"The fact is that the reviewer is always in a situation to go up against the paper by finding the weak spots of it in the function of improving the text and readability of the paper."

This is not the case as I had have mentioned: "The answers given were satisfactory", but it is a fact that the authors did not put the required effort to improve the paper. They only reply not correct the paper significantly.

"Namely, there is no possibility (and it is not necessary) to compare the presentations and results in papers Mihailović et al. (2018) – (P1), Bessafi et al. (2018) – (P2) and paper by Bessafi et al. (2019) – (P3), which is under the reviewing process."

You are wrong. This is what is called "literature reviewing". This is a crucial part in the paper specially when there are two quite similar papers in the same topic that, if do not state it properly may look like "milking the cow" with another set of data. Instead of La Reunion it considers a bit wider area of same region.

Answering to authors reply to my previous concern number 3: I stand with my comment. Maybe the problem is the Lempel-Ziv compressor that is not suitable to lead with this type of strings. You should try other compressor.

Regarding the answer to comment 4: The results emphasize something that it is already well known and that authors also acknowledge in section Coding theorem and Algorithmic probability method.

Answer to 5: Now it is in standard mathematical notion. When presenting notation and results this must be carefully treated as it might lead to confusions to the reader.

Answer to 6: How can an "inverse of a parabola" be a Gaussian PDF? A Gaussian is defined using an exponential function. This must be removed or restated otherwise is completely wrong.
"The fact is that the reviewer is always in a situation to go up against the paper by finding the weak spots of it in the function of improving the text and readability of the paper." 

Verily, this is the statement where we and reviewer#2 reached a complete agreement. Finding the weak spots of the paper is the main reviewer’s task looking scientifically and ethically as well. Only correction we can add is “those weak spots must to be found and placed in the framework of the paper’s idea but not in the world of ideas which are outside of idea in this paper, and then pushing them in its framework”. The idea of the paper is quite clear and easy to be readable: “improving the quality of the KC complexity using new encoding scheme, typically for short strings”. Nothing less! Nothing more!

This is not the case as I had have mentioned: "The answers given were satisfactory", but it is a fact that the authors did not put the required effort to improve the paper. They only reply not correct the paper significantly.

Looking epistemologically this statement is true. The author (authors) cannot invest adequate effort to improve enough any model or theory, i.e. paper (simple reading of the Gödel's incompleteness theorems are a good guide). The last statement is a trivial phrase against we have no answer (not only the authors of this paper – but nobody).

"Namely, there is no possibility (and it is not necessary) to compare the presentations and results in papers Mihailović et al. (2018) – (P1), Bessafi et al. (2018) – (P2) and paper by Bessafi et al. (2019) – (P3), which is under the reviewing process."

That is true. This is just a part of our answer on your ascertainment in the round#2: “1) no improvement in the presentation of the differences for other 2 papers”, when we answered on your comments from round#1, by the above statement.

You are wrong. This is what is called "literature reviewing". This is a crucial part in the paper specially when there are two quite similar papers in the same topic that, if do not state it properly may look like "milking the cow" with another set of data. Instead of La Reunion it considers a bit wider area of same region.

Answer:

Thank you the reviewer for this helpful comment. Although, we think that these two papers are not “quite similar” this comment is given from the position of the potential reader and it is very valuable. We agree that literature reviewing is a crucial part of the paper. Therefore, in the revised version we invested effort to better highlight the readability in the presentation of the subject of the paper in regard to the previous studies. We propose inserting the following sentence in the introduction:

«Furthermore, they mentioned the ability of KC computation to give an estimation of daily solar radiation predictability but emphasizing the limitation of this method for shorter time series. To embedding the purpose of this study, it is pointed out that the two previous studies [8, 9] focused on complexity of solar radiation at La Réunion with spatially sparse daily dataset. The former study was achieved using 11 laboratory solar radiation stations through the 2013-2015 period to assess the spatial complexity pattern over the island. A weighted...»
Hamming distance is introduced to smooth the geographical complexity and overcome the sparsity of the data. The results reveal that the spatial complexity pattern is well-related to the spatial local meteorological weather pattern prevailing at La Réunion (trade winds that circulate in a “flow around” regime). The latter study used higher dense but still sparse daily solar radiation dataset using 32 stations of Météo-France (French meteorological office) available for the 2011-2015 period. This study showed that the spatial complexity pattern over La Réunion is also variable in time and sensitive to large scale climate event (2011–2012 La Nina event and preceding a very strong El-Nino 2015–2016). Following these two studies, the question is how the complexity looks like spatially and temporally for a larger area at infra-day time scale. Solar radiation satellite data can contribute to give an answer. Moreover, such data are commonly spatially available at regular grid with infra-day time sampling as well. But the first task is if we would like to extend those previous predictability studies on daily solar radiation to infra-day sampling data with KC computation used in the previous studies we should have in mind that it is inapplicable for hourly solar data time series. 

