Reviewer 1:

- Comments on the manuscript by Leonhard Schulz et al. entitled "The m-Dimensional Spatial Nyquist Limit Using the Wave Telescope for Larger Numbers of Spacecraft"

Correction to the first author name: It's Leonard instead of Leonhard.

General comments

- The study is concerned with spatial aliasing phenomena in spacecraft array wave vector estimation techniques when more than the minimum number of probes needed to identify all wave vector components are available. To generalize one-dimensional approaches employed in studies of the temporal aliasing effect for unevenly sampled time series, the authors apply concepts from number theory to determine a suitable elementary cell in the lattice of reciprocal vectors. While results are shown for the wave telescope technique, the underlying geometrical arguments are of general nature so that the approach should be applicable also to other wave vector estimators.

  The overall quality of the manuscript is convincing, the logic is sound, the organization is clear, and the figures are of general publication standards. In the core sections 3.2 an 3.3, the development of the mathematics is rigorous. In sections 1, 2, and 3.1, some motivational arguments are confusing and need further clarification.

The authors thank the reviewer for the general, positive assessment of the manuscript. Specific comments below will be addressed one by one in the following.

Specific comments

- Since the focus of this study is on the phenomenon of (spatial) aliasing, the authors could clarify its conceptual basis in more detail: How does the ambiguity called aliasing expresses itself, and what are the algebraic conditions? In the context of spacecraft array wave analysis, without reference to a specific estimator, one of the earliest considerations was offered by Dunlop et al. (1988) who based their argument on wave vector ambiguity in the representation of the phase, see also the presentation by Chanteur (1998) using reciprocal vectors based on mesocentric position vectors. The current setup of the paper, starting the presentation with a detailed review of the wave telescope technique in section 2.1, suggests that the spatial aliasing analysis is restricted to a specific method while it is more general.

Regarding the first part of this comment concerning aliasing: We agree that the topic of aliasing, which is elementary for the study, has been introduced too briefly. Therefore, we want to add additional explanation and formulae to the beginning of section 2.2. Please also refer to the answer made in the specific comments on line 114-120 below for more details on proposed changes.

Regarding the second part of the comment: Indeed the spatial aliasing and the findings of this study concerning it are not restricted to the wave telescope. This is mentioned on several occasions, e.g. in the abstract and conclusions. However, we agree that this fundamental aspect should be emphasized on other occasions as well, especially in section 2. Therefore, we would like to add an introductory paragraph to the section prior to the detailed review of the wave telescope technique. As the current study is focused on the wave telescope technique, the assessment of other estimators goes beyond the scope of the paper. Thus, we will explain the study's sole focus on the wave telescope but emphasize the general nature of spatial aliasing. Additionally, we want to cite the two suggested references in section 2.2 in the sentence (lines 121-123) "The concept of aliasing and frequency domain periodicity can be directly transferred from frequency space to k-space in general (Dunlop et al., 1988; Chanteur, 1998), in particular for the wave telescope (Neubauer and Glassmeier, 1990; Pinçon and Motschmann, 1998; Glassmeier et al., 2001; Narita and Glassmeier, 2009)." We think this describes exactly what the reviewer is aiming at as well.

- Lines 114-120: With the addition of a conceptual characterization of spatial aliasing as suggested above, it may be worthwhile rewriting the beginning of section 2.2. In its present form, the first paragraph on Fourier transformation appears incomplete and partially incorrect (see below under "Technical corrections") while its main (and only?) purpose seems to be the introduction of aliasing and the Nyquist limit (Nyquist frequency, or critical frequency) in the context of regular (evenly sampled) time series. Furthermore, as textbooks on (array) signal processing (e.g., Bendat and Piersol, 1971; Pillai, 1989) and also the cited literature on the wave telescope or k-filtering technique (Pincon and Lefeuvre, 1991; Pincon and Motschmann, 1998) show, power spectral density (PSD) estimation can be accomplished by a range of different methods, with Fourier techniques only a special (and, due to the availability of the Fast Fourier Transform FFT for evenly sampled time series, a numerically most efficient) approach. Even in the key 1D aliasing references mentioned in this paper, PSD estimation takes center stage using a Bayesian perspective (Bretthorst, 2001), through a spectral window function, convolved with the true spectrum to yield the spectrum obtained after irregular sampling (Eyer and Bartholdi, 1999), or generalized periodogram estimation (Mignard, 2005).

