Physical & Environmental Sciences
UNIVERSITY OF TORONTO
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Dear *Biogeosciences* publication team,

Thank you for the opportunity respond to reviewer comments posted about our manuscript entitled "**Quantifying the influence of wood carbon fractions on tree- and forest ecosystem-scale carbon estimation in a temperate forest.**" We appreciate the reviewer comments, which we found both supportive and constructive. Notably, all comments have been addressed in our revised manuscript.

***Biogeosciences* Editor Comments**

To avoid further delays, I decided to make a decision based on the reports of the first referee. The referee recommends minor revisions and suggests a few technical changes and some comments. I agree, and have therefore decided to go for a round of minor revisions, which should no longer be sent to referees. Looking forward to receiving the new version of the manuscript.

Thank you kindly for making a decision on our paper in this manner. It is much appreciated. We have attended to all the points below, though wish to note that some of the comments here were factually incorrect. We noted those in turn, and edited where necessary.

***Biogeosciences* Reviewer: 1**

General Assessment

This study combines detailed inventory data from over 39,000 trees in the Haliburton Forest Dynamics Plot (HFDP) in Ontario, Canada, with open access wood carbon fraction (CF) databases to systematically evaluate how different wood CF assumptions affect carbon estimates from individual trees to whole forest scales. The results show that using species specific CFs versus generic values (e.g., the 50 % assumption or IPCC defaults) leads to significant differences, and these discrepancies compound across spatial scales – the 50 % assumption may overestimate temperate forest biome carbon stocks by 2.2–2.5 Pg C.

Overall, this is a methodologically rigorous, data rich, and practically relevant study. The topic addresses a central source of uncertainty in forest carbon accounting, the study design is appropriate, the analytical approach is sound, and the conclusions provide clear guidance for improving forest carbon estimation practice. Below I offer specific comments and suggestions for the authors' consideration.

Thank you kindly for your continued attention.

Major Concerns

1. Insufficient treatment of intraspecific variability

The authors acknowledge in the discussion that "the relative contribution of inter vs. intraspecific variability in wood CFs remains only loosely disentangled." However, in the methods, for the 11 species lacking species specific CF data (39 % of species, though representing only 0.1 % of carbon stocks), the authors substituted mean temperate angiosperm values. While this is a reasonable practical choice, the paper does not fully discuss the uncertainty introduced by this substitution.

We would argue that this discussion introduces limited (if any) uncertainty into our primary results. Specifically, because these tree species account for only 0.1% of forest biomass/ carbon stocks, the uncertainty that this introduces is negligible in comparison to the wider findings of our study. We have acknowledged that species sensitivity analysis that characterizes the role of intraspecific variation, would be a key next step in this line of research (Pages 11-12, Lines 406-426).

2. The sample size at the hectare scale limits statistical inference
The authors honestly state that "due to sample size limitations at this scale these results are interpreted cautiously" (p. 9), which is commendable. Nevertheless, n = 10 does limit the statistical power at the hectare scale, especially in ANCOVA models with multiple predictors (some parameters in Table A4 have relatively large standard errors).
Agreed. This is indeed why in the previous analysis, we added a number of instances noting that we interpret these results cautiously (Page 9, Lines 308-309).

3. Several methodological details need clarification
Equations 1–3: The notation "β_wood|DBH^wood2" is ambiguous.
I suggest writing y_"wood" =β_"wood1" × 〖"DBH" 〗^(β_"wood2") to match the standard form in Lambert et al. (2005).
In fact our current equations match exactly the format and form published in Lambert et al. 2005, so are retained here.

Treatment of missing species for Ctree1: The authors state that "in the cases of the less common and small understory trees that did not have species specific wood CF data, Ctree1 estimates are based on mean wood CFs from temperate angiosperms" (p. 5). Does the HFDP contain any conifer species lacking CF data? If so, were they assigned angiosperm means or conifer means? Please clarify.
No, only a small number of angiosperms were lacking species-specific wood CF data. This has now been clarified (Page 5, Lines 156-158).

IPCC default for Level 4: The authors use 0.47 (47 %) as the IPCC default. This is consistent with the IPCC 2006 default for temperate conifers, but the IPCC actually provides different defaults for different forest types. Please justify why 0.47 was chosen as the representative value, and discuss whether conclusions would change if another IPCC default (e.g., for temperate broadleaved forests) were used instead.
In the IPCC 2006 guidelines, specifically their Table 4.1, the IPCC recommends a default wood CF value of 0.47 for "All" tree parts and species. We have edited this for clarity. As for discussing whether or not our conclusions would change if another IPCC value was used: our analysis is specifically designed to evaluate both the IPCC default value and a 50% wood CF assumption. Adding in another suite of IPCC default values would make our analysis quite extensive, without making additive contributions to our main result that species-specific wood CFs influence tree- and forest-level C stock estimates.

Minor concerns

Terminology: The paper uses “carbon fraction (CF)”, “wood carbon fraction”, and “wood CF” interchangeably. I suggest adopting “wood carbon fraction (CF)” throughout and defining it clearly at first mention.
We have now defined wood CF on first use in the abstract (Page 1, Lines 17-18) and have reviewed and ensured that “wood CF” is used throughout.

Figure 2 readability: The figure contains eight panels and is quite dense. It would be helpful to repeat in the caption the specific comparison for each panel (e.g., “Ctree1 vs. Ctree2”) to make it easier for readers to interpret without referring back to the text.
Done. We have revised our Figure 2 caption accordingly (Page 16, Lines 467-480).

Tables 1 and 2 layout: These tables span pages 13–14 and are rather crowded. Consider splitting central tendency and distribution statistics into separate tables, or using a more streamlined design.
Here, the reviewer is talking about Table 1 specifically (and not Table 2). We would prefer to retain Table 1 in its current form, in order to avoid having multiple tables with descriptive statistics. Otherwise, we would have two tables with redundant information (namely, all three of the left-most columns in the current Table 1 would be replicated twice in the manuscript).

Reference completeness: Confirm the publication status of Doraisami et al. (2024, 2026); if the 2026 reference is “in press” or forthcoming, please mark it accordingly and provide a preprint link if available.
Both of these articles are published, and the citations appear correct on our end.

Code and data availability statement: The statement reads “No custom code were used” (p. 21). However, the ANCOVA and mixed effects modelling clearly required R scripts. Please clarify whether this means “no new software package was developed” rather than “no code was written”, and consider depositing the analysis scripts in a public repository (e.g., Zenodo or GitHub) to enhance reproducibility.
Done. We have edited accordingly.