Biogeosciences Discuss., 10, C7159–C7167, 2013
www.biogeosciences-discuss.net/10/C7159/2013/

Biogeosciences Discussions

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.

I. N. Fletcher et al.

inf201@exeter.ac.uk

Received and published: 4 December 2013

Response to S. Pueyo

The authors would like to thank the reviewer for his time and effort in reviewing our manuscript. We highly appreciate the thoroughness of the comments, and are pleased that he agrees that an approach based on scale-invariance could potentially be a suitable alternative to existing fire spread parametrizations. Based on the reviewer's suggestions, and those of the other reviewers, we have made considerable changes to the manuscript, and hope that they satisfactorily address any concerns the reviewer has about the validity of the methods or reliability of the results. Below, we first of all outline the main changes we have made, and then address each of the suggestions/comments individually. We have included in each case the exact comment that we are addressing, for the sake of clarity, as well as the section of the comments in which the comment was made .

Main changes to the methods

- A new distribution is used, which allows for a tail at both ends of the distribution: $n_{X \geq A} = aA^{-b} \exp(-\frac{1}{A} - \frac{A}{\theta})$ $= n_f A^{-b} \exp(1 - \frac{1}{A} + \frac{1-A}{\theta})$
- Only one parameterisation is presented, with a comprehensive explanation of its physical interpretation

Response to comments

"The power law distribution and the Pareto distribution are exactly the same distribution." (Section 2)

We appreciate the thorough explanation of this point. We agree that there is much confusion in the literature about this, hence our mistake. In the original manuscript, when we described the "Pareto" as continuous and the "power-law" as discrete, we realise we used the wrong terminology. We simply meant that using the cumulative distribution was preferable because the non-cumulative form of the distribution allows many fire sizes to have the same frequency, hence making it harder to fit the distribution to the data. Additionally, it is difficult to incorporate fire sizes that do *not* occur into the distribution without either resorting to binning the data, or resulting in overestimation of burnt area. For example, from the burn scar data, P(X=100) = 0.000083, P(X=101) = 0 and P(X=102) = 0.000083. If P(X=100) and P(X=102) could be accurately estimated, however, it follows that P(X=100) $\leq$ P(X=101) $\leq$ P(X=102).

“As a fitting method, [least-squares regression] is arguably sub-optimal, but I consider it to be acceptable (certainly, it is widely used). As refers to the goodness of fit, it is clearly unacceptable." (Section 3.1)

We agree completely that least-squares regression is not ideal for parameter estimation. However, likelihood-based estimation using the non-cumulative distribution function does not really work. To show this, we have included a figure (below) which is the same as Figure 2 in the revised manuscript, but using maximum-likelihood regression rather than least-squares.

We have included an explanation of our choice of fitting method in the revised manuscript, pasted below:

> We check that this distribution fits the data by estimating parameters b and θ using least-squares regression on Eq. (4), and comparing the resulting fitted cumulative frequencies to the data points. This is not an optimal fitting method, since a condition of least-squares optimization is that the errors be independent of one another. This is obviously not the case when cumulative frequencies are used. However, alternative methods such as maximum likelihood regression or the method of moments are not suitable in this case. These methods are commonly used for similar problems in the literature, using binned data (e.g. Pueyo, (2007), Pueyo et al., (2010), Moreno et al. (2011)). Binning the data results in the loss of information about extreme fire sizes, hence our reluctance to use this technique in this instance. If the data is used unbinned, we encounter the problem of trying to fit a continuous, monotonically decreasing probability density function to a set of data in which many sizes can take the same frequency, and some intermediate fire sizes do not occur at all (this pattern can be seen in the

BGD
10, C7159–C7167, 2013

top-right plot of Fig. (2)). Ultimately, this results in a clear underestimation of fire frequencies. Least-squares regression, although not a perfect option, provides decent approximations of the parameters.

Since the use of R^2-values as a measure of goodness-of-fit is not suitable, we have instead modified Figure 2 (for the single distribution being tested) to include:

(a) a log-log plot of the observed and fitted cumulative frequencies against fire sizes,

(b) a log-log plot of the observed and fitted frequencies against fire sizes,

(c) a log-log plot of fitted against observed cumulative frequencies with a 1:1 line, and

(d) a log-log plot of fitted against observed frequencies with a 1:1 line.

We feel that this makes it easier to assess the fit of the distribution to the data, at least visually.

"A careful examination of the cumulative plots (Fig. 2) makes it clear that neither model fits the data." (Section 3.1) This was a clear oversight on our part. We agree that neither of these models is suitable, hence our decision to use the new distribution detailed above which includes a tail at both ends of the distribution. We have checked that this new distribution does not produce a total burnt area estimate that is unrealistically far from the observed value (observed: 16,384 km^2, estimated 15,532km^2, as shown on the revised Fig. 2)

"I see a large problem in assuming the same $\max(A)$ for all cells." (Section 3.2) We agree that this makes little sense, physically. This possible parameterisation has been removed from the manuscript.

