Biogeosciences Discuss., 10, C5586–C5590, 2013
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Biogeosciences Discussions
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Interactive comment on "Simulated impacts of mountain pine beetle and wildfire disturbances on forest vegetation composition and carbon stocks in the Southern Rocky Mountains" *by* M. K. Caldwell et al.

Anonymous Referee #2

Received and published: 1 October 2013

General Comments: 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

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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?

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).

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.

Specific Comments: 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.

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.

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

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

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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.

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.

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.

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

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

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.

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.

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

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.

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.

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.

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.

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.

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?

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