Dear Reviewer 1,

We would like to thank the reviewers for their comments and suggestions, which have enabled us to make a substantial improvement to our manuscript. Thank you very much for your kind assistance.

Yours sincerely,

Nantachai Pongpattananurak, Ph.D.

Assistant Professor, Forest Biology

Corresponding Author

Dear Reviewer 2,

We would like to thank the reviewers for their comments and suggestions, which have enabled us to make a substantial improvement to our manuscript. We have responded to the comments and modified the manuscript as suggested.

**General observation.**

The authors made significant improvements to their manuscript. However, I believe they could reconsider the use of statistical tests to support their arguments. The authors propose that "Fire suppression can lead to accumulation of fuel loads and alter the composition of the understory and tree communities". The potential explanation is that the fuel load and understory community composition will be altered. Altered, I understand, can include anything from non-significant to significant changes. I assume the authors are looking for non-random trends to propose solid conservation strategies, which can be derived with statistical analyses. The advantage of statistical analysis is that it will allow discerning if results derived from the effect of a variable independent variable or by random chance.

* Thank you for suggestion. We have now added the statistical analysis on the community data according to your suggestion.

**Particular observations**

**Authors**: We added a paragraph to address overall hypotheses regarding what we expected to see in our study site, based on the previous literature as suggested by the reviewers.

The sentences presented in lines 49 to 62 suggest that a single fire regime and history of cover change will alter the understory plant composition of dipterocarp forests. The authors propose that "Fire suppression can lead to accumulation of fuel loads and alter the composition of the understory and tree communities". The potential explanation is that the fuel load and understory community composition will be altered. Therefore, there is one source of variation (fire regime) that will modify two variables (fuel load and understory community composition). By deduction, it can be interpreted that fuel load will increase, but such an assertion is implicit, when it should be explicit; and it is not clear for species composition alterations. Thus, I suggest that they can propose a hypothesis that addresses the response that they expect to get, for example, that areas with a controlled fire regime will have a higher fuel load than unmanaged areas. In this regard, the authors present a parametric test to test this hypothesis. However, species composition lacks a direction of the results that authors expected to obtain before conducting the field work and a statistical test to determine if effect of the independent variable is real or due to chance. The authors present a list of species (species composition or life forms) at the sampling sites but omitted to use a statistical test to determine whether the variation between and within groups of the independent variable (fire regime or forest type) is because of these variables or to chance.

The authors' justification for omitting a statistical test to analyze species composition is since it corresponds to another document. However, in the current manuscript it is mentioned that the species composition will alter, so it is necessary to use an analysis that determines that the observed variations are because of the fire regime and not to chance. The document presents a table (2) with the most dominant species by forest type and a supplementary file with the species recorded, information that can be used to determine if the species composition before and after a fire varies statistically. I consider that if authors limit themselves to report only a list of species in the study sites, then the document will have a high regional component, where readers who are in contact with these species may be interested in the manuscript; but if they apply statistical tests, then the results they present may be used by a wider audience, since they will give more weight to the effect of the fire regime or forest type on the species than individual response of species.

Finally, I suggest that the hypothesis could be more explicit and avoid that reader inferring what the authors want to test. For example, that sites with fire management will have lower biomass than unmanaged areas, which can be tested with the non-parametric statistical test that use. In the case of species composition, the word alter implies change, hence differences. Therefore, I suggest their hypothesis expresses what the authors want to test. If test differences, then they should use a statistical analysis.

**Authors**: The current study focuses on the effect of the fire on biomass and forest structure, in particular the life forms of understory plants, because the study was designated as a non-hunting area, and the data on availability of herbivore food source is a priority for the management of this area. Therefore, the comparison of difference in species composition and richness before and after fire is a subject for another study.

