

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

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### General comments

This paper explores abiotic drivers of woody vegetation density and crown size across African savannas. Here, woody cover is seen as a function of tree density and crown size. The authors find that increasing woody cover results in the literature are attributable to increasing crown size rather than increasing tree density, which is a controversial topic and would contribute significantly to terrestrial carbon pool debates, as well as the bush encroachment sphere. The authors also assess abiotic drivers of regular vegetation patterns and the possible ecohydrological processes behind the patterns.

This was an interesting paper to review – interesting content and thorough methods

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section. The content is appropriate to the scope of BG and does contribute to land-vegetation interactions in general.

The paper was mostly well-written and I noticed almost no errors in text. The discussion was particularly well-written, with interesting links to other studies and well-thought out exploration of explanations for the relationships between woody vegetation properties and the abiotic template. The data processing and methods section were detailed and clear, with parameters, limitations and steps clearly presented. Most of the technical considerations/challenges/limitations that occurred to me after I read the abstract and got a first broad overview of the methods were explicitly and thoroughly dealt with in the methods section. The authors are well aware that the crown size/density metrics are probably not that accurate, but that there is still merit in assuming that the error has been propagated consistently over space and it is in the relative differences that insight into vegetation patterns are shown. That said, I have a few points:

1) For me, the hinge-point of this study's methods are that tree crown center points are derived from relative NDVI differences. While this might be valid (and from an eye-ball of Figure 3 it seems to work), there are no references discussing this method. I would suggest this method be backed up by previous references. I would also like to know what the limitations of this method are. I would also like to see some form of validation stats (e.g. Kappa) for the accuracy of the woody cover/forest mask, crown size, crown density outputs. Perhaps some test sites could be manually evaluated and compared with the semi-automated approach. You mention “uncertainty in the accuracy” of your metrics on ln 151 so perhaps the authors have already performed an error test and haven't reported it? It would be interesting for readers to know how well these methods performed (and it would increase your citations!)

2) The counterpoint to well-written discussion, is the introduction is not the same quality and reads like a rough draft. The introduction lacks the key “introduction linkage” points made both in the abstract and the discussion. The introduction and discussion should book-end the findings, and the introduction was inadequate in this regard. While the

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content for the motivation and aims for the study were available if one was looking for it, they were not presented in a clear flow and it felt weak. There were also several lines that would be better suited in the methods section. I have made suggestions on how the introduction could be improved below in the specific comments.

3) I intuitively felt an important part of this study was mentioned in the discussion for the first time. Ln229-230: The results of your study suggest that increasing woody cover trends from multiple previous research articles are related to increasing crown size, rather than increasing density. This is huge and forms the central finding but is only mentioned once! There are important implications for global carbon cycles (see Poulter et al. 2014 Nature and Liu et al. 2015 Nature Climate Change), bush encroachment etc. This would be a finding that other scientists would explore further. You need to develop this theme. I want to know more!

4) PVPs: This aspect of your study is mentioned briefly in the beginning, forms a large chunk of your results and more than half of your discussion. This leaves the manuscript unbalanced and the reader is left wondering why PVPs are so important and why it was decided to explore it so heavily. If the focus is on PVPs, that needs to be reflected in the abstract (it isn't mentioned once until the end) and introduction (it is mentioned as a phenomenon but no why they are worth exploring or what the question is about them. While I am no specialist in PVPs, I would also suggest that no lit review section of PVPs is complete without a mention of Max Rietkerk's work, particularly Rietkerk & van de Koppel 2008 Trends in Ecology and Evolution. I was also missing mention of Bromley et al. 1997 Journal of Hydrology which specially mentions West African PVP's and 'tiger bands'.

#### Specific comments

1. Title: The title has the word "Savannas" in it. Yet, later on the authors mention 'drylands' (Ln 40) which contains large areas not typically counted as savannas. In Figure 2 the vegetation area of interest is labelled 'rangelands', as well as in Ln 87.

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Why use Ellis & Ramankutty's anthropogenic biome for an abiotic-vegetation study on savannas when you could use a climatic-disturbance based biome which defines savannas? This study does not consider the human component. Whichever term the authors choose require clarification and should be used consistently. Why not provide a map of the savanna extent? For example, the 'rangeland' areas in Morocco, Algeria and Libya are traditionally not considered savannas. The authors could use the extent used by Sankaran et al. 2005 Nature as it is widely accepted. It could even be interesting to see the relative differences in abiotic influences on your sites divided into the "stable" and "unstable" savanna categories, if they agree with Sankaran et al. Just a thought. The other issue with the title is the word "structure" when your metrics measure woody cover and tree density. My understanding is that 'structure' implies height metrics or SCD's.

2. Abstract: PVPs not mentioned until Ln 28. They need to be introduced earlier if they are the focus of the study.

3. Ln 12-15 Very concise and clear summary of your introduction and aim in these abstract sentences. This idea also needs to be explicitly stated upfront in the introduction and well referenced. I probably lost the impact of this point in the introduction because of poor flow and structure.

4. In33-34 "While humans often play a dominant role in many systems..." I did not understand the point of this statement and it feels out of place here. Either remove it or expand on it.

5. In 38 "...future stability and productivity..." 'stability is a loaded term in savanna literature. Perhaps rephrase this. This idea would form a nice link to bring up again in your conclusion to tie your manuscript together.