Thank you for this important comment. As said above, we agree that the explanation of aliasing and its implications for time and frequency space should be explained more. Nevertheless the authors think that discrete Fourier transform (DFT) serves as a good explanatory example when introducing aliasing. The DFT is, as the reviewer noted, due to the availability of FFT a numerically most efficient transform and it is the most widely used and very basic transform when wanting to acquire the frequency domain. Additionally, the introduction of the DFT not only serves as an introduction for aliasing and the Nyquist limit but also as a precursor for the later introduction of the technique of zero adding for irregularly sampled time series, explained in section 3.1. Also DFT is needed for the preparatory work on time series when wanting to apply the wave telescope, as the time series b(t,r) first needs to be transformed to a frequency series b(w,r) (see equation 1). Therefore, the authors would like to leave the definition of the DFT in, but want to incorporate it into a more expanded explanation of aliasing and the Nyquist limit. Additionally, the authors want to add the backwards transform and remove one false claim made as requested in the technical corrections.

Considering the reviewers remarks that Fourier transform is only a special way to estimate a PSD: We agree to this remark. Combined with the remark of another reviewer, we want to replace the statement that the WT is a Fourier transform estimator by “With a plane wave model, the wave telescope can be interpreted as a power spectrum estimator substituting power spectral density estimation via a spatial Fourier transform (Motschmann et al., 1996; Plaschke et al., 2008; Narita and Glassmeier, 2009).” (line 92-93). Throughout the paper, there are numerous occasions, where the claim was made that the wave telescope estimates a Fourier transform. We want to replace this by “power spectrum estimation” as this describes more precisely what the wave telescope is doing. Additionally, we want to remark in the study (in the beginning of section 2.2.) that “In order to yield a spectrum, one normally estimates the power spectral density (PSD), which can be accomplished by a range of different methods. Nevertheless, here we want to focus on Fourier transform as an illustrative example...”.

- Lines 149-153, "However, as an important remark, ... Brillouin zone ": Since this is labeled as an important remark, it seems worthwhile explaining it in more detail, and what exactly is contrary to comments made in earlier papers.

We understand that this statement has to be explained in more detail. In our paper, we adopt the definition of a unit cell from crystallography, for example as presented in (Souvignier, International Tables for Crystallography (2016). Vol. A, Section 1.3.2, pp. 22–28.). They present 2 differentiations of unit cells, one the one hand the “primitive unit cell”, which is synonymous to the parallelepiped construction of the periodic cell in our paper and in previous wave telescope publications. On the other hand there is the “Voronoi cell” equal to the first Brillouin zone, which is defined by a Wigner-Seitz cell approach (constructing the perpendicular bisector at half the distance to each lattice point and find the smallest volume of the intersecting bisectors lines or planes, depending on the number of dimensions). This cell is the locus of points in space that are closer to the origin lattice point than to any of the other lattice points (Souvignier, 2016). Only these point in reciprocal space constitute the points at which no aliasing is taking place. Thus it is the only unit cell within which there are no aliased points contrary to the parallelepiped cell.

All this is in contrast to specific statements in papers on the wave telescope:

Neubauer and Glassmeier 1990: “The subvolume closest to k= 0 can be described by (equation 12a)” -> not correct, as the subvolume closest to k=0 would be the 1st Brillouin zone and not the primitive unit cell calculated in their equation 12a.

Narita and Glassmeier 2009: “Reciprocal vectors are not always mutually orthogonal but have in general various angles. In such a case the Brillouin zone forms a diamond-shaped cell in the wave vector domain.” → This is not the 1st Brillouin zone, but the parallelepiped cell.

Narita et al 2022: “Therefore, it is conventional and safer to search for wavevectors within the first Brillouin zone spanned by the reciprocal vectors […]” → The Brillouin zone is not spanned by the reciprocal vectors, this again is the parallelepiped cell.

We want to replace the current wording with a compacted version of the above, stressing the difference between the first Brillouin zone and the primitive unit cell. As the concept of these two unit cells is well accepted in solid state physics and crystallography and the choosing of the right unit cell is important to the problem of aliasing, we think it is beneficial to stress this terminological differentiation to avoid future confusion. In addition, we have noted that in our study, further distinction is needed of “spatial Nyquist limit” and “periodic cell”. So far, this has not been clear throughout the paper and we want to change especially Figure captions to make this clearer, always bearing in mind the explanations made in the above paragraph.

