# Rebuttal letter (entropy-1919488)

We thank the reviewers for their careful reading of our manuscript and their useful comments. We have considered all of them and improved the manuscript accordingly. Our responses to all reviewer comments (listed below in *italic*) in the following.

## Reviewer 1

*Introduction: a clear formulation of the mathematical problem, the state-of-the-art in terms of the existing literature, a summary of the solution proposed and of its pros and cons are totally lacking.*

According to the both Reviewers‘ comments we restructured and extended the introduction to make our approach clearer. In the revised version we improved also the literature review in the introduction. The summary of the solution proposed, as well as pros and cons are provided in the conclusion section, as we believe this section is more suitable place for their presentation.

*Equation (1) is poorly introduced. Although the authors cite a reference ([1]), the x\_i, x\_j vectors, as well as the parameters d and N, are not explained. The "trajectory x\_i", for example, is introduced only at the end of page 1. Moreover, R\_{i,j}(\epsilon) is not explicitly stated and explained.*

In the revised version of the manuscript we have explained all variables and parameters in equation (1). So, an unfamiliar reader could also easily follow the method.

*(lines 20-21) "Furthermore, the correlation structures of higher dimensional spaces can be resolved in the recurrence-derived Fourier-spectrum." What do the authors mean with "correlation structures of higher dimensional spaces"? And what do they mean with the term "resolve"?*

The RP encodes arbitrary dimensional trajectories and its tau-RR serves as a plug-in for the auto-correlation function (explained in the same section), which is why we were talking of "correlation structures of higher dimensional spaces”. In fact, this can be read from the Fourier spectrum of the tau-RR. We clarified this and restructured the according sentence.

*Figs. 2C, 2F: the "Inter Spike Spectrum of perfect DC" is equal to the "Inter Spike Spectrum of randomized DC", although the time domain signals (Figs. 2A, 2D, respectively) are not. This might prove that the new tool introduced in the manuscript is not invertible: Definitely a major issue.*

Yes, you are right. It is not invertible due to the sparse optimisation used. This is also stated and elaborated on in the Method-section.

*(beginning of Sec. 2) "Let s(t\_i) be the normalized signal we want to transform...". It is unclear how s(t\_i) is obtained (i.e. "normalized") from x(t).*

We thank the reviewer for pointing out this negligence. We gave the necessary information in the text (min-max normalization is used).

*The way in which basis functions are defined is confusing.*

We tried to minimize the confusion by using Fig. 3 as a complementary illustration of the method.

*The authors state that they "either use" the LASSO or the STLS methods to obtain β. It is not clear when the authors prefer one method over the other one, and why. Moreover, although a direct comparison between the methods, namely the different behavior of the regularization parameter, is shown in Fig. A3, a thorough comparison between the two methods is lacking: Figs. 2, 4 and 5 appear to be obtained by using only the LASSO method. One of the few mentions of the STLS method is in the Discussion section, where it is stated that (lines 236-237) "[...] the two different sparse regression algorithms [...] yield different results for the same desired ρ". How do the authors support this statement? There is no clear way for a reader to either compare the two methods, or to understand why the authors did not consider a single method from the very beginning.*

We have expanded Section 2 in this regard and thank the reviewer for this advice. We support the statement you mentioned with the behaviour shown in Fig. A3.

*At line 51 the authors introduce the term "loading": The authors should explain the meaning of this term.*

We thank the reviewer for this comment that this terminology was indeed inconsistent. We have restructured the manuscript to avoid this term and refer to it simply as weights.

*With regard to the analysis of power grid frequency time series (Sec. 3.3, Fig. 6) The criterion followed by the authors to evaluate the importance of the spectral peaks is unclear. […]*

*Overall, the details of the power spectrum estimation procedure are not fully described: in particular, no information on windowing is provided. It is thus hard to evaluate the significance of the "peak splitting" discussed by the authors in Sec. 3.3 and concerning Fig. 6(A): could this splitting be due to spurious effects such as spectral leakage? It is worth noting that the information on windowing is instead provided in the case of the "evolutionary" spectra of Earth's orbit data.*

In the main text, reference is made to Appendix B, which contains all the technical information on downsampling, batching/windowing, etc.

However, you are right in the sense that we did not explicitly explained the splitting and thank the reviewer for pointing out the possible leak-effect cause. We took this as an opportunity to integrate a further explanatory figure in the Appendix B and also to deal with the non-existing leakage effect of our proposed method in the Method Section and the Conclusion.

