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Dear Derik,

I enjoyed meeting you and listening to the presentations on March 10 in Sacramento on the Forest Protocol white papers.

I reviewed the Soil Carbon white paper and am including some thoughts I had and wrote down.

I am very interested in your progress and I think it is a great idea to try to search for ways to utilize forest soils as a carbon sink.

This is a very complex subject and the effort of the reviewers shows the difficulty in grappling with all the studies that show disparate results.

Please do not take my comments too personally as I support this effort. My comments are directed to help fill holes and make the review more readable with greater emphasis on unifying principles. At some point, there will be issues where the data simply do not provide a very clear answer. I think that some level of critical review and ability to separate studies that are not as credible as others will be very important.

Good luck with your effort.

Sincerely,

Kim Mattson
Ecologist and owner Ecosystems Northwest
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Comments on Gershenson et al. Accounting for carbon in soils.

The paper summarizes a complex issue and in general, does a good job. But it needs significant amount of more work.

In a field where the conclusions are still somewhat fuzzy but becoming more clear, this review tends to dwell overly on the confusion of studies and less on the unifying principals. Secondly the review may be relying on single studies a bit too much from which to buttress or draw conclusions.. I suggest the authors take a more "weight of the evidence" or common sense approach. Clearly there are general trends we expect carbon to follow. I think much of the confusion in the literature is due to poor designs, or poor field sampling, or using study data in manners that they were not intended. Basically poor science. I think the authors need to reject certain studies that are confounded by other variables. Finally I suggest they review models and use models logic to help them sort through the variable study results.

1. My first suggestion is to try to paint a more unifying picture. For example, Johnson and Curtis showed that coniferous soils do seem to accumulate carbon under a sawlog scenario. Other studies, particularly where there is soil disturbance as part of the site prep, soil C seems to decline. So, one conclusion may be that when logging debris or slash is left on site, it can end up in detritus and eventually soil organic matter. If the sites are disturbed, soil C likely declines.

In the review of the literature, several examples of misstatements are: soil organic matter varies on micro and macro scales and it varies with depth. While this statement is not overtly incorrect, it is too general and leaves the reader that it is hopelessly variable. In fact, while soil organic matter has relatively high variation on a small spatial scale, there are discernable patterns on a larger scale. The soil organic matter does varying with depth, but again the variation has a discernable pattern. The fact that soil carbon varies with an observable pattern is quite different than being simply "variable." These two patterns are readily described: soil organic matter increases with latitude and with rainfall, and soil organic matter decreases with depth. Knowing these sorts of patterns are a good thing in that we understand basic processes that contribute soil organic matter accumulation in soil. Instead of citing literature that does not make sense why not cite the literature that makes more sense. Are there results that show increases in soil organic matter? What have we learned from the Rothamsted studies?

2. I think the review highlights not so much that soils are highly variable, but instead, the results from field studies are highly variable. This is an important distinction. I personally believe that much of the variation we see in the literature is as much due to poor sampling as it is to natural variation. I think you cite Yanai..her paper does a good job of bringing up problems with field sampling.

3. There is no discussion of the success of models. The CENTURY model appears to be relatively good at predicting soil organic matter in agricultural systems. If soil organic matter was hopelessly variable, the model would be abandoned. Most models of soil carbon implicitly assume that carbon transfers from plant material to soil C. Based on this logic, it is presumable that leaving more debris on site will

4. This literature review relies more on individual cites of results. I think they should try to give quantitative estimates from the reviews and from models and be more critical of the studies they review.

The soil carbon literature is notoriously full of contrasting results. Part of the problem is that the field methods do not measure carbon the same. Time is important. Disturbance history.

There are likely ways forests can be managed for soil carbon. Length of rotation, avoidance of disturbance (fires or insects), management of woody debris. Conversion of sites to forest assumes a short term loss of carbon.

Discussion of measurement error. Bulk density, rock content, forest floor and mineral soil separation. More figures and data..less anecdotal writing. Less focus on the problems and more on what trends do appear to be established. Good discussion of N. No cites of Berg and McClaugherty—probably the best and most sophisticated discourse on carbon cycling.

Here are some specific comments I made on my second reading through it.

P2. line 5: The phrase…"or disturbance on contours" is a bit confusing. It appears to be a double negative. It seems to say that CAR does not consider any disturbance to soil to have an effect if the disturbance were not oriented along a contour? In other words, CAR thinks disturbance along contours would have an effect? This doesn't make sense.

Line 6. Monitoring remains elusive.. The results from monitoring that show clear effects are elusive—not the monitoring. I would take some issue that monitoring does not show clear effects.. It may be more accurate to say that monitoring show variable effects, depending on the study.

First bullet, again, I get the impression you are trying to put up a smoke screen that the research is hopelessly variable (what multiple other factors?). Again, I think a more accurate way to cast the state of science is that study results produce variable interpretations. Though, it seems to me that the level of disturbance to the soil is the most important factor, followed by the particular techniques used by the study. The effect of tree species may embody other factors. For example conifer sites in the SE US probably have greater site prep following harvest and with replanting. Tree species will vary with climate and climate may be a larger factor. It seems reasonable to mention soil type is a good factor to consider.