- Lines 163-174: Please consider to rewrite the paragraph or at least amend it with additional explanations and clarifications. Apparently, the text is meant to introduce the aliasing problem for irregular sampling using the Fourier transform of time series, but the presentation remains unclear, and may be partially incorrect (see below under "Technical corrections").

Please refer to the answer to the respective comment under “Technical corrections”.

- Line 249, "The regular sampling point case ...": A brief characterization of the underlying three-S/C configuration for 2D wave vector estimation should be added. Does the term refer to an equilateral triangle, or to any configuration of three S/C in two dimensions?

The terminology and difference between regular and irregular sampling points is explained in section 3.1 on the 1D case. Nevertheless, we understand from the reviewer’s comment, that there still is some ambiguity regarding the use of “regular sampling points”. Thus we want to reword the sentence in question to better show that regular sampling points refers to the number of spacecraft rather than their configuration. We also want to add that the regular sampled 2D case is only represented by a three spacecraft configuration not forming a line in space (thus covering all 2 spatial dimensions). Additionally, we will explain that the specific spacecraft configuration used as an example for such a regular sampling points case, resulting in the spectrum of Figure 5, is indeed an equilateral triangle, here.

- Line 253, "Different deformations of the outer region of the maxima result from numerical artefacts": Please clarify. Are the numerical artefacts related to the necessarily finite representation of numbers in computer memory (machine accuracy)?

We apologize for this incorrect statement, this has been overlooked in final checking of the manuscript and the sentence will be removed.

- Line 256, "2D situation and four irregularly positioned S/C": How would a configuration of four regularly positioned S/C look like? Or does the term "irregular in the 2D context simply mean "more than three S/C"?

As said above (comment on line 249), the terms regular and irregular indeed refer to the number of spacecraft. In the paper, we want to explain this in more detail in the above paragraph (see the response to comment on line 249), thus we argue that at this point in the text it should be clear to the reader what is meant by irregular in the context of a 2D situation.

- Line 388, "for all regularly spaced subsets of sampling points": Please clarify. Here, in 2D with 4 S/C, does "all regularly spaced subsets" refer to all subsets comprising three S/C? How would the procedure look like with 5 S/C (and still in 2D)?

Again, we would like to refer to the response to the comment on line 249. There, we want to explain in more detail what is meant by regular and irregular spaced sampling points. Thus, this should then be clearer to the reader. However, to be even more precise, we would like to add that a regular spaced subset of sampling points is constituted by a subset of m+1 sampling points that span the m spatial dimensions.

Technical corrections

- Lines 114-120, "at these frequencies the discrete FT equals the values of the continuous FT", Eq. (9): The discrete Fourier Transform (FT) can be defined in different ways, depending on the purpose and the context (e.g., Eriksson, 1998). In the present manuscript, Eq. (9) yields Fourier analysis (forward FT). To clarify the purpose and the normalization used here, consider adding also the formula for Fourier synthesis (backward FT). The statement "the discrete FT equals the values of the continuous FT" does not seem correct for general time series as the continuous FT is based on the continuous signal b(t) and thus uses information also in between the sampled times. If the statement is supposed to be kept, the definition of the corresponding continuous FT needs to be added, and the reasoning explained.

Thank you for this comment. The authors agree that the adding of the back transform (synthesis) of the FT adds value and will integrate this. Also we agree that the statement "the discrete FT equals the values of the continuous FT" is indeed incorrect. The authors apologize for this error. Please refer to the above answer to the specific comment on lines 114-120 for additional changes the authors want to implement regarding this paragraph.

- Lines 163-174: As in the previous comment on Eq. (9), the formula in Eq. (16) needs clarification and additional explanations. Apparently, it is based on a FT formula (Fourier analysis) for nonuniform sampling which is amended by the addition (insertion) of zeros, but then the corresponding formula for (nonuniform) Fourier synthesis should be included. The Fourier transform of the time series after "zero adding" may formally produce the same output, but it remains unclear in which sense the result gives the coefficients of a harmonic series expansion (and in which form exactly), and how it can be interpreted as a starting point for PSD estimation. It appears as if the paragraph is supposed to provide a shortcut to the spectral window function approach presented by Eyer and Bartholdi (1999), but there the argument is more complete, explaining the connection to the true power spectrum through a convolution. Note that the concept of "zero adding" (inserting zeros into an irregular time series) differs from the popular technique of "zero padding", often used in the time series context to amend an evenly (!) sampled time series with zeros to arrive at a sample length that takes advantage of the computational efficient FFT.

