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Interactive comment on "Rapid response of habitat structure and aboveground carbon storage to altered fire regimes in tropical savanna" *by* Shaun R. Levick et al.

Anonymous Referee #1

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– General comments –

This manuscript reports the effects of various fire treatments, including combinations of early/late season burning and variation in fire frequency over more than a decade, on aboveground vegetation biomass and structure in a savanna habitat in northern Australia as quantified by airborne lidar. The results are relatively straight-forward and offer quantitative data that may be useful in future models of carbon dynamics and management of vegetation structure or heterogeneity. There is an interesting spatial interaction effect in which the results of a fire treatment depend on soil moisture and soil depth; the paper would be improved if these results were further explored and

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elaborated upon. I would have appreciated seeing a validation of their model results on out-of-sample data to assess the accuracy of their results and the significance of the soil depth/moisture gradient. Some of the conclusions with respect to what is driving the observed decreases in woody cover with increasing fire intensity (i.e., greater tree mortality or reduced accumulation of woody biomass) appear to be unsubstantiated and require further explanation. Overall the paper, while not especially novel, does represent an important contribution to the literature by quantifying the effects of various fire regimes on 3-dimensional structure and aboveground biomass in northern Australian savannas.

– Specific comments –

Abstract: In the abstract there are inconsistent statements about the temporal scale of the experiment and how to interpret the results with respect to time. On page 1, line 3 the experiment is referred to as 'long-term', whereas on page 1, line 12 the results as said to have occurred over time scales as 'short' as a decade. It is important that the authors represent a consistent message: in their expert opinion, do structural changes occurring over a decade represent short-term or long-term responses? The title suggests that the interpretation is one that these are rapid changes and therefore observing these plots over ten years is not a particularly long time in the savanna tree cover cycle.

Page 2, lines 11-22: While declines in faunal populations are certainly important, I was surprised by the one-sided discussion of negative effects of savanna fires (e.g., the effects of savanna fires on greenhouse gas emissions). I felt this section of the manuscript lacked a balanced discussion of fire as an evolutionary force in savannas that, when suppressed, can have negative effects on savanna flora and fauna. True that some faunal populations are influenced but what about savanna specialists or species that rely on grass cover? Are there no species in these savannas that benefit from fire? Given the global and historical significance of fire in savannas, I advocate for a more balanced discussion of fire as a natural part of savanna landscapes that, when

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well-managed, can have beneficial effects.

Page 2, lines 28-29: Is the significance here only that the approach is novel for savannas? Because lidar has been used to study fire effects in many other systems. Also, why is Smit et al. 2010 and your 2009 paper (Levick et al. 2009) not credited with studying fire effects on savanna vegetation structure using lidar? The Smit et al. 2010 paper was squarely aimed at "...assessing vegetation biomass and structural diversity responses to experimental fires"

Page 2, line 34: aim 1 is somewhat weak considering that lidar has been used successfully to study vegetation biomass and structure in so many other systems. It seems that we already know the answer to the question about reliably detecting vegetation and biomass and structure by airborne lidar is 'yes'. This first aim also puts the emphasis of the paper on methodology and thresholds of detection, which, in my opinion, changes the nature of the paper and requires more of a methodological approach. My suggestion is to leave this part out of future versions and focus on the effects of fire in this system.

Page 3, Table 1: This table legend is incomplete – are these mean fire intensity values? Also, I suggest you include standard errors or ranges for the fire intensity values (i.e., range for E5 and +/- SE for others).

Page 4, eqn (1); is there a different equation for multi-stemmed shrubs? Are they a significant part of the carbon pool?

Page 6, lines 8-10: this seems like a very comprehensive model which fits the data well (e.g., Fig. 3), but I am worried that there was no validation on out-of-sample data, which is the gold standard of model assessment. Perhaps it is challenging due to the paucity of lidar data, but is there any capacity to validate the model on out-of-sample data to get a better sense of model accuracy? It will also provide a means to understand the generality of eqn (2) to represent aboveground woody biomass with lidar derived data from this study (versus having to derive a new eqn for woody biomass at a different site).

Page 7 and results section throughout: I strongly advise that when values are being reported, such as 75% or 45% canopy cover, the authors include some reasonable representation of error or variation (be it standard error or standard deviation, doesn't matter).

Page 7, Fig. 3 legend: text is incomplete. One should be able to look at the figure and legend and understand what information is being conveyed. This figure legend leaves much to be desired (location, sample size, where the data came from, refence to the model, etc.).

Page 8, lines 1-4: I found the fire * block interaction to be very interesting and worthy of some further exploration or analysis. I think your audience would be interested to know more about this interaction – are there other ancillary data that could help you explore this soil/moisture effect? To begin with, the directionality of the interaction is never reported – does greater depth/moisture increase or decrease the effect of a given fire treatment on woody cover and biomass? At the very least this should be reported. Further, once the directionality is presented, what is the mechanistic nature of this interaction? Is it related to quantity or composition of the fuel as depth and soil water availability changes? This question would be helped by data if you have it, otherwise perhaps a few sentences in the discussion are in order.

Page 8, lines 22-24: like my comment above, I did not find this conclusion or aim very compelling since we already know these methods work well and this is not a methods paper. I recommend sticking to the ecological effects of fire in these tropical savannas as the main focus of the paper.

Page 8, line 30: is this interpretation entirely correct? Wasn't there an interaction effect between fire treatment and block suggesting that the fire treatments did not simply 'persist' but in fact 'changed' with soils moisture and depth (i.e., the interaction effect). I suggest a re-evaluation of this simple interpretation and better presentation of what are interesting interaction effects.

Page 9, lines 2-3 and page 10, lines 1-3: I do not understand how this conclusion (that decreasing biomass was the result of decreasing biomass accumulation rather than mortality) was reached from this study. The text and the citation of Fensham et al. 2017 suggests that the result and conclusion come from another study rather than this one – is that the case? Moreover, the statement on page 10 is confusing because it suggests that your interpretation of the data is that mortality from fire is a driving factor in the observed patterns (in direct contracts to the sentence on page 9). Either way, clarification and rewriting are required here, as we don't know where these conclusions are coming from and there is no evidence that the current study can provide demographic data of the nature being described here.

Page 10 & 11: If my interpretation is correct, Figs 6 and 7 are representing the same data. Consequently, it may make more sense to represent Fig. 7 as a difference from the control plot rather than as the same data presented in Fig. 6 (would that make sense?).

– Technical corrections –

Page 7, Table 2: delta AIC for the top model should be reported as 0.00.

Page 8, line 27: should read "…in woody canopy cover…" or "…in woody canopy structure…"

Page 10, Fig. 5 legend: should the legend read: "Correlation between change in fire intensity and difference in woody canopy cover…"? Also, it needs to be clear what is meant by change in fire intensity; is this control – treatment or some other metric. More text and greater clarity (which is the case with almost all the figure legends in this paper).

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