*Reviewer comments are followed by responses in red text*

**Q1**

The authors tested several hypotheses related to the future persistence of pitch pine, a native conifer, in one of its more northerly locations. Trees in post-fire stands were compared to trees without a recent (>70 yr) fire history. The main finding appears to be the two tree groupings were not significantly different over a large set of traits, when adjusted for site elevation differences.

We thank the reviewer for their helpful comments on our manuscript. Below we respond to each comment and indicate how the manuscript was subsequently revised. All revisions are indicated with red text in the revised manuscript.

**Q2**

Strengths of the paper include detailed observations of both site properties and a suite of traits associated with tree growth potential. Limitations include underlying assumptions that drive the hypotheses, but that need more careful examination. In addition, descriptions of the associated plant communities (including their relationships to fire disturbance and soil properties) would have been helpful.

We appreciate these concerns. In response, we have clarified the hypotheses. We have also added information about other species in the community to the Introduction. However, community interactions were not a focus of our study and, as such, detailed composition data were not taken.

**Q3**

Site selection criteria were not explained. Their mapped locations do not seem to reflect randomized or stratified placement, which could be problematic. Another concern arises from the use of elevation a both a binary (high/low) and continuous variable. Also, sample sizes vary among various statistical tests, and the correction applied (Type II ANOVA) does not address the likelihood of Type 1 error with so many individual comparisons. More important, the underlying 2X2 model could be read as N=1 (no replication of treatment sectors) or N = 15, with the individual sites per sector treated as independent replicates, rather than pooled. Given that some sites appear to be clustered, either as groups or along elevation contours, they may not qualify as independent replicates.

We have clarified our site and individual selection in the Methods. Individual selection was done using a combination of systematic and cluster sampling methods with non-fixed intervals between trees (1m to 4m) and non-fixed directionality (employing nine compass points). Populations were selected based on their fire history and elevation to achieve individuals at relatively high and low elevation that had and had not experienced the 1947 fire. Note that elevation was measured for each individual and used as a continuous variable in all statistical analyses.

With regard to the statistics. Elevation was only used as a continuous variable and was not used as a categorical (or ‘binary’) variable in any of our models. Sample size did vary by site and dependent variable and sample sizes are listed in the Supplementary Material. A different model was used for each dependent variable and the number of models run did not impact the likelihood of Type I error. We treated individual samples taken at each site as biological replicates throughout. Given differences (albeit small in some cases) in elevation (an independent continuous variable in our models), we stand by this definition of replicate.

**Q4**

Is the English language of sufficient quality?  
- Yes  
  
Is the quality of the figures and tables satisfactory?  
- Yes  
  
Does the reference list cover the relevant literature adequately and in an unbiased manner?  
- Yes  
  
Are the statistical methods valid and correctly applied? (e.g. sample size, choice of test)  
- No  
  
Are the methods sufficiently documented to allow replication studies?  
- No  
  
Are the data underlying the study available in either the article, supplement, or deposited in a repository? (Sequence/expression data, protein/molecule characterizations, annotations, and taxonomy data are required to be deposited in public repositories prior to publication)  
- Yes  
  
Does the study adhere to ethical standards including ethics committee approval and consent procedure?  
- Not Applicable  
  
Have standard biosecurity and institutional safety procedures been adhered to?  
- Not Applicable

Please see response to comment Q3 for clarification on the methodology.

**Q5**

Pitch pine is a species found almost entirely in the east with distinct ecological and cultural histories, and this paper extends studies of the conifer to some of its geographic limits, while probing into its relationships with habitat, including fire disturbance history. This experimental work has many interesting aspects, but I have several concerns about its underlying premises, its design and analyses, the writing style, and the interpretation of results.  
  
First, the premises. P. rigida is not a globally rare species as stated, but is well-distributed in the northeastern US. In Maine it receives a NatureServe conservation status (S) rank of SNR (i.e. not ranked), and in nearby states its rankings indicate a low degree of endangerment (e.g., S5 in both Massachusetts and New York). Many types of pitch pine barrens communities, on the other hand, have been declining due to several factors such as fire suppression, and are considered rare and threatened entities. This is an important distinction that needs clarification. In addition, fire-dependence is a more complex trait than indicated here. In discussing geographic differences in cone serotiny, the authors note that their populations may not express this trait, but do not explore the possibility of differential fire dependence. Ledig and Fryer (1974 USDAFS RP WO-27) offered evidence of large-scale clines in differential serotiny, which may indicate differential fire dependence. Fire dependence of pitch pine needs to be explored in the context of soils, climate, and forest succession. Moist climates of NE NA inhibit wildfire and favor more competitive tree species that are not fire tolerant, and pitch pine seems to owe its persistence to its tolerance of poor soils as much as to frequent fire (wild or managed by Indigenous Americans). One aspect of this paper that needs clarification is whether the pines studied are embedded in fire-dependent plant communities (e.g., heath, other resprouting shrubs, and graminoids that promote and benefit from fire). The photographs in Figure S1 might suggest otherwise. Furthermore, it should be noted that pitch pine, long-recognized for its tolerance of poor soils, has been planted for decades as a reclamation species on degraded Appalachian strip-mine sites. It has also been planted across much of South Korea to reclaim deforested slopes with extremely nutrient-poor soils. Fire management has not been applied in either case to maintain these introduced populations. In other words, well-established natural stands of pitch pine may persist in absence of fire, provided underlying soils remain relatively infertile.