Thank you for this remark. As done before, we want to add the backwards transform of the presented DFT subject to “zero-adding”. We are aware that we describe a different technique than zero-padding, but we now understand that we should explain this also to the reader and thus want to add a sentence like ”…not to be confused with zero-padding…”.

Considering the remark that “The Fourier transform of the time series after "zero adding" may formally produce the same output, but it remains unclear in which sense the result gives the coefficients of a harmonic series expansion (and in which form exactly), and how it can be interpreted as a starting point for PSD estimation.” We would like to argue that the concept of zero adding is very illustrative in showing how the effective sampling distance respectively the effective Nyquist frequency comes into being. Although the DFT presented in formula (16) has to be transformed to a more complex form using different running indices for the sine and cosine transform part of DFT as presented in Bretthorst (2001) - we would like to keep this formula as is. Bretthorst himself introduces the very explanatory concept of zero adding (although he does not use this terminology) to explain the effective sampling distances (in his work largest effective dwell time) for the DFT. It is also mentioned in VanderPlas (2018) that the concepts shown in Eyer & Bartoldi (1999) can be understood as an additional window with zeros at some points added to a uniform dataset. While he approaches the problem from the other side, the interpretation is synonymous to the concept of zero-adding. Thus we think it is sufficient to rewrite the paragraph in question emphasizing that we use the DFT formula here just as an illustrative example, mentioning that the presented formula can’t be seen as a transform of DFT to non-uniform sampling as this is more complex (and refer to Bretthorst (2001)).

References

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Reviewer 2:

- This interesting study is original, innovative and well-written. I only have a minor remark to add:

  The authors may consider to refer in their study to a recently published paper on nine-spacecraft HelioSwarm observatory (Broeren et al., Frontiers 2021).

  Refererence

  Broeren T, Klein KG, TenBarge J M, Dors I, Roberts OW and Verscharen D (2021) Magnetic Field Reconstruction for a Realistic Multi-Point, Multi-Scale

  Spacecraft Observatory. Front. Astron. Space Sci. 8:727076. doi: 10.3389/fspas.2021.727076

The authors want to thank the reviewer for the general positive review. The study mentioned considers implications of higher numbers of spacecraft to determine a better reconstruction of the magnetic field and thus current densities as e.g. the Curlometer method does. It does not refer to the determination of k-space energy spectra. Thus we think this study is not directly connected to what is done in our study and referencing it does not provide needed information for the reader. Also regard that a reference to the nine-spacecraft HelioSwarm mission is already given in the introduction.

However, we are aware of a very recent study that was not published when our study was submitted. Namely (Broeren, T. and Klein, K. G.: Data-driven Uncertainty Quantification of the Wave Telescope Technique: General Equations and Demonstration Using HelioSwarm, The Astrophysical Journal Supplement Series, 266, 12, https://doi.org/10.3847/1538-4365/acc6c7, 2023) show extensive statistical error quantifications of the wave telescope technique. We want to add a reference to this publication in section 5 (the conclusions) stating a possible combination of both studies to precisely quantify the wave telescope's capabilities for configurations of more than 4 S/C.

Reviewer 3:

- The study reveals interesting features of high resolution wavenumber power spectra for non-uniform sparse spatial sampling. Previous results from time series analysis with non-uniform sampling are shown to be transferable to the Capon beamforming estimator, and are generalized to higher spatial dimensions. Before final publication I would suggest minor improvements and clarifications:

We want to thank the reviewer for the assessment of the manuscript. Specific comments below will be addressed one by one in the following.

- The conclusions could state more precisely what is new in this study. As far as I can see this would perhaps be
    - by modeling the study verifies that for non-uniform sampling also the wave telescope technique shows the same aliasing features as in time/frequency the ordinary DFT (Bretthorst, 2001) and the Lomb-Scargle periodogram (VanderPlas, 2018).
    - in higher dimensions the spectral structures repeat themselves in periodic cells in k space because of aliasing. Within a cell non-uniform sampling can lead to side maxima.

