

Interactive comment on “Structural, physiognomic and aboveground biomass variation in savanna-forest transition zones on three continents. How different are co-occurring savanna and forest formations?” by E. M. Veenendaal et al.

Anonymous Referee #2

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Summary & Major Comments

In this manuscript authors present analyses using a huge field-based dataset of vegetation structure and composition from savanna-forest transition zones in Africa, South America and Australia. The authors attempt to frame their analysis in terms of the ‘alternate stable state’ paradigm that has been popular in recent global and regional savanna analyses. While the underlying database collected by this large team of well

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respected authors appears to be unique and impressive, the presentation does not do justice to those data and the analysis in its current form contributes little or nothing to the body of savanna or ecological theory.

In general, the paper is poorly written and presents the authors' data in a rather random and barely comprehensible fashion. For this undoubtedly huge and impressive dataset to have the impact it deserves the paper at a minimum needs extensive editing to improve the English and should be greatly shortened to focus on a specific message. At the moment we have a large number of data-plots that lack a coherent 'story' linking them together. The analyses that purport to relate to alternate stable states in savannas provide, in my opinion, little or no new insight: the data are not presented in a way that informs alternate state ideas (see Points # 1, 2, 5-8 below).

I would encourage the authors to rethink what it is they want to say, and how they can best say it, with the potentially transformative data available to them. I hope my comments below will provide some useful suggestions to that end, but I have selected examples, rather than list all potential issues, assuming that other issues will be resolved in revision.

SPECIFIC COMMENTS 1. The alternate stable state argument: it seems the authors' main thesis is that, if shrubs are considered part of the woody canopy, then fewer discontinuities will be observed (e.g. the discontinuities observed in the global satellite products which ostensibly ignore trees < 5 meters tall). I understand how this could be a valid and interesting point, but failed to see a clear demonstration of it in the figures presented. I struggled to see how the data presented (or the form in which they were presented) contributed to our understanding of (or detection of) discontinuities related to fire (or other) feedbacks.

2. Figures 2a-e show summed canopy areas for all trees and shrub strata (on the x-axis) against an overlap-adjusted "crown cover" for a subset of strata (on the y-axis). As shown by equation 1, the x-y space in Figure 2a-e is necessarily saturating, and

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I infer that these plots depart from the perfect saturating curve only because one or more strata are omitted, in turn, from the Y-variate in each plot. Thus while I agree it is interesting to see the contribution of different strata to the overall cover, it's not clear why the authors mention 'saturation' as if it was an interesting or unexpected result. More particularly, it is not clear that this plot contributes any new insight into the alternate state 'debate'. Figure 2f shows herbaceous decline with increasing woody canopy cover: not a new result, but interesting nonetheless.

3. "Axylale": I had never encountered this word before, and found only 2 or 3 rather obscure references to it on the web. It appears to be a direct synonym for 'herbaceous'. Since we all understand the latter word, couldn't we just stick with it rather than introducing an intimidating and confusing new word?!

4. Personally I found there were far too many curly Greek symbols. I was not able to retain all their meanings in my head. Granted, that may reflect my failing memory, but I also suspect the authors mixed them up in places: using fewer and more familiar Greek symbols would be less confusing. Defining them clearly – perhaps in a table – and being consistent would help.

5. Figure 5 is demonstrative of the problem I found in many of the plots. It shows the response of smaller (i.e. non upper-canopy) forest and savanna species to the overall canopy cover. The authors interpret that forest species become more common (in the sub-canopy layers) at higher tree cover, while savanna trees decline slightly. However, they fail to recognize that the observed responses may simply reflect the hidden, and probably more causal, correlations between tree cover, species composition and rainfall. Thus I would interpret this Figure to conclude that "forest species become more common in wetter environments", rather than "forest species become more common with increasing canopy cover". A rather obvious conclusion. More generally for this paper, many of the plots are confounded in this way (i.e. by plotting two within-site variables against each other, the effect of rainfall becomes noise rather than signal). In many cases I think plotting structural variables against rainfall would provide more

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insight.

6. Figure 6 is a good example of the previous comment. Why is it interesting to correlate crown cover with canopy height? What is the point, reason, hypothesis, theory here? Fundamentally a single tall tree could occupy a hectare just as easily as could a thousand trees (e.g. sites with and without some recent mortality event). The correlations observed here therefore reflect the correlation between rainfall and cover, where trees are bigger in wetter systems. This would be interesting to show against an index of rainfall, but in my opinion is not interesting to show against canopy cover. All the same comments apply to Figure 7.

7. Plots in Figure 8 are far too small and appears to lack some of the shading referred to.

8. Figure 9. Why are the axes inverted in this plot? Should we infer that water availability is responding to cations? If x and y axes were swapped around, how would we interpret the ~horizontal ellipse that would now be ~vertical?

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