Answering to authors reply to my previous concern number 3: I stand with my comment. Maybe the problem is the Lempel-Ziv compressor that is not suitable to lead with this type of strings. You should try other compressor.

Answer:

Thank you the reviewer for this comment. We were not concerned with exploring different compression algorithms (mathematical tools approximating the KC for short strings). Our focus was set on improving the approximation of KC using new encoding scheme (eliminating the threshold). Our approach gave good results and there is no need for exploring other methods, which could not be adequate in this case. In summary, our idea is stated explicitly. If we accept changes suggested by the reviewer it means that we should leave the crucial features of the main idea in the paper and have to change whole structure of the paper.

But we agree with the reviewer that there is a need to better clarify our approach and lift up its readability for the reader. Therefore, in the revised version we propose the following sentence:

Studies on hourly solar radiation complexity is also of an important interest to make solar energy and its exploitation more efficient and economically acceptable. Therefore, we still focus on Kolmogorov complexity using Lempel-Ziv algorithm but for the hourly solar radiation observed over a larger geographical area which embedding La Réunion. Nevertheless, to overcome the limitation of KC of computation for daytime hourly data, we have inserted a new encoding scheme instead of threshold one used in the original KC method (threshold scheme to binarize data plus Lempel-Ziv compression with normalization) to be able to have a measure of complexity of short string. Our approach can namely be considered as an extension of KC with a new encoding scheme (hereafter KC-ES) without threshold process.
To assess the reliability of this extended KC method (encoding without threshold plus Lempel-Ziv compression with normalization), we have compared our method with another method based on an algorithmic and probabilistic approach. Suggested by Delahaye et al. and Soler-Tsocano et al. [11, 12], coding theorem and the algorithmic probability (hereafter AP) is able to compute complexity for short time series. The AP method is used here as a benchmark method to evaluate the KC-ES method.

Regarding the answer to comment 4: The results emphasize something that it is already well known and that authors also acknowledge in section Coding theorem and Algorithmic probability method.

Answer:

Thank you the reviewer for this comment. We agree that AP method is a well-known method to compute complexity for short strings. We’ve also mentioned that AP method has never been used for hourly solar radiation. What we emphasize is that AP can be used as a benchmark method for the comparison with the KC-ES method we introduce in this study for computing the complexity of hourly solar radiation. Additional text as proposed to the previous answer will clarify the use of the AP method in this study.

Answer to 5: Now it is in standard mathematical notion. When presenting notation and results this must be carefully treated as it might lead to confusions to the reader.

Answer:

Thank you the reviewer for this helpful comment. We agree with the reviewer for this comment.

Answer to 6: How can an "inverse of a parabola" be a Gaussian PDF? A Gaussian is defined using an exponential function. This must be removed or restated otherwise is completely wrong.

Answer:

Thank you the reviewer for this helpful comment. We agree with the reviewer with the comment and we recognize that there is some confusion. In fact, this is an inverse parabola. We agree Gaussian is an exponential function of a negative square of the argument. The confusion is related to the fact about the plot and we agree with the reviewer that some confusion with lead the reader.

The plot is a semilogarithmic plot of the PDFs. Thus when we use the log scale for the vertical axis that give us an inverse parabola.

For proof, let’s write \( y = e^{-\frac{x^2}{\sigma}} \) then \( \log(y) = -\left(\frac{x}{\sigma}\right)^2 \). If we plot \( \log(y) \) versus the variable \( x \) we have an inverse parabola.
We agree with the reviewer to avoid such potential confusion for the reader especially to read the plots of Figure 6, we propose the sentence in Figure caption of Figure 6:

«Figure 6. ) Semi logarithmic scale of normalized experimental PDFs of the increment $\delta x_\tau$ of hourly clear sky index for time lag $\tau$ ($\tau = 1, 2, 3, 4, 5$ and 6 hours) for the period 2004-2006 record for a) Longitude 43°E; b) Longitude 52°E; c) Longitude 57°E and d) Longitude 62°E and 30°S-15°N latitude band. Red line is the Gaussian PDFs. »

In the text, we propose the sentence:

«Figure 6 shows the distribution of the time increment of solar radiation $\tau$ ($\tau = 1, 2, \ldots, 6h$). This is displayed using a semilogarithmic scale (along the vertical axis). The departure from the Gaussian distribution is an indicator of the intermittency [9]. »

Finally, we deeply grateful to the reviewer who helped us to significantly polish this work.