Thank you for this comment. We agree that the key findings of the study haven't been prominently represented in section 5. Therefore, we would like to reword especially the first paragraph of section 5, presenting the spatial Nyquist limit as the main finding of the study while including the 2 above made points and referencing the their referenced literature.

- Lines 92-93: "With a plane wave model, the wave telescope technique can be interpreted as a Fourier transform estimator". Capon describes his method as "... estimation of the frequency-wavenumber spectrum." Which is not the same as a Fourier transform of the signal. Particularly the output of the technique does not contain an estimation of the phase of the signal, only of the power, equations (6) and (7). This is ok, normally one is mainly interested in the power, not so much in the phases. But to me the statement "Fourier transform estimator" seems not accurate: the Fourier transform is used to derive the method, but a spatial DFT is then not explicitelyly performed in the algorithm?

Thank you for this comment and bringing this to our attention. We agree that the term “Fourier transform estimator” does not correctly describe what the wave telescope is doing. However, for the plane wave assumption, we think the term “power spectrum estimator substituting power spectral density estimation via a spatial Fourier transform” describes the wave telescope technique best. Here, we think it is important to directly mention that the wave telescope is a substitute for the spatial Fourier transform as is described in the cited literature. As there are also other occasions where the wave telescope is described as a Fourier transform estimator, we will replace those by statements in the sense of the above (e.g. the abstract).

- Similarly, lines 409-411, "As the wave telescope acts as an estimator for a spatial Fourier transform in multiple dimensions, the results from this study can be directly transferred to different fields of research that are using multidimensional Fourier analysis or its estimators with irregular sampling points." I'm skeptical whether the wave telescope should be termed an "... estimator for a spatial Fourier transform ...". Replacing this by "... estimator of the wavenumber power spectrum ... " would be more accurate?

As said in the above answer, we agree and would like to replace this by “power spectrum estimator substituting spatial Fourier transform PSD estimation in multiple dimensions”

- Lines 109-110: "... that is estimating the spectral power P (k) for any given frequency." --> perhaps clearer would be "... that is estimating the spectral power P (k) for a suitable range of wave vectors and at any given frequency."

Thank you for this remark, we agree that this makes the sentence clearer and want to adapt it. In accordance with the two above answers, we want to change the beginning of the sentence as well to “…we focus on the limits for estimating the spatial power spectrum…”

- Lines 125-127: "The wave telescope, however, estimates a spatial Fourier transform of the complex data set B(ω) (Eq. 1). Thus, the spatial power spectral density obtained is not symmetric around 0." The complex B(ω) does not seem to be the origin of the generally asymmetric spatial wave power spectrum? In equations (2) the data covariance matrix is an ensemble average of B(ω) B†(ω) which is real. To prepare the B measurements for equation (1) one could instead of a Fourier transform, for example, use a cosine transform or a real-valued Morlet wavelet transform which both would give real B(ω). I suspect that still the P(ω, k) came out asymmetric in k?

In following the authors would like to confirm the statements made in the study regarding the specific remarks and questions posed here. Precisely, we would like to confirm that the complex B(ω) is indeed the cause of asymmetry of the spatial power spectrum. For this, please review the attached pdf. There, we show plots with a k different from 0 (k=0.05). Normal wave telescope analysis (equal to the one made in Figure 3) shows that as said in line 214-215 “A different, larger wave number would result in a shift of the maxima in k-space, but does not change the periodicity itself.”

The second figure in the pdf shows the exact same power spectrum but now only the real part of B(ω) has been subject to the wave telescope analysis. Clearly, the spectrum is now symmetric around 0. This shows that the imaginary part of B(ω) is crucial to obtain an asymmetric spectrum.

This also becomes clear when considering the suggested cosine transform instead of Fourier transform. This is shown in the third Figure of the attached pdf. Again, a symmetric spectrum around 0 is visible (contrary to the assumption of the reviewer). The different appearance of the cosine transform wave telescope analysis discernible is not topic of this study and thus not discussed further.

All this shows that indeed the complex nature of B(ω) due to the Fourier transform in time is the reason for the asymmetric spectrum. Although as correctly stated by the reviewer the data covariance matrix M computed by taking an ensemble average over B(ω) B†(ω) is real, the antisymmetric, imaginary parts of B(ω) contain indispensable information, that is lost when only considering the real part of Fourier transform or not even regarded when using a “symmetric” transform such as the cosine transform. Thus, and in order to not overload the reader with information, the authors would like to keep the part in question as is.

