## RESPONSE TO REVIEWERS' COMMENTS

Regular font: original comment by the reviewer
*Italized text:* response by the authors
*"Italicized quotes":* revised text segments

## Response to Comments by Referee #1

*Comment:* 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.

*Authors' response: We thank Dr. Thompson for these extremely thoughtful and constructive comments. We took the following steps to assure that the statistical tests and the resulting estimates of significance are reported correctly.*

*First, we emphasize that the statistical analyses were done on square root transformed data, but the data provided in Table 3 is the non-transformed observational data. We have clarified this in the methods section on Page 10: " Statistical analyses were conducted on square root transformed data when assumptions of normal distribution were not met, although non-transformed observational values are reported within the text (Table 3)."*

*Second, we clarified our statements regarding the significant differences in C contents in top and subsoils across the transect. We believe that the confusion arose from the fact that we made statements in the text (both abstract and main body) that were not reflected in the statistics reported in Table 3. The results presented in Table 3 are based on a very conservative approach (Tukey's honesty significance) to determine significance of the means. This test yielded the letter designations reported in Table 3. The ANOVA model uses and distributes the variance for the entire dataset and the 36 individual comparisons. In essence, the high variability, particularly in the A-horizons, is distributed across the entire model. Consequently, an ANOVA model taking all 36 horizons into account yielded no significant differences, even though the means in some cases seemed different from an ecological perspective (2.3 versus 8.5% in the A horizons and 0.4 versus 0.6% in C/Cg horizons). Because we were primarily concerned with comparing C contents across individual horizons along the transect (e.g., comparing A horizons across upland, transition and lowland position), we had decided to conduct additional ANOVAs on a horizon-basis. Those tests indicated that both A and C horizons in upland and lowland positions are significantly different. We had presented the results from this analysis, including the p*

*values indicating significant differences among the horizons, in the abstract (Ln: 20-22) and results section (Pg. 11 Ln:21-25 ). In the revised version, we explain these additional comparisons in the text and added the full results table to the SI (Table S7). Additionally, Table 3 was updated to indicate the significance based on horizon groups. We hope that these measures will clarify this issue.*

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.

*Authors' response: We thank the reviewer for this suggestion. We believe the above explanation of the statistical approach taken to support our interpretations resolves the confusion about the stats letters.*

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.

*Authors' response: We thank the reviewer for this suggestion, but believe that the measures taken above to clarify our statistical approach sufficiently address this concern.*

Abstract

Comment: 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.

*Authors' response: We revised the abstract in response to the specific comments provided below. Most importantly, we ore explicitly included the data that allowed to arrive at our conclusions.*

Comment: 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.

*Authors' response: We agree that the terminology used should be more consistent as 'seasonally flooded mineral soils'. The abstract and manuscript will be updated accordingly. These ecosystems (seasonally flooded mineral soils; i.e. vernal pools, ephemeral ponds, etc.) have little information published research focused soil C cycling, especially with depth.*

Authors' changes: *Ln13-15: "Among wetlands, seasonally flooded mineral soils are likely the most vulnerable to increased severity and duration of droughts in response to climate change.*

*Yet, the relative influence of associated changes in oxygen limitations, root dynamics, and mineral protection on C cycling in seasonally flooded mineral soils is largely unknown."*

Comment: Ln 16: Need to specify here that the lowlands are periodically flooded and the uplands are not if 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.

*Authors' response: This point has been clarified in the abstract. The lowlands are in fact seasonally flooded whereas the transition and upland position are not.*

*Authors' changes: Ln15-18: "To address this knowledge gap, we combined seasonal monitoring of soil moisture, redox potential, and $CO_2$ efflux with a characterization of root biomass, C quantity, and mineral and organic matter composition in both top- and subsoils along upland-to-lowland transects in a temperate deciduous forest. Specifically, we contrasted mineral soils in lowland positions that experience seasonal flooding with adjacent upland soils that do not."*

Comment: 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 isâA Ť˘also could give A or B or O classification) had higher C concentrations a higher abundance. . .than the uplands."

