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Interactive comment on "Coded continuous wave meteor radar" *by* J. Vierinen et al.

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We thank the reviewer for valuable comments that help improve the quality of this paper. It is obvious from the comments, that the referee is very knowledgeable on the topic of meteor radars and gone through a lot of trouble to improve the quality of our paper.

The responses to the individual points are listed below, with ">" preceding the referee comments.

> This is quite ironic - I would very much like the authors to look at this paper Elford, W., and D. Robertson, Measurements of winds in the upper atmosphere by means of drifting meteor trails II, J. Atmos. Terr. Phys., 4 , 271-284, 1953.

> I would very much like to ask the authors to adjust the title and to remove the intro-

ductory statement of the abstract, and give better recognition to those who have gone (long) before.

The Webster dictionary defines the word "irony" as one of three possibilities:

a : the use of words to express something other than and especially the opposite of the literal meaning b : a usually humorous or sardonic literary style or form characterized by irony c : an ironic expression or utterance.

We would like to clarify that no humour was intended when writing the paper, nor was it written as a humourous piece. Our intention was to state that we are presenting a new type of a concept for a meteor radar, which we hope others will find useful.

The Elford and Robertson paper describe a bi-static meteor radar that transmits a low carrier wave and a high power pulse train. The uncoded carrier is used to obtain Doppler shift and the pulses are used to determine range. This is essentially a combination of a radar that uses pulses to range targets, and frequency offset from a carrier to measure Doppler. These are both concepts that preceed Elford and Robertson. Already Breit and Tuve used short pulses to measure range in 1925, and already Watson-Watt and Wilkins used uncoded carrier to measure Doppler in 1935.

While Breit and Tuve; Watson-Watt and Wilkins; Elford and Robertson; and a large number of other people have done very impressive pioneering work in the early days of radar, we believe they have not described the combination of phase coding and numerical analysis that we are we present in the paper. In our case, we describe a method that derives range and Doppler from a constant amplitude phase coded continuously transmitted pseudorandom waveform. We do in such a way that the same RF band is shared by multiple independent transmitters, due to the statistical orthogonality of different pseudorandom waveforms. This has a lot of advantages that we have listed in the paper. We have not seen anyone apply this concept to meteor radars before, but we do not claim to have invented phase coded radar transmissions.

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We have never claimed that we are introducing an uncoded CW radar or pulsed radar, as the author says when stating that we are "re-introducing a 60 year-old technique, at best." The trailing "at best" implies that what we present in our paper is probably not even as good as the techniques presented in 60 year old papers. We feel that this statement and implication is patently false.

We do not know what is wrong with the title: "Coded continuous wave meteor radar". We believe it accurately describes the technique presented in the paper. Our intention was not to disrespect prior work or to be comical, our intention was to bring forward a new coding and signal processing technique that we believe hasn't been applied to meteor radars before in the way presented in our paper. We also have not seen the measurement model and analysis equations presented before, so we have included them.

To improve our paper, we will add the suggested references and make the wording on our contribution more specific to avoid a possible interpretation that we are claiming to have invented the pulsed radar technique, or the uncoded CW Doppler radar technique.

> Page 7881, line 16 - "meteor trails are typically point-like in range" - this is wrong. The meteor trails can be 5 - 10 km long in range. The specularly reflecting point is potentially point-like (though it is usually considered to cover a Fresnel zone) but the trail is NOT!

The referee is correct. The language was not precise enough. We will change this to point-like in delay, although in some cases radar echoes are spread in range.

> Page 7881, line 11 - not sure why Holdsworth is referenced here for winds - there were many papers before this one - at the least, say (Holdsworth et a., 2004, AND REFERENCES THEREIN)

Thank you for pointing this out, we'll make the correction suggested by the referee.

> Page 7883 line 22 - the equation "N = c(d)...etc" - the origin of this is unclear. Is it

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supposed to be obvious? Or is there a reference? How/when were typical values of c(d) found? What is d? Is it supposed to be distance?"

The referee has a valid point. The d refers to distance. This is obtained experimentally using a multi-static meteor radar deployed in Germany. We will add clarification.

> Page 7884 section 3 - the authors mention both frequency domain and time domain analysis - but then say little about the frequency domain. A link to a paper that does use long coded pulses and does the analysis in the frequency domain might be useful to some readers e.g. Hocking et al., “Windprofiler optimization using digital deconvolution procedures”, JASTP, 118(A), 45-54, http://dx.doi.org/10.1016/j.jastp.2013.08.025, 2014, uses this strategy to obtain ∼50m resolution with a 1 km pulse. Might also be a useful reference at the bottom of p. 7881 and top of p.7882.

We have not discusssed the frequency domain method because we do not use frequency domain techniques for this specific application. We believe frequency domain techniques are not as flexible as the time domain method presented in the paper. The time domain method allows removal of spikes without biasing the results, treating edge effects, and missing measurements due to transmit pulses correctly. In the case of a square matrix, the matrices presented in the paper can be diagonalized using a discrete Fourier transform. This has several numerical advantages, but at the same time result in a less general equation. The frequency domain method cannot be used to deconvolve multiple transmitters simultaneously without making an assumption that the radar echoes are weak and the transmissions are orthogonal – in the case of meteor radars, this is rarely the case.

We have modified the text accordingly.

> Page 7885, equation (1). This is not well described. It says "our measurement equation is.." Measurement of what? What is m? Is it complex amplitudes? I assume so, but it is not stated. In addition, on lines 5-6, it says ".. target backscatter coefficient at a given range gate and coherence time i.." - is the "given range gate" represented

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by "r"? I assume so, but it is not specified.

