

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

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This work reports on the preparation of a sample of 876 records with woody vegetation properties and an analysis of differences in those properties in response to environmental variation in terms of soil characteristics, rainfall quantity and distribution and slope. The authors are to be commended for their great effort of generating a considerable and interesting data set based on high resolution image data using a generally well-described methodology. It would be great if the data were made available to other scientists.

I am less convinced of the rigor of the subsequent analyses, though. Given my background, I have focussed my review on methodological issues.

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.

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.

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?

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!

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?

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.

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7) The analysis employs a mix of resolutions (support sizes) but I am unsure on how 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 m<sup>2</sup> regions used or were patterns within the 240 m cells also considered?

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.

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 arises 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.

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 averages. 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

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pattern should be explained!

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