

## ***Interactive comment on “Saltwater reduces CO<sub>2</sub> and CH<sub>4</sub> production in organic soils from a coastal freshwater forested wetland” by Kevan J. Minick et al.***

### **Anonymous Referee #1**

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**General Comments** The authors recognize the threat of saltwater intrusion caused by sea-level rise on non-tidal coastal forests and, using laboratory incubations, test whether additions of salt and coarse woody debris (CWD) change biogeochemical and microbial outputs. They find, among other factors, that salt water reduces total and soil organic carbon and microbial biomass, increases general seawater ions (SO<sub>4</sub>, Na, Cl, NH<sub>4</sub>, NO<sub>3</sub>, PO<sub>4</sub>, Ca, Mg, K), and over time, and stabilizes pH and Eh more quickly in the presence of CWD. Some enzymatic activity shifts, especially with coarse woody addition, δ<sup>13</sup>C effects are largely unchanged with CWD but significant effects in absence of CWD. Cumulative CO<sub>2</sub> and CH<sub>4</sub> emissions are reduced with salt, but CWD with FW addition only stimulates CH<sub>4</sub> production.

C1

As noted, there is not a large literature on seawater intrusion into these non-tidal systems (I suspect because tidal systems will experience salt intrusion first, thus are the more timely systems of concern), but the postulated scenarios are reasonable, thus providing relative insights into responses of these systems. I appreciate the synthetic discussion and request a few details in my comments to help the reader advance from point to point in the same way the authors have.

1. Does the paper address relevant scientific questions within the scope of BG? yes
2. Does the paper present novel concepts, ideas, tools, or data? I'm not sure about novelty
3. Are substantial conclusions reached? 4. Are the scientific methods and assumptions valid and clearly outlined? I'd like to see hypotheses clearly stated
5. Are the results sufficient to support the interpretations and conclusions? Yes, with some specific clarifications requested
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? yes
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? adequate
8. Does the title clearly reflect the contents of the paper? I think so, but a comment included below seems to contradict the title and Figure 2
9. Does the abstract provide a concise and complete summary? yes
10. Is the overall presentation well structured and clear? yes
11. Is the language fluent and precise? Yes, with some subject-verb agreement errors and a few run-on sentences (L83) [There are many cases where subject-verb agreement is not in alignment. e.g. L280 activity...were should be activity...was; L299 “enzyme ... were” should be enzyme ... was] L317 should be “a” one-way ANOVA, no?
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? yes
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Might include some of the data driving equations in supplemental sections
14. Are the number and quality of references appropriate? perhaps
15. Is the amount and quality of supplementary material appropriate? No supplemental received

C2

Specific comments Introduction I would have preferred to see clear hypotheses outlined in the last paragraph of the Introduction. The next to last paragraph reads more like Methods to me

Methods I cannot speak in depth to the methods used for isotopic analyses or microbial enzymatic processes. The authors do not disclose the methods used by the NCSU laboratory for the samples they sent to that unit for analysis; I would prefer they do (presumably ion chromatography, and NDIR?). Have the authors any general physico-chemical descriptions of the field soils from where the incubation matrix was collected to help contextualize the work? It seems that other terminal electron acceptors (specifically nitrate) would be useful covariates across the plots that might affect whether a system reaches sulfate reduction, perhaps. It isn't essential that this be provided, but I suggest an interesting consideration if the data are available. . . The temperature and precipitation data provided are useful (L152), but I'd also like to see the range of these values since over such a long timespan. Might the authors comment on the saltwater treatment levels they selected? These are rather high for a non-tidal system, and the high treatment would be oligohaline in a tidal system. Have levels this high been seen in some nearby areas? L210: Please allay any concerns of positive pressure effects in the chambers during the ~2week intervals between sampling toward the end of the incubation.

Results L339-341: The authors fall into a common trap suggesting that even though a mean is of a different magnitude, that the results vary. They do not. The statistics do not support that wood-amended soils were depleted – the statistics suggest equivalency if all of them are denoted with an “a”. (and discussion) Figure 2. I'd like to see something in the discussion related to the pattern of CO<sub>2</sub>:CH<sub>4</sub> reported in the Results. The trend in wood free is parabolic but linear upward in wood-amended. Is that useful? Does this suggest that there an optimal ratio of CWD and salinity that might be targeted to minimize GHG emissions as sea-level rises? L396+: I believe this interpretation follows the same trap noted in L339-341. It is accurate to say MBC was lowest in the dry

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treatment across un-amended treatments and lowest in the 5ppt amended treatments.

Discussion L424-425: what C cycling processes are the authors suggesting balance out the reductions in CO<sub>2</sub> & CH<sub>4</sub>? L426 & Figure 2L: I must be missing something, so I suspect other readers will as well. Panels B & E show that the wood-amended plots drop CO<sub>2</sub> and CH<sub>4</sub> with salt water addition (+2.5 & +5.0 ppt), but the text says it enhances CH<sub>4</sub> under saltwater additions. Can you provide clarity? If this is actually referring to the difference (panels C & mostly F), then it seems that the CH<sub>4</sub> emissions with CWD are essentially on balance (at the 0 line), no? I've interpreted that saltwater is different than freshwater amendment (A vs B), but the saltwater additions seem to cross the 0 line with the variance. L432: the sentence is almost verbatim earlier in the manuscript (L154). Please revise so each occurrence is unique and not redundant Minor quibble: the hydroperiod operates constantly. I suggest these systems RESPOND over short time scales, but to state they operate on short time scales seems a bit misleading. Even no water is reflected in the hydroperiod in some way, isn't it?

Technical corrections (in addition to a few pointed out previously) L126: The sentence beginning on L126 (“Although many studies. . .”) is unnecessary. That statement was clearly outlined previously in the introduction and does not narrow their research into what they will test and what they expect to find (via the recommended hypotheses addition).L142: why note 13 plots if you only used 4? L199: what year were the trees harvested? L202 & L204: are the 6 rings mentioned in 204 the mean of the 5-7 rings in 202? L248: add (MBC) after spelling out microbial biomass C L286: enzyme XYL is not defined in the 5 above L385: please be more precise than “the last couple” L421: recommend authors use the defined abbreviation “SLR” instead of sea-level rise (else, why define it earlier?) L466: over time (add space) Table 1: please provide units of the ions Figure 2: Please confirm that the labels for panels B & E follow those of C & F (and not A & D). Would you consider a different title for panels C & F? It took me a while to understand that you were reporting the DIFFERENCE between the two, and it wasn't some sort of range (the hyphen notation threw me off). Perhaps “Difference

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between wood-amended and wood-free”?

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