

Interactive comment on “Structural effects of liana presence in secondary tropical dry forests using ground LiDAR” by A. Sánchez-Azofeifa et al.

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Comments to the Author

In their Discussion paper Sánchez-Azofeifa et al. present a study on the effect of liana presence on forest structure in secondary dry forest in Costa Rica. They use the VEGNET ground LIDAR system to study the vegetation structure of forest stands (with and without lianas) along a successional gradient. The topic of the paper is important, the idea has merit and I believe that terrestrial LIDAR scanning has a lot of potential in this context. However, the study setup is rather limited, the methods and analysis are not well presented and some important information is missing. The analysis is too weak to support the conclusion that the authors make. I can therefore not recommend

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this manuscript for publication in Biogeosciences.

Response: We thank referee 1 for the comments. In effect LiDAR has a lot of potential to identify lianas and vegetation structure in forest of different stands, but few studies have been conducted assessing the potential of ground LiDAR to differentiate among stand ages or stands that differ in composition of functional groups such as woody vines, thus where the contribution of the paper lies.

We believe our data shows solid evidence of the capabilities of LiDAR technology and its measured variables (PAI, RG) to detect and differentiate forest with lianas and forest with no lianas, over a successional gradient. This evidence has positive implications for the use of ground remote sensing platforms in long term forest monitoring, and for the future use of airborne point cloud data to detect liana infestation over large areas in tropical dry forests.

After addressing major comments from referee 1 below, we believe our paper deserves publication. Moreover, as of today, no studies have been conducted in tropical dry forest environments to understand the effect that lianas, from a structural point of view, can have on succession, and our study is intended to provide new insights on how to understand these processes using remote sensing

Major comment 1 There is some important background information missing on the setup of the study. In the first place it is not clear why some stands have lianas and others not. Have they been artificially removed? Or has the presence/absence a natural cause? It is important to describe why lianas are present or absent. The factor that is determining the presence of lianas (e.g. the soil) might also influence the forest structure independently from the lianas. . . This would mean that the observed patterns might be caused by other factors than lianas.

Response: We have not conducted any removal of lianas. The study area is part of a long-term project conducted by the Tropi-Dry Research network at Santa Rosa National Park (<http://tropi-dry.eas.ualberta.ca>). Thanks to our previous work there, we

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have been able to identify areas within the park with and without lianas that we were able to use for the current study. We do not have soil data on the plots sampled with the Vegnet to disentangle the different factors influencing the presence of liana in the region. However, we considered this does not necessarily preclude the understanding of our findings, as one of the goals is to evaluate whether terrestrial laser scanning can be used to evaluate whether liana presence could cause a change in the successional trajectory.

Major comment 2

- The stands in the study area have been classified in different age classes. This classification is done in a rather non-transparent way (page 17158, lines 14-23). Multiple criteria have been used for this classification, but it is not clear how much weight is given to each of the criteria. And in addition, forest structure appeared to be an important factor in determining the age classes! This is not really a good setup to test differences in forest structure afterwards.

Response: The stands in the study area have been classified in a transparent way. We mentioned on page 17158 that sites were classified based on land use history, age since land abandonment, using remote sensing data and field inventories. We provided a brief description because we cited previous studies where the methodology of site selection is explained. Nonetheless, we can expand the description of the study sites for the next version of the manuscript to improve clarity. Here, it is also important to highlight that the criteria to select plots were the same for sites with lianas and no lianas, so there should not be any biases for comparison across stand ages. The selected plots and their characterization are the core elements of several papers published over the last 15-years such as the papers by Arroyo-Mora et al (2005), Kalacska et al. (2004, 2005), Sánchez-Azofeifa et al. (2009 Forest) and Castillo et al. (2012), which are currently cited in the manuscript.

Major comment 3

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The story needs more focus. Currently the results are describing both the impact of succession and the impact of lianas. However, none of both topics is well developed. Maybe the authors should choose one of both topics to be developed in detail.

Response: We disagree with the reviewer in that the paper needs more focus. The paper deals with both succession and the impact of lianas. If the editor considers it is needed to broaden the introduction, we can do that in the next version of the manuscript. However, we consider the scope of the paper is very clear, as we are using our sampling design with plots of lianas and no lianas to evaluate whether the presence of lianas, over a chronosequence, could be detected by the terrestrial laser scanning. Our method is also intended to get insights about the role of lianas in succession, by assessing whether lianas could be modifying the trajectory of succession in tropical forests as previously hypothesized by Schnitzer et al (2000), Paul & Yavitt (2011). This is another contribution of the study, as research on lianas in secondary forests is rather scarce (see Paul & Yavitt 2011, Duran & Sánchez-Azofeifa 2015).

Major comment 4

- In that respect I also wonder if the number of studied stands is statistically sufficient to study both patterns (succession and liana presence) simultaneously. The studied stands differ in a lot of aspects (age...), I doubt if they can be really considered as repetitions.