BGD

10, C7159–C7167, 2013

“The method of estimation ($\max(A)$ as a function of n_f) is circular. When they decide a given q they determine the b that they will obtain. The variability in b does not reflect the real differences in b among cells, but the fact that Eq. 10 is not fully accurate in this case.” (Section 3.3)

Equation 10 ($\log(\mu) = q\log(n_f)$) is not intended as an explanation of how $\max(A)$ relates to $\log(n_f)$ within the confines of the distribution. Instead, its purpose is to estimate $\mu \approx \max(A)$ using observed relationships, so that μ can then be used to estimate the other parameters. So we are not so concerned about whether Equation 10 is valid in terms of the distribution, although it is of course reassuring that the distribution confirms that Eq. 10 is not unreasonable.

In terms of the circularity of the relationship, where fixing q also fixes b, this is no longer the case in the new distribution.

“Taking a constant b is very likely to be closer to reality than taking a constant $\max(A)$." (Section 3.4) However, some problems remain:

1. **“While this might be an acceptable approximation, I do not think that it is entirely realistic"** We agree that it is not entirely realistic, but for the sake of model simplicity, we feel that fixing b is a suitable approximation, especially in the new distribution being considered. See Figure 3 in the revised manuscript for our justification of this. We have included a discussion about the physical interpretation of a fixed b in the Dicussion:

    > Parameter b represents the gradient of the distribution, i.e. the underlying rate-of-decay of fire sizes. We are assuming that this is predominantly dependent on land-cover, and since we are only considering tropical forests, there is no reason to allow b to vary. This does not mean that the rate-of-decay is fixed across all grid cells, since the value of θ can have a large effect on the distribution.

2. **"I find it less likely that it is acceptable taking a fixed $A_{\mathbf{up}}$."** We have changed this in the new results. A_{up}, renamed θ, is calculated as a function of b and μ ($\approx \max(A)$). This improves the likelihood that the model will be adaptable to other regions.

3. **"Some sound method has to be implemented for estimating b and $A_{\mathbf{up}}$ at least at the beginning"** As mentioned earlier, we use least-squares regression to produce initial estimates of b and θ (formerly A_{up}). Parameter θ is limited to positive value. The new methods used in the model itself to estimate the parameters (keeping b constant, and calculating θ as a function of μ (hence a function of n_f) and b) are further justified in the revised text of the manuscript, hopefully to your satisfaction.

4. **"The data should really follow a Pareto or tapered Pareto"** This has been resolved by using a new distribution which does fit the data.

"The burnt areas can be calculated more accurately by integrating Eqs. (3,6), but I do not think that this is a big problem" (Section 3.5) We agree that this would be a more accurate method. However, for the new distribution, integration of the cumulative density functions is not possible.

"The plots [in Fig. 4] do not satisfy the conditions for linear regression, which implies that their RMSEs are not very informative. Taking logarithmic axes would probably give more information." (Section 3.5) We have done as suggested and taken logarithms of the predicted and observed burnt area before calculating RMSE, as well as plotting Fig. 5 (previously Fig. 4) with logarithmic axes.

"Fig. 2 suggests that, if there is scale invariance in their data set, it does not start much below 200 ha, which seems consistent with the issue of resolution" (Section 4) Although we agree that the tail that can be observed for fires smaller than 200ha may suggest a lack of scale invariance below that point, the problem we faced

BGD

10, C7159–C7167, 2013

when dealing with this is that these fires need to be included in the model in order to produce an accurate estimate of burnt area, since they account for the majority of burnt scars in the study region (approximately 70%) . Additionally, if the only input to the model is going to be the total number of active fires, then we have no way of knowing how many of those are greater than 200 ha. Hence, we modified the distribution instead to include this tail. Although this may now contradict the theory of scale-invariance, the important thing in this study is being able to predict burnt area, regardless of the underlying theory.

"I do not agree that this should imply self-organized criticality. SOC is just one among many possible explanations for the Pareto distribution, however popular it might be." (Section 4) We have removed all discussion about SOC from the manuscript.

"I do not think that the hypothesis that *vegetation and climate variations do not affect the distribution parameters directly, but instead influence the number of fires or fire fronts that occur* is realistic." (Section 4) We phrased this hypothesis carelessly in the original manuscript. We have now reworded it:

> In this study we hypothesize that the effects of climate variations on active fires and fire spread are closely correlated, and hence, if fire counts are known, then the distribution parameters can be estimated from this single input variable, without the introduction of a weather variable.