*Authors' response: We revised the sentence as suggested.*

*Authors' changes: Ln18-19: "We found the lowland soils had lower $CO_2$ effluxes than the upland soils. Lowland surface soils (A-horizons) had higher C concentrations and a higher abundance of high molecular weight and chemically reduced organic compounds than the uplands."*

Comment: 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 abstract. 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?

*Authors' response: We revised the abstract to more clearly distinguish subsoil from topsoil results as well as C stocks from C concentration.*

*Authors' changes: Ln20-21: "These results indicate that selective preservation of organic compounds during anaerobic periods caused C accumulation in seasonally flooded surface soils (A horizons). In contrast, seasonally flooded subsoils ($C_g$-horizon) had lower C concentrations."*

Comment: 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.

*Authors' response: This statement was implying that 'non-redox active' Al phases, measured as ammonium oxalate extractable Al, are more strongly correlated with C than ammonium oxalate extractable Fe, which we considered "redox active". However, we agree this sentence is confusing/misleading and have revised it.*

*Author's changes: Ln 23-24 : "C concentrations in the subsoils were strongly correlated with ammonium oxalate extractable Al phases rather than redox potential."*

Comment: 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).

*Authors' response: We recognize that the statements within the abstract are not presented with the data to support these claims. We have revised this statement to reflect that there were correlations found between C and roots, Fe and redox potential and the supporting data is found in the main manuscript.*

*Authors' changes Ln 24-25: "Our linear mixed effects model demonstrated that redox potential was the better predictor for C concentrations in the topsoils (A horizon) of seasonally flooded soils, suggesting that anaerobic conditions are responsible for C accumulation."*

Comment: 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.

*Authors' response: We have addressed this issue in our revision of the abstract.*

Introduction

Comment: The introduction does a nice job setting the stage although I suggest line edits below.

*Authors' response: We appreciate this comment and the subsequent revision suggestions for clarity.*

Pg. 2

Comment: Ln 14-15: Revise for clarity.

*Authors' response: The authors have revised the sentence as suggested.*

*Authors' changes: Ln14-15: "Seasonal wetlands can be considered early warning ecosystems (Brooks, 2005); forecasting the impacts of climate change on permanently flooded mineral wetlands."*

Comment: Ln 16: Maybe not "model ecosystems" but essential "endmembers".

*Authors' response: We agree that 'endmembers' is a more suitable representation of these ecosystems.*

*Authors' changes :Ln18-19: Thus, seasonally flooded wetlands represent essential endmembers to study the effects of climate change on larger permanently flooded wetland soils (Brooks, 2005).*

Comment: 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.

*Authors' response: Thank you for noticing and correcting the endash/emdash inaccuracies. We have corrected all throughout the text.*

Comment: Ln24: "seasonal wetlands" or seasonally flooded mineral wetlands, choose one term and stick with it through-out the MS.

*Authors' response: We agree the terminology needs to be more consistent. We have revised the manuscript to be more consistent with the term 'seasonally flooded mineral soils'*

*Authors' changes: Ln24-25: "Determining the controls on C cycling within seasonally flooded mineral soils thus requires specific consideration of the fluxes and dynamics across these terrestrial-aquatic transitions."*

Pg. 3

Comment: Ln 5: "catalyze"

*Authors' response: We revised the sentence, replacing "catalyzing.." to "..which catalyze..".*

*Authors' changes: Ln4-6: "The resulting oxygen limitations inhibit the activity of oxidative enzymes which catalyze the depolymerization of higher-molecular weight OM into smaller, assimilable compounds (Megonigal et al. 2003)."*

Comment: Ln 10: instead of chemically-reduced, "lower valance" would be more precise.

*Authors' response: The authors appreciation this suggestion. To be consistent with other publications on the subject, we would like to retain the 'chemically-reduced' terminology, but also reference "oxidation state" in the revised version.*

*Authors' changes: Ln9-11: "Anaerobic conditions limit microbes to utilizing substrates that are chemically more oxidized, in turn preferentially preserving more chemically-reduced organic compounds (i.e., compounds with lower C oxidation states) in soils and sediments (Boye et al. 2017; Keiluweit et al., 2017)."*

Comment: Ln 17: ", but the impact of roots on soil C. . ."

