Editor's Comments to the Author:  
Subject Editor: 1  
Comments to Author :  
  
The referees of GCB-23-1228 found aspects of the study compelling that could make for an interesting study in GCB; however, they also found the paper significantly flawed in a number of ways, most notably in terms of questionable statistics and some uncertain methods.  Thankfully, they provide detailed reviews with the recommendations for improvement, which could allow the study to improve sufficiently to gain acceptance upon re-review. Both referees favored a chance for revision. Thus, the paper should be rejected but the authors  be allowed to resubmit an improved manuscript that addresses the referee's concerns.  
  
  
Reviewer(s)' Comments to Author:  
  
Reviewer: 1  
  
Comments to the Author  
Overall, this was an interesting manuscript which addresses issues of increasing fire frequency and possible implications for carbon storage in the boreal forests of Alaska. The study uses sites from two main geographic areas – one upland and one lowland, in the Alaskan boreal forest. The authors discuss the patchwork of fire history in these two regions and have utilized a suite of field measurements to assess carbon storage across these sites (measuring aboveground biomass and belowground soil carbon, for example). The manuscript is relevant to the fire science community and is also relevant to the modeling community as it provides detailed field data which could be used in models of carbon stocks.  
  
However, there are a few areas where the manuscript can be improved prior to acceptance. My main concerns with the manuscript are related to the field work methodology and analysis, and more details need to be provided by the authors:  
- While the upland and lowland sites are in separate geographic locations, the subset of the sample plots within each of these two geographic locations were near one another (plots are at least 90 meters apart, which is still relatively close), and spatial autocorrelation could be an issue. This should be addressed with the statistics within the analysis (such as mixed effects modeling which could account for spatial autocorrelation, or another suitable technique), or at least discussed as a source of uncertainty within the analysis.  
- The field data was collected in 2018 and 2019, ~14-15 years after the most recent fire occurred. This could be an issue with measuring soil carbon stocks at the sites and should be addressed in the text.  
- It is also noted in the text that tree ring data was used to verify the age of the sites, in conjunction with aerial and remote sensing imagery. When sampling in these frequently reburning stands it can often be patchy and difficult to tell the timing of the last fire at specific points across the landscape. More details on this method would be helpful to the reader. Table S2 provides a chart of the tree ring data collected, and this chart does help, however it was a slightly confusing chart. Often, the median age is greater than the max age, which doesn’t make sense - shouldn’t the max age always be greater than the median age? Adding a few details, even in the supplement, could assist the reader in understanding the methodology used here.  
  
Line by line notes:  
  
Line 108 – How did you scale up your randomly selected 100 m plot?

Measurements were scaled to m^2. We’ve clarified the language in the draft (L109).   
  
**Line 135 – Why were multivariate Bayesian regression models chosen for this analysis? Can you provide more detail into this choice and a citation showing that this is an appropriate method for this research? Will this method account for spatial autocorrelation?**  
**Line 147 – It was not fully clear how soil organic layer depth was measured, even after review of Harden et al (2006). In Harden et al (2006), the authors reference Harden et al (2004) for the calculation of their soil layer depth. Also, your field work would provide you with post-fire soil organic layer depth (or depth remaining) as your research was conducted after fires occurred – how is this giving you an indication of burn severity since you do not know the pre-fire conditions? It does not appear that you use an adventitious root method to determine pre-fire depth, so more detail in the explanation of the methods, and then in the discussion of the results, would be helpful to the reader.**  
Line 154 – There is a Table S2, but no Figure S2 (at least that I saw). Could you alter the text or add the figure?

Figure S2 has been added back into appendix.   
  
Line 165-166 – The figure caption could be clearer by saying that green represents Aboveground Biomass including the combined pool of carbon within live overstory and understory biomass. It was confusing since green is already listed as representing Aboveground biomass, but then the text referenced something different (although I recognize that you are providing the components within the aboveground biomass). Also, there are dots/points in Figure 2 which are not mentioned in the caption or legend – please explain these.

We’ve clarified the caption, and added an explanation for the dots/points (which represent outliers).   
  
**Line 168 – You mention that the fires are 13-16 years after one another. You are analyzing multiple fire events in the analysis and a table detailing the fire events in Figure 1 would be helpful through showing the year of the fire events, seasonal start/end dates of the event, length of time to the next fire event, and perhaps the number of plots within each event. Some of this information is in Table S2 in the appendix, but a second table in the appendix which more easily shows this information would be helpful.**Line 175 – Figure 2 is mentioned here, however I believe you meant Figure 3.

