RE: Manuscript #EAP18-0548

Dear Dr. Pan

Thank you for providing the thoughtful reviews of the paper titled “Estimating uncertainty in the volume and carbon storage of downed coarse woody debris.” We have addressed these comments on the attached revision and provide point-by-point responses below in blue. The reviewers’ constructive suggestions have improved this manuscript and we hope you will now find it suitable for publication in Ecological Applications. We appreciate this opportunity and look forward to your editorial decision.

Sincerely,



John Campbell

on behalf of all authors

Reviewer(s)' Comments to Author:

Reviewer: 1

Comments to the Author

The authors used FIA data to characterize uncertainty associated with north temperate forest DCWD volume and carbon estimation. The manuscript was a pleasure to read and overall is an excellent and timely contribution, requiring only minor revisions prior to publication.

My primary comment/question, which I suggest the authors address through additional discussion and, if they choose, new analysis, is related to the two approaches to acquiring information on parameter uncertainty and whether this difference could affect the estimation and interpretation of uncertainty. The first approach draws from a very broad distribution of published data (i.e., for density, carbon) and the second uses more constrained onsite repeated measurements (e.g., diameter). Given the authors’ first approach and underlying assumptions, the use of a fully random (ln 210) distribution of published wood density values could inflate the apparent uncertainty associated with this parameter under some circumstances. For example, my assumption is site/region-specific and species-specific densities typically are used when available, and therefore one might expect the associated confidence interval to be smaller than that which the authors derived from a random unfiltered and narrower distribution.

The reviewer’s point about how the use of published vs. site-specific measurements can inflate the uncertainty is an important finding of the manuscript (i.e., line 503, 511). Density data for all species and decay classes are not available at the local, regional, or even national level, and we recognize that this contributes to the uncertainty. This is why we suggested on line 394 that uncertainty in density could be reduced by making site-specific measurements: “A more comprehensive and systematic sampling of DCWD density would improve estimates of carbon storage in forests and could potentially be implemented as part of routine FIA field data collection procedures once the methods for measuring DCWD density have reached an acceptable level of efficiency and precision.” Please note that we sampled from published density values, but the sampling was not fully random, as the reviewer seems to have believed. On line 211 we state that we sampled density by species and decay class. Based on the reviewer comment, we added an additional sentence on line 384: “Because data from the literature come from many different regions and are obtained with different methods, applying these data gives higher uncertainty than would be obtained by sampling from the population of inference.”

Related, I understand the value of generating uncertainty using published values (because indeed this is what is typically done), but ‘true’ uncertainty associated with density and carbon would be better obtained by comparing actual site-specific estimates with a random (or more restricted) assessment of uncertainty in which values from the literature are used.

We fully agree with the reviewer that quantifying the uncertainty with site-specific measurements would help refine the values. Since these data are not currently available, the published values are our only option. There were a few places where we had made this point, for example on line 386 we stated that “Density measurements for each individual piece of DCWD would be ideal…” As stated on line 458, making additional measurements of carbon concentrations would not be worth the effort and expense because it would do little to reduce the uncertainty. We altered the text on line 511 so it more directly identifies the advantage and disadvantage of making measurements of the collapse of individual pieces of DCWD. The sentence now states “Alternatively, the height of individual pieces of DCWD could be measured in the field, which would obviate the need for the use of collapse ratios by decay class and would improve estimates of DCWD volume. However, this would be a time-consuming measurement and destructive for partially buried logs.”

These points merit consideration and acknowledgement, even if additional analyses are not conducted. In either case, the findings from this study provide a valuable contribution to the broader discussion of uncertainty when estimating an important terrestrial carbon pool.

Minor comments:

Ln 397. Please include a reference for standing live tree DBH uncertainty.

We calculated the uncertainty in standing live tree diameter for a publication that is in preparation. Because this source is not yet citable, we modified the sentence to more clearly indicate how uncertainty in live tree diameter was determined. The sentence now states “We evaluated standing live tree data from FIA plots using the same approach as for DCWD and determined that the measurement uncertainty in diameter at breast height was 0.14 cm, or 0.7% of the mean tree diameter, whereas the DCWD measurement error was 1.0 cm or about 7% of the mean DCWD diameter.”