*Authors' response: We thank the referee for this revision, which adds more clarity to the sentence.*

*Authors' changes: Ln17-18: "Roots are the main contributors to C stocks in upland soils (Rasse et al., 2005), but the impacts of roots on soil C stocks in wetlands is less clear."*

Comment: Ln 18: "growth due to low DO (Day. . .)"

*Authors' response: We altered the sentence as suggested.*

*Authors' changes: Ln18-19: "Water saturation directly inhibits root growth due to low dissolved oxygen concentrations (Day and Megonigal, 1993; Tokarz and Urban, 2015)."*

Comment: Ln 25: "distribution of high surface area minerals that are excellent sorbents for C in soils"

*Authors' response: We thank the reviewer for this constructive edit and revised the sentence as suggested.*

*Authors' changes: Ln24-25: "In addition to restricting microbial metabolism and root growth, water saturation also influences the concentration and distribution of high surface area minerals that are excellent sorbents for C in soils …"*

Pg 4

Comment: 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.

*Authors' response: We thank the reviewer for pointing out these highly-relevant, recently published papers. We incorporated the following citations accordingly.*

Comment: Ln 10: See Chen et al 2018 ES&T and Chen and Thompson 2018 ES&T on these topics

*Authors' response: We incorporated these citations as requested.*

*Authors' changes: "Further, Al oxides, rather than Fe oxides, are the predominate mineral phases contributing to OM retention in forested floodplain sediments because their solubility is controlled by pH rather than redox conditions (Darke and Walbridge, 2000), and may thus play a critical role in mineral protection in seasonally flooded soils (Chen et al., 2018; Chen and Thompson, 2018)."*

Comment: Ln 17: See Barcellos et al 2018 Soil Systems on this topic

*Authors' response: We incorporated these citations as requested.*

*Authors' changes: "How the relationships between C and important biogeochemical controls differ in systems that undergo longer, yet not permanent, periods of water saturation is still in question – especially with depth (Barcellos et al., 2018)."*

Comment: Ln 25: “measurements of soil. . .”

*Authors’ response: We revised this sentence as suggested.*

*Authors’ changes: Ln23-25: “To accomplish our first objective, we related soil $CO_2$ efflux at three landscape positions (upland, transition, and lowland) spanning the transect over the course of a full drainage and flooding cycle to measurements of soil temperature, moisture, water table depth and redox potential.”*

Methods

Comment: 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.

*Authors’ response: We agree that a more detailed description of the statistics used to generate table 3 are needed. In the revised version, we detail comprehensively how the data was binned and compared for this analysis. Briefly, included the R packages we used to determine letter designations using Tukey HSD comparisons (agricolae and MulticompView) where the alpha value was set at 0.05 for different letter designations. Within the analysis, the samples were tested for significance based on the landscape position and horizon. Additionally, as mentioned previously, we will include Table S7 in the supplemental material that provides the horizon-based ANOVA analysis mentioned above.*

*Authors’ changes: ‘ Statistical analyses were conducted on square root transformed data when assumptions of normal distribution were not met. Tukey’s honestly significance difference (HSD) tests were conducted in Rstudio using packages agricolae (de Mendiburu, 2017), and multcompView (Graves et al., 2015) with an alpha value of 0.05 used for different letter designations. Due to the conservation nature of using a Tukey’s HSD on our dataset, we additionally tested differences among horizon groups (i.e. across A or C/Cg horizons) using analysis of variance (ANOVA) tests (Table S7).’*

Results

Comment: 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).

*Authors’ response: We hope that additional explanation regarding the statistical tests and inferences provided above make it clear that there are statistical differences among A and C/Cg*

*horizons, respectively. This statement was not made based on the ANOVA results for the full data table, but for individual comparisons on a horizon-basis.*

Pg. 11

Comment: Ln 9: Assuming that Feb – June is the wet period, but you should tell readers that explicitly.