Good catch! Updated to Figure 3. Line 184 – You generalize and say that there is more carbon in organics than in mineral soil, can you include a citation here?

Specified the language to black spruce forests and added citations.

**Line 190-191 – You say “plots” twice in this sentence – I think it just needs a little rewrite for clarity.**Line 192 – “twice less carbon” is an awkward way to phrase this statement – could you say “two times less carbon”, or something similar, instead?

Changed.

**Line 196 – Figure 5 – Your axes are somewhat small and hard to read. Also, you use abbreviations here that do not appear in text (SOL and BA). While many readers will know the meaning, it is best to have abbreviations spelled out before their use.  
  
Line 203 – Use of the word “sites” – throughout the manuscript the use of sites, plots, and locations appears frequently. Standardizing the terms you use and then being consistent throughout the manuscript will help the reader. For example, perhaps you have upland and lowland geographic locations, and then you have unique plots within these locations, etc.  
  
Line 219-220 – The phrasing of this sentence is a little confusing where you say “our lowlands had lower” – I’m guessing they had lower aboveground biomass, but it might be best to just state it for readers. Also, could you state the range that you found for your measurements? It would be easier for readers to relate your measurements to the regional average if you wrote it out in the text.**Line 224 – Figure 6 – It might be worth somehow alerting readers to the changing y-axis scale in the three plots.

Added line in caption.  **Line 228-229 – While soil organic layer carbon can recover quickly, this does not account for the legacy carbon lost through frequent reburning. It is important to somehow make this distinction in your discussion.  
  
Line 235-236 – You mention that differences between upland and lowland areas (topographic variation) could affect your analysis – are there other statistics you could have used to test this? So much of the research into soil organic layer changes has accounted for differences between upland and lowland areas, and north and south facing slopes, that it seems incomplete to not test it or at least discuss it in more detail in the discussion.  
  
Line 262 – In the discussion of limitations, you could also include spatial autocorrelation, topographical differences not tested here, and sampling after so many years post-fire.  
  
Line 284 – It is good that the dataset is available on Zenodo, but it is difficult to find the actual data files within the link. Would the NSF Arctic Data Center be an appropriate place for this dataset as your research was funded by NSF?**  
Other citations related to reburning/resilience to consider for this manuscript or future work:  
  
Johnstone, Jill F., F. Stuart Chapin, Teresa N. Hollingsworth, Michelle C. Mack, Vladimir Romanovsky, and Merritt Turetsky. “Fire, Climate Change, and Forest Resilience in Interior AlaskaThis Article Is One of a Selection of Papers from The Dynamics of Change in Alaska’s Boreal Forests: Resilience and Vulnerability in Response to Climate Warming.” Canadian Journal of Forest Research 40, no. 7 (July 2010): 1302–12. https://doi.org/10.1139/X10-061.  
  
Brown, C. D., and J. F. Johnstone. “How Does Increased Fire Frequency Affect Carbon Loss from Fire? A Case Study in the Northern Boreal Forest.” International Journal of Wildland Fire 20, no. 7 (2011): 829–37. http://dx.doi.org/10.1071/WF10113.  
  
Brown, Carissa D., and Jill F. Johnstone. “Once Burned, Twice Shy: Repeat Fires Reduce Seed Availability and Alter Substrate Constraints on Picea Mariana Regeneration.” Forest Ecology and Management 266, no. 0 (2012): 34–41.  
  
Hoy, Elizabeth E., Merritt R. Turetsky, and Eric S. Kasischke. “More Frequent Burning Increases Vulnerability of Alaskan Boreal Black Spruce Forests.” Environmental Research Letters 11, no. 9 (August 2016): 095001. https://doi.org/10.1088/1748-9326/11/9/095001.  
  
