Nick,

I am not near a computer until now. This is the best I can do to provide a synopsis of responses to questions by the Editor and two reviewers. I have chosen to select responses which I think are relevant to the current ms since it is 95% different (Intro and discussion) than before.

Editor

1. It appears we have done an adequate job of covering the 1947 fire history, differences in populations and status according to topography.
2. Trees were not selected completely haphazardly. For example I selected trees which were at least 5 in or 13 cm in caliper. Also these were selected to form either a cluster or 'path' depending on the population, alway switch an eye towards separating them at least five feet from each other. Even so if you look at the similarities between low and elevated populations you will find very proximate elevation between the low elev and a similar proportion between trees at the two higher elevation sites. In other words there was some homogeneity between trees within each population even if not between all four.
3. In order to account for objections by the editor and one reviewer, the analysis was recomputed within the covariate scheme (right?)
4. Soil was adequately explained by type, depth and association with either a flat, cliff or ledge population. Not only was an analysis performed for both organic and mineral content but a sieve and visual inspect for charcoal fines in order to test the assumption that pyrogenic carbon (fines) are retained in sufficient volume to be noticed some 75 years after the fire (in non-burned populations we found no charcoal fines either).
5. Changing number values (N=?) are explained according to vagaries of collection (limitations on time, resources and site access).
6. The needle sampling is explained although we could mention one sample per tree per population during active growth as the deciding factor.
7. About fire creating an erasong impact, we would suggest that this is explained in a number of ways, including reference to C removal after the fire; C and N removal from exfoliation and the loss of serotiny as a response not to presence but absence of fire.
8. 8. Obviously we did not conduct a manipulative experiment but did conduct observations of traits in their current phase after 75 years of uninterrupted fire absence in the same topographic location. There was no way to manipulate the measured space or trees nor anyway to replicate a 75 years absence in any other part of MDI--that is why using data from tree and locations not subject to fire represented a means to compare fire and non-fire output.
9. There are several possible mechanistic explanations which we offer. The first is the effect of the loss of serotiny; the second is the effect of topography and the way in which we can predict a certain degree of plasticity (of growth for example) based on trees either in the fire path or not, as examples o populations in recover or resilience as well as features like location (slope, aspect, solar exposure) affecting population advances in succession.

   Reviewer 1

1. There do not seem to be too many problems in responding to reader one based on the changes to the introduction and explanations in the discussion.
2. We could possibly add a little as to further detail on implications.

Reviewer 2
1. We wholeheartedly agree with statements by reviewer 2 with regard to the difficulties of correctly stating and making claims for the original model which is abandoned. Thus, the claims for a need to clarify plasticity versus long-term change (selection) are met by a reworking of the hypotheses to the extent plasticity becomes a driver of arguments towards persistence based on specific trait responses (e.g., iWUE measuring drought tolerance (C depletion) or growth (abundance). We completely agree with the assessment that (previous 403-405) that density should not be characterized by the term expansion but by terms like succession, reaction to disturbance (example of environmental pressure). At that point it is more likely that arguments can be made about how colonization is affected by conditions or properties associated with succession (reaction to solar availability, resources and so on).
2. We agree with the reviewer that there should be some clarity as to treating response measures as independent influences on outcomes so they may be studied independently or in conjunction with other traits. That is why we utilized elemental and mineral soil analysis coupled with SWR, and further the cluster of parameters including aspect, slope and elevation.
3. Serotiny has now been addressed.
4. In doing away the previously proposed model we were able to turn some attention to the interaction between succession and how it plays out on flat, cliff and ledge terrain, or or more specifically with regard to other traits such as SWR.
5. Not sure if we need to deal with single linear vs multiple linear models.
6. Assume we have done a better job of rationalizing the assertions about the dominance of elevation as the most key factor between fire history and elevation but perhaps more is needed there
7. We have made an effort to better define some of the characterizing language about populations but perhaps we could say a little more about differences in disturbances if that is required