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Interactive comment on "Saltwater intrusion into tidal freshwater marshes alters the biogeochemical processing of organic carbon" *by* S. C. Neubauer et al.

Anonymous Referee #1

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Summary. Understanding carbon cycling in wetland ecosystems has important implications for global and regional carbon cycling and is important for understanding the long-term fate of tidal marshes in response to global change (especially sea level rise). This project is timely and interesting. The central focus of this work is a long-term (3.5 year), field-level addition of salt water to a tidal freshwater marsh. Previous publications have discussed the field-scale responses to this manipulation. In the present manuscript, soils were collected at the end of this manipulative experiment and analyzed for rates of carbon mineralization (CO2 and CH4 production), soil chemical composition (e.g., C:N) and enzyme activity. The results of this experiment demonstrate that long-term saltwater intrusion decreases the rates of carbon mineralization (this is

supported by a decline in enzyme activity) and results in lower C content and generally decreases the lability of soil at this site. In contrast, a complementary short-term laboratory salinity manipulation experiment demonstrates that salt water intrusion actually increases rates of carbon mineralization – seemingly in opposition to results from the field-based experiment. Overall, this is a well written manuscript describing a unique experiment. I have a few comments for the authors to consider.

General Comments. This is a very solid paper – I enjoyed reading it and appreciate the logistical magnitude of the field experiment being described. The biggest questions I have with this paper center around the interpretation and conceptual model presented by the authors. The authors state that the short-term stimulation of carbon mineralization and the long-term decrease in CO2 production are inconsistent. Their conceptual model suggests that short-term effects are due to biogeochemical impacts where are long-term effects are due to changes in vegetation community. This seems logical to me but this argument could be supported by a bit more data – do plants in the +salt plots have higher C:N ratios for example. It also seems to me that there is a second (non mutually exclusive) hypothesis that could explain the long-term patterns and still be consistent with the short-term amendments. If salt-water initially stimulated decomposition of labile organic matter (resulting in increased CO2 production) than wouldn't there be less soil carbon left behind and wouldn't it be lower quality? Again, this isn't mutually exclusive with the plant community conceptual model, but it seems important and in line with all of the results presented here. Could back of the envelope calculations be done to explore how long the stimulation of CO2 production observed in the short-term amendment experiment would need to persist to explain the reduction in soil C observed in the field?

I personally found the first 2 paragraphs of section 4.2.4 "Linking Soil Biogeochemistry..." to be a bit anti-climatic. It is reassuring that the lab estimates are in line with field measurements, but I didn't quite see the point of this discussion. The authors are wise not to scale their bottles up to an ecosystem C balance and I don't think that is the point of this mechanistic study. The fact that field CO_2 and CH_4 flux measurements across treatments agree with the general patterns from the incubations presented here is interesting and merits discussion. The lack of an initial stimulation of net CO_2 flux is also interesting. The slurry:field ratio just wasn't particularly compelling to me – too many reasons why these might not add up in meaningful ways. Ending with the conceptual model and a discussion on different long- vs. short-term change mechanisms would give the closing of this paper a stronger punch.

Specific Comments. You mention that fluxes were "typically linear" over time and based on the median correlations coefficients this seems to be the case. I wonder however if there were any non-linear (what was the cutoff to define this?) fluxes and how they were handled. If all fluxes were linear, consider removing the word "typically."

As it reads now the Results section flows from long-term experiment results (3.1.1) to short-term amendment results (3.1.2) back to long-term experiment results (3.2 and 3.3). I realize that the authors do this to couple CO_2/CH_4 results in the same section (3.1). I wonder if discussing the long-term results completely and then discussing the short-term experiment would result in a smoother flow. If you keep the current structure, consider reminding readers that the data in 3.2 and 3.3 were from the long-term field experiment.

It seems to me that the measurement of CO_2 and CH_4 production measured here represent potentials rather than anything like "in situ" rates. These soils were at 4C for 1-2 months before being measured. I would make this explicit – call these potential production rates.

Section 2.2.2. I would consider removing the first sentence here ("As described below, . . .). I think it is stronger to start with the conceptual justification for the short-term amendments rather than directing readers to data which have not yet been shared.