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Interactive comment on “Carbon stocks and soil sequestration rates of riverine mangroves and freshwater wetlands” by M. F. Adame et al.

Anonymous Referee #1

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General comments: In this manuscript, Adame and colleagues measured carbon pools in vegetation, downed wood, and soils of seven mangrove sites, one peat swamp forest, and one herbaceous marsh within Mexico's La Encrucijada Biosphere Reserve (LEBR). They also measured soil N pools at all sites and rates of soil C sequestration at the mangrove sites. The peat swamp and marsh sites appear to have been included only for the sake of making casual (rather than statistical) comparisons, since there was just one of each of those site types. The primary hypothesis was that soil C and N stocks and soil C sequestration rates would be higher in upland vs. lowland mangroves.

This is a very descriptive place-based study. Although the authors made some suggestions about why some sites store more carbon than others (e.g., perhaps it is related

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to geomorphology or the dominant tree species), this manuscript didn't leave me with any new ideas or insights about how wetlands "work." I didn't feel that it told me anything more than some specific information about one specific location, and I think that will limit its appeal to the diverse readership of Biogeosciences. As detailed below, I also have concerns about the number of sites (are they sufficient for scaling up to the landscape level?) plus some questions about the collection of data and presentation of results.

Specific comments: 1) A question about the wood density values that you used: in the Zanne et al. wood density database, there are multiple wood densities for each of the mangrove species at your site. How did you decide which values to use? For example, there are seven different *R. mangle* wood density values in the database (range: 0.810–0.105; average = 0.898; median = 0.890); why did you decide to use the value of 0.84? 2) p. 1030, lines 9-14: This calculation of C loss due to fire assumes that all vegetation and all soil C is converted to CO₂ when the site burns. Is that a reasonable assumption? If the marsh and peat swamp forests burn "frequently" (p. 1023, line 5) and all of the soil C is lost, it seems highly unlikely that you would end up with soils that have up to ~30% organic C (and this 30% average value is at 30-50% depth, Table 5). Unless you have evidence that says otherwise, it seems unreasonable to suggest that 100% of the ecosystem carbon in the marsh and peat swamp is oxidized to CO₂ whenever there is a fire. 3) p. 1016, lines 9-10, "We hypothesized that riverine wetlands have large C stocks. . ." and p. 1018, lines 112-13, "We predict that the riverine wetlands within the LEBR have large ecosystem C stocks and high C sequestration rates." Large C stocks and high C sequestration rates compared to what? Compared to non-riverine wetlands? Compared to a terrestrial forest? Compared to something else? 4) I had some trouble with the different ways that you described the sites. a. For the mangroves, you had three classes based on plant vigor, with Class I corresponding to upland mangroves and Class III corresponding to lowland mangroves. According to your text, Class II mangroves have intermediate vigor, although you don't provide enough information to let the reader know if Class II mangroves are a) intermediate in

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location between upland and lowland mangroves; b) upland mangroves that are less productive than the typical (Class I) upland mangrove; or c) lowland mangroves that are more productive than the typical (Class III) lowland mangrove. [Edit: some text in the Results section (p. 1026, lines 15-16) indicates that lowland mangroves include Classes II and III but I don't think the reader should have to scour the Results section to learn such basic details about your study sites, especially when one of your predictions/hypothesis is about the differences between upland and lowland wetlands.] b. How are upland and lowland mangroves defined? Is it based on elevation? Do you have elevation data for your sites? c. Then, I wasn't sure how your sites fell into riverine vs. non-riverine categories. Your prediction on p. 1018 (lines 13-16) suggests that some of your sites are riverine (perhaps some of all of the upland mangroves) whereas others (the lowland sites?) are non-riverine. However, in the Abstract (p. 1016, lines 11-13) suggests that all of your sites were riverine wetlands; in that sentence the values given are the same* as those from Table 6, which shows all of your study sites (* the standard error for the peat swamp differs between the Abstract and Table.) d. In Tables 4 and 5, you have sites named Zapotón and Tular. I assume that one is the peat swamp and one is the marsh, but you don't specify which is which. 5) Some of the numbers in the text do not agree with those in the tables. a. One example, compare p. 1025, lines 8-9 with Table 3. The text says that Las Palmas and Esterillo each had vegetation C stocks > 620 Mg C ha⁻¹ yet, according to Table 3 *none* of the sites had vegetation C stocks that were that high. The rest of the sentence then says that Santa Chila and Zacapulco had vegetation C stocks < 340 Mg C ha⁻¹. Since these sites actually had vegetation C stocks of 132 and 196 Mg C ha⁻¹, the authors' sentence is technically true but I wonder why they said "< 340 Mg C ha⁻¹" when "< 200 Mg C ha⁻¹" would be a better descriptor of the data. b. Another example: Table 7 reports a total of 27,477 ha of mangrove in the LEBR. Multiplying this by the average soil C sequestration rate of 1.3 Mg C ha⁻¹ yr⁻¹ produces a LEBR-wide C sequestration rate of 35,720 Mg C yr⁻¹; you report 27,762 Mg C yr⁻¹ (p. 1029, line 23). Using class-specific areas and sequestration rates, I come up with 34,013 Mg C yr⁻¹ which is still quite different from the number

