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Interactive comment on "Patterns in Woody Vegetation Structure across African Savannas" *by* Christoffer R. Axelsson and Niall P. Hanan

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We thank the reviewer for constructive feedback on the methodology. Here are our responses:

Specific comments:

1) Section 2.1 of the paper should include a proper definition of the sampling universe as well as a description of the sampling frame. The section lists sampling criteria, but these seem to address a pragmatic approach for dealing with issues that occurred while preparing the data set rather than a design approach targeting the intended population.

Response: We have modified the manuscript to clarify that the sampling frame for the

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analysis was sub-Saharan African savannas with a minimum of anthropogenic disturbances. We also added that within-image site-selection followed a systematic sampling approach and was guided by a 0.04° longitude/latitude grid.

2) Methods section 2.3 (Crown delineation) contains discussion (lines 135-139 and 144-146) which is improperly placed in my view. The methods section should just describe the methods, as used. Alternative methods can be described in the introduction while potential flaws in the results caused by the used methods should be described in the discussion section.

Response: These lines describe how this delineation method relates to previous delineation approaches, and concerns related to this and other delineation methods. As the purpose is to describe the method and its strengths/weaknesses, we do not think these sentences are inappropriate for the Methods chapter. We understand the reviewer's concerns, but feel that it is better to keep the description of this method in a single section instead of dividing it between Methods and Discussion.

3) Same section (lines 148-150): Is it really enough to balance rates of falsely divided and falsely grouped crowns? I guess one wants to minimize those errors. How was this achieved?

Response: This is a general statement about the consequences if crowns are systematically falsely divided or falsely merged, which is an issue with all crown delineation methods. Originally, fine-tuning of the delineation method was done by visual inspection of the crown polygons overlaying the high resolution imagery. With the added Appendix, we also refer to the validation of the Kenyan sites.

4) Same section (lines 150-151) The authors seem to have validated the results by visual inspection which showed the results to "look realistic". That is by no means a scientific validation!

Response: Here we will refer to the validation of the Kenyan sites, which is a quanti-

tative validation. The visual inspection does, however, also play a role since it helped us determine that the delineation was consistently executed across all landscapes. In many scenes, individual trees can be identified from visual inspection.

5) The validation exercise described in the appendix concerns a small dataset in Kenya. In the sample, common large umbrella thorn acacias were claimed to be overrepresented and given their problematic behaviour in determining crown size and crown density they were excluded when computing R2. So, how can the results from this exercise be generalized to the entire dataset?

Response: The large majority of our sites do not contain this type of trees with particularly large and spread out crowns which are relatively rare across all of African savannas. Since all four sites in Amboseli were dominated by them, we determined they were overrepresented in the field data. We acknowledge that the delineation method underestimates crown sizes for trees with large spread out crowns, and will mention this problem when referring to the appendix.

6) It remains unclear to me how vegetation periodicity was characterised. In line 185 (section 2.5), "spotted, labyrinthine, gapped or banded patterns" are briefly referred to (between brackets). This seems to suggest periodicity was identified on a single image. Since periodic behaviour plays an important role in the analyses and conclusions, it is necessary to explicitly describe whether or not multiple images were used and to be very clear on its characterisation.

Response: We will clarify how sites with periodic patterns were identified. We have also added images of sites with periodic patterns, as suggested by the previous reviewer. We visually inspected each site individually and determined if it had a periodic vegetation pattern. This is straightforward for clear cases of banded and spotted patterns. There were cases where it was less straightforward, e.g. weak gapped patterns, and for those we tried to be consistent with the assessment.

7) The analysis employs a mix of resolutions (support sizes) but I am unsure on how

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these were integrated. It is mentioned that the TRMM data were resampled by bilinear interpolation, but for the other data sets it remains unclear to me at what resolution the analyses were performed. For example, were average slopes over the 240 x 240 m2 regions used or were patterns within the 240 m cells also considered?

Response: We only considered the center point of the sites and extracted raster values based on nearest neighbor in all cases except the TRMM where we used bilinear interpolation. We will clarify this in the text.

8) There are several changes of tenses throughout the text (also mentioned in the review of Penny Mograbi). My understanding is that the present tense is reserved for presenting either well-known facts or statistical inferences from sample statistics that are generalised to entire populations. However, in this paper no formal hypothesis testing is performed; all results should thus be in past tense since they concern the used (sample) data set.

Response: We have modified the text to correct tenses.

9) The previous comment points to a major weakness of the work: One might doubt whether the analyses support drawing general conclusions about woody vegetation properties in response to environmental variation in African savannas. The sampling method would only allow to do so under the assumption that the sample is representative. This should then be explicitly stated and supported by proper argumentation. Furthermore, at some places the authors acknowledge that the used data are not error free. This implies that we are uncertain about the true environmental properties and the woody vegetation characteristics. The question then arrises whether the observed differences or relationships exceed uncertainty bounds. How did the authors decide whether an effect was "clear positive", "weak" or "absent"? The inference mechanism should be described.

Response: While the sample set was affected by various factors, including image availability, cloud cover in images, and anthropogenic disturbances, we do not see any of

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these factors creating a bias when relating the woody structure estimates to environmental variables. The inferred “clear positive” or “weak”, relationships were based on the trends in the partial dependence plots. We agree that interpretation of results from boosted regression trees relies to some extent on a qualitative assessment of these trends. When describing the methodology we have added: “The influence of individual predictors was estimated from their relative importance in the BRT models, and the directions of relationships were inferred from their trends in partial dependence plots.”

10) The grey dots in Figures 5 and 6 are claimed to represent fitted values for each of the 876 sites considering a single environmental variable with the other variables fixed at their avarages. For MAP, rain seasonality, sand content and slope this seems to indicate erratic behaviour at very minor changes of the environmental variable under consideration. For "fire frequency" a vertical banding pattern is observed which suggests the BRT produced multiple outputs for the same fire frequency. How come? This pattern should be explained!

Response: Only the partial dependencies (red lines) account for the average effect of the other variables. We will clarify the text and avoid the term fitted function since it might cause confusion. The erratic behavior (overfitting the data points) is often seen in partial dependency plots of boosted regression trees and is perhaps a weakness of this method. It means we need to focus at the main trend of the response function. Many sites had the same fire frequency (based on the number of fires in the period 2001-2015) which causes the striped pattern. The fitted values are model predictions based on all predictors and will thus vary. We have updated the text to clarify this.

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