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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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The paper addresses the potential power-law distribution of wildfires. As the existence of such a distribution is relevant for both hazard assessment and theoretical understanding with regard to other types of hazards, the paper may make an important contribution. But while finding it overall interesting, I feel that a significant amount of work should be spent before it can be published as a regular paper in NPG.

First I would like to mention that the writing appears to be a bit rushed. The number of typos and errors in grammar is quite high, in particular in relation to the number of authors. I also found small inconsistencies in the references, e.g., Malamud et al.

(2005) on page 1566 must be either 2005a or 2005b. Unfortunately I did not find enough time to check everything in detail and therefore apologize that my comments may also be a bit rushed. However, I believe that these formal issues can be fixed with some more diligence.

I also got the impression that the methods are not explained very well. As an example, Eq. (3) describes the original maximum-likelihood estimate for unbinned raw data presented by Clauset et al. (2009), while it is explicitely mentioned some lines above that the version for binned data (Virkar and Clauset, 2014) was used in this study. Similarly, I did not understand Sect. 4, in particular Eq. (4), without looking into the original paper of Lehsten et al. (2014), and I am not sure whether it is explained correctly here. This also applies to the last sentence on page 1562 where I simply do not understand why CFS should be a straight line for a power law.

However, my most serious concern about the results refers to the consideration of power-law distributions. This paper addresses the statistical distribution of cumulated burned areas in spatial and temporal bins, while at least the majority of the literature addresses the size distribution of individual fires. Both statistics may be related in some sense, but it is not clear. The interpretation of the exponent given at the beginning of Sect. 3 clearly refers to the widely used fire-size distribution, and I do not understand how it can be transferred to the distribution of monthly sums. Here, a small value of the exponent simply indicates a high variablity in this values, and I would attribute this at first to a high spatial variablity of fire activity. The relationship to the contribution of small and large fires is not clear to me. In order to avoid confusion, the meaning of this distribution and its potential relationship to the widely used fire-size distribution must be clarified.

In addition, everything written about the potential relationship to self-organized criticality remains on a vague level.

Beyond this, I am not completely convinced about the statistical treatment and would like to point out the following aspects:

1. The Kolmogorov-Smirnov test is not a very strong criterion in general. This test is ok for rejecting a hypothesis formally in some cases, but the opposite, speaking of significance if the test does not fail (in particular, at the confidence level used here), is a bit optimistic. This particularly applies if binned data with a quite small number of bins are considered as it is done here. The problem is also reflected in Fig. 3 where even the EFFIS data do not look like reasonable power laws.

2. When comparing the power-law distribution to alternative distributions (Table 3), at least the results for the power law with cutoff seem to be strange to me. The power law with cutoff includes the original power law, so how can the power law without cutoff provide a better fit? By the way, “positive values of the ratio” or of its logarithm in the caption of Table 3?

I would like to apologize again for not going deeply into all details and minor points due to the lack of time at this stage. I think that the points mentioned above are somewhat fundamental for the interpretation, so that they should be addressed first. I would, of course, be glad to review a revised version.

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 1553, 2015.