

Biogeosciences Discuss., referee comment RC2  
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## Comment on bg-2021-102

Anonymous Referee #2

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Referee comment on "Strong temporal variation in treefall and branchfall rates in a tropical forest is related to extreme rainfall: results from 5 years of monthly drone data for a 50 ha plot" by Raquel Fernandes Araujo et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-102-RC2>, 2021

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The authors present a unique analysis of canopy disturbances over the Barro Colorado Island 50-ha plot using a high-temporal density drone dataset. The high temporal resolution of this dataset allows the authors to relate the occurrence of canopy disturbance events to meteorological conditions with far greater precision than was previously possible with 5-year census intervals. The authors (surprisingly) conclude it is not horizontal wind speed, but high rainfall intensity events that cause canopy disturbances. Overall I think this is a very interesting analysis of a unique dataset, but I think it suffers from some analytical pitfalls that limit its utility for forest dynamics. I believe this will be a notable contribution if these issues can be addressed.

General comments:

There are some issues with the statistical analyses that I suggest be addressed (see line comments).

The size distribution of canopy disturbances is important. Table S3 seems like a really key piece of this study and should be in the main text. I suggest the authors include the equations of the distributions in the main text, and calculate some metric of uncertainty for each of the distribution parameters. It seems the lambda and k parameters of the

Weibull distribution change quite a bit depending upon the minimum disturbance size. Although the Exponential distribution does not have the lowest AIC, the parameters don't shift as much.

When calculating the hypothetical total canopy disturbance area from 1 million events, the Weibull and Exponential suggest near equivalent total disturbance area from the (fit 2m<sup>2</sup>) parameter set, but the Weibull only simulates 33% of the area simulated by the Exponential from the ( $\geq 25$  m<sup>2</sup>) parameter set.

I see the authors used Python in the github repo (kudos for organizing the code), but in R it would be:

```
# Minimum size: 2 m2
```

```
# weibull and exponential agree
```

```
sum(rweibull(1e6, scale = 55.860, shape = 1.03))/sum(rexp(1e6, rate=0.018))
```

```
# Minimum size: 25 m2
```

```
# The weibull fit simulates only 33% of the total from the exponential fit
```

```
sum(rweibull(1e6, scale = 6.745, shape = 0.448))/sum(rexp(1e6, rate=0.02))
```

If the end goal is to use these parametric distributions to estimate the total amount of canopy gap area being created, this discrepancy could have important implications for

scaling. It would be nice to see a more thorough exploration of these distribution differences (and maybe check the Tweedie, Negative Binomial, LogNormal, Generalized Extreme Value dist.). I suggest along with the AIC, the log-likelihood also be presented. Apart from these, it would be useful to know which has the lowest mean absolute error between the observations and (simulations) from the fit distributions, and which fit distribution produces the total simulated canopy disturbance area closest to the sum of the observations.

Would the Weibull distribution still be the best fit distribution if the data were not binned (see: White, Enquist & Green 2008 Ecology)?

It would be nice to see a histogram of the canopy disturbances on the raw untransformed scale, perhaps discretized by a few canopy depth classes. I suggest this could be added as a panel to one of the other figures. It would also be nice to see the canopy disturbance shapes in Fig 2 with a colorbar corresponding to the canopy depth. A 2D-density plot might be a way to present the distribution of the canopy gap size and depth.

I question the utility of reporting the canopy disturbance rate with respect to percentiles or thresholds, specific to the Barro Colorado Island met station. I urge the authors to reconsider this analysis with standard units (e.g. wind speed in  $\text{m s}^{-1}$ , rainfall in  $\text{mm hr}^{-1}$ ). This would make the findings from this study more comparable with other studies, and potentially useful for parameterizing wind disturbance in ecosystem models. On this topic, the max wind speeds in Figure S1 seem low - or is it the 7-day mean of the 15-min maximum? If so, it would be more useful to see the wind speeds unsmoothed because the effect of a strong storm gets washed out when averaged by week or month.

Figure 6c is very interesting and odd. Could the plateau in frequency of the smaller canopy disturbance area be related to a measurement bias? For example, perhaps all disturbances  $\geq 25 \text{ m}^2$  are visible from above the canopy, but perhaps smaller disturbances could be (partially) obscured by overtopping vegetation? Or could the canopy surface model not have sufficient resolution to identify smaller and shallower canopy disturbances on otherwise green canopies? Overall, I am not entirely convinced the plateau in Fig 6c is not

caused by measurement bias.

The following are suggestions that I hope the authors will consider addressing:

P1 L24: Confusing, power function and Weibull are very different.

P1 L26: Check units? (35.7 mm hour<sup>-1</sup>)

P1 L29: "large spatial scales" ~ This seems relative. The spatial scale of this study is akin to the footprint of one MODIS surface reflectance pixel.

L30: confusing wording "linkages to drivers"

L32: I suggest ending this abstract with a more conclusive statement about what was found, rather than a list of (potentially very difficult to accomplish) suggestions for other studies.

L35: The Pan 2013 reference is very old now, and was questionable to begin with. Surely there is a better reference at this point with the many radar/LiDAR RS studies?

L38: Were either of these really theoretical? McDowell 2018 was more a review with a bit of speculation rather than a statement of theory, and Brienen 2015 presented a GAM of some sort for the Rainfor plots.

