for "corrections" to the measured signal in time and space to be made so that, if the target parameters are chosen correctly, all samples coherently integrate (thereby the need to sweep the target parameter space). Beamforming to determine DOA is a type of spatially distributed matched filter in this sense. This has the advantage of being a more efficient coherent integration then simply relying on the much shorter natural coherence time of the target itself (if models of the target and signal work well). Another good example of this is the measurement of resident space objects, e.g. [J. Markkanen, et. al, Real-time space debris monitoring with EISCAT, 2005], where it is called match function or matched-filtering (MF) method.

Line 134: 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. If this is indeed what your concern was about, we will add these remarks to the manuscript!

Line 140: We will add the remark about instrumental effects! On the existence of other users on the same frequency, this is essentially handled by using the MUSIC algorithm: as the signal sub-spaces 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. We will add these remarks.

Line 145: Unfortunately, we do not exactly know how the "phase calibration" is performed since this is a commercial system. We are simply given a set of phase calibrations with no insight into the specific code. The question can probably be raised to Genesis software. Do you think this is a major issue and that we should pursue an answer from them?

Line 169: A vinculum is used in many areas with many different purposes. We are used to expected values being written as $E[x]$ or $\langle x \rangle$ and then that a function (the sample mean $\tilde{\mu}$) is an estimator of those. However, as the method is completely invariant to a real constant multiplication we will simply add the $1/N$ to the formula thus making it a mean value to avoid further confusion.

Regarding the second point, please see the comment on (1.). Again, if we have interpreted the paper correctly this time, we will of course revise the manuscript to reflect this!

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