*Authors' response: The authors realize there had not been a clear definition of the growing and non-growing season designations within the methods section. We have added this definition to page 10 under statistical analyses. We added reminders in brackets on page 11.*

*Authors' changes: Pg10, Ln9-10: "Regression analyses were conducted for the entire year-long dataset, and for the growing and non-growing seasons defined as May through September and October through March, respectively."*

*Page 11:*
Comment: Ln8-9: Assuming that Feb – June is the wet period, but you should tell readers that explicitly.

Authors' response: We added a line to more clearly point out the flooded period of the lowland position in relation to the growing season parameter.

*Authors' changes: Ln9-10: "The flooded period (February through June) of the lowland position extended into the first two months of the growing season."*

Comment: Ln 11: "significantly lower than in. . ."

*Authors' response: We have revised the sentence, but where the reviewer suggested 'lower' it should actually remain 'greater'.*

*Authors' changes: Ln11-13:" This general difference became even more pronounced when cumulative $CO_2$ emissions were normalized to C content, with the upland position showing significantly greater emissions than in the transition (p-value <0.001; Tukey's HSD) and lowland (p-value <0.001, Tukey's HSD) positions."*

Comment: Ln 13: "season lowland VMC. . ."

*Authors' response: We revised the sentence as suggested.*

*Authors' changes: Ln21-22: "Soil moisture was consistently the greatest in the lowland position; during the growing season lowland VMC was 20% greater than the upland position (p-value < 0.05; Tukey's HSD), and 15% greater in the non-growing season (p-value < 0.05; Tukey's HSD) (Table S1)."*

Comment: 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.

*Authors' response:* *The variations in pH values across our site is minimal and within the margin of error (4.98-5.43). Correcting the Eh values for pH would change Eh values by less than a decimal point. For example, an Eh value of 400 mV would correspond to Eh7 values of 399.91 mV at pH 5.43 and 399.88 mV at pH 4.98, respectively. These two pH values cover the range of values observed at our site, so we concluded that normalizing Eh to pH wouldn't change the numbers sufficiently to warrant inclusion.*

Comment: Ln 23: change "mineralogy" which is the study of minerals to "mineral composition". Do this elsewhere as well.

*Authors' response:* *We agree with this correction and have corrected it throughout the manuscript.*

Pg. 12

Comment: Ln 1: The data are more complex than this statement suggests. Please revise.

*Authors' response:* *We have revised the topic statement as suggested.*

*Authors' changes:* *Ln7: "3.3 Relating carbon concentration to root biomass and mineral composition across upland-to-lowland transitions"*

Comment: 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.

*Authors' response:* *We're thankful the reviewer caught this misspelling. We replaced subsoil with topsoil.*

*Authors' changes:* *Ln11-12: "C concentrations in the lowland position topsoil were two and four times greater than the transition (p-value < 0.01; Tukey's HSD) and upland positions topsoil (p-value < 0.001; Tukey's HSD), respectively."*

Comment: Ln 4: although this was not statistically significant, correct? I suggest adding that information.

Comment: Ln 7: Although again this was not statistically significant, right? Tell the reader that.

*Authors' response:* *In regards to the two comments above, we included a more detailed explanation of our statistical approaches as outlined in our response to the more general comments above. Again, these statements were supported by our horizon-based ANOVA results (Table S7). Using a Tukey's HSD (which is reported by letter designations in Table 3) is an overly conservative approach for our purposes as it accounts for the variance within the whole dataset. Conducting a horizon based ANOVA test allowed us to compare only the comparable horizons and include the variances within those horizons when testing for differences. The ANOVA results show a significant difference among positions when comparing the A-horizons*

*(p-value = 0.02), in the upland B- and transition and lowland C-horizons (p-value = 0.04) and the upland C- and transition and lowland Cg-horizons (p-value = 0.01). These findings support our statistical significance of the differences in C concentrations among the three positions in the adjacent horizons. To clarify the statistics for the reader, we have adjusted Table 3 to reflect the ANOVA results reported in Table S7. We have also added these p-values with the text ANOVA, to signify where the significance is derived. We have also included references to Table S7 when referring to the C concentration significance.*

