

Interactive comment on “Nonlinear analysis of the occurrence of hurricanes in the Gulf of Mexico and the Caribbean Sea” by Berenice Rojo-Garibaldi et al.

Anonymous Referee #1

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The manuscript's goal is to evaluate the chaotic nature of a hurricane time series reconstructed using historical data and a hurricane dataset. This manuscript presents several problems that I will try to elucidate here:

1) Conception of the study: -it is not clear how the dataset analyzed is constructed and what are the relations with the HURDAT database. No comparison with already-existing hurricane datasets is shown.

-Figure 1: is the linear fit showing a significant reduction in the number of hurricanes? If the fit is significant, this means that your analysis cannot be performed because the series would be issued from a non-stationary process.

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-Why the authors perform a nonlinear time series analysis on the time series on the number of hurricanes? What is supposed to be the underlying “dynamical system” that generate the hurricanes count in certain regions? How can the “attractor” of the number of hurricane occurrences give any information on the predictability of the phenomena as claimed at the end of the introduction?

-The phase space reconstructions in Figure 2 shows a noisy fixed point structure. This is coherent with the fact that the hurricane occurrence seem to be Poisson or Compound Poisson distributed. The rest of the analysis just show trivially the consequences of this.

2) The language: I won't comment here on the English language but only on the use of scientifically wrong expressions. Just to make some example:

- Hurricanes are not complex systems. They are extreme phenomena that occur in a complex dynamical systems (the climate system).

- Line 7-8 chaotic edge of what?

- The last sentence seems broken or is impossible to understand what you mean by “category”. Do you mean hurricane strength?

- “Lyapunov exponent is a key point”: actually it is a dynamical systems metric.

- What is a “chaotic movement”?

3) The references are not updated: they are mostly coming from the (excellent) scientific knowledge of dynamical systems in the 80s/90s. There are only very few references from 2000. Of course, since this date there have been several improvements to the methodology and the problems the authors want to address but they seem completely unaware of this body of literature.

4) The conclusions are practically inexistent (this problem is certainly related to the wrong conception of this study, as detailed in my point 1)

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For all these reasons, I firmly advise against publication of this manuscript in NPG. I encourage the authors 1) to analyze different time series than the number of hurricanes in the Caribbean region to infer dynamical properties, 2) to review the recent literature on dynamical systems metrics 3) To use carefully the scientific jargon pertinent of dynamical systems community.

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