The authors would like to thank the referee for the very helpful comments and suggestions, and we will take them into consideration in the revised manuscript. We would like to respond to several points below:

"… [The manuscript] develops a robust method for estimating aboveground woody biomass in open savannas from LiDAR-derived tree height x 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 R2). This is important because both height and cover can be independently estimated, freeing biomass estimation from the requirement to have airborne LiDAR coverage."

We used both the R^2 and the standard residual error during the LiDAR model selection process. The AIC or BIC will yield the same conclusions in this case since the number of additional parameters in the more complex models is low relative to the sample size ($k = 3$ vs. $n = 121$), and the performance improvements were negligible even before taking the number of parameters into account. We can include these metrics if you are unconvinced. However, we disagree with the conclusion that the strong predictive power of airborne-measured height and canopy cover "frees" biomass estimation from requiring LiDAR data – it is already free. Allometric equations, applied to field-measured inventory data (e.g. stem diameter, height) yields more accurate estimates than field-measured height and canopy cover, if one's goal is to estimate biomass from the ground. The advantage of airborne LiDAR to biomass estimation is wall-to-wall mapping at the landscape scale, revealing spatially explicit patterns that would otherwise require a laboriously detailed grid of hundreds or thousands of field plots.

"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."

The pattern may be visually obvious on some granite landscapes (e.g. the Skukuza flux tower), but there was little evidence to date that the granite catena pattern continues into the northern, mopane-dominated parts of the park. On the basalts the woody biomass pattern we find clear in the airborne data is much less clear on foot, if detectable at all. We have yet to find mention of this monotonically increasing biomass from crest to drainage line on basalts in the literature on Kruger – if you are aware of any, please provide the reference and we will include it.

"It is also surprising that the riparian biomass itself is excluded (it is not clear whether it actually is)."

Yes, the riparian biomass is excluded. Major riparian corridors represent approximately 2-4% of the land area (depending on what rivers are included) and require a separate calibration that was outside the scope of this study. We will make this more explicit.

"The findings on soil depth and seepline 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… There is a recent tendency to rediscover findings from that period and claim priority."

We cite grey literature where it finds novel discoveries and represents expert opinion, and we will consider the two references you provide above.

"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 shales and basalt are indeed separate - there is a typo on line 2 that is causing confusion; it should read "At low rainfall (MAP of 450 mm yr−1), we observe both very high and low AGB in the Lower Sabie region of the park (Fig. 4b)." However, the following sentence is correct – we found high biomass on the shales, which deserves mention. We already note that the cause of this high biomass requires more investigation, but we will modify line 8 to better reflect this.

"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."

The discussion in section 4.3 does not use the hydrological rooting-depth argument, as a straw man or otherwise (we do mention it in the introduction – it is an overgeneralization to say it has been sufficiently disproven to not include in the introduction). Rather, our biomass dataset suggests that, on the basalt landscapes, edaphic factors are not proximal control on biomass, where disturbances such as fire and herbivory dominate. Although we feel the large area covered by our biomass dataset provides a novel line of evidence, we do not claim to be the first to come to this conclusion, and we will gladly cite "grey references" of novel discoveries and theories (see the introduction for examples). Yet the debate is hardly settled as to what fire-herbivore-soil interactions are the strongest controls over woody biomass on Kruger's basalts. Also, it is not a trivial conclusion since we found the opposite on Kruger's granite landscapes, where herbivory and fire have no statistically significant effect on woody biomass near the exclosures, and topo-edaphic properties are a much stronger correlate to biomass at the local scale (<1 ha). Overall, this section of the paper is intended more to reconcile what is known about fire and herbivore interactions with our results, as opposed to attributing our biomass results solely to substrate type, as if they existed devoid of these disturbances. We are open to suggestions to better achieve this goal.