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Geoscientific Instrumentation Methods and Data Systems Discussions

Interactive comment on "Validation of the k-filtering technique for a signal composed of random phase plane waves and non-random coherent structures" *by* O. W. Roberts et al.

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We thank the referee for his/her comments. We have revised our paper in respond to the comments. In the following, we will list his/her comments and give our response in bold. All the changes in the manuscript are in bold.

The k-filtering technique has been introduced in space plasmas at the occasion of the PhD thesis of J.L. Pincon (1989), in preparation of the Cluster project (see also Pinçon and Lefeuvre, 1991). It has been improved and practically used for studies of physical interest a few years after the successful launch of this project (Sahraoui et al, 2003)

and at many occasions since this date. Similar techniques, based on the same mathematics (Capon method), like the so-called "Wave Telescope technique" (Neubauer and Glassmeier, 1990, Glassmeier, 2001) have been developed and used simultaneously. They are quite equivalent. Beyond the tests done in the original works, few works have been devoted since then to checking the validity of the method although, when using it extensively, some spurious results may justify some wariness about its use.

The present paper by Roberts et al. is therefore quite welcome in this context. Among the necessary conditions to be verified in the data to make the results valid, it investigates the possible errors due to the signal "coherence". This follows a remark of the same authors (Roberts et al., 2013), who suspected that too much coherence could invalidate the results. The conclusions are rather reassuring since no major errors seem to come from this point in general. I would nevertheless ask for some changes in the paper before declaring it suitable for publication. Some clarifications are needed to make the paper convincing. General:

The questions related to the signal coherence are presented as evident conditions for the k-filtering validity. Even in the abstract, it is said that the technique requires "that the fluctuations can be described by a superposition of plane waves with random phases". I am ready to accept that this is true, but where does this necessity of random waves come from? I don't remember any reference telling it clearly. I just remember one analytical example in Tjulin et al. (2005), which seems to plead in this sense, but as far as I remember, the original mathematical papers don't even mention it. Comments and references are strongly needed about this point.

We agree a comment is added to discuss this theoretically on page 4 line 262."The requirement of the k-filtering technique that waves have random phases may be illustrated here. For instance, two waves similar to those in Eq. (7) are given by $\sin(\mathbf{k}_1 \cdot \mathbf{r} - \omega_1 t - \phi_1)$ **and** $\sin(\mathbf{k}_2 \cdot \mathbf{r} - \omega_1 t - \phi_2)$**, the combined wave field will be**

$$2\sin\left[\frac{(\mathbf{k}_1+\mathbf{k}_2)\cdot\mathbf{r}-2\omega_1 t-(\phi_1+\phi_2)}{2}\right]\cos\left[\frac{(\mathbf{k}_1-\mathbf{k}_2)-(\phi_1-\phi_2)}{2}\right] \quad (1)$$

If $\phi_1 - \phi_2$ is fixed (the two waves are coherent), then the above equation basically produce a new wave field with a wavevector at $(\mathbf{k}_1 + \mathbf{k}_2)/2$ at the same frequency ω_1, very different from the original two waves. However, if $\phi_1 - \phi_2$ is randomised (at each wave period), the ensemble average of the k-filtering technique will recover the two original random phased waves."

And references are given in the following sentences on page 4 line 289: "This is due to the requirement of random phases, since two or more waves (of the same frequency) with fixed phase differences will superpose which will result in a periodic resultant amplitude. In previous tests of the method the need for incoherent signals is pointed out Capon 1969,Pincon 1991,Motschmann 1998"

We have also added a sentence referring to Pincon 1991 on page 2 line 99 "Here we quote the comments of Pincon & Lefeuvre (1991) when the k-filtering was first designed: "Measurements of the electric and/or magnetic field in space plasmas commonly show fluctuations in time and/or space on all observed scales; such fluctuations are mostly random in appearance and therefore are considered as turbulent phenomena."

The notions of "random phase", "non random phase", "coherent structures" used in the tests should be defined and discussed much more completely from the very beginning of the paper.

We agree we have appended the text in the updated manuscript on page 1 line 61 "Here we use the terms random phase plane waves to refer to waves where the phase is randomised in each waveperiod, and non-random plane waves to refer to the case where the phase is fixed." and in later on page 2 line 88 "In relation to the signal we use the term coherent structure to mean an intermittent magnetic

field signature which is characterised by a radius and a mean spacing between them."