- Figure 4 shows a symmetric spectrum over k, probably because the artificial signal has only a single component at k≈0. It might be illustrative to see also a spectrum with a component k away from 0. Probably it would be asymmetric.

We agree with the reviewer that such a spectrum would be asymmetric around 0. Please refer to the above reply and the attached pdf to see proof of that. We agree that adding such a plot is a good example that will aid in understandability. Therefore we propose to change Figure 4 to have a panel a and b. In a, the original Figure will be shown, while panel b will show the same plot that is different only in the fact that the input wave vector is changed to k=0.25 km^-1. We would like to reference this plot in line 214-215 “A different, larger wave number would result in a shift of the maxima in k-space, but does not change the periodicity itself.” In order to retain text flow and fit the new panel b nicely into the section, some sentences in section 3.1 will have to be changed.

- Lines 128-129: "This periodic cell is spanned by a set of skewed linearly independent basis vectors ki." There are degenerate cases: For example, for m=2 three of the spacecraft could be on a single line. Then obviously the basis vectors ki are not linearly independent. Obviously such cases need to be excluded.

Thank you for this remark, we will write a sentence stressing the exclusion of such degenerate cases.

- Line 133: "w. l. o. g." stands for "without loss of generality", as Google would reveal to the reader. But I would recommend to spell out the expression, which is not so common in a journal like EGUsphere.

As we adapt the sentence below (refer to the answer to the below comment), this sentence would be removed.

- Less cryptic would be something like "Here, the spacecraft translation vectors di are the difference between position vectors ri of spacecraft nr i and an arbitrarily chosen reference spacecraft 1 (i.e. the set of basis vectors ki is not unique):"

Thank you for this comment, we will adapt this sentence in order to assist understandability.

- Section 3 The Nyquist Limit for Irregular Spaced Sampling Points 3.1 1D case
  A relatively popular method to handle irregularly spaced sampling points is Lomb-Scargle. E.g. VanderPlas (2018) recently discussed the method in some detail. He describes an effective Nyquist limit which seems to be determined as in this manuscript for the 1d case. Eyer and Bartholdi (1999) are referenced as the original peer-reviewed article discussing the problem, which is earlier than Bretthorst (2001).

Thank you very much for mentioning the very illustrative study by VanderPlas. Although the work of Eyer and Bartholdi is earlier than the one of Bretthorst, the latter in our view gives a more extensive and illustrative explanation of the matter. This is the reason why Bretthorst (2001) is the first study mentioned in section 3.1. However, Eyer and Bartholdi's study from 1999 is mentioned as well at sensible position in section 3.1.

- Figure 2 "The synthetic plane wave number used for modelling the artificial signal is k = 10^−10 km−1", also Figure 3 and lines 201-215. The wave length of the synthetic plane wave is really large. Why not just k=0 km^-1, i.e. spatially constant values? Is there any benefit from using a small but non-zero k instead of just k=0?

As one has to divide by k at some point in the calculations (see for example equation 5), the value of k can be set very close to zero, but not to 0. This is more a technical limitation in coding than a physical reason behind it. As such a small difference in wave vector does not change the appearance of the spectra we don't want to overload the reader with very specific side information and thus propose to leave the respective sentences as is.

- Figure 4 and discussion: As discussed in VanderPlas (2018), side maxima at regular spaced intervals are an effect of windowing (his Figures 6 and 7), while non-uniform sampling adds "noisy" features in the spectrum, his Figure 9. This should be true also for sampling in the spatial domain: since the data points in space do not extend infinitely, there is implicitely a window applied before the Fourier transform. The Capon method effectively uses an adaptive window, depending on k. Therefore the method suppresses window effects and they are normally not visible. Here the artificial signal contains only one k≈0. Could this be the cause of the regularly spaced side maxima?

> Here in Figure 4 the side maxima "do not represent a true signal detection but are artifacts stemming from the different sampling distances (line 230)". But the larger side maxima do seem to occur at regular intervals. Wouldn't this rather be an effect of spatial windowing? Figure 2 seems to show no windowing effect, is this a matter of scaling?

We want to address the above two comments together. The reviewer uses the term “side maxima” in a different way than we do in our paper. Please refer to the labeling with arrows in Figure 4 and the corresponding explanations in the text. Will will use that terminology in the following answer.