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you reported. 6) I question whether you have enough sampling sites to come up with a good estimate of C stocks across the entire LEBR. There are three issues: a. There is a lot of variability from site to site. For vegetation (Classes II and III), downed wood (all classes), and soils (Classes II and III), the C stocks vary by a factor of two or more between sites of the same class. For Classes I and III, this variability fortunately cancels itself out such that total ecosystem C pools are reasonably similar within a single class. However, for Class II, there is a 2-fold difference in ecosystem C between the two sites. If Esterillo is the more typical Class II site, then you have underestimated (by ~25%) the amount of C in the ~7000 ha of Class II mangroves. Conversely, if Esterillo is an outlier and Santa Chila is a more-typical Class II site, you have overestimated C in this class by ~50%. Of course, with just two sites, you can't say which site is more typical of Class II mangroves. b. All of your sampling sites appear to be located along rivers. However, there are large areas of the LEBR that appear to be 1-2 km (or more) from a river. You have not justified that C data from riverine wetlands are comparable to mangrove areas that are far from rivers. c. You had one marsh site, which you then scaled up to the landscape scale. Is one marsh site representative of 32,000+ ha of marsh, which represents ~70% of all wetlands in the LEBR? If your measured C stocks at the single marsh site are not representative of all marshes in the LEBR, your estimate of total C stocks in the LEBR wetlands could be wildly inaccurate due to the large area of marsh. 7) The reporting of statistical results was poor; I counted only 5 p values in the entire text but quite a few instances of saying that something was "higher" or "lower" in a particular site/class. Should I assume that such comparisons don't have any statistical support behind them? The authors present a hypothesis about differences in soil C and N stocks between upland and lowland mangroves. Statistical results were reported for the C stocks (p. 1026, line 5) but only after one site was removed from the analysis – why was the site removed? Is it a statistical outlier? There were no statistics comparing soil N stocks between mangrove types. I don't think that the authors tested to see if there were differences in total (ecosystem) C stocks between sites. 8) Did your study design lead to a double counting of root biomass? You estimated below-

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ground biomass using allometric equations and you also measured soil C. There was no indication that roots were removed from the soil samples before the soil C content was measured.

Technical comments: 1) p. 1016, line 17: I think the units should be Mg C, not Mg C ha⁻¹. 2) p. 1019, line 1: Should this be LEBR, not LEBRE? 3) p. 1021, lines 2-4: Change start of sentence to read “Belowground root biomass for mangroves was calculated...” 4) p. 1021 and Table 2: As requested on the Dryad site for the wood density database, you should cite the Chave et al. (2009) article and the database itself (Zanne AE, Lopez-Gonzalez G, Coomes DA, Ilic J, Jansen S, Lewis SL, Miller RB, Swenson NG, Wiemann MC, Chave J (2009) Data from: Towards a worldwide wood economics spectrum. Dryad Digital Repository. <http://dx.doi.org/10.5061/dryad.234>). 5) Table 2: For belowground biomass of *L. racemosa*, why is DBH first raised to the power of 2.22 and then to the power of 1.11? Is this a typo? 6) p. 1022, line 16: hydrochloric does not need to be capitalized. 7) p. 1023, line 3, replace “of” with “and” 8) p. 1026, line 6: Is “downland” a synonym for “lowland”? 9) p. 1026, line 23: Change “de N” to “N” 10) Table 7: In the table itself, “emissions” is misspelled. 11) p. 1029, line 8: According to the wood density database I cited earlier, the maximum wood density for *A. germinans* is 0.90, not 0.99 g cm⁻³. 12) Figure 1: On the part of the figure that shows NDVI classes, there are 8 mangrove sampling sites (indicated by black squares) but you only had 7 actual sites. It looks like the “Esterillo” site on the NDVI figure is not shown on the larger map of the entire LEBR.

END OF REVIEW

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