

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

### **Anonymous Referee #1**

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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 this manuscript for publication in Biogeosciences.

Major comments:

- There is some important background information missing on the setup of the study.  
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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.

- 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. . .

- 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. 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.

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

- 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.

- Related to the comment above, it is a bit confusing why PAI as a function of RG is

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studied in order to study successional trajectories (fig 3). Why were the indices not studied along an axis of stand age? What does the PAI-RG relation actually mean?

- 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 suspect there will also not be a significant trend for the non-liana stands if you only consider the two oldest succession stages.

- 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.

- 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).

Minor comments:

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

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