The authors' response to omit the use of statistical tests to compare composition, richness and abundance is inconsistent with what is presented in the study. In line 58 of version 2 they refer to "alter the composition of the understory and tree communities" and from line 164 to 185 they report a summary of the taxa recorded. Therefore, they should present an analysis that evaluates the alteration of species composition depending on fire management and forest type. The reader will be able to identify more clearly the emerging pattern with results that indicate that there is an effect of the independent variable on species composition, and not by because of chance. With respect to abundance, the authors present a table (2) in which they report the most dominant species, but it is also necessary that these values be supported with statistical tests that allow the authors to give non-random responses. Finally, in lines 180-182 the authors report "The pattern of species richness in these forest types was similar to that before the fire except for the unburned plots, where MDFB showed the most diverse understory plants". In this regard, the similarity in the pattern of richness must be supported with an analysis that proves that the number of species is statistically similar and more diverse.

**Auhors**: The PERMONOVA and/or SIMPER analyses could certainly be done with the current dataset, but we believe that the results will distract the main point of the comparison of life forms and biomass and after the fire.

The objective of PERMANOVA and SIMPER is compared and identify the species that lead to these differences, respectively. The PERMANOVA test allows to identify significant differences between categories of one or two sources of variation, which is the multivariate variant of ANOVA and Kruskal-Wallis test. I believe that the test would not distract the reader; on the contrary, if authors use these analyses, then they would reduce the number of statistical tests that the authors would use. For example, to compare biomass they had to perform 10 statistical tests, and this analysis cannot evaluate the interaction of fire regime and forest type. The two-way PERMANOVA allows them to analyze the two sources of variation, report the radius of variation between and within groups (F value), the probability value and the matrix of comparisons between the categories of sources of variation in the same analysis, which would not distract the reader. On the other hand, Table 2 presents the dominant species of each vegetation type. But these abundances and biomass differ between fire management regimes? The PERMANOVA test will allow you to determine whether it is the more abundant species that are causing the differences or the less abundant species. In addition, if the study is oriented to the vegetation consumed by herbivores, where each species has a differential use of resources and tends to select its food, then it is necessary to identify in statistical terms which are the plant species (dominant or non-dominant) that present differences between fire regimes and forest types.

**Authors**: The rarefaction is normally used when the sampling efforts are uneven. Since the number of plots before and after the fires are the same, meaning the same sampling efforts. We are not attempting to compare the species composition between the forest types here, and therefore do not see the need for this particular analysis.

The authors argue that a "The pattern of species richness in these forest types was similar to that before the fire except for the unburned plots, where MDFB showed the most diverse understory plants". However, they omit to present an analysis to support this statement.

**Authors**: The Figure 3 showed the significant decreases of forbs and grasses with p-value less than 0.05 in the burned plots (left panel). The interpretations are already consistent with the reported p-values.

In this regard, I apologize, I was referring to the interpretation of the first part of that sentence (lines 179-180) in which they refer to "When considering each life form separately, we found that AGB of bamboos 179 and tree seedlings showed a **slight increase** (p > 0.05, Figure 3)". The probability is greater than 0.05, so referring that there is a slight increase is incorrect, since the acceptance threshold of the null hypothesis is that the value must be equal to or greater than 0.05. In this case they report that it is greater than 0.05, so there is no significant relationship between both variables.

**Authors**: The community analysis (species-by-site multivariate analysis), while it could be done, was beyond the scope of the current manuscript, as we are attempting to estimate the biomasses of the understory for the management purpose. Adding another section on community analysis will involve discussing and dissecting effects of fire on individual understory species, which will add to the considerable length of the manuscript.

The test would synthesize the tables and text presented by the authors, since a graph would display the species that are most frequent depending on the type of forest and the fire control regime, which would summarize Table 2, the paragraph found from line 164 to 185 and the supplementary file.

Having revised the manuscript with the reviewer's comments and suggestions, we hope it is now acceptable for publication in *Sustainability*. We look forward to receiving a positive response at your earliest convenience.

Thank you very much for your kind assistance.

Yours sincerely,

Nantachai Pongpattananurak, Ph.D.

Assistant Professor, Forest Biology

Corresponding Author