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Interactive comment on “Topo-edaphic controls over woody plant biomass in South African savannas” by M. S. Colgan et al.

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This paper is a very useful contribution. Its most significant findings are hidden in Appendix A and B, where it develops a robust method for estimating aboveground woody biomass in open savannas from LiDAR-derived tree height \times cover, and shows that more complex algorithms are less predictive (I would have expected the use of the Aicke Information Criterion or the Bayesian Information Criterion to be invoked here to reject the less-parsimonious models, rather than relying solely on R^2). This is important because both height and cover can be independently estimated, freeing biomass estimation from the requirement to have airborne LiDAR coverage. The paper goes on (in figure 2) to show that the biomass relation depends on the vegetation composition, a crucial point which is rather glossed over.

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The method is then used to describe the patterns of tree biomass over large landscapes, both spatially in relation to topographic position, and overall. These quantifications are a first for this part of the world, although the patterns they describe are visually obvious rather than a revelation. They note that the tree biomass is high on crests in the granite landscape, declines in the mid-to-toeslope (not exclusively the toeslope, as they state – there are cases where the toeslope may in fact have high biomass) and highest adjacent to the drainage line (a better term than ‘stream’, since these valleys seldom carry flowing water). Given the latter finding, it is surprising that they omit the alluvial soils that support the highest biomass from their topographic discussion, since the water and nutrient dynamics there are somewhat different from the illuvial processes that drive the catena. It is also surprising that the riparian biomass itself is excluded (it is not clear whether it actually is – they do not do a separate calibration for it. The inclusion or exclusion should be made explicit on page 969 line 6) on the grounds that it makes up a small area. They show that on the flatter, clayier basaltic landscapes the overall biomass is lower and increases monotonically from the crest to the drainage line.

Where the paper is on shakier ground is in relation to their claims to have made novel theoretical findings. The paper more than justifies its publication on technical and descriptive grounds, without having to dress itself up in these partly borrowed, somewhat threadbare and out-of-date clothes. The hypotheses (line 25-30 on page 961) smack of post-facto construction. They are either trivial (biomass is high on crests, low in the midslope and high in the valley), or poorly defined (biomass is more sensitive to parent material than rainfall – where there is a co-dependence, this statement is meaningless), or already well-established (lower tree biomass on the basalts is proximally due to fire intensity, and only indirectly due to hydrological or nutrient conditions). The abstract (line 17-19) implies that the latter hypothesis is a novel suggestion on their part, whereas a reading of the literature they cite shows that this hypothesis has been around for years and is widely accepted. They reopen a long-discredited rooting-depth niche separation discussion on page 959 line 22-23. Technically, this is only one form

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of niche separation in savannas argument – others, such as separation on a temporal niche, fare much better.

The findings on soil depth and seepage distance in relation to topographic position (In 21-22 page 962), attributed recently to Khomo (2008) and Levick et al (2010) have been known from about 1990 and 1982 respectively, and are in dissertations by C Chappel and B Olbrich. Grey literature, I know, but nevertheless prior knowledge – and the research was done in the same department where one of the authors worked, so he would have had easy access to it. In the period up to 1994 South African science was deliberately isolated from the global publication mainstream. There is a recent tendency to rediscover findings from that period and claim priority.

The discussion of sodicity (line 29 on pg 961) misses the key point that the area is rendered inhospitable for trees ('down-regulation' is not an appropriate term) by seasonal waterlogging above the impervious sodic B horizons, not by the presence of salts, as could be inferred here.

The statement regarding less competition due to more grazing near 'streams' (970 In 2) is entirely speculative. The comments on the high tree biomass on shales are also entirely speculative and should be dropped (pg 971 lines 8-10). Somehow the shales seem to have been conflated with the basalt landscape (line 2 on page 971)– they are adjacent to it, but quite separate.

The key discussion in section 4.3, in which the authors claim on the basis of their work to have dismissed the argument for soil type as a proximal cause of low tree biomass and support an argument based on fire and herbivory, is weak in several respects. Firstly, the hydrological rooting-depth argument has long been dismissed (see Scholes and Walker 1993 and many authors since then). To erect it as the standard hypothesis through selective citation, only to be able to shoot it down now, is disingenuous. Secondly, the study only incidentally addresses the hypothesis – it includes no analysis of spatial fire or herbivory data, rather relying on inference from LiDAR coverage of her-

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bivore and fire experimental plots set up to test (and refute) this hypothesis years ago. A visual inspection of the plots provides the same insight –it hardly needed a biomass measurement. Finally, the conclusion is overgeneralised. For instance the hypothesis regarding the shrink-swell action of smectitic clays being disadvantageous to trees refers to Gilgai soils, which are not a prominent feature of the Kruger landscape.

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