Reviewer comments, author responses and modifications for BGD-10-12919-2013, Caldwell et al., Simulated impacts of mountain pine beetle and wildfire disturbances.

Reviewer 2 comment 1: I found this paper to be somewhat difficult to evaluate. In general, it examines a relatively important set of issues – that is, examining the potential for stand structure, species composition, and carbon storage changes that occur after wildfire or insect outbreaks in lodgepole pine forests of Colorado. Those are certainly important and compelling issues to explore. My problem is that they have either largely been explored already, and/or have been treated with more rigorous study than this manuscript presents.

The main issue with this paper, in my mind, is its modeling approach. That is not to say that one cannot ask interesting question or get interesting answers with FVS; I believe it can and that there is a use for such approaches in the literature. It is not clear to me, however, what the benefit of using FVS-FFE is when others have already studied the same questions with empirical data. Don't we already know that stands following MPB will recover faster than those following wildfire? Aren't there already good data about structural and compositional trajectories after each of these disturbance types in Colorado and Wyoming?

Author response: Thank you for taking the time to review the paper and for the helpful comments. It is true that are many good studies have addressed questions similar to ours. However, we feel that our paper does present an informative set of comparisons that have not been previously made in a single integrated analysis. We had attempted to articulate this in the Discussion (p12946, line 11+) with the statement "Strengths of our approach include the simultaneous consideration of changes in both carbon stocks and vegetation composition after two major disturbances in lodgepole pine dominated forests, and a more complete effort to model regeneration after these disturbances than has previously been published (references)." To make this specific contribution of our study more clear, we added a sentence to the Introduction, after the sentence starting with "In this paper….." that reads "Our approach built on the strengths of previous studies by comparing changes over time in multiple variables across two major disturbance types simultaneously."

To summarize briefly here, most previous studies of MPB disturbance have either (1) described forest change based on empirical data collected immediately after MPB outbreaks (Klutsch et al 2009, Collins et al 2011, Diskin et al 2011, Kayes and Tinker 2012), (2) collected on empirical data collected within 30 years of less severe outbreaks (Pelz et al 2012), or (3) made projections using FVS and/or other models (Collins et al 2011, Pfeifer et al 2011). Other studies have collected data after wildfires (e.g. Anderson and Romme 1991, Turner et al 1997, Buma 2011) and/or have simulated the effects of wildfire on carbon using models (e.g. Smithwick et al 2009, Kashian et al 2006, 2013). We mention all these studies in the last paragraph of the Introduction and then tried to make extensive comparisons between their approaches and results with those of our study in the discussion. Please let us know if there is something we missed.

We feel our approach is useful because none of the studies above addressed potential changes in both species composition and carbon stocks for extended time periods for this region, and none made comparisons between the disturbance caused by MPB and the (better understood) disturbance caused by wildfire. Our belief was that modeling potential long-term changes in species composition and carbon stocks for undisturbed, MPB, and wildfire scenarios simultaneously provides information that

was not necessarily unique, but represents consolidated and accessible information for scientists and land managers interested in comparing the potential short- and long-term impacts of both MPB disturbance and fire.

Reviewer 2 comment 2: *I found the carbon data most troubling, because C stocks are extremely sensitive to the method of sampling and estimation of stocks from biomass, and yet there is no way to evaluate these things in the manuscripts. Instead, some field data is added to a model, "stirred", and then the reader is asked to believe it. It is thus nearly impossible to evaluate whether things were done correctly, because we have no idea how the model works. In this light, why would we not believe field studies instead? All we can do is evaluate assumptions that go into the model, and many of those seem suspect in this case (see below).*

Author response: FVS is a complex but well documented model. It is beyond the scope of this manuscript to fully explain the inner workings of FVS; however, the paper could clearly benefit from a little explanation. Therefore, we added new text to section 2.3 'Modeling methods' giving a short overview of the inner workings of FVS. However, we strongly encourage readers to see the FVS citations listed or visit their website (http://www.fs.fed.us/fmsc/fvs/index.shtml) to fully evaluate the model.