We do not dispute that there it is likely that there is a relationship between weather and the distribution parameters for some regions and some distributions, as you found in your 2007 paper. However, in the context of our study, we found no clear relationships between a range of different weather variables and the parameters used in our distribution. There may well be some complex relationships that are beyond the scope of this work.

We have added more of the motivation for avoiding the introduction of additional variables in section 2.3.2:

> The maximum size a fire can take in a grid cell is dependent on many factors. From a purely statistical viewpoint, the more fires in a cell, the larger $\max(A)$ is likely to be. $\max(A)$ also depends on local climatic and ecological conditions. For example, fragmented fuel or a high fuel moisture content can severely limit fire spread, while high winds and a high litter load encourage fire propagation. Additionally, the largest potential fire size is not necessarily similar to the actual achieved $\max(A)$, which makes this a difficult value to predict.
>
> The estimate used in this model is simple: it is a log-linear function of fire counts, described by Eq. (1).
>
> $$\log(\max(A)) \approx q \log(n_f) \quad (1)$$
>
> This obviously takes the statistical likelihood of large fires given the sample size into account, and restricts $\max(A)$ to 1 pixel if there is only one fire, which is a reasonable assumption. Also, since fire occurrence is itself dependent on the same climatological and ecological conditions as fire spread, we would expect $\max(A)$ and n_f to covary. We see a correlation between the logarithms of the two variables of between 0.73 and 0.85, for the range of resolutions, and this relationship can be observed in Fig. (4). While the introduction of additional input variables could potentially improve the estimates of $\max(A)$, the added complexity of the model and errors present in the input datasets may counteract any potential improvement in the model performance.

Interactive comment on Biogeosciences Discuss., 10, 14141, 2013.

BGD

10, C7159–C7167, 2013

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

BGD

10, C7159–C7167, 2013

Interactive Comment

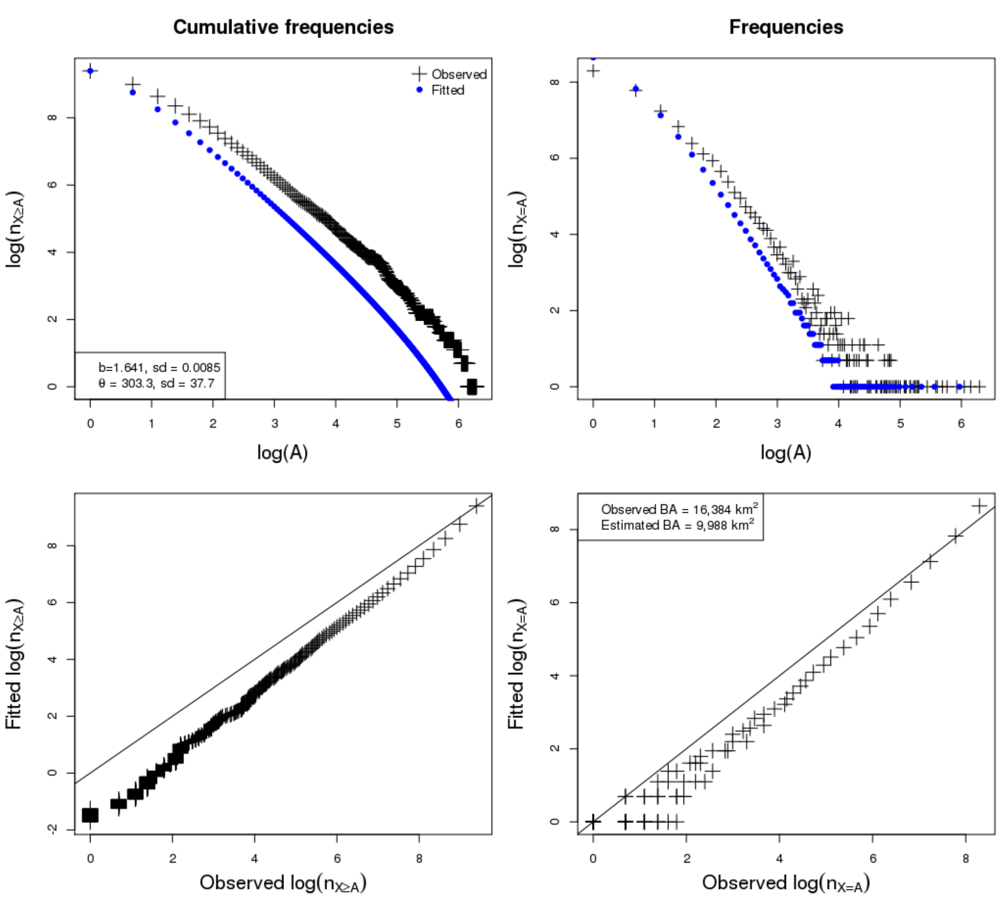

Fig. 1. Results of fitting the distribution to the data using maximum-likelihood estimation

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper