Nonlinear Processes in Geophysics

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Discussions

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Interactive comment on "Power law distributions of wildfires across Europe: benchmarking a land surface model with observed data" *by* B. Di Mauro et al.

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This contribution attempts to characterise the statistical properties of wildfires across Europe. The authors use data from two different sources: the European Forest Fire Information System and a recent version of a Land Surface Model (LSM). Their main goal in the paper is to benchmark the LSM with a broad observational data set.

The analysis has great potential. Forest fires prediction is a key issue for society, specially taking into account upcoming warming scenarios. However, in my opinion, the analysis is rather limited and constrained to the comparison with the LSM. But this model performs quite badly in comparison to the observational data. That can be seen

from figure 2 directly, without any sophisticated analysis. The authors also point it out.

I think that the paper in general needs a substantial amount of work before it reaches a publishable form. In the following, I will give a list of the main issues that I recommend to address.

* The LSM produces very coarse data in space and time which may affect the values and the interpretation of power law exponents. I think the paper would greatly benefit from further analysis on the observational data. In particular, the effects of changes in the minimum fire size, the binning in the temporal and spatial and alternative goodness of fit measure to Kolgomorov-Smirov statistic could be discussed on detail.

* The Introduction could be revised with care as it has some grammatical mistakes. I think that in some parts of this section the writing stops and confuses the reader.

* I also have concerns about the Classification section. It is clear how the classification is done, but does it mean that the authors group all the data from a corresponding class? This is not said specifically until later in the text. This brings a question to me: How robust are the results under changes on the classification? I have great concern that this strong classification grouping, together with the binning, are choices with great potential to make the analysis not robust.

* In the Power law fitting section the authors only explain some details of the Method of Clauset et al., 2009, but they do not explain the one they actually use for the data that has been binned, Virkar and Clauset, 2014. An explanation of the fitting method would make the paper self-contained and improve its quality: could the authors explain which is the modification for binned data? Previous analysis used Probability Density Functions instead of the Cumulative. I recommend that the authors also discuss that a bit.

* I also recommend performing an error estimation of the exponents. In figure 5 a progression in the exponent values can be observed but we do not know if 4.92 or

4.83 are different or not, as this would depend on the error. Moreover, this progression could also be related to different number of data. Table 2 may be more complete if the number of total data and number of data out of the fitted range were mentioned.

* In the same way, Figure 3 and Figure 4 would be more informative if they would include error bars.

* In the Discussion session, the connection with the Self-Organised Criticality (SOC) paradigm could be discussed with more detail. In the text is mentioned that "Power law generating mechanisms play a fundamental role in the interpretation of the results" but possible mechanisms are not proposed or discussed. Only the SOC mechanism is mentioned.

* Finally, I also recommend working a bit more on the Conclusion section. For example, the conclusion "a power law scaling behaviour is likely observed only for natural environments, and deviation from the power law can be ascribed by the influence of human being on natural fire regimes" could be more elaborated, it sounds bit speculative to me. Could the model be used in order to test that?

I do not detail here other minor mistakes or minor points. I do not recommend the paper to be published in its current form, but I think that with some work and if the previous points are addressed the paper has a great potential to have a big impact and to be an important contribution. I am willing to review a revised version.

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