The revised text for section 2.3 is:

We conducted analyses with data from 97 plots that had an overstory dominated by lodgepole pine and an understory dominated by lodgepole pine, aspen, or subalpine fir. We excluded 22 plots that had an overstory dominated by subalpine fir or Engelmann spruce. We initialized FVS with data from the 97 lodgepole-dominated plots, and ran simulations of forest growth over a 200-year time period to model three scenarios described in detail below.

The FVS, created and applied widely by the USDA Forest Service, is an aspatial growth and yield model that estimates forest growth based on tree recruitment, growth rates, and mortality, informed by algorithms for tree- (e.g., mean annual increment) and stand-level (e.g., basal area) production (Keyser and Dixon, 2008). FVS uses separate growth models for small and large trees, where the breakpoint DBH between small trees and large trees varies among species. The small tree model predicts height growth first, and diameter is predicted from height growth, as height is a driver in tree regeneration when trees compete for space and resources. FVS predicts small tree height growth based on equations using stand density, crown ratio and site characteristics (Keyser and Dixon, 2008). The large tree model predicts diameter growth and changes in height using a suite of functions with predictors including species-level coefficients, DBH, tree age, stand-level basal area, and site index (Keyser and Dixon, 2008). Background mortality levels are predicted for individual trees based on DBH. Density-related mortality is predicted for the stand based on stand density index and the maximum stand density index. After stand-level density-related mortality is predicted, it is allocated to individual trees as a function of contribution of each tree to stand-level basal area, species weights, and crown ratio (Keyser and Dixon, 2008).

Carbon stock estimates were made for 4 pools, representing total aboveground carbon, standing-live, standing-dead, and downed-dead biomass using the Fires and Fuels (FFE) extension to FVS (Rebain, 2010; Reinhardt and Crookston, 2003). The standing-live carbon pool measured carbon in live trees, including stems, branches, and foliage, but not roots. The standing-dead pool included stems and

branches and foliage, but not roots of dead trees. The down-dead wood pool included all downed-dead wood, regardless of size. The total aboveground carbon pool included all of the above categories as well as carbon contained in the biomass of herbs, shrubs, roots, litter, and duff. FVS estimates carbon in these pools (except litter and duff) by multiplying biomass by 0.5 (Penman et al., 2003). Litter and duff in the downed-dead biomass pool is converted to carbon by multiplying estimates by 0.37 (Smith and Heath, 2002). Biomass of tree boles and crowns is estimated in FVS using national biomass equations (Jenkins et al., 2003) or the default FVS-FFE methods (Rebain, 2010). We selected the default FVS-FFE methods, which are considered to be more accurate because they use region-specific allometric equations from or the National Volume Estimator Library (see Keyser and Dixon, 2008) to predict volume. Volume is converted to biomass with species-specific density values for boles (Brown et al., 1977) or using the equations in Brown and Johnston (1976) for crowns. During each cycle of an FVS simulation, some crown and bole material is transferred to fuel and litter pools. Transfers to fuel, litter and duff biomass pools in the model are based on ecological processes such as tree growth and mortality and snag dynamics (Rebain, 2010). FVS simulates decomposition of surface fuels and litter over time using a constant proportional loss model from four size-specific decay rates. Duff decay is estimated from a single decay rate where two percent of decayed matter from each fuel and litter pools are added to duff each cycle (Rebain, 2010). Snags are also modeled to decompose, decaying from hard dead wood to soft dead wood over time, or reach 64% of the snags' original density (Rebain, 2010), which is a logarithmic relationship to decay rate for a particular size class.

Reviewer 2 comment 3: *I am recommending major revisions because I think the questions are valid and compelling. To warrant publication, however, the authors will need to convince the reader that they are doing something new and improved over what has already been done with empirical data in an experimental way.*

Author response: Thank you. We're glad you also see our questions as compelling and valid. We did our best to address your concerns in our response to Reviewer 2 comment 1.