We thank the reviewer for their comments. Throughout the manuscript we have adjusted our wording to remove rarity and instead focus on the ecological importance of pitch pine and the aspects threatening its persistence now and into the future. It should be noted that this species is found in both the eastern coastal and interior portions of the northeast U.S.; evidence of serotiny persists in the mid-Atlantic but not New England pitch pine communities. Thus, reference to this phenomenon is pivotal in importance where both low and high elevation populations at MDI exhibit no tendency towards serotinous cones, which offers a perspective on topographic variation which may portend similar trends in the future in more southerly refugia.

We appreciate the comment about soil fertility and agree that fertility likely plays a role in where pitch pine persists. Although we did not aim to directly test this with our study, we now note in the Discussion that low fertility may be more important in our system than fire.  
  
Second, the selection of the 60 experimental sites, as laid out on the maps, needs more explanation. Were they randomly assigned? Are the gaps between site clusters devoid of pitch pine? What other considerations were in play? For example, were efforts made to identify sites with similar slope aspects, or was aspect a completely uncontrolled factor (Table 2)? More important, for the reduced replication used in many of the tests, are those subsets of the same 15 sites per sector, or taken elsewhere? Table S1 would be more informative if we could see a matrix explaining what was done at each site. For analytical purposes, I would recommend correlation matrices of the dependent variables, perhaps one for nutrients and one for habitat and tree traits. As it stands, the large number of ANOVA comparisons are difficult to navigate and may also inflate the probability of Type I error. Also, the tables need degrees of freedom and slopes to be fully interpretable. I think a more straightforward and clearer approach would be ordinations of the sites by type on major axes, with tables of variable loadings on the selected axes. Also, the treatment of elevation was confusing. The indication in Methods indicate is that it was used either as a continuous or fixed variable, I think the former for the regression figures and the latter for ANOVA models, but the rational for those choices was not clear. As with ANOVA tables, the large number of graphs could be condensed and summarized by multivariate models.

We have clarified the information about site and individual selection in the Methods. In response to the reviewer questions, we selected 15 individual trees at each of four sites, so there were 60 individual trees in our study, not 60 sites. Site selection was done to obtain relatively high and low elevation sites that both experienced and did not experience the 1947 fire. For each tree, topographical features (aspect, slope, and elevation) were measured. Individual selection was done using a combination of systematic and cluster sampling methods with non-fixed intervals between trees (1m to 4m) and non-fixed directionality (employing nine compass points). We did not try to control for topographical features within a site. For metrics without 15 samples, the subset was of the same individuals.

We chose our statistical analyses based on our original hypotheses regarding the impact of fire history and elevation on the measured dependent variables. We appreciate the suggestion of ordination analyses (or correlation matrix), but these would not directly test these hypotheses, but rather group individuals by similarity in measured variables. As noted above, elevation was used as a continuous variable throughout. Also, the number of ANOVAs run does not impact Type I error inflation as each model was fit independently. Each ANOVA table has (and had) degrees of freedom. We have added slopes for plotted lines to the figure legends.  
  
The writing is mostly concise but is occasionally florid (“ferocious winds” in a “frenzy”) or normative (“tragic” consequences). In addition, causation is implied (e.g., nutrients “impacted” by elevation or “reduced” by fire), whereas the results are correlations.

We have removed florid, normative, and causal language throughout the revised manuscript.  
  
Finally, with respect to interpretation, I found that some opportunities were missed, and others could be improved. Among the missed opportunities was the chance to test for correlations between soil and leaf nutrients, which would strength results and validate the choice to run so much lab work. Another possible opportunity would be to determine whether the presumptive “populations” are true cohorts in the case of the post-fire trees, although pitch pine is known to produce multiple annual tree rings, and so size may need to substitute for age. It would strengthen the work if we could infer which trees survived the fire and which were their counterparts that escaped it.

We appreciate the suggestion of multiple analyses. With such a large dataset (in terms of measured variables) there are many correlations that we could have done. However, we have chosen to stick with the analyses in our previous submission as these best match our hypotheses. We do note that we have published the dataset for use by other researchers who may want to address different hypotheses. We appreciate the point about aging our trees, but unfortunately do not have these tree ring data. In the manuscript we now note: “While no coring was used to determine a dendrological metric for each cohort, we noted similarities in tree height and dbh, especially in fire-present precincts (Gorham Cliffs and South Cadillac Trail), which underscored assumptions about similar ages amongst the post-fire samples.”  
  
Perhaps the major needs for improvement are related to the discussions of climate change and pitch pine persistence. As demonstrated here and elsewhere, these trees are capable of persisting in locations where fire is infrequent or rare. The species itself spans a large latitudinal gradient, and if its northern outposts become warmer, pitch pine may persist where other native tree species decline. For example, some northern conifers are known to have optimal growing season temperatures that already have been exceeded, whereas some pitch pines may be more tolerant to warmer days as well as poorer soils. And the concluding paragraph generalizes well beyond the study’s results, particularly with respect its invocation of plasticity, something that is both inherent and heritable. For long-live plants, plasticity is not uncommon (thus their long lives), although individual trees may vary. However, the notion of “future plasticity” requires a population-level shift in future generations, a phenomenon beyond the scope of this study.

We thank the reviewer for their notes about the scope of our discussion. We have revised the Abstract and Discussion to better align with the scope of our study, while also pointing out avenues for future research.