*Authors' changes: Ln9-11: "Along the upland to lowland transects, C concentrations in the surface horizons significantly increased (p-value < 0.05, ANOVA) whereas concentrations in the subsurface horizons decreased along the transect (p-value < 0.05, ANOVA) (Table 3 and S7)."*

*Authors' changes: Ln12-15: "In contrast, the subsoils in the upland positions had nearly double the C concentrations than the subsoils of the transition and lowland positions (p-value < 0.05, ANOVA, Table S7)."*

Comment: Ln 9: True, except in the upland, right (Table 3 indicates it is not significant).

*Authors' response: Although we're not entirely sure, we believe the reviewer is referring to the silt and clay data. The statement has been adjusted in regards to significance.*

*Authors' changes: Ln14-16: "Silt and clay content increased from the upland to the lowland positions, particularly in the subsoil, although shifts in silt and clay contents were not statistically significant (+33%, Table 3)."*

Comment: Ln 12: Change 'determine' to 'predict'

*Authors' response: We have made the suggested change.*

*Authors' changes: Ln24: "To predict the relative influence of roots, mineral composition and $E_h$ on C concentrations in each landscape position…"*

Comment: Ln 18: "concentrations decreased along..."

*Authors' response: We have made the suggested change.*

*Authors' changes: Ln4-6: "The model results show that as the importance of redox-active $Fe_o$ as a predictor for soil C concentrations decreased along upland-to-lowland transects, the importance of $Al_o$ increased."*

Comment: Ln 20: Change 'identify' to 'predict'

*Authors' response: We have made the suggested change.*

*Authors' changes: Page13, Ln7: "To predict the influence of the biogeochemical variables on soil C concentrations with soil depth…"*

Comment: Ln 24: Maybe not Eh, but likely O2, right?

*Authors' response: We agree that it is oxygen availability, and not Eh, that is likely what causes higher C concentrations. We have adjusted the statement to reflect this detail.*

*Authors' changes: Page 13, Ln11-13: "These results indicate that, among the tested biogeochemical variables, $E_h$, a proxy for oxygen availability, has a predominant influence on C concentrations in the surface soils, while $Al_o$ has the strongest influence on C concentrations at depth. "*

Comment: Ln 25: Change 'effect' to 'influence'

*Authors' response: We have made the suggested change.*

*Authors' changes: Page 13, Ln11-12: "while $Al_o$ has the strongest influence on C concentrations at depth."*

Pg. 13

Comment: Ln 17: "across the upland to lowland transect. . ."

*Authors' response: We have made the suggested change.*

*Authors' changes: Page14, Ln4: "increases across the upland to lowland transect"*

Comment: Ln 18: ". . .(-11%) moving from upland to lowland."

*Authors' response: We have made the suggested change.*

*Authors' changes: Page 14, Ln5: "lipids decreases (-11%) moving from upland to lowland (Fig. 6b, Table S5)."*

Comment: 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.

*Authors' response: We thank the reviewer for this suggestion and have modified the figure to include color for a clearer designation between positions and depths.*

Comment: Fig. 2: Are the Eh values on these graphs corrected for pH? To allow comparisons between the depths/sites?

*Authors' response: We refer to our comment above about the minimal effect of pH on Eh values.*

Discussion

Pg. 13

Comment: Ln 22: Change ‘demonstrate’ to ‘suggest’

*Authors’ response: We have made the suggested change.*

*Authors’ changes: Page14, Ln10-11: “Our results suggest that the factors regulating $CO_2$ emissions and C accumulation shift…”*

Comment: Ln 23: “...transects, but exhibit potentially inverse trends in the subsurface.”

*Authors’ response: We have made the suggested change.*

*Authors’ changes: Page 14, Ln10-12: “Our results suggest that the factors regulating $CO_2$ emissions and C accumulation shift as predicted in surface soils along the upland to lowland transects, but exhibit potentially inverse trends in the subsurface.”*

Pg. 14

Comment: Ln 1-2: delete sentence.