Reviewer 2 comment 4: *P12920 L24-26: It is more standard and easier to read if carbon pools are reported as concentrations – in the case of the atmosphere, as ppm.*

Author response: We reported atmospheric carbon pools (P12920 L24-26) for comparison with North American terrestrial carbon pools (P12921 L10-12). Pg C were the units of the results presented in the State of the Carbon Cycle Report (King et al. 2007). These units made the most sense to us for comparison between atmospheric and terrestrial pools as it would be difficult to compare ppm and g/m^2 or Mg/ha.

Reviewer 2 comment 5: *P12921 L7-10: This is an example (there are several) of a long sentence with multiple ideas in it. Suggest breaking this down and simplifying it for the reader.*

Author response: We simplified the sentence to read 'The global atmospheric carbon pool (estimated for 2003) holds about 705 petagrams of carbon (PgC; 1Pg=1015 g), of which 535 PgC are from non-anthropogenic sources and 170 PgC are from anthropogenic sources (King et al., 2007).'

Reviewer 2 comment 6: P12921 L10-12: Again, report in g/m2 or Mg/ha for forests.

Author response: Please see the response for Reviewer 2 comment 4

Reviewer 2 comment 7: P12921 L24-25: This is not true. Extent, possibly, though we don't really have the historical data to show it (thus it is speculation). As for severity, there have been numerous examples of MPB outbreaks with this level of severity (% mortality), even within the last several decades. The problem is not even that the extent is large so much as that multiple large outbreaks were synchronized across North America. That synchrony suggests an environmental cue that may not have existed before. This is the concern.

Author response: The synchrony of outbreaks and possibility of a novel environmental cue is interesting and important, but was not the focus of this paper. We were interested in potential long-term changes in species composition and carbon stocks following disturbance with the belief that these could be especially important for such an extensive outbreak. The sentence you highlighted was intended to describe the considerable magnitude of the current outbreak. We changed the text in P12921 L24-25 to tone it down a bit and removed the implied comparisons to recorded and unrecorded history. It now reads "However, the extent and severity of the current outbreak is remarkable, affecting nearly 3 700 000 ha in the conterminous United States by 2009 (Mann, 2012)." We also changed the text in the abstract that read "In the Southern Rocky Mountains, a recent outbreak of mountain pine beetle (Dendroctonus ponderosae; MPB) has caused levels of tree mortality that are unprecedented in recorded history." to "In the Southern Rocky Mountains, a recent outbreak of mountain pine beetle (Dendroctonus ponderosae; MPB) has caused remarkable levels of tree mortality."

Reviewer 2 comment 8: P12922 L10-14: This does not make sense, because one could argue the same thing for wildfires – since relatively little C is lost in these systems as a result of the fire. Most C loss occurs due to decomposition after the fire – and the same is true for insect outbreaks. It is not just storage that is important, but the stability of the carbon pools where that storage is happening.

Author response: We changed the text on P12923, L7 to read 'Similar to insect outbreaks, the proportion of biomass in the standing-live, standing-dead, and downed-dead pools changes as needles, twigs, branches, and trees fall. However, unlike insect outbreaks, wildfires consume a proportion of live vegetation, dead surface fuels, and organic soil layers.'

Reviewer 2 comment 9: P12923 L8-9: Yes, there is more C loss compared to insect outbreaks, but it is still relatively low (probably 10% or so) – at least in lodgepole pine.

Author response: We removed the word 'substantial' from the sentence.

Reviewer 2 comment 10: P12924 L17-19: Not clear here. Are you looking at the MPB/fire interaction? Or just wildfire alone?

Author response: We recognize this wording was potentially confusing. We did not look at MPB/fire interactions. To make this clearer, we changed the sentence to "In this paper, we characterized how species composition and carbon stocks in lodgepole pine forests might change in response to each disturbance; MPB and wildfire."

Reviewer 2 comment 11: P12925 L23: This is an issue, as these surveys are known to be highly biased and variable between observers.

