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Interactive comment on "Shifting Mineral and Redox Controls on Carbon Cycling in Seasonally Flooded Soils" *by* Rachelle LaCroix et al.

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Overall I think this is a valuable dataset and a well-executed study. I support its eventual publication. The surface horizon data are well established and I have just minor comments there as indicated below. However, the subsurface depth data are problematic, mostly because there are different overall soil depths in each of the sites and different horizon designations. This has led also I think to some statements that are not well supported by the statistics or that the statistics used are not well presented. For instance, reading the abstract while looking at Table 3 raises several questions if we interpret "significantly lower" to mean different statistical lowercase letter assignments, which most readers will. Much of my confusion occurs in section 3.3, where it appears in most cases the differences described are not statistically significant as shown on

Table 3, but this is not pointed out in the text. At one point here the authors refer to a Tukey test for the topsoils (although I think they misplaced the word subsoil on pg. 12, ln 2) with a p value < 0.01 for the transition vs. lowland, but the those share a lowercase letter assignment in Table 3, which suggests they would not have a p value < 0.01. Correcting these presentation or interpretation issues is critical. Assuming the stats letter values are correct, I think this could be resolved by looking at C stocks rather than C concentrations at depth and backing off on some of the subsurface interpretations that are not fully supported by the stats. If the authors have data bined at finer depth intervals, that might help clarify things as well, but if not I suggest using C stock down to 68 cm, in which case one could compare equally across all the sites. One could examine surface C stock (0- 25 cm) and then a subsurface C stock value (25 – 68cm). Outside of this major issue, I think the paper has a lot of promise and the combination of field CO2 data and molecular-scale carbon chemistry is exciting.

Abstract I read and reviewed the abstract without looking at any other parts of the MS to mimic a reader looking at the abstract on-line. Read alone, I am not clear on the findings and implications and thus the abstract needs to be clarified. I give a couple of specifics below in the line edits, but I encourage the authors to have someone unfamiliar with the study read the abstract alone after revision.

Ln 14: Is it really true that this is largely unknown? If this is just for seasonally flooded mineral soils (compared to wetlands in general), then this point escaped me on the first read. Perhaps it was the shift from "seasonally flooded soils" in the previous sentence to "seasonally flooded mineral soils" in this sentence. Use one term and stick with in, especially in the abstract where space is tight. Ln 16: Need to specify here that the lowlands are periodically flooded and the uplands are notâĂŤif that is indeed the case. I am assuming that, but one could have uplands that are also periodically flooded due to high rainfall and perched water tables. Ln 17: This sentence is hard to follow. I read it twice and was still not sure what it was saying, where C was higher? I suggest "We found the lowlands had lower CO2 effluxes than the uplands. Lowland surface

soils (0-20 cm. . .or whatever it isâĂŤalso could give A or B or O classification) had higher C concentrations a higher abundance. . .than the uplands." Ln 20: Here I was confused again by subsoils slipping in there. I think you need to be much more upfront about this distinction as it is one of the main points of the abstact. At the end you also start to talk about C stocks (depth integrated concentrations), which would take into account bulk density. Consider discussing that here instead of concentration? Ln23: It is not clear what non-reducible Al phases are being relied on for here? I assume mineral protection, but best not to have readers assuming in the abstract. Ln 24-25: The three reasons given for why you see more C in the topsoils than the subsoils are not supported in the abstract by any data. Either include this data upfront (i.e., lowland had low/zero O2, whereas uplands had O2 above X%; also data on roots and Fe presence/abundance) or you could simply state that these C findings correlated with O2, roots and Fe, implying the data is in the paper, but not fully presented. What you are asking the reader to do here is accept this statement without any sense that it is supported by data in this paper and that is not comfortable to many readers (and me I suppose). Ln26: Again, without O2 data or mineral protection data, how could you conclude this. I assume it is in the rest of the paper. . .but I have not read that yet if I am most readers.

Introduction The introduction does a nice job setting the stage although I suggest line edits below. Pg. 2 Ln 14-15: Revise for clarity. Ln 16: Maybe not "model ecosystems" but essential "endmembers". Ln 19: This is an "endash" and you want an "emdash" here. A longer dash, that should not have spaces around it. On MS-Word you hit dash twice between words without adding spaces and word turns it into an emdash. Do this elsewhere in the text. Ln24: "seasonal wetlands" or seasonally flooded mineral wetlands, choose one term and stick with it through-out the MS.