The synthetic signal is here supposed to be the superposition of several (eight) plane waves. Each of these individual waves is said to be "random phase" or "non random phase". This would of course have no sense if these individual waves were monochromatic since a monochromatic wave has a single phase ϕ. Reading the paper in more details, one understands that each individual wave is actually not monochromatic: it has indeed a given central wave vector ki, but a phase which varies. This point is not precisely described. Is it a space variation: $\phi(r)$? Or a temporal one: $\phi(t)$? In the so-called "random phase" case, it is said that the phase variations consist in random jumps at "each period". Why jumps and not continuous variations? If the variations of the phase have the same period as the central one (related to ki if the variation is supposed spatial, i.e. if $\phi = \phi(r)$), this should change considerably the spectrum and give a quite broadened line.

We agree that this is not clear in the manuscript and have added some discussion of the phase on page 3 line 206:The randomisation process is a 'jump' in the phase and not a continuous variation as a function of time $\phi(t)$. A continuous variation in the phase is not expected over short timescales. For a series of unrelated wave trains made up of wave packets (or a number of coherent structures) we would expect the phase to jump with each new wave packet in the train. We do not investigate changes in phase in the spatial domain, since these are assumed to be small on the scales of spacecraft separation.

If the signal is simply Fourier transformed, what does the spectrum looks like? It should be shown in a Figure. Since the goal is to compare the result of the k-filtering technique with what is expected, I think that the first step is to calculate what is expected. Why not working directly on the phases in Fourier space, keeping a controlled profile for the spectrum line? Additional question, important in the context of this paper: is this kind of signal supposed to fit correctly what can be observed in the data? Comments are really needed about it.

To make it clear what we expect as a result from the analysis we have added the sentence on page 4 line 282 “Note mathematically, the power of an individual wave is a singular point located at the crosses.”

We also addressed the referee’s concern by adding the following text to the second last paragraph in the introduction page 2 line 99: Here we quote the comments of Pincon 1991 when the k-filtering was first designed: “Measurements of the electric and/or magnetic field in space plasmas commonly show fluctuations in time and/or space on all observed scales; such fluctuations are mostly random in appearance and therefore are considered as turbulent phenomena.” And page 2 line 109 (including short lived coherent wave packets). The paper is also motivated by studies that discrete waves have been indeed observed in the solar wind Jian2009,Jian2010 and discrete waves can survive in a turbulent environment for a limited time at least theoretically Ghosh2009.

The presence or not of “coherent structures” in the signal is certainly not decoupled from the previous question, but it is studied independently in the paper. It seems for me that it is just another way of constructing synthetic signals that are more or less coherent. Is it? This second method consists in taking monochromatic waves and tapering them in a series of windows (squared or Gaussian, it is shown to give no major difference). The windows are just distributed with separations and widths that are chosen randomly, with Poisson statistics, around given mean values. The mean values are indicated in the results, but I don’t see any trace of deviations from this mean value in Figure 2: the structures seem to be arranged as in a crystal. What is the value of this mean square deviation? It should be indicated. And the role of this deviation on the results should be investigated. Furthermore, if the positions are random and the phases constant in each window with respect to this position, I means, I think, that each structure is coherent but that the different structures are incoherent with respect to each other. Was it the goal? It should be indicated.

For a Poisson distribution the varience is equal to the mean, for clarity we have stated this in the caption. With regards to the comment that the structures appear in a lattice, to answer another point brought up we have plotted a case where $d \gg \lambda$. We hope that the differences will be more apparent in Fig. 2 panel c. For the final point, we thank the reviewer for pointing this out, indeed this was one of the goals of this work and it should be stated more clearly, which has been done on page 2 line 93. This makes it like a coherent wave packet. However with respect to other structures they are likely to be incoherent, and a key goal of this work is to determine the effect of coherency on the determination of the power of waves or structures when the k-filtering technique is adopted” Note that this point is also relevant to one below which is also bold and italicised.

The coherent structures are said at several occasions (beginning in the abstract) to be “advected by the flow”. I don’t understand at all this claim. First, I don’t understand why it should be true: why do these structures could not be of magnetosonic nature? If they are Alfvenic, they can be advected by the flow only if the flow is perpendicular to the static magnetic field. Assuming that they don’t propagate with respect to the flow could also amount to considering that they are all of “mirror type” Is it the case? Anyway, I don’t understand either why such a claim is necessary in the demonstration. What would be changed if the structures were propagating? What is important in the signal processing is the velocity of the structure with respect to the spacecraft. The speed of the flow has no specific interest at this stage, before identifying the modes.