Author response: In the case of this study, the aerial surveys were simply used as a guide to help to determine where to place plot locations on the landscape so that we could attempt to include the entire range of variability of post-outbreak conditions in our sampling effort. We felt that the survey data was useful and adequate for this purpose. Because of known issues with the accuracy and precision of the aerial survey data at fine scales; however, we quantified actual mortality and time since mortality for trees in our plots rather than assuming that the surveyors' designations of these variables were correct.

Reviewer 2 comment 12: P12965 L14-18: 12 cm is an awfully high cutoff for trees. This is especially true for the seedling class, even if it is not likely that one would find a plant that large that doesn't reach DBH. In any case, trees, seedlings, and saplings are meant to capture some sort of age structure, and this high cutoff hinders that effort.

Author response: The 12 cm DBH breakpoint is very similar to the 5 inch breakpoint used to distinguish trees and saplings in the U.S. Forest Service Forest Inventory and Analysis (FIA) Program. We used this DBH breakpoint for trees to ensure that our results for tree density and basal area reflected potential changes the dominant canopy tree species. Moreover, the criteria chosen for defining tree seedlings in our study (DBH of < 12cm and a height of < 1.4m) did not differ substantially from those used in a recent study modeling post-MPB outbreak future stand development (trees <2.5 cm diameter at 1.4 m; Collins et al. 2011), and would thus not preclude cross-study comparisons.

Reviewer 2 comment 13: P12927 L7-12: Two points here. First, there should be some treatment of why using FVS-FEE is better or comparable – other than being easier – than using allometric equations, which is the standard practice when doing carbon work of this type. Someone in the literature must have made this comparison. Secondly, you are doing aboveground carbon estimations, not total carbon. As belowground carbon is not trivial – I think that new Kashian et al. Monograph reports it near 10%, and it is the most stable– this correction should be made throughout the manuscript.

Author response: We added a short subsection to the beginning of the methods section explaining some of the details about how FVS works, and hope this clarifies our decision to use this approach. Please see our response to Reviewer 2 comment #2 for more details.

Also, we appreciate the point regarding belowground carbon. We changed references to 'total carbon' to 'total aboveground carbon' throughout the manuscript.

Reviewer 2 comment 14: P12929 L9-11: This is probably not very realistic, because regeneration success is not constant among tree species.

Author response: Regeneration success certainly varies among tree species, and our approach was intended to address this. Our method systematically introduced some variability in seedling densities for different species at levels proportional to the overstory composition measured during our field inventories. We assumed this variability would reasonably reflect the potential for differential contribution of pre-fire overstory composition to post-fire seedling composition based on factors influenced by propagule abundance: composition of post-fire seed banks in canopies, forest floors, and soils, as well as vegetative resprouting. Nevertheless, because FVS simulates mortality, not all of the seedlings we introduced survive; the majority of them did not.

Reviewer 2 comment 15: P12929 L15-16: If you read these papers carefully, you will see that cone serotiny is highly variable across these study areas. 64% and 65% are averages; in Yellowstone, for example, most of the landscape is occupied by lodgepole stands with cone serotiny < 5%, but the few pockets of very high serotiny raise this value up. In any case, if adjusted by area, the number would be nowhere near 65%. If the model is very sensitive to this parameter, this could be a serious problem.

Author response: We agree that serotiny levels can vary within and among landscapes. However, the MS thesis by Carissa Aoki (2010) was conducted in a study area overlapping our own, and for this area (the west side of Rocky Mountain National Park), she measured mean serotiny of 64%, a median of 69%, a range of 14-91%, and a standard deviation of 18. Based on the small standard deviation, we feel that variation in serotiny among stands in this area may not be as great as in Yellowstone NP, and that our selection of the mean value of 64% for use in our analyses was reasonable.

However, at the high seedling densities we used, FVS is not very responsive to the variable (initial seedling density) that reflects serotiny. We found during preliminary sensitivity analyses that our results would differ if the seedling densities were low, in hundreds of trees per ha, instead of thousands of trees per ha. Therefore, our decision to use a serotiny level of 64% is unlikely to have significantly altered the outcome of the post-fire simulation.

