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Interactive comment on "Biological soil crust communities 12–16 years after wildfires in Idaho, USA" *by* Heather T. Root et al.

Anonymous Referee #1

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The authors document species of biological soil crusts at four sites in Idaho on the border with southwestern Montana in an effort to see their recovery 12-16 years following fire. The topic of biological soil crust recovery after fire is important and one that is understudied in North America in general. Also, I appreciate that the authors have attempted to account for secondary disturbances such as grazing. However, there appears to be a lack of understanding about the ecology of the system that they are working within as well as a lack of understanding of these two disturbances. I suggest some reworking of the paper that uses the strength of this dataset, the species identities and cover along gradients of elevation and precipitation. The authors could present which species were present at each site both inside and outside of the fire. It appears that there are some obvious compositional differences amongst sites given the ordinations. Although it may be too late for this manuscript, I suggest that the au-

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thors consider adding a coauthor on future projects to fill the knowledge gaps that are apparent in regards to the disturbances and ecology of this system. My issues with the documentation of disturbances include only noting cow pies in 1m2 subplots. Cattle move across a landscape so this measure of cow pie cover, at this scale, is not appropriate when looking at animals that trample and graze over landscapes. Many folks use a piosphere approach for this reason. Table Mountain was a prescribed fire and prescribed burning is generally conducted outside of wildfire season. Its ecological effects should be different from the others and it looks like you see a wide range of BSC species at this site. My issues with the ecological knowledge used in this study applies to the authors identifying Artemisia as the dominant shrub, which does not resprout following fire. So why do the burned plots at the Fenster Creek fire have greater shrub cover than the unburned plots? Burned and unburned plots at Table Mountain have similar amounts of shrub cover. This does not make sense. Did these plots actually burn? Did something happen with the data? Was the dominant shrub species misidentified? If these are sage steppe sites as the authors seems to imply, the expected fire return interval would be at least 100 years but we are led to believe that 12-16 years is a “long” time since fire. Given that the authors had to look for patches free of tree cover, I’m guessing that this is not sage steppe and maybe something with a more frequent fire return interval such as ponderosa pine? The reader isn’t told what the dominant tree is. Also, there is a lot of discussion about invasive annual grasses and cheatgrass-fire cycles for a study on the edge of a forested system where only one site has annual grasses. Cheatgrass is shade intolerant. See work by McGinness and Keeley. Again, being on the edge of a forested system, I think that you are in a different ecosystem. It is great to add to what we know about BSCs where there is not a lot of cheatgrass but this is a different system and that should be discussed. I’m also thinking that fire severity was not accurately captured in this study. See work by Kolden related to the ecological meaningfulness of MTBS. MTBS data is 30m2 pixels so the average value is given for the change in reflectance of vegetation at that scale. You are comparing this with your 1m2 subplots on which biocrusts were measured. You would have to

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assume that the fire burned along the soil surface evenly over a 30m2 pixel for this to be the right scale at which to assess fire severity on BSCs. Specific Comments p.1 Line 9- Saying that BSC richness is 65% greater when comparing burned to unburned plots seems to conflict with your telling us throughout the paper that there were dramatic differences in richness amongst sites. This kind of statement needs to include the range in differences or what they were for each site since there were only four. Line 29- Be more specific about which lichen crusts. Also “thick crusts” needs a definition. p. 2 Line 9 It would be nice to see examples of the different BSC groups described. For example, what are “tall growth forms”? Line 16-17 Need a citation. p.4 Line 8- How were your points randomly selected? Line 14- You say that dNBR and RdNBR varied but from your maps it looks like you only surveyed the low severity end of the spectrum. I don’t see how this range tells us anything about the meaningful of the data. p.5 Line 7- You should state your reasons for using generalized linear mixed models. Was there something different about the distributions of your response variable? Please clarify this. Figure 2- It would be valuable to see the sites differentiated here. How much is the change in cover driven by one or two sites? Given the ordinations, it looks like two sites are driving this relationship. Figure 4. Define the biocrust growth forms presented here. A superscript of which sites each species occurred on would be really interesting.

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