

Interactive comment on “Effects of topography, soil type and forest age on the frequency and size distribution of canopy gap disturbances in a tropical forest” by E. Lobo and J. W. Dalling

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GENERAL COMMENTS This contribution examines the distribution of gap sizes and gap area fraction across Barro Colorado Island (BCI) in Panama and assesses possible drivers of variation in these gap metrics. The authors, in general, use adequate methods, and find significant, and in some cases, unexpected results. It seems that slope is the major correlate of variation in both metrics, but not in the manner expected: steeper slopes have a higher proportion of small gaps and lower total gap area fraction. The authors attribute this to the rather shallow range of slopes present on BCI. Stand age, soil type, aspect, and exposure have varying effects on the gap metrics. This

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is one of the first studies to assess causes of variation in the distribution of gaps at the landscape scale using remote sensing in combination with detailed on-the-ground data of the potential causes and thus would represent a useful contribution to the literature. However, a few issues, outlined below, need to be addressed prior to potential publication.

SPECIFIC COMMENTS >Pg 7106, line 25: While I agree that BCI being unaffected by cyclones allow it to be representative of large areas of lowland tropical forest, I think the caveat should be added that it is an island in the middle of a lake, and thus likely experiences a very different wind regime than most other tropical forests.

Further, are we sure that the taxa that occur on BCI are adapted to deal with such a wind regime, which is relatively novel for them? I am not suggesting that other tropical tree communities are at equilibrium in species composition with respect to their environment, but BCI definitely is not. For example, see Feeley et al. (2011. *Ecology*. 92(4): 871-882), which shows directional changes in the composition of the 50 ha plot. It would be interesting for the authors to briefly discuss their study and results in light of that paper, either here or in the discussion.

>Pg 7107, line 20: Can we assume that the soils correlate fairly well with the geomorphological units? I see now in section 2.3.3 that soil type was actually the variable used in analyses, rather than geomorphological unit. Perhaps this should be briefly mentioned here.

>Pg 7108, line 5: Just to be clear, this reviewer does not know much about LIDAR methods, so cannot evaluate the validity of this part. It sounds good!

>Pg 7112, line 5, pg 7114, line 25, and Figure 5: Aspect is a circularly distributed variable (e.g. values of 1 and 359 are quite close to each other), but it seems that it was modelled as a standard continuous variable. Based on Fig. 5, it looks like no matter how it is modelled, it probably won't be found to have a significant effect on lambda, but it clearly affects gap fraction. While fitting a polynomial works, many other

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options could be considered and may be better (e.g. check out the package circular in R). These alternative modelling approaches might also be helpful in a multivariate analysis of gap area fraction that I advocate (see comment immediately below).

>Pg 7112, section 2.4.2 and pg 7116, section 3.2: Why was a multivariate analysis not conducted for gap area fraction? It would be good to assess potential interactions between the explanatory variables for this response variable as well. This needs to be done.

>Pg 7116, line 20: Don't forget that in many gaps, regeneration comes from damaged trees that survived (i.e. resprouts).

>Pg 7117, line 25: Perhaps it could also be that, on steep slopes, trees fall before they can get too big, thus preventing big gaps (same goes for gap area fraction in section 4.2). Perhaps this could be checked by assessing a relationship between tree DBH and canopy height with slope.

>Pg 7118, lines 14-15: This could be explained in a little more detail. Perhaps soil type did not have a strong effect because it covaries somewhat with forest age (based on looking at Fig. 1, the authors' comment here, and the comment in line 25 on this page). That age stayed in the overall model and soil type did not does not definitively negate a role for soil type (i.e. because of possibly significant covariance). As topography (other than slope) had little effect on lambda on its own, I am less worried about its covariance with forest age.

>Pg 7119, line 20: Were not the lake vs. terrestrial exposure analyses intended to help sort this out? Results concerning these two measures were not given for the gap area fraction.

TECHNICAL CORRECTIONS >Table 1: 1st line of Morphological features for Brown fine loam. Presumed typo: 'with little or clay increase...'

>Fig. 4: I think the lettering in the legend got mixed up here. Shouldn't it be a,d,g that

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give the slope frequency; b,e,f that give lambda relationship with slope; and c,f,i that give the relationship between gap area fraction and slope?

>Fig. 6: I think the lettering in the legend got mixed up here as well.

>Pg 7120, line 6: insert comma after results and before variation

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