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Interactive comment on "Carbon degradation in agricultural soils flooded with seawater after managed coastal realignment" *by* Kamilla S. Sjøgaard et al.

Anonymous Referee #2

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General Comments: Sjøgaard et al. investigate the effect of seawater flooding on the metabolism of soil organic carbon in soils from a reclaimed wetland that had either been cultivated for 140 years or allowed to become a reedswamp. This is an important issue globally as coastal land managers turn to a program of "depoldering" to restore the functions and ecosystem services of intertidal coastal habitats. The author's use bottle and core incubations to explore patterns of carbon mineralization both through time and with soil depth. They conclude that seawater sulfates rapidly accelerate carbon degradation upon flooding, but soils quickly regain a new equilibrium as mineralization slows over time, resulting in only 6-7% of the original soil organic carbon being lost, which they conclude indicates seawater flooding will result in a negative feedback on

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Discussion paper

atmospheric CO_2 concentrations by preserving C. While the analysis conducted were generally well executed and the results comprehensive, they were not designed to test the hypotheses posed in the introduction, specifically (H1) that soil carbon degradation is related to the lability of organic matter, which is not assessed in the current study, (H2) that flooding preserves organic carbon or (H3) there is a negative feedback with soil flooding and atmospheric CO_2 concentrations. The results presented are sufficient to support a comprehensive study of the effects of seawater reintroduction to reclaimed coastal lands, but the hypotheses and conclusions must be significantly re-framed to be acceptable for publication. I recommend major revisions.

Major specific comments:

-The abstract is the first mention of coastal realignment but through the manuscript it is discussed as a relatively novel concept about which little is known. There is an extensive body of literature on "managed realignment" also call dike-breach restoration, or depoldering. While I believe the author's data is amongst the most detailed laboratory study of carbon degradation in this body of literature, making it a unique and important addition, they have not used this literature to their advantage and have neglected some key publications, among them the studies of Portnoy and Giblin (Eco. Apps. 1997 pp1054), recent publications by Ardon et al. (GCB 2013 pp296 and Biogeochemistry 2016 411), for a review of biogeochemical changes due to salinization see Herbert et al (2015 Ecosphere) and for reviews of dike-breach restoration see Burdick & Roman (2012) Tidal Marsh Restoration: A Synthesis of Science and Management (Springer)

- There are two problems with the authors' central argument that flooding soils enhances carbon preservation and therefore has a negative feedback with atmospheric CO_2 concentrations.

(1) The authors do not show that flooding soils enhances carbon preservation (over what?). The reader may assume that the authors intend to say that flooding the soils preserves more carbon than would be preserved if the land was not subjected to flood-

ing. In fact, the data they present shows flooding increases carbon mineralization, at least initially. While the authors' supposition is not unfounded (intertidal soils on average accumulate carbon at 5-10x the rate of terrestrial soils e.g. McLeod et al 2012) the authors analyses cannot show this because they did not measure mineralization rates in the cultivated and uncultivated soils in the absence of seawater flooding. At the very least the authors could provide a comparison of published rates for similar marine sediments, similar reedswamp sediments, and similar agricultural soils, however this would only be sufficient to suggest, not confirm, enhanced carbon sequestration.

(2) The authors have confused preserving stored carbon with negative carbon-climate feedbacks. Preventing carbon from entering the atmosphere (i.e. through flooding of soils) at best has a null impact on atmospheric CO2 concentrations. To have a negative feedback on atmospheric CO2 concentrations a system must remove CO2 from the atmosphere which is a process not explored in the current MS. Mackey et al (Nature Climate Change 2013) provide an excellent perspective on this. This may well be the case if intertidal vegetation is established etc. but is not the case in the current study.

- Hypothesis 1 & 2: while the dependence of mineralization on content is investigated, the authors do not make any measurements of lability or the origin of organic matter, thus these are weak points of argument that should not be the focus of the manuscript. Instead, the bulk of the analysis are targeted toward bulk organic matter degradation and the pathways of degradation.

-The strongest way to re-frame the data in hand would be a comparison of the effects of seawater flooding on cultivated versus what seem to be freshwater wetland soils focusing on the rates of carbon loss, the proportion of initial carbon lost, and the pathways of mineralization. There are obvious differences in the two sites that lend themselves to this discussion and the topic is still highly relevant to efforts to re-flood former agricultural land (cultivated) as well as restore artificial freshwater impoundments (Portnoy & Giblin, Bouldoc & Afton etc.) or the migration of saltwater into historically freshwater areas (absent of restoration).

-Section 4.1 It is the production of small polymers/monomers small enough for microbial uptake that is considered the rate limiting step for carbon degradation (e.g. the enzymatic latch hypotheses) not the generation of DOC which can be highly recalcitrant. DOC is not equivalent to easily degradable materials.

-The authors do not sufficiently address the caveats of long core/bottle incubations and the various experimental artifacts introduced.

Minor specific comments: Abstract o Ln. 6 delete “continue for centuries and” o Ln. 8: delete “So far”; what kind of soils? o Ln. 9: delete “In this study”

Section 1 o Ln. 20: This paragraph invokes far too many specificities related to SLR. Suggest compressing into a single sentence. This paper is about managed realignment with SLR as one of the justifications, not specificities of SLR or scenarios. o Please revisit this argument. It is the hydrolytic enzymes in conjunction with radicals that can depolymerize refractory compounds. There are also multiple other arguments for accelerated decomposition in aerobic environments, including free energy of alternate terminal electron acceptor pathways and other metabolic constraints. o The last two sentences of the first full paragraph starting on page 2 are confusing. Where is “here”? Do the authors intend to say soil organic matter of terrestrial origin may be difficult for marine organisms? o Soil organic carbon is generally abbreviated as SOC

Section 2.1 o Give details about the reclamation: was the area diked and drained? o Because the authors are using so many acronyms for different carbon compounds, the use of “C” and “UC” for the sites can make for difficult reading. Suggest switch to agricultural field (AF=C) and reedswamp (RS=UC) as they are more descriptive. o Was the reedswamp freshwater?

Section 2.2.3 o Were vials flushed to remove oxygen prior to the incubations?

Section 2.3 o The budget calculation is unclear. Please clearly describe which data sources are utilized for the carbon budget.

Section 4.4 o Reduced iron (FeII) responsible for buffering sulfide accumulation (Reddy and DeLaune 2008) appears to increase through most of the study and show no substantial declines (particularly in station C) over the course of the year, indicating there should be sufficient Fe buffer for sulfide generated over longer time scales (>1 year) since metabolic rates appear to decline over time (See Schoepfer et al. 2014. JGR: Biogeossciences).

Technical corrections:

2.2.1 Give simple details of flow injection analysis (model of instrument). Was Zinc added to prevent H2S interference for CO?

Figure 2b. Re-scale y-axis to fit data.

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