The reviewer is completely right that aliasing and thus the repetition of maxima can be explained by windowing, most prominently by the Dirac comb window as thoroughly explained in VanderPlas (2018). In the presented study, the authors however chose not to include explanations of windowing as it would increase the already high complexity of the presented matter and in our view would make the work less accessible. As VanderPlas (2018) notes, windowing “…is *one* way to motivate the famous Nyquist sampling limit, *which approaches the question from the other direction*…”. As stated in a response to another reviewer, we want to expand section 2.2 to explain aliasing in more detail. Nevertheless, we think that such an explanation is possible without going into the details of windowing.

As visible in Figure 2, there is clearly a Nyquist limit respectively a repetition of maxima visible. As said above, for Fourier transform, VanderPlas attributes this Nyquist limit to the Fourier transform window of the Dirac-comb. Therefore, contrary to the reviewers comment, one might well argue that window effects are visible in Figure 2. Also for Figure 4, where the spacecraft respectively sampling points are now irregularly spaced, the individual repetition schemes of the main maxima and side maxima may be explained by windowing. As the reviewer states, Figure 9 in VanderPlas shows that irregular sampling points add “noisy” features in the spectrum. While this might be true for the complicated window function used there, we have a much simpler window in Figure 4 with only 3 delta peaks (the 3 sampling points). Therefore, the window will not look “noisy”. It might well explain the repetition scheme visible in Figure 4, where you can see that about 3 different frequencies are overlaid, producing the visible spectrum.

Despite all this explanation, we think that a discussion of main and side maxima periodicity through windowing is out of the scope if this study. As explained for the 2D case in section 4, the repetition can be explained by the regular subsets of sampling points having their own spatial Nyquist limit. This also applies for the 1D case presented in Figure 4. For 3 spacecraft, there are already 4 different spatial Nyquist limits: The main one, determined by the gcd of the spacecraft distances, and three other ones, taking every combination of 2 spacecraft. In our paper we chose the way of explaining the side maxima through the combination of these spatial Nyquist limits and the respective periodicities. This is what creates the different underlying periodicities in Figure 4. Thus, the “regular” spacing of side maxima as noted by the reviewer is completely to be expected and needs no further explanation.

As a final remark, and as stated in the study (line 214-215), the change of the value of k does not change the periodicity. The spectrum just shifts by the value of k. This can be seen in the attached pdf as well (first Figure). The addition of another wave with a different wavelength will change the appearance of side maxima, but not the underlying periodicities (as can be seen in the fourth Figure in the attached pdf).

- Lines 293-298: The LLL algorithm is here described in such a way that a typical reviewer or reader of EGUsphere probably cannot follow what is going on. Especially "α-reduced" and the following equation (26) with undefined symbols like β should either be fully explained or just be replaced by an understandable wording, refering for the details of the algorithm to the text books.

The authors agree that the word “α-reduced” is confusing as well as the explanation of the parameter β and thus equation 26 is incomplete. However we think by replacing “α-reduced” with “reduced” and explaining the purpose of the parameter β, a typical EGUsphere reader will be perfectly capable of understanding. We want to point out to the reviewer that we have to find a balance between explaining the complex LLL algorithm in full detail, which would overload the reader with information, or not mentioning at all what the algorithm does, which many readers would leave there confused and unsatisfied. Therefore, we think it is a good balance to mention the Gram-Schmidt orthogonalization as well as termination criteria to very broadly describe what the algorithm does and keep formula 26 in to show what the result of the algorithm is. Again, we think with an additional explanatory sentence, this will help the reader in gaining a basic understanding of the algorithm.

- Line 362-363: "In reality, position errors of the S/C will enormously alter the calculated spatial Nyquist limit ..." Isn't for the wave telescope technique also a problem that by orbital mechanics the relative positions of the spacecraft vary within the duration of the time intervals over which a temporal analysis needs to be performed?

This is true, however a detailed assessment of the influences of orbital mechanics goes beyond the scope of this paper. Such errors would just be another aspect in position errors beside e.g. error of position determination by timing inaccuracy. Thus we want to add this aspect as a wave-telescope specific argument for the incorporation of position errors into the lattice calculations.

Reference:

- Jacob T. VanderPlas (2018), Understanding the Lomb–Scargle Periodogram, ApJS 236 16 DOI 10.3847/1538-4365/aab766