Reviewer 2 comment 16: Section 3.2. I am somewhat uncomfortable with these results because I know nothing about how FVS calculates C, and there is no description of it here. As it stands, the paper is simply a description of model runs that I can't judge very well.

Author response: We added text to the methods explaining more about how FVS runs and calculates carbon. Please see our response to Reviewer 2 comment 2.

Reviewer 2 comment 17: *P12933 L22-23: This makes very little sense, given the regeneration ecology of lodgepole pine. There is no ecological reason that lodgepole density would suddenly increase 50 years after a disturbance; gradual and continuous increase, perhaps, but certainly not suddenly.*

Author response: We appreciate this point; this statement could certainly have caused questions for other readers. The confusion is a consequence of how we define trees (DBH > 12 cm) and we were remiss in specifying that these results only referred to 'trees' - not trees, saplings, and seedlings - at the start of the paragraph.

We changed the paragraph starting at page 12933, line 20 to read: "In the wildfire scenario, there was an initial large increase in lodgepole pine density (seedlings and saplings), but it took 50 years of growth before individuals were considered trees by our definition (DBH ≥ 12 cm) and thus, lodgepole pine tree density remained extremely low (median of 63 TPH) through the 2060s, then increased rapidly to a median value of 1500 TPH by 2090, and then gradually declined to a median of 560 TPH by 2210 (Fig. 2c)."

Reviewer 2 comment 18: *P12934 L14-15: Again, aspen sprouts after disturbance, so one would expect density (perhaps not basal area) to peak early, not in the middle. This would certainly be true with fire, where density would be highest in the first 5-10 years after.*

Author response: Again, this is an artifact of the way trees are defined. See our response to the previous comment. We changed the paragraph starting at page 12934, line 13 to read: "The density and basal area of quaking aspen trees were low initially, and then increased near the middle of all simulation periods as saplings matured, but with subtle differences among the scenarios in the timing and magnitude of the increase."

Reviewer 2 comment 19: *P12939 L6-7: Why would it take longer for basal area to recover from the MPB scenario vs. the wildfire scenario, when advanced regeneration should jump-start recovery after MPB? I am skeptical about this result.*

Author response: The differences in recovery times are not large, about 20 years, but we agree that this result appears somewhat surprising at first glance. More advanced regeneration and surviving trees were present initially in the MPB scenario. We believe that two factors may explain the difference in basal area recovery: 1) the number and growth rate of new seedlings (both greater after fire) and 2) the influence of shading from residual canopy trees on growth (this effect was probably smaller and thus tree and growth was probably faster, after fire.)

To clarify further, a large number of lodgepole pine seedlings were introduced after the wildfire and their growth was the primary driver of basal area recovery in this scenario, as we note at 12940 L 9-13. These seedlings grew rapidly into trees and ultimately basal area in the wildfire scenario overtook the MPB scenario. In the MPB scenario, REPUTE added few new seedlings, and changes in basal area were largely the result of the growth of the existing trees and advanced regeneration that we measured during our field inventories. Growth could have been slowed by shading effects of the surviving large trees too. We added a sentence to address how residual canopy shading may have influenced these results at 12940 L14 to read: "Moreover, the growth of trees after the simulated fire was probably less affected by shading from residual canopy trees than in the MPB scenario, which may have contributed to the slightly more rapid recovery of basal area after the fire."

Reviewer 2 comment 20: *P12838 L17-27: Why beat yourself up over these numbers? Fact is, percent mortality varies a lot from place to place. In fact, each of these studies is reporting an average, and at any one place mortality will vary. The same is probably true with your study – and it would be nice to see those kind of numbers. Did you get 65%/71% at every plot? What was the range?*

We compared mortality levels in our study area to previously published estimates to help readers interpret our results to previously published studies. You are correct in pointing out that mortality is variable from place to place. Because we used the existing aerial survey mortality data to stratify our sampling locations across the landscape, we captured much of the variability, and the percentage of dead trees and dead basal area across our plots ranged between zero and 100%. However, mortality tended to be high as indicated by the mean values we reported.