*The claimed robustness to noise is evaluated by showing the results of the method applied to the Roessler system with 5% additive Gaussian white noise (Fig. A2). While the results indeed appear to be unchanged with respect to the noiseless case (Fig. 4), a single example is hardly sufficient to claim that the method is "robust to noise". At which signal-to-noise ratio does the method fail to provide reliable results?*

We agree with this comment and added a more sophisticated analysis, which results in two additional figures in the manuscript (Fig. 5 and Fig. A3). In Figure 5 we basically show that the peak positions found in the inter-spike spectra for different noise levels (ranging from 1%-50% additive noise) do not change at all or only slightly, whereas the amplitudes do change. However, this depends on the underlying system as well as on the regularisation parameter chosen for obtaining the inter-spike spectrum. In Figure A3 we show the deviations of the noisy inter-spike spectra with respect to the noise-free spectrum by using the Wasserstein distance.

*The authors unduly use the term "powerspectrum". However, this word does not exist in English. They might consider, for example, power spectral density.*

We do not agree with this criticism. "Powerspectrum" (or "power spectrum") is common term in data analysis, see, e.g., <https://mathworld.wolfram.com/PowerSpectrum.html> (or simply Google’ing it). Nevertheless, we have modified the term to "power spectral density” as the reviewer suggested.

*The abbreviation "DC" for "Dirac comb" is introduced four times (lines 27, caption of Fig. 2, beginning of Sec. 2, line 248).*

We believe that in principle it should be possible for the reader to understand certain sections if they are read individually. Since this abbreviation is central to the method presented, we introduce it in the introduction, the method-section and the conclusion.

*The abbreviation "s.t." in Eq.(6) is unclear. Does it stand for "such that"? Why not using "where"?*

"s.t." means "such that" or "so that" and is frequently used in equations. Nevertheless, we have now explicetely given "such that" (which is correct, and not "where", because the following part is the condition for variable $n$).

*(lines 241-244) "This is not a drawback of the proposed method, but rather a drawback of the particular application method that we have heavily used in this article and which was the main motivation for developing the proposed method." This sentence is unclear: which "application method" are the authors referring to?*

It was referring to the previous sentence, but we have now clarified that sentence.

*Some typos:*

*in page 2, Wiener-Khinchim ---> Wiener-Khinchin;*

We corrected this typo.

*in the Conclusion, "Wee chose LASSO..." (unless Wee is a surname).*

We corrected this typo.

## Reviewer 2

*Starting a detailed assessment from the beginning, it is necessary to emphasize the comprehensive and impeccable introduction to the issue of the construction of a novel kind of power spectrum, which is the inter-spike spectrum.*

We have extended the introduction and hope it is now more clear.

*Moreover, the Authors explained the algorithm in great detail (in numerous pictures). By the way, also with outer -spike. Literature examples below review.*

We have added some further references on spike-train analysis and, in particular, on spike-train power spectra. Thanks for the literature suggestions, which were really helpful!

*The illustrations of tau-recurrence rate based spectrum (Fig. 1) and the transformation diagrams of the series of Dirac delta function (Fig. 2), including the two proposed inter-spikes spectrum, deserve emphasis. A small remark - regarding verse 35: what will be with stochastic resonance, because we are dealing with impulse disturbances.?*

TBD

*Chapter 2 presents the signal decomposition into set of appropriate basis functions using Dirac comb. Here, too, we can distinguish a graphic representation of the decomposition procedure (Fig. 3). Nevertheless, the Authors could cite a position in the literature that discusses other methods of decomposition.*

We took the reviewer's advice and think this is a good point. Hence, we extended the Methods-section and added the corresponding references.

*A small note on Appendix C: It would be interesting to present an inter-spike spectra also for one of the nonlinear systems from Duffing, Mackey-Glass or Chen systems.*

We thank the reviewer for this suggestion. In fact, we were computing numerous inter-spike spectra for different systems. But regarding the manuscript, we decided to present a variety of possible applications, which includes the analysis of exemplary nonlinear system (like the Roessler we showed or systems you suggest) as well as "real-world" data (power grid frequency data) and data stemming from a geophysical context (orbital data). Each of these applications could, of course, be expanded or other applications could be shown, but here we must concentrate on presenting the method itself as concisely as possible while keeping the size of the manuscript within tolerable limits. Moreover, when analyzing a nonlinear system we would potentially be interested in different dynamical setups, which would, in turn, lead to a variety of aspects we would additionally need to consider and cover with the proposed method. The idea of this paper is to outline the proposed method and provide the code/package for its use. We would certainly be pleased if researchers decided to publish a thorough study of a number of different nonlinear systems using this method.