Pg. 3 Ln 5: "catalyze" Ln 10: instead of chemically-reduced, "lower valance" would be more precise. Ln 17: ", but the impact of roots on soil C. . ." Ln 18: "growth due to low DO (Day. . .)" Ln 25: "distribution of high surface area minerals that are excellent

sorbents for C in soils"

Pg 4 I point out three of our recent papers that are highly relevant to this introduction/discussion, but which were not likely available when this was drafted. Ln 10: See Chen et al 2018 ES&T and Chen and Thompson 2018 ES&T on these topics Ln 17: See Barcellos et al 2018 Soil Systems on this topic Ln 25: "measurements of soil. . ."

Methods Well done, except that more description of the stats used are required potentially to clarify issues I raise above and below with regard to Table 3 lowercase lettering.

Results Main issue in this section is the depth that is considered 'subsurface'. How does one determine this for soils with different depths or thicknesses? Normally, this doesn't matter, but in this case the authors are making a key argument about the C and Fe interactions and chemistry "at depth". Examining the C horizons, total C is actually higher in the lowland than in the upland and this would be true even if we examined C concentrations at 25 cm across the sites. If we go deeper, then the Cg of the transition and the lowland are equally low and the upland is higher, but not statistically higher based on the lower case letter assignments. The same is true in inverse for the lowest depth for Fe-o, it is highest at the lowland, but this is not significant from the other sites. This makes statements like "C concentrations were significantly lower in the lowland than in the upland subsoils", which is in the abstract, incorrect based on the authors' assignment of letters (see Table 3). Pg. 11 Ln 9: Assuming that Feb – June is the wet period, but you should tell readers that explicitly. Ln 11: "significantly lower than in. . ." Ln 13: "season lowland VMC. . ." Ln 21: Maybe it would be helpful to calculate the EH7 values here so that these could be compared with other studies and compared between the surface and subsurface horizons. Ln 23: change "mineralogy"âĂŤwhich is the study of mineralsâĂŤto "mineral composition". Do this elsewhere as well. Pg. 12 Ln 1: The data are more complex than this statement suggests. Please revise. Ln 2: Do you mean topsoil here???? Because actually it is over 8 times the subsoil, but according to the letters, the lowland and transition topsoil are equal within error. Ln 4: although this was not statistically significant, correct? I suggest adding that information. Ln 7:

Although again this was not statistically significant, right? Tell the reader that. Ln 9: True, except in the upland, right (Table 3 indicates it is not significant). Ln 12: Change 'determine' to 'predict' Ln 18: "concentrations decreased along. . ." Ln 20: Change 'identify' to 'predict' Ln 24: Maybe not Eh, but likely O2, right? Ln 25: Change 'effect' to 'influence' Pg. 13 Ln 17: "across the upland to lowland transect. . ." Ln 18: ". . .(-11%) moving from upland to lowland." Fig. 2: Symbols are hard to tell from one another. Consider using squares, triangles and circles. Cool could help too since other figures are in color. Fig. 2: Are the Eh values on these graphs corrected for pH? To allow comparisons between the depths/sites?

Discussion

Pg. 13 Ln 22: Change 'demonstrate' to 'suggest' Ln 23: ". . .transects, but exhibit potentially inverse trends in the subsurface." Pg. 14 Ln 1-2: delete sentence. Ln 4: "Our field data support our hypothesis that reducing. . ." Ln 16-20: Clarify this section. Ln 24: Note that the figure shows topography that is not flat. Pg. 15 Ln 7: Consider using C stocks instead of concentration, which would help get around the depth issue. Pg. 17 Ln 23: OK, but Cg in the lowland is 2nd highest across ALL sites/depths, so this statement doesn't ring fully true for me.

Conclusions Ln 10: change 'related to' to 'correlated with' Ln 12: But, again what about Fe-o in the lowland Cg?????

References (1) Chen, C.; Thompson, A., Ferrous iron oxidation under varying pO2 levels: The effect of Fe (III)/Al (III) oxide minerals and organic matter. Env. Sci. Technol. 2018, 52, 597-606.

(2) Chen, C.; Meile, C.; Wilmoth, J.; Barcellos, D.; Thompson, A., Influence of po2 on iron redox cycling and anaerobic organic carbon mineralization in a humid tropical forest soil. Env. Sci. Technol. 2018, 52, 7709-7719.

(3) Barcellos, D.; O'Connell, C.; Silver, W.; Meile, C.; Thompson, A., Hot spots and

hot moments of soil moisture explain fluctuations in iron and carbon cycling in a humid tropical forest soil. Soil Systems 2018, 2.

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