We will clarify this, but you are essentially right. m is a vector of measurements.

> Page 7886, equation (7) - maybe I missed it, but Sigma does not seem to be defined. And the authors talk about the "standard formula for complex linear least-squares problems". Standard or not, a reference would still be a good idea.

Sigma is a covariance matrix. The equation of the generalized linear least-squares equation for a linear inverse problem with proper complex Gaussian errors. We will add a reference to this method and add discussion about the error covariance matrix.

> Figs. 4, 5 and 6 are very hard to read.. the layout could be a lot better. I suppose the idea is that since the paper is to be shown on the web, the reader can zoom in, but for someone who prefers to read a paper version, they are very hard to read. In addition, the labels seems funny - the ordinate reads "Counts x 30 mins" - which I suppose means "Counts at 30 min intervals" (maybe "Counts @ 30 min" might be better?)

We will improve the readability of the figure.

> Page 993, line 22 - the authors mention the range-Doppler ambiguity problem with pulsed systems. Yet I would expect the coded systems might start breaking down if the velocities get very large - which the authors agree is true at a later stage when they discuss measurement of head echoes (p. 7894, lines 17-18). I did not get a clear idea of the velocities needed before this problem sets in, but it does seem to be something that is worth discussing. The authors are clearly intent on "selling" their system, and take every opportunity to show the advantages of the CW system over the pulsed system, but not much is discussed the other way around - - some better level of "balance" would be worth thinking about.

We acknowledge that short pulses perform better than long pulses for high Doppler shift targets, if the radar target model does not take into account the Doppler shift. But we do not agree that this is a fundamental limitation of long coded pulses or coded cw

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measurements.

The fundamental measurement theoretical limitation is defined by how well determined the problem is. If the posteriori covariance matrix is singular or near singular, then the measurement cannot be made. This happens if there are less measurements than unknown parameters. The number of parameters needed depends in the number range and Doppler extent of the echoes. In the case of meteor trails and meteor head echoes, the number of parameters is very small, because the target is very localized in range and Doppler. I would not expect the coded method to break down with large Doppler shifts. If one wanted to support large Doppler shift, the bulk Doppler shift can be added into the model. This is what we discuss in the paper. This is discussed e.g., by Volz and Close (2012). We did not add the Doppler term in this paper, because it is unnecessary for meteor trail echoes. Coded long pulses are routinely used with high power large aperture radars for studies of meteor head echoes with radial velocities of up to 70 km/s (See e.g., Sulzer 2004). Coded CW measurements are routinely used to image asteroids with tens of km/s Doppler shifts (Ostro et.al., 1993). The asteroid measurements would probably be impossible if a pulsed radar were to be used, because there would be nowhere near the required radiated power.

Volz, Ryan, and Sigrid Close. "Inverse filtering of radar signals using compressed sensing with application to meteors." Radio Science 47.6 (2012).

Ostro, Steven J. "Planetary radar astronomy." Reviews of Modern Physics 65.4 (1993): 1235.

Sulzer, M. P. (2004). Meteoroid velocity distribution derived from head echo data collected at Arecibo during regular world day observations. Atmospheric Chemistry and Physics, 4(4), 947-954.

We have no intention of commercially "selling" meteor radars. We simply think that what we describe would result in a useful meteor radar system that is capable of increasing the number of meteors observed over a geographic region and allow more

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science to be extracted from a radar system.

The only advantage that we can think of for the traditional pulsed radar or uncoded CW radar transmissions is that the data can be analyzed with very primitive signal processing hardware and software. However, there are no more computational limitations that existed in the past.

In any case, we will make an attempt to change the wording to avoid overselling our contribution. We are not in any way affiliated with any commercial business selling meteor radars or meteor radar signal processing software. We want to avoid any perception that what we present in the paper is intended as sales material and we thank the referee for pointing this out. The goal is to try to objectively describe our method.

> I could not see where the authors have discussed the height resolution of the CW system.

In our measurements, the height resolution is 1.5 km due to the bandwidth used (100 kHz). We will make more effort to talk about this in our paper. By adjusting the transmit bandwidth, the range resolution can be increased. This applies to pulsed and continuous transmissions in exactly the same way. This is a well known principle of radars, and we feel that this does not belong to this paper.

The paper very long as it is. We feel that adding more detailed plots would better fit in a future paper. This paper is focused on the overall concept and the signal processing principles.

> Another point of note is that because the new design relies more critically on low elevations, (or at least I assume it does) then it is also potentially more susceptible to ground-level interference. I am not sure if this is important, but some discussion might be warranted.

One advantage of the method that we present in our paper is that it is more tolerant to radio interference. This is explicitly mentioned. We will add more text regarding this.

> I did not see much detail about the specifics concerning the codes used - not even a table of

The code is random phases generated with a pseudorandom number generator. The random seed used for these measurements is 1. We plan to use random seed 2 and 3 when we build the second and third transmitter. Both of these are not very important, as nearly any pseudorandom code is close to optimal in terms of a posteriori covariance matrix structure. This is very much different from the case of a short pulsed phase code, where the specific details of the code are important. We will again add more text on this.

> p 7895, lines 19 and 20 - "However, with careful planning and surveying, these issues are not prohibitive". This is not proven and is speculation - which is not advisable in a scientific paper.

We will address this point.

We will address the typographic and grammar issues pointed out by the referee. We will be very careful to avoid irony in the next version of our paper.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 7879, 2015.