  
Reviewer: 2  
  
Comments to the Author  
Summary (S)  
S1: Increased fire frequency is a consequence of climate change that may release carbon accumulated in the ecosystem back into the atmosphere, creating a positive feedback loop. In order to better anticipate whether ecosystems will be carbon sinks or sources, it is necessary to understand the extent to which increased fire frequency will alter the distribution of carbon stocks in the ecosystem. The manuscript proposed by Hayes et al. deals with the effects of increasing fire frequency on carbon stocks in boreal forests. The study focuses on interior Alaskan forests in which fire intervals are reduced compared with historical trends. The authors compare the quantities of carbon stored in the main ecosystem reservoirs between two modalities: the number of recent fires (1, 2 or 3) and the topographical position of the sites studied (lowland versus upland). Unburned stands (number of fire = 0) are used to define references in terms of carbon reservoir size. The authors show that the increasing fire frequency and severity will tend to make Interior Alaska's forests future sources of carbon.  
S2: The subject covered in this study is of great importance in generating knowledge that will enable us to better anticipate some of the effects of global change on the carbon cycle.  
S3: Overall, the manuscript is well written and easy to understand.  
S4: Nevertheless, I noticed several major flaws and weaknesses (see below) which lead me to believe that the manuscript should be rejected. My main concern is the lack of statistical analysis and the possibility of misinterpreting the results, which could lead to erroneous conclusions. Authors should consider to be advised by a statistician.  
S5: only major issues are reported below.  
  
Major issue (M)  
**M1: Fig. 1 needs some improvements. It lacks grid coordinates and north arrow. It is difficult to distinguish the overlapping fire areas, maybe other symbols and/or colours may help. Upper case letters in caption and legend titles should be removed. Some plots do not seem to match the number of fires according to the fire areas: for upland sites, the 3 fires plots intersect only 2 fires (1967 and 2005), and the eastern plots are very close but have different number of fires ; for lowland plots, the 3 fires plots intersect only two fire areas. My main concern is about inconsistencies between the map published in Ecosphere (Hayes et Buma, 2021) using the same design and study sites: 1974’s fire area looks like different ; in Hayes et Buma 2021, the map shows a fire in 1957 that does not appear here, and a fire in 2005 that is annotated in 1996 in Fig. 1. The data must be carefully checked and examined, as it determines the entire subsequent analysis.  
  
M2 : I am very concerned about the data analysis. On lines 130-131, the authors state that they test for the interactive role of topographic position (and I assume reburning) by directly comparing the size of carbon pools. We understand later that they plot the results of carbon stocks by pools, number of fire and topographic position, and that the comparisons are made by visual inspection. Visual inspection is not a robust test. The author must use proper statistical analysis according to the study design, for example ANOVA or linear mixed models.  
  
M3: to answer the second question about the difference in tree density, forest composition and soil consumption correlating with carbon stocks, the authors used a Bayesian regression model. The authors stated on line 132 that they test direct and indirect effects, but the model used here is not appropriate. In fact, to test direct and indirect effects, the authors can use path analysis. This is not a problem to use Bayesian regression modelling, but I am concerned about the model specification. The response is live aboveground biomass or soil carbon. The explanatory variables are tree density (to represent forest structure), deciduous tree basal area (forest composition) and soil organic layer depth (index of fire severity). The study design include lowland versus upland sites (as an index of soil drainage): this factor must be included in the models. Also numerous studies have shown that time since fire is a major factor of carbon dynamic in boreal forests, we expect to see time since fire as covariate in models.  
  
M4: using soil organic layer depth as an index of fire severity is problematic, because it is not certain how much soil have been consumed by the last fire. Why to not use the same method used previously by Hayes and Buma 2021 ? Or to include an interaction term stand age \* organic layer depth as in Pacé et al. 2019 (https://doi.org/10.1007/s10021-019-00425-2) ?  
  
M5: overall, the results are not well presented and organized, and incomplete. Units need to be harmonized in figures and texts. Figure titles need to be removed. Upper case letters in legends are unnecessary. Figure 2 and 3 are redundant. Plot design for figure 4 could have been reproduced for Fig. 2-3. The lack of statistical analysis is a serious problem here. Description of results is sometimes wrong , i.e., line 179-181 the authors state that 0.09% compared to 0.46% is not different but here there is a x5 factor !, and need more numbers.  
  
M6: to my understanding of Figure 5, when posterior estimates 95% intervals cross the 0 value is equal to no effect, when below 0 is negative effect, and when higher than 0 is a positive effect. From this point of view, the text describing the results of Bayesian regression modelling is wrong.  
  
M7: the discussion section is not using enough comparison from other studies. I am not sure that Fig. 6 is relevant and the text describing the results with Fig. 6 is sometime wrong (lines 230-232).  
  
M8: some references in the dedicated section are not in text (lines 319, 326, 343, 404).  
  
M9: appendix 2 (line 154 and 161) is not in the document.**