Response: We disagree with the referee in the sense that these plots cannot be really considered as repetitions since they have been extensively studied and information from these plots has been already published on several ecological journals. In fact these plots have been monitored every year over the last 10-years as part of our long-term studies at the Santa Rosa National Park. We will clarify this on the next version of our paper. Furthermore issues such as age, structure and composition have been already published in several journals as indicated above. As per the number of plots, the small number is similar to other studies from other tropical dry forests in the Americas,

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specifically those studies conducted for example by Balvanera et al. (2002) in Mexico. We will acknowledge this in the manuscript specifically in the discussion and also on the conclusions of the paper.

Balvanera, P. E. Loot, G. Segura, C. Siebe, A. Islas. Patters of Beta Diversity in a Mexican Dry Forest. *Journal of Vegetation Science*. 13(2): 145-158.

Major comment 5

- Why are the VEGNET test measurements done at night? (page 17160, line 17) are the actual measurements also done at night? Why?

Response: All measurements conducted by the Vegnet, are performed using a visible wavelength of 635 nm. Measurements must be conducted at night, in order to avoid sunlight irradiance interference at the same wavelength with the VegNET laser light. Night time measurements ensure an optimal environment for recording all returns and avoiding interference. We will clarify this in the paper.

Major comment 6

- The RG metric is introduced technically in detail. But for me it was not clear what the actual meaning of this metric is in terms of forest structure. It is not clear why the authors hypothesize that RG would increase with succession but not in case of liana presence. Is RG used here mainly a proxy of biomass or as a measure of vertical canopy structure? It would be interesting to relate the lidar data to actual biomass data (based on inventories) of the stands.

Response: The idea of conducting a linkage to the site biomass to the vertical profiles of the Lidar is very interesting but we think that it is out the scope of the paper. In addition, to conduct a vertical study of the vertical distribution of biomass is something that we have not done; therefore we cannot integrate this component in the current the study. In the case of our scope and our data, Plant Area Volume density (PAVD) provides a measure of the area covered by photosynthetic and non-photosynthetic mate-

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rial present in a given forest height. The PAVD vertical profile (Figure 2) then describes the distribution of the plant volume in the forest. We then use RG as a single measure to describe the distance between all PAVD values and the centroid of the vertical profile. This single measure provides a proxy to general forest volume. The greater the overall height, stratification and biomass of the forest (as a function of succession), the larger the RG value. The relation between RG and succession is evidenced in the RG values obtained in Early, Intermediate and Late sampling plots with no lianas (Table 1). The presence of lianas affect forest structure, and this is registered in the distances between PAVD values and the centroid of the vertical profiles of liana infested plots, therefore affecting RG values. We will insert a better explanation of this hypothesis in the next version of the manuscript.

Major comment 7

- Related to the comment above, it is a bit confusing why PAI as a function of RG is studied in order to study successional trajectories (fig 3). Why where the indices not studied along an axis of stand age? What does the PAI-RG relation actually mean?

Response: Forest structure changes as a function of succession. PAVD and RG are proxies of forest structure. See explanation above, in t, in the response to comment 6.

Major comment 8 - On page 17163 (line 17) the authors observe that there is no significant trends in fig 3 for stands with lianas. And that there is a trend for stands without lianas. However the liana stands are only available in intermediate and late succession: : : I suspects there will also not be a significant trend for the non-liana stands if you only consider the two oldest succession stages.

Response: We assess the pathway by using all the available plots, and we found that the regression for the plots where lianas are absent show a statistically significant trend ($P < 0.05$), while the trend with plots where lianas are present do not show any significant trend. We can improve clarity in this point by adding the regression equation. What is interesting here is that for the plots with lianas despite the regression is not

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significant; the slope is negative, while for the plots without lianas the slope is positive and significant. We acknowledge our small sample size, and as stated above we make suggestions rather than stating that our data confirms that lianas in fact are modifying the pathway. We can add the equations, P-value and R square for both trends to improve this section.

Major comment 9

In the discussion the authors state that they “evaluated the role of VEGNET as a methodology to assess. . . However, the presented study is not an evaluation of the VEGNET tool. An evaluation of a tool should include a comparison with other methods, or at least one other method. And this is not the case in this study. The VEGNET methodology has probably been tested/evaluated in other studies, but this study should in my opinion not be presented as an evaluation of the VEGNET methodology.

Response: We appreciate the comment. We will remove it rephrase this in the discussion, to specify that the laser terrestrial scanning was useful to detect changes in structure among plots, rather than the results are showing the utility of Vegnet to assess successional changes.

Major comment 10

- The authors conclude on page 17165 (line 11) that their results suggest that lianas may be modifying the successional path for these forests. Although I believe that this phenomenon is very likely, the presented results are not strong enough and the setup is too limited to support this conclusion (see my comments above).

Response: We acknowledge our small sample size, and the limitations of our study and that's why our conclusions in the manuscript are stated as suggestions and hypothesis. The fourth paragraph in the discussion suggests that lianas may be modifying the successional pathway, and we state the reasons of why this may be happening, and as mentioned in the introduction this is a hypothesis previously formulated in the liter-

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ature. We could moderate the writing in this part if the editor considers it necessary to improve clarity.

Minor comments:

- Also refer in the introduction to the recent paper of van der Heijden et al. 2015 in PNAS

Response: We will include this paper in the revised version of the manuscript.

Interactive comment on Biogeosciences Discuss., 12, 17153, 2015.

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