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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 #2

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Fires in tropical forests are important from ecological and climate change perspectives and current remote sensing approaches have difficulties in fully capturing their extent. For DGVM's where remotely sensed data can only be used for validation for the time period when satellite data is available the situation is even worse and this paper presents a new approach to estimate the size of fires (or their distribution) when ignition patterns are known. It could therefore be an important addition to the literature.

A previous reviewer has provided valuable insights with regard to the size distribution and the methodology. Below I will give additional feedback.

One important issue is that one cannot evaluate the burned area dataset used which is key to the work. The reference leads me to a Proceedings chapter in Portuguese. It would be very helpful for the reader if the authors give more information about this

product than they do in 2.1. In particular, how does this product compare to the burned area products they use for comparison?

This comparison is important for several reasons but partly because the authors end with a statement that their method yields higher burned area estimates than other burned area products, which I think has nothing to do with the work presented but stems from higher burned area datasets from Lima et al. (2009) than their comparison dataset.

It is unclear how the active fire data was used to calculate the largest fire size. If the largest fire size was not calculated from active fires then please explain how you got this metric outside the Lima et al. (2009) study region. In general I feel the paper is too concise (normally a good thing) and lacks discussion about methodology and implications.

In the discussion section we should get confidence in the model because it rightly captures interannual variability and spatial patterns of burned areas. One could also argue that this is the case because active fire and burned area from MODIS are very well correlated over larges scales. Please elaborate

To put things more into context, please see recent work by Randerson et al. (2012) titled 'Global burned area and biomass burning emissions from small fires'. That work aims to merge active fire and burned area data and has a useful discussion about the differences in both products which are very relevant for your work.

The paper is framed to be useful to improve DGVM's. However, active fire data to constrain the maximum fire size seems and spatial distribution are not available for the future so I am not sure if the current work will improve future (and past) predictions very much. Please also elaborate

In Figure 6 not only the tropical forest is shown where the method was validated (at least for part of the Amazon) but also estimates are given for savanna areas. There is no corresponding text and the total estimates are an order of magnitude above observed patterns. While a case can be made that current observations are low in tropical forests, the current generation of burned area estimates is capable in savannas (Roy and Boschettin (2009) 'Southern Africa Validation of the MODIS, L3JRC and GLOBCARBON Burned Area Products') where the majority of burned area is observed so the order of magnitude difference is suspicious to me. This is somewhat acknowledged in 3.3 but it is unclear when the authors think an overestimation indicates deficiencies in the data they compare and when it may be due to their approach.

In the Figures, please make use of sub- and superscript to make it easier to read (Aup -> A_{up}, R2 -> Rˆ2 etc)

In conclusion, I think the work is potentially interesting but it requires a substantial amount of work to persuade the community that this method yields more useful information than current fire models. In particular, if I understood the paper right then several key results are more related to agreements between active fires and burned area than with the model. To really make a strong case for the model it has to be tested against other relatively high resolution burned area assessments in other regions, or the authors have to refrain themselves from applying their methods (validated in Brazil) to all of the tropics where fire processes can be very different from the Brazilian case.

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