Lns 424-429. Would the authors recommend, given their findings, that species be recorded for the purpose of DCWD volume and carbon estimation?

Species is recorded, and contributes little to the overall uncertainty. We revised the paragraph on species identification (line 436) to better address this question.

“Tree species identification contributed relatively little to the overall uncertainty in carbon storage in part because agreement was high between production and QA crews (Fig. 7). Tree species identification, in combination with decay class, is used to determine DCWD density, but taxonomic class is not as important as decay class in predicting variability in density (Fig. 8, Supplement S1). Tree species (hardwood vs. softwood) is also used to estimate carbon concentrations, but this was a negligible source of uncertainty.”

Consider including the numeric data plotted in figure 8 in a supplementary table, since these values may be useful to others when scaling/estimating uncertainty.

As suggested, we added a supplementary data file and metadata for the plot-level downed coarse woody debris volume and carbon storage data shown in Fig. 8 (see supplement S2) along with citations for it in the text (line 306).

Reviewer: 2

The manuscript provides a clear and concise review of a simulation effort to quantify the uncertainty in volume and carbon estimates for coarse woody debris sampled on FIA plots in the US. The article is important in that it identifies opportunities to improve the precision of estimates and highlights where additional sampling or other data collection is needed to reduce uncertainty. The approach used is sound and is well described in the manuscript. Interpretations are insightful and appropriate. I recommend acceptance after very minor revisions for clarity.

The conclusions are interesting, and suggest opportunities for more efficient data collection (eliminating measurement of hollows) without losing precision of estimates. It seems subsampling for improving density estimates would also be called for.

Subsampling would be the best way to obtain a density estimate, but is expensive. Non-destructive methods would be ideal. These points are raised in the discussion on line 386.

One suggestion for an addition that might be informative would be to show mean and variability of carbon Mg/ha for the five decay classes. I would guess that carbon density is higher in the lower decay classes and variability may be higher in the upper decay classes. If so, this could inform efforts to improve field protocols- time may be better spent on standardizing measurements that are easier to implement on low decay classes.

Expressing carbon by decay class in units of Mg C/ha would reflect the amount of wood in each decay class (rather than changes in density) which is illustrated in Fig. 6. Since there are few pieces of highly decayed wood, there is less Mg C/ha in the highest decay class. Perhaps what the reviewer is suggesting is to show the decline in carbon with decay class. This relationship is a function of the density of wood (as shown in Fig. 3b) and not the carbon concentration (as discussed on line 456). Interestingly, the variability in density reduction ratio does not indicate clear increases with decay class as the reviewer expected (see Fig 3b). To highlight this finding we added text (line 371) stating “Although density decreased with decay class, there was not a clear increase in variability with level of decay when all species are combined (Fig. 3b), which reflects different rates of density loss with decay class by species (Harmon et al. 2008).” Because variability in the reduction ratio did not increase with decay class, it precluded us from making any statements about modifying field sampling protocols.

Specific suggestions: Line 130: Text refers to Figure 1 and mentions 109 plots in a dataset, but Figure 1 mentions 79 and 31 plots, which would total 110.

We appreciate that the reviewer caught this discrepancy. We changed “109” to “110” to correct the error (line 130).

Line 143: Text mentions one 15m transect, but Equation 1 and Figure 2 refers to these as two transects per subplot totaling 14.6m. Either using “Approximately” with the 15m or being precise would help avoid confusion.

The inconsistencies the reviewer identified were corrected. We indicated that the length of the transect was 14.6 m on line 143 and in the equation (line 168). In Figure 2, the direction of the transect is shown along with diameter of the subplot (14.6 m). Therefore, in the figure legend and caption we removed the text stating that the DCWD transect consisted of “2 x 7.3 m lengths” to avoid redundancy and confusion.

Figure 6. It seems that the same data used for 5 graphs in Figure 6 could be more easily portrayed in a table such as in Figure 5. Such a contingency table format (called “heat map” in the manuscript) is a common and widely understood method of portraying agreement between categorical datasets.

We agree that the information in Figure 6 could be simplified by converting it to a heat map. We altered the graph as suggested and modified the caption accordingly.