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Interactive comment on "Fractal properties of forest fires in Amazonia as a basis for modelling pan-tropical burned area" *by* I. N. Fletcher et al.

Anonymous Referee #1

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The general issue addressed by this paper, which relates to the model behavior of forest fires, is important in the literature. Such models are commonly complex, and any method that claims to provide a simplified explanation is worthy of consideration. Thus, there is no doubt that the material is relevant.

The specific methods addressed by the paper involve a consideration of whether two specific distributions (Pareto and the tapered Pereto or Generalised Pareto) fit the fire count data. The paper argues (p14144, lines 4-11) that the data exhibits self-organized criticality (SOC); essentially arguing for a fractal-type of behavior. The evidence for this viewpoint is taken from the fits of the two distributions to the processed MODIS data, as well as some of the literature (p14144 lines 12-25). In this sense, a crucial test of the authors' hypothesis is whether the data conforms to one of the two distributions.

A great deal of the paper (p14147 - p14151) is taken up by considerations of fitting the Pareto and tapered Pareto distributions to the processed data, and I am not convinced that all that space is necessary to describe the process. As a general rule, fitting of the Pareto distribution by the simple methods described in the manuscript is sensitive to outliers, particularly at the extremes of the data dynamic range. Specialized methods of fitting are available from the literature and these would generally be preferred, unless there are very good reasons for using the methods in the manuscript, and I cannot see such a justification.

The comparison between the Pareto and tapered Pareto is obvious and probably spurious. One should not be surprised if the tapered Pareto, with an additional parameter, gives a better fit. In fact it would be rather surprising if it did not do so. Figure 2 gives misleading information concerning this comparison, in two ways. First, the R-squared value has little meaning in this context unless it is defined separately and uncertainty quoted (and I don't see much justification for doing so). Second, the plots of fits in Figure 2 appear to me to both demonstrate a poor fit, with evidence of short tails with respect to the theoretical distribution in the data. If the two distributions are to be compared, then a formal criterion ought to be used, such as a likelihood-based approach or some other accepted alternative. The reason that formal methods would be favored in this case is that it is very difficult to tell by eye whether the tapered Pareto is really better in Figure 2, or whether the way that the data is plotted makes the data look better. Personally, I do not trust my eyes.

As an aside, if a figure such as Figure 2 is to be included, then all the parameters ought to be written on the plot, in the form they are used in the text. So, this would involve adding a_0, b, and A_up as values along with their estimated uncertainties. The latter will come out from the fitting analysis. As written, Figure 2 has to be re-interpreted from the text, which is distracting.

The justification for the use of a specific fitting procedure finally used (p14152 lines 12-20) is by way of the results from Figure 5. I am not sure that these images tell us much, except that the specific fitting method chosen gives something reasonably close to the observed burnt areas. Given that the variations in the fitting method used seem to be so high, this gives me less confidence in the method chosen, rather than more confidence (which is presumably the authors' intention). Of course, the comparison of the total burnt area value (p14153 line 2) is good, but it is difficult to come to a general conclusion on the goodness of the model based on only one such comparison.

The discussion (p14155) notes doubts in the literature concerning fractal-based models, particularly the paper by Reed & McKelvey (2002). Their paper argued (rather strongly in some respects) that the power-law approach was fundamentally flawed, except for special cases of spatial scale where the distribution might be expected to hold. In a sense, the authors' manuscript is at one end of a spectrum of modelling approaches. Their paper is largely empirical in nature, and it could be justified on the basis of the parsimony of the number of parameters involved. A physically-based model would be at the other end of the spectrum, and the Reed & McKelvey paper describes such an approach. Which is better? Well, if the data fits the empirical model then I have no problem with the model, provided it is accepted that the model itself is a contrivance to fit the data. There is no need to have to justify the model as an example of SOC for it to be acceptable, provided it is a good model. So, the authors' discussion concerning justification of SOC is essentially irrelevant as far as I am concerned, provided the model is a *good* model. Unfortunately, I do not think the model is particularly well-justified, and on balance it is probably not a good model.

The data for the fitting process (p14145 lines 17-25) come from the MODIS MOD09 product. Since the band 1 and 2 data are 250m and the band 6 data is 500m, the resolution of the MOD09 derived products is no better than 500m, and probably somewhat worse as a result of the data processing stream. So, the use of a 500m cell size is possibly over-optimistic as a basis for detecting fire events, although it is an arguable issue.

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