*Authors’ response: We deleted the suggested sentence.*

*Authors’ changes: Page 15, deleted sentence “Our results show how seasonal flooding affects redox conditions, root biomass, and mineral composition as well as their impact on $CO_2$ efflux, C accumulation, and C chemistry across the upland to lowland transects.”*

Comment: Ln 4: “Our field data support our hypothesis that reducing. . .”

*Authors’ response: We have made the suggested change.*

Comment: Ln 16-20: Clarify this section.

*Authors’ response: We revised this section for clarify.*

*Authors’ changes: Page 15, Ln4-9: “Although the lowland soils become oxygenated in the non-growing season due to the water table drop, we observed near equal $CO_2$ emissions from the three landscape positions (Table 1, Fig. 2a). A possible explanation for this convergence in $CO_2$ emissions during the non-growing season could be the low seasonal temperatures (-1.7 to 10°C) which inhibit microbial activity. In other words, even when drainage allows for aerobic metabolism in the lowland soils, respiration rates still remain limited due to low temperatures.”*

Comment: Ln 24: Note that the figure shows topography that is not flat.

*Authors' response:* *We appreciate this correction, and have updated the statement to reflect figure 1.*

*Authors' changes:* *Page 15, Ln9-10: "Given the proximity of our three positions and minor change in elevation, aboveground litter inputs can be considered equal across the transect."*

Pg. 15

Comment: Ln 7: Consider using C stocks instead of concentration, which would help get around the depth issue.

*Authors' response:* *The authors agree that using C stocks would be beneficial. Due to the lack of accurate bulk density data for this site, however, we have chosen to focus on concentrations.*

Pg. 17

Comment: 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.

*Authors' response:* *Despite the Cg horizon in the lowland positions being the 2nd highest horizon, it still had overall lower concentrations of both extractable Fe and Al compared to the other positions. Which has led us to conclude that even though new C inputs could occur belowground, there is still a limited mineral protection capacity within the lowland positions.*

*Authors' changes:*

*Pg. 16, Ln 9-11: "An observed, but insignificant, increase in $Fe_o$ concentrations in the lowland $C_g$-horizons are likely a reflection of vertical transportation of soluble Fe phases. Despite this trend, there were overall lower concentrations of potentially reactive Fe and Al in the lowland $C_g$ horizon when taking into account both extractable Fe and Al contents (Table 3)."*

*Pg. 18, Ln 14-20: "Additionally, seasonal flooding over pedologic time scales has resulted in a total overall loss of reactive minerals and metals and thus diminished the potential capacity of the soils to accumulate C through other means. Recent studies suggest that colonization by deep-rooting upland plants will offset some of the C loss upon drainage of former wetlands through additional C inputs (Gorham et al., 1991; Lal 2008). The overall lower concentrations of reactive Fe and Al phases observed in seasonally flooded soils investigated here suggests a low capacity for new C inputs to associate with reactive Fe or Al phases, and, consequently, a low potential to offset the losses of anaerobically protected C upon drainage."*

Conclusions

Comment: Ln 10: change 'related to' to 'correlated with'

*Authors' response:* *We have made the suggested change.*

*Authors' changes: Ln21-22: "In the subsoil of seasonally flooded soils, anaerobic protection of C appears to be less important. C accumulation was low and primarily correlated with to $Al_o$,…"*

Comment: Ln 12: But, again what about Fe-o in the lowland Cg?????

*Authors' response: The reviewer points out here a trend in our dataset that we do not fully address, an apparent increase in $Fe_o$ within the Cg horizon in the lowland position. While it is clear that Fe is lower in the A and C horizons, this increase in Fe in the Cg horizon is likely due to vertical transport of soluble Fe. To address the slight increase in Feo in the lowland Cg we have added a comment in the 4.2 discussion section.*

*Authors' changes: Section 4.2, Page 16: Ln 9-10: "An observed, but insignificant, increase in $Fe_o$ concentrations in the lowland Cg-horizons are likely a reflection of vertical transportation of soluble Fe phases and reprecipitation at depth."*