Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-188-AC1, 2018

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.

Shaun R. Levick et al.

slevick@bgc-jena.mpg.de

Received and published: 9 August 2018

Reviewer comment: 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.

Author response: Thank you, we do hope that this work proves useful to the modelling community.

Reviewer comment: 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 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.

Author response: We agree with the points you have raised and have addressed them in detail in the specific comments section below.

Reviewer comment: 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.

Author response: We have modified the sentences in question for clarification.

Reviewer comment: 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.

Author response: Thank you for your comments, they have helped shape a much stronger manuscript.

Reviewer comment:: 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.

Author response: Very true, thanks for highlighting this. We tend to think of the experiment as a long-term one as we plan to maintain it for decades to come. However we agree that the current timespan of the experiment is short in context of savanna tree cover cycles, which is why we were impressed with the degree of change that has occurred and used ‘rapid’ in the title. We have made changes to our terminology throughout to avoid this ambiguity and no longer refer to it as a long-term experiment.

Reviewer comment: 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 well-managed, can have beneficial effects.

Author response: Good point. We have restructured this section as suggested and have now presented a more balanced perspective, including the importance of fire in savanna ecosystem functioning.

Reviewer comment: 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”

Author response: We have rephrased this section. Although the Smit et al 2010 and Levick et al 2009 papers are relevant here in that they utilised airborne LiDAR across

fire experiments in savanna, but neither of paper quantified biomass and its variation across fire treatments (only height).

Reviewer comment: 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.

Author response: Thanks you - valid comment. The goal here was indeed to focus on the fire effects, so we have restructured the aims to focus more squarely on the ecology. We agree that the answer to reliable detection of vegetation structure by LiDAR is “yes” – but what needs deeper consideration is the sensitivity of these techniques to detecting change. In our case, is the degree of structural change caused by fire manipulation greater than the uncertainty associated with LiDAR biomass estimation?

Reviewer comment: 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).

Author response: Updated as suggested, with SE included.

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

Author response: Very good question. Shrubs are generally ignored, and are considered to be a minor part of the carbon pool. However they are an important part of the ecosystem and some represent future trees. We have not accounted for shrubs well in either our fieldwork or our airborne LiDAR. We have now made this clear in the manuscript and have added it to our limitations section. As a side note we have started new projects exploring the shrub component with ground-based LiDAR.

Reviewer comment: 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).

Author response: We agree that out-of-sample data would be ideal for further independent validation. Unfortunately this is not possible with the data we have, and with the time that has passed since the LiDAR flight was conducted. Despite this, we have added our field estimated C values to Figure 4b so that the value for each 30 m X 30 m plot is now overlaid on top of the LiDAR derived values in the box plots. A key point here is that interpretation of biomass changes across the fire treatments does not differ if using the original field data or the LiDAR derived model – providing greater confidence in the ecological conclusions we are drawing.

Reviewer comment: 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).

Author response: Updated as suggested.

Reviewer comment: 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.).

Author response: Agreed – updated accordingly, and figure legends improved throughout.

Reviewer comment: 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.

Author response: Very good point – we have expanded on this interaction and have made the directionality clear. We have also expanded on proposed mechanisms in the Discussion.

Reviewer comment: 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.

Author response: Agreed – we have restructured the aims to focus squarely on the ecological effects.

Reviewer comment: 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.

Author response: Thanks for picking up this point – agreed and modified as suggested.

Reviewer comment: 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.

Author response: It was not our intention to suggest that decreasing biomass accumulation rather than mortality was the driver. In hindsight we can see how it could have been read like this and have modified this section to avoid any confusion. Likewise we have rewritten these sentences to remove ambiguity between interpretations from our study and the literature referenced.

Reviewer comment: 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?).

Author response: They are similar although Figure 6 showed mean vertical profile and 95% CI for all treatments, while Figure 7 shows only unburnt, 2 year early season and 2 year late season with SE of the mean. We have tried the suggestion of plotting Figure 7 as the difference to the unburnt, however we consider the direct comparison of the unburnt condition and the different season burns to be valuable. We have expanded and clarified the figure legends.

Reviewer comment: Page 7, Table 2: delta AIC for the top model should be reported

as 0.00.

Author response: Thank you – corrected.

Reviewer comment: Page 8, line 27: should read "...in woody canopy cover..." or "...in woody canopy structure. . ."

Author response: Fixed.

Reviewer comment: 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).

Author response: Thank you, we have clarified this legend and have been through all the other legends to provide more detail.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-188, 2018.