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Interactive Comment

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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Review on the paper "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.

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)

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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. * 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. * 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 phi. Reading

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the paper in more details, one understands that each individual wave is actually not monochromatic: it has indeed a given central wave vector k_i, 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 k_i if the variation is supposed spatial, i.e. if phi=phi(r)), this should change considerably the spectrum and give a quite broadened line. 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. * 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. * 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

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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.

Details. Line 46: perfomred –> performed 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. 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). 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. 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?) Line 236: a closing parenthesis is missing. 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. 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. . .

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?

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