We have changed the text to mention that some structures may propagate in the plasma frame with a certain velocity. Typically this is not an issue for the solar wind since the bulk velocity is much larger than the Alfven speed, however we accept that this may not be the case in other plasma environments and have changed the text accordingly at page 2 line 80. “or those propagate with a small velocity v ÂńVsw in the plasma rest frame (e.g dipolar Alfv′en vortices Petviashvili Pokhotelov (1985, 1992)).”

Details.

Line 46: performed-performed **This is corrected in the revised manuscript**

Line 61. When summarizing the history of the k-filtering applications in space physics, I think that presenting the paper Eastwood et al. (2003) as the first paper of this kind is not fully correct. This paper is actually very rarely cited (even by its authors) and I just discovered it myself here. Before that, there was already a paper of "first results" about the wave telescope technique in Annales Geophysicae (2001) and, concerning the k-filtering proper, Sahraoui got his more famous paper accepted during the same year 2003 in JGR (while submitted in mid-2002), the main results of this paper having already been presented in several international conferences since 2000. The paper Sahraoui et al., 2004 indeed completes this first –pioneer- one, and can be cited also, but it has certainly not the same anteriority as this one.

Glassmeier 2001, and Sahraoui 2003 are now cited in the revised manuscript.

Line 64. The reference "Eastwood and Balogh" is incorrect. It should be Eastwood et al. (The bibliography is not correct either: many authors are missing).

This reference has been corrected in the revised manuscript

Line 97. The first step of the k-filtering technique is indeed a temporal (windowed) Fourier transform. I would like to read here a comment on the fact that this initial step, when investigating real signals involving trends, may spoil the determination of the phases of the signals and therefore alter the quality of the results.

We thank the reviewer for this comment, indeed it is an important limitation, which is sometimes neglected. The text is now updated in the revised manuscript and a few sentences added at page 2 line 143. "It also should be noted that a key limitation due to the use of a windowed Fourier transform, is the requirement that the time series are at least weakly stationary so the goal of achieving ensemble average, a key requirement of the k-filtering method, can be

realised by using time averages. This is an important restriction when selecting intervals for analysis. Intervals are chosen such that the magnetic field time series are devout of shocks, discontinuities or any trends so that the quality of the results are not impaired."

Lines 151 to 163. This long explanation is unclear (and maybe not very useful as it stands: I am ready to trust the authors when they say that they are able to construct a B-field which is divergence-free). Otherwise, it would be useful to better explain what is the z direction, what is the direction of the static magnetic field and what is the polarization supposed for the waves (are they Alfvenic?)

To make the paper clearer, on page 3 line 182, we have changed B to δB. Our discussion is rather general and we are not limited to Alfven waves. The static field can be in any direction. For simplicity, we have defined the z component of the wavevector to be zero.

Line 236: a closing parenthesis is missing. **This is corrected in the revised manuscript**

Figure 2. It would be useful to show what the signals look like in the different cases investigated, in particular: $\lambda >> d$ and $\lambda << d$.

We agree. We have revised figure 2 to include a plot where lambda«d. hopefully the difference between structures radii, and spacing will be more apparent to the reader with the inclusion of this panel. Note that this point is related to a previous point.

Line 255. The claim "This criteria is well satisfied in space plasmas" seems much too strong. The example given (Alexandrova et al., 2006 and 2008) just shows that this criteria seems to be sometimes satisfied in some contexts of space physics. . .

We agree and have toned down this statement in the revised manuscript, now reads "often satisfied in some space plasmas"

In conclusion, the paper should be acceptable for publication if the authors agree with my different remarks and can correct the text accordingly. I would also appreciate if the conclusion could appear clearer and in better agreement with the initial claim that random phases are necessary for the k-filtering technique. The reader may remain skeptical: finally, was it true or not?

We agree and have expanded the conclusion in the revised manuscript on page 8 line 360 "We have demonstrated that waves at any frequency are required to be incoherent, since the k-filtering technique is unable to resolve two waves with a fixed phase difference. A series of randomly located coherent structures arranged with characteristic mean radius and spacing can be assumed to be incoherent and can be resolved by the k-filtering technique. While they are 'coherent' within their radius they are incoherent with respect to each other due to their random locations. For turbulent environments such as the solar wind, where waves and structures are both present the incoherency of the signal is likely to be satisfied, and the application of the technique justified."

Interactive comment on Geosci. Instrum. Method. Data Syst. Discuss., 4, 435, 2014.