L40: I suggest placing the citation next to each disturbance (e.g. lightning strikes (Yanoviak et al., 2017)), instead of lumping them together at the end.

L43: I suggest referencing climate change rather than emissions scenarios, which is the driver of climate change.

L49-50: This seems surprising. What about following drought? At the very least, this statement is dependent upon the climate regime of the tropical forest in question.

L59: "easy" -> "easier"

L60: Suggest replace "stem density" with "stem basal area"

L61: disproportionately useful to ...?

L62: I think it could be argued that windthrown but (temporarily) surviving trees will have reduced lifespans and their necromass is part of the "committed" emissions from necromass.

L65: "don't" -> "do not"

L78: See paper "Death from above" by Deborah Clark. Branchfall might not be fatal to the tree losing the branch, but may be a large driver of understory mortality.

L80: "5 years" -> "five years"

L83: "expect" or "hypothesize"?

L94: decimal degrees might be better

L96: Given that wind is an important part of this study, perhaps some statistics about wind gust speeds could be given (long term mean of max annual wind gust speeds, or some distribution?).

L106: So would a 1 second wind gust of 60 m/s have the same reading as a 14.9 minute sustained wind speed of 60 m/s? This might be an important point for the lack of a horizontal wind speed effect being found.

L126: "images for 1-ha square subplots" -> "images of 1-ha square subplots"

L133: I suggest not using red to delineate the polygon on a green background because red/green is difficult for colorblind people to differentiate.

L133: Minor issue: The Height bar goes from 162-186 m, but this is clearly not tree height. So maybe "Canopy Surface Elevation" would be more accurate?

L149: I am unclear why the 237 day interval was excluded. Was this a data gap?

L160: Why linear regression as opposed to a glm or gam?

L172: with respect to the CDF plot, should this be referenced somewhere?

L175-180: Are the size distributions being fit with all canopy disturbance drop heights? This would be a bit odd, as a canopy gap extending to the ground has different implications than say a shallow canopy gap that only extends 1 meter.

L179: Unclear. Correlation with?

L180: Please include the functional forms of each distribution as equations in the main text. There are multiple forms of the power, and Weibull functions - so this will keep things clear.

L185: I suggest trying to explain this part in more detail. Most readers will not want to dig up the other paper to understand a core part of the methods for this manuscript.



L185: I suggest the log-likelihood also be presented (table 1).

L188: suggest "last three years" -> "final three years of the time series"

L190: I think the standing dead trees may be an issue for relating the tree falls to specific meteorological events. A standing dead tree may take years to fall, so it would be a misattribution to relate its death to a high wind speed event.

L187: Was there any field validation to determine if the branchfall and treefall classifications were correctly assigned?

L199: Is it possible to color code the branchfalls and treefalls (with a legend)? I suggest not using red to both outline the plot and indicate where two disturbances occurred.

L206: "parallel variation" is unclear.

L215: I think the y-axis units are a bit misleading. It looks like the data gaps prevent analysis on a one month time step. For example, there is no way to know the monthly canopy disturbance rate around 2016 because the sampling interval is several months. Perhaps it is better to report the sum of disturbed area per sampling time block?

L223: Why not present the early/late Dry season? Or better, put all in the same figure. I do not think the p-value adds much value here and its calculation is not specified in the methods. Considering the skew in the data, the varied sampling intervals, and the intrinsic spatial dependency in the data, reporting simple p-values from (t-tests?) might not be statistically appropriate.

L235: Linear regression does not look like the right analysis for overdispersed data. It looks like the one large outlier exerts a lot of leverage to drive the  $r^2$  metric. I suggest the authors consider modeling this with a negative binomial or Tweedie generalized linear model.

L253: Why not use color in panel a?

L254: Is the correlation with height drop and canopy disturbance area, or the log of canopy disturbance area? I suggest the authors use a generalized additive model to overlay the trend on the points.

L255: Should the exponential fit also be plotted?

L282: It might be worth noting that the horizontal wind speed was measured at ground

level, and therefore might not really be representative of canopy surface wind conditions.

L295: High rainfall (mm), or high rainfall rate (mm hr<sup>-1</sup>)?

L327: The domino effect of falling trees causes spatial autocorrelation (effectively inflating

sample size), which ideally would be addressed in any of the regression analyses. In practice, this is difficult and would probably not change the conclusions of the manuscript.

L338: I am confused by what is meant by self-organization here. The wind storms are an exogenous force.

L341: detection frequency -> measurement bias?

L351-354: I suggest splitting this very long sentence in two.

L367: I am not sure about calling these 'rainfall events'. I suggest swapping "extreme

rainfall events" with "extreme storms". The trees are not falling down because of hard rain, they're falling because of the strong wind gusts accompanying these storms. The met station may be able to accurately measure rainfall intensity, but I think it's unlikely a 15-minute interval is going to be able capture the difference between sustained high wind speeds and very short gusts, so I think calling this "rainfall events" might be misattributing the cause to rain instead of wind.

L374: This is a unique and valuable dataset. Will both the raw and processed data will be published in the Figshare repository?

Fig S1: This is very surprising, the max wind speed never got above 7 m/s?

Fig S2: Is the canopy gap disturbance counted as one polygon, or three separate polygons in panel F? Could these types of decisions have much influence on the distribution size fitting?

Fig S3: Why not present this as a color coded time series for each year of the study?

Fig S7: I suggest adding the fit parameters for each distribution to the figure.