

Interactive comment on “Saltwater reduces CO₂ and CH₄ production in organic soils from a coastal freshwater forested wetland” by Kevan J. Minick et al.

Kevan J. Minick et al.

kjminick@ncsu.edu

Received and published: 18 July 2019

Sorry for the delayed response! The authors and co authors responses are given below and appear following the reviewers comments. We will upload the revised manuscript once we have completed responses to the second reviewers comments.

Interactive comment on “Saltwater reduces CO₂ and CH₄ production in organic soils from a coastal freshwater forested wetland” by Kevan J. Minick et al.

Anonymous Referee #1

Received and published: 21 June 2019

C1

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 (SO₄, Na, Cl, NH₄, NO₃, PO₄, 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, δ¹³C effects are largely unchanged with CWD but significant effects in absence of CWD. Cumulative CO₂ and CH₄ emissions are reduced with salt, but CWD with FW addition only stimulates CH₄ production.

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

C2

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;

This has been corrected

L299 “enzyme . . . were” should be enzyme . . . was]

This has been corrected

L317 should be “a” one-way ANOVA, no?

An “a” has been added

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

These are very common equations and are not necessary to include.

14. Are the number and quality of references appropriate? perhaps 15. Is the amount and quality of supplementary material appropriate? No supplemental received

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

We have added objectives to this section and changed the wording to sound less like methods.

Methods:

I cannot speak in depth to the methods used for isotopic analyses or microbial en-

C3

zymatic 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?).

We have added this information.

Have the authors any general physicochemical 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.

We measured soil ions in a study from 2013, using ion exchange probes (PRS probes) (Minick et al. 2019). These probes collected anions and cations over one six week period from July to August 2013 in the same plots used for this study. NO₃⁻ concentrations were very low and likely contributed little to the potential pool of electron acceptors. Alternatively S and Fe availability were much higher than NO₃⁻ in the hummocks, as measured using the same PRS probes. Given that the soils were completely saturated (e.g. flooded) with either fresh or salt water, and numerous ions were measured (regrettably not Fe though), we feel this represents an acceptable

Minick, K. J., Kelley, A. M., Miao, G., Li, X., Noormets, A., Mitra, B., and King, J. S.: Microtopography alters hydrology, phenol oxidase activity and nutrient availability in organic soils of a coastal freshwater forested wetland, *Wetlands* 39, 263-273, <https://doi.org/10.1007/s13157-018-1107-5>, 2019a.

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.

We have kept this section as is.

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

C4

system. Have levels this high been seen in some nearby areas?

Yes, the saltwater in the sound to the east (only a mile or so) ranges from approximately 1-5 percent saltwater, another couple miles into the sound and towards the ocean the water is up to 10-20 percent. So these values are reasonable. The 2.5 percent is a more likely, or relatively short term scenario, while the 5 percent represents a more extreme or long term scenario.

L210: Please allay any concerns of positive pressure effects in the chambers during the 2-week intervals between sampling toward the end of the incubation.

The lids were left loose between sampling periods. This is stated in the following lines (L211-212).

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)

The end of the sentence has been removed.

Figure 2. I'd like to see something in the discussion related to the pattern of CO₂:CH₄ 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?

Our experiment was not intended to determine different levels of CWD inputs with all incubations receiving the proportionally same amount of wood additions and so we cannot test the combination of varying effects of wood and salinity. With that said, we think the reviewers observation is a good one and worth noting. We have added discussion on the CO₂:CH₄ trend in the discussion to this specifically

C5

L396+: I believe this interpretation follows the same trap noted in L339-341. It is accurate to say MBC was lowest in the dry treatment across un-amended treatments and lowest in the 5ppt amended treatments.

We have made these changes

Discussion:

L424-425: what C cycling processes are the authors suggesting balance out the reductions in CO₂ & CH₄?

We have clarified this sentence

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 CO₂ and CH₄ with salt water addition (+2.5 & +5.0 ppt), but the text says it enhances CH₄ under saltwater additions. Can you provide clarity? If this is actually referring to the difference (panels C & mostly F), then it seems that the CH₄ 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.

We have added clarification in the text to address this potential confusion. Panels C and F show the difference between wood free and wood amended soils which gives the wood-associated CO₂ and CH₄ production. So within the wood free or wood amended treatments, salt water generally reduced gas production. But when comparing wood free and wood amended gas fluxes for each specific gas, we actually see that wood additions highly reduced CH₄ from freshwater but enhanced it in salt water incubations. This is just another way of looking at the results in order to derive some interpretation of how wood versus non wood treatments influence gas production when incubated with fresh or salt water.

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

C6

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)

We have made changes to the sentence in the discussion and changed operates to responds. We agree with the reviewers assessment

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).

We have made some changes to this paragraph but have kept this sentence because we think it helps guide the reader in this summary introduction paragraph.

L142: why note 13 plots if you only used 4?

We have mentioned the thirteen plots because it is part of the description of the site. We feel it is important to note that this site is part of the Ameriflux network, which follows certain experimental design protocols. Of the thirteen plots, four of these are more intensively monitored for plant and soil processes. We have added information to this sentence to highlight why we chose four plots, to hopefully clarify why we chose to mention this.

L199: what year were the trees harvested?

2010, we added that it was harvested then.

L202 & L204: are the 6 rings mentioned in 204 the mean of the 5-7 rings in 202?

We have revised this section. It was six tree rings. We reduced mentioning it to only once.

L248: add (MBC) after spelling out microbial biomass C

C7

This correction has been made

L286: enzyme XYL is not defined in the 5 above

This information has been added

L385: please be more precise than "the last couple"

We just removed that part of the sentence, due to it being somewhat subjective and not adding much to the overall results or interpretation

L421: recommend authors use the defined abbreviation "SLR" instead of sea-level rise (else, why define it earlier?)

This has been changed

L466: over time (add space)

This has been changed

Table 1: please provide units of the ions

This information has been added

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 between wood-amended and wood-free"?

We have added a sentence to the figure caption to show this.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-174>, 2019.