Second bullet is somewhat soft. I had trouble reading it and making sense of it. It wasn't clear to me how thinning or veg control may alter site fertility per se. Maybe you are trying to say that soil carbon is a function of site production. This makes sense. I doubt that simply thinning or veg control will increase soil carbon. You may be trying to get at the effect of N on humus or stabilized carbon? I am not convinced that studies show that leaving detritus on hardwood sites causes loss of carbon. N-fixing plants may increase soil carbon on young sites.

Third bullet is good, but poorly worded.

Page 3 bullet on thinning appears to contradict statement on thinning on page 2. I wonder how thinning may increase productivity of a site. I can see how individual trees left behind will grow faster due to the release. But the overall site productivity should diminish as you remove leaf area.

Bullet on soil carbon monitoring is too general and is not very correct. Soil carbon monitoring can be precise, it is not very expensive (or expense is a relative term), and it is not necessarily very time consuming. At least state what you are comparing the monitoring to. Perhaps modeling is less expensive (after you have developed your model). Maybe you mean monitoring on every site? Maybe you mean monitoring to detect 5 % change? I think monitoring is essential to helping determine whether certain sites or disturbances affect carbon. A better recommendation might be to state the importance of monitoring and but that it involves basic research that the CAR cannot do. Still it is in the best interests of CAR to support monitoring and the development of better data sets so this uncertainty might be reduced. I think a better way to state your problem is that the state of science in soil carbon dynamics is still too young and is still developing and it cannot provide the sorts of answers with high degree of precision that CAR needs.

Page 10: first paragraph. Where you state soil carbon accounts for 48 % of forest carbon, are you citing Woodbury. Otherwise, if this contradicts your general statements earlier of soils containing more carbon than biomass. Also this paragraph start with the idea that soils can sequester carbon but ends with the idea that soils are more inert…

Second paragraph paints too simplistic of a picture. The last sentence suggests that the CENTURY or other models of carbon are not accurate.

Page 14: Your reasoning for stating that conversion of non-forest to forest will result in short term declines in soil carbon are not clear. Unless there is disturbance to the soil, it is not clear how planting to trees may cause short term losses.

Page 15: Your reasoning that increased production of litter will cause increased decomposition is not clear. Do you mean increased decomposition of the newly added litter or of carbon on the site before addition of litter? It doesn't make any sense to say that the increased decomposition of the newly added litter is higher than sites without the added litter.. because in sites without added litter, decomposition is, by definition, zero (no litter is added so no decomposition of this non-added litter can occur). So, this causes some confusion for me. I see you cited Cheng for priming effects of root exudates. Is this what you mean?

Page 18: the section on thinning seems to presume that since thinning helps the remaining trees to grow faster, that overall net primary production on a thinned site will also be higher. This is counter-intuitive from the prespective of leaf area. If you thin, you will initially reduce leaf area and this should reduce the net photosynthesis of the site and the net primary production.. I am pretty sure Powers data show this.. that thinning reduces overall site carbon fixation but the left over trees do show a release response. It is not too surprising that a couple of cited studies show declines with thinning.

The conclusions on page 19 first bullet do not seem correct. I fail to see how thinning will add soil carbon per se to infertile sites. In any event, it seems better to address this via some sort of process model that may show ways that adding litters may help them become incorporated into the soil carbon pools.

Page 20: It may be more correct to paraphrase Johnson and Curtis review as showing studies can be highly variable in the reported change of soil C. But the over all mean of the studies they cited showed small increases. To say that overall, there are no significant effects of harvesting on soil carbon misses the important points of harvest type and degree of disturbance. If you say there are no significant effects, you should at a minimum add the adjective no "statistically" significant effects. But clearly this is comparing apples with oranges. Given the wide range they report, it is not too surprising that the mean happens to fall near zero. I think the big point is that, if disturbance is moderate or light, there are potential gains to the soil carbon pools via enhanced transfers.

I was running out of time, the remaining sections are not too bad. I think I could start to see some general trends emerging out of the text.

Derik,

I took a look at the woody debris white paper. I have done both plot and line intercept measures for woody debris.

I enjoyed reading this white paper--it is more finalized and better prepared than the soil paper.

I learned a lot by reading the review of the methods of measurement. I maybe would have like Ducey to more strongly recommend a best method. It does seem like he favors the Perpendicular distance sampling.

I used line intercept in a triangular arrangement and was very satisfied in relatively flat stands in Alaska where there were high densities of downed wood. I also stratified my sampling lengths by size classes--short transects for wood under 10 cm dia and longer transects for larger pieces.

I think one topic is missing from his review. The transfer of woody debris to humus or long term carbon is not addressed. This is not addressed very much in the literature as it is implicitly assumed that wood decomposes in place and it eventually all goes to atmospheric CO_2. Whereas, models of soil carbon presume that some of inputs to soil carbon do come from above ground sources including wood.

Sincerely,

Kim Mattson
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