6. In 44-45 Great to bring up fire's influence. Recent work by Smit et al. 2016 Journal of Applied Ecology show that SCD's are affected by high intensity fires, including tall tree (large canopy size?) loss. I understand that fire intensity can't be ascertained with

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MODIS data, but it does need to be mentioned that intensity plays a role.

7. In 40-50 This may be a personal style preference, but worth a mention (word limit permitting). The first half of the paragraph lists abiotic driver influences on woody veg properties, the 2nd half specifies how these drivers can influence the specific metrics of the study (individual: crown size; population: crown density, woody cover) and provide an example of how the same woody cover can have different ecosystem functioning. This is a natural flow, but I wanted a bit more on both topics. Could these two sections be expanded to their own paragraphs?

8. In 64-65. Please reference mention of vegetation bands. Rasmussen? Bromley?

9. In 69-70. Both studies the authors cite for “African savannas” are from W Africa. Could other African studies be included?

10. In +-73-81 This paragraph seems more suited to the methods section. Perhaps you could reduce these details to a sentence or two, linking the methodological processes to the general aim, rather than mention details here and then details again in the very next paragraph? Figure 1 should also only be mentioned in the methods.

11. In 80 The PVPs identified in the study sites, were those sites derived from the literature or were they found by the authors. Please mention this. If the latter, it would be nice for the reader to have image examples of the different kinds of PVPs. Are they very easy to spot?

12. In 90. Does this mean spring in the northern and southern hemispheres? Could you be more specific?

13. In 91-92 It's not necessary to mention that another on-going study influenced this one's parameters unless some of the data from that study are included in this paper. Perhaps leave this out.

14. Methods: The sections on preprocessing and classification were thorough. Thanks!

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15. In 112-115 This section isn't really necessary for the article, although I do understand the feeling of wanting the time and monumental effort taken for analyses to be recognised by the readers!

16. In 132 Is there a reason for the 40 m limitation to crown size?

17. In 143-154 This is a well-needed section and I like that the limitations are mentioned. However, it needs bolstering with supporting literature. A quick google search has shown that crown delineation techniques with multispectral, high resolution satellite data exist and it would be useful to see a comparison of the trade-offs to back up the method you have used. This ties in with my request to see support for the NDVI crown centre identification method. Accuracy statistics would be a useful addition here.

18. In 172 Was a 20 m cut-off used for Ripley's L because that is where the sill occurs on all the curves in Figure 5?

19. In 192 Mentions Figure 4. Figure 3 was never mentioned. Please include it where relevant.

20. Results: The subheadings seem strange. You have one sentence on vegetation characteristic differences followed by a subheading “3.1. Mean crown size, density and woody cover”. Surely the previous paragraph (of one sentence) fits into this subheading? Or was the subheading meant to be related only to BRT results?

21. In 194 It was not clear to me from Figure 4 that arid sites had higher levels of aggregation. Perhaps because the colours did not come out well in that panel?

22. In 197 “Woody cover and mean crown size both had strong relationships with the local environment...” What factors in the local environment? Could you be more specific?

23. In 209-211 Nice findings. The sentence that starts “ These are factors that influence ecohydrological processes...” at the end of the paragraph is better suited to the discussion section and needs to be referenced.

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24. In 219. "...aggregation reaching a minimum at around 25 meters." Consistency with meters/m. This sentence also needs a figure reference at the end. Figure 7?

25. In 219-220. This is a discussion point.

26. "Heading 4.1. Dividing woody cover into density and crown size components" as well as In 226-228 are concepts that should be addressed in the introduction. This is a key part of what makes this study novel as most research deals with woody cover without addressing density/crown size differences. These lines are the coherent aim and motivation I was missing in the introduction.

27. In 229-231 Great finding! Make a meal of it. The authors need to discuss this vs. bush encroachment findings in the literature.

28. In 243-245 Low woody cover unrelated to rainfall seasonality. This section needs mention of the large role of disturbance agents in "unstable savannas" (sensu Sankaran et al. 2005). The authors do mention elephant impacts in a sentence, but this needs more unpacking and forms part of the caveats to this study's results as biotic disturbance was not included. Together with acknowledging effects of fire intensity on SCDs.

29. In 254 "In accordance with previous literature..." There are no references at the end of this sentence. Which literature?

30. In 270 "...and short-range facilitation through modified microclimate close to nursery plants" needs a reference.

#### Technical corrections

1. In 20-30 Be careful of the change in tenses. Generally, methods and results should be reported in the past tense.

2. Journal editor preference, but Figure mentions should normally be in parentheses, rather than mentioned in the sentence.

Eg. "Frequency distributions of the four woody properties, separated into three rainfall

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categories, are shown in Figure 4." To "The more arid savannas (<400 mm/year) typically feature smaller crown sizes, lower crown density and woody cover, and higher levels of aggregation than sites in the wetter categories (Figure 4)."

3. In 118 Insert spaces between "240x240" and shouldn't 'meter' be 'm'. Be consistent throughout the manuscript. Either change previous mention of 'meter' to 'm' or vice versa.

4. In 124 'ID' or 'point' rather than 'id'

5. In 241. This is the only occasion a discussion sentence refers to a results Figure. Either include more links to the results where appropriate, or remove this one. Consistency. e.g. In 228-229 could also use a figure reference?

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