

Reviewer #1 (Anonymous)

General comments:

In this manuscript, Kirwan and coauthors examined the temperature sensitivity of organic matter decay in brackish tidal marshes using a pair of experimental approaches. In the first experiment, the decay of standard cotton strips was quantified in brackish marshes from South Carolina to Nova Scotia in order to provide a wide range of soil temperatures. In the second experiment, the decay of cotton strips and belowground root+rhizome biomass were measured over ~1 month intervals over the course of an 8 month period, where the seasonal variability within the site provided the variations in temperature. Both experimental approaches yielded temperature sensitivities (Q10 values) in the range of 1.2-1.5, values that are toward the low end of temperature sensitivities reported in other tidal marsh studies. The authors made the interesting observation that these Q10 values are similar to the temperature sensitivity of O2 diffusivity in water, even though much salt marsh decomposition happens through anaerobic processes. The final point made by the authors is that the temperature sensitivity of salt marsh decomposition is lower than that of salt marsh primary production. In other words, the authors argue that warming should increase primary production more than decomposition, thus leading to an increase in rates of marsh organic matter accumulation. This would have implications for marsh vertical accretion and resilience to sea level rise.

Overall, the manuscript was well written. As noted below, I had some relatively minor questions about experimental details. I think that the authors should consider analyzing their cotton strip data using the exponential decay equation (the same approach applied to the litter bag data) instead of assuming a linear decay rate. Over the short time periods of their study (5-20 day deployments for the cotton strips), there are not likely to be large differences between exponential and linear decay models, but running the calculations using an exponential model would mean that the authors don't need to use their "calibration equation" to convert the cotton strip loss rates to k values; more on this point below in the specific comments below.

The largest issue I have with this manuscript is a conceptual one relating to the context and presentation/interpretation of the data. This entire study examines the initial decay of organic matter over time periods of up to 1 month. This is great for understanding the fate of labile organic matter. However, the fraction of organic matter that is resistant to decay is what can ultimately be stored and preserved in the soil. In other words, what happens to the labile organic matter is largely irrelevant in the context of longterm organic matter storage. Any temperature-related change in the fraction of organic matter that is resistant to decay or change in the rate of decay of the recalcitrant fraction (since even "recalcitrant" material can decay at a slow rate) is going to have a larger effect on tidal marsh organic matter storage than will changes in the decay rate of the labile fraction. I am skeptical that this study can actually provide any information about the effect of temperature on marsh carbon accumulation and the ability of tidal marshes to avoid submergence by rising sea levels (that is, the big picture context provided by the authors).

This is a good point, and deserves clarification in the revised manuscript. Recalcitrant organic matter is more important for carbon storage and marsh accretion, but by definition decays at timescales (decades to millennia) that are impossible to study with field-based experimental methods. Lab experiments can provide insight into temperature sensitivity of more recalcitrant pools, but cannot fully represent natural conditions. Therefore, our work- which is among the first and most robust measures of decomposition sensitivity to temperature in marshes- does provide important insight into the processes governing marsh elevation and response to sea level rise. Experiments from terrestrial ecosystems offer some guidance on how to apply our results. Some evidence suggests that temperature sensitivity does not vary with the lability of organic matter (Fang et al., 2005), which suggest our results are directly applicable to all carbon pools. Others suggest sensitivity decreases with lability (Craine et al. 2010), which suggest we may underestimate the Q10 of more recalcitrant pools. In response, we have added a caveat sentence to the next to last sentence of the revised manuscript. It reads, "How temperature sensitivities of labile carbon decomposition relate to the more refractory carbon that is important for long-term carbon burial, is poorly understood [Fang et al., 2005; Craine et al., 2010]." We have also specified that our measurements are for labile organic matter decomposition both in this paragraph and in the

abstract, and have added the word “potentially” to the last sentence of the abstract to more cautiously frame our implication for resilience to sea level rise.

Specific comments:

1) p. 6020, lines 3-4, “where ecosystems accumulate organic matter to build soil elevation and survive sea level rise” As written, this sentence from the Abstract implies that marshes accumulate organic matter in order to survive sea level rise. It would be more accurate to say that marshes accumulate OM (because of high productivity and low decomposition), and that accumulation over time helps them avoid being submerged by rising seas. Marshes do not accumulate OM with an ultimate purpose in mind.

We have changed this sentence to read, “Approximately half of marine carbon sequestration takes place in coastal wetlands, including tidal marshes, where organic matter contributes to soil elevation and ecosystem persistence in the face of sea level rise.”

2) *Latitudinal gradient experiment: When during the year were the cotton strips deployed? Beginning of growing season? End of growing season? Peak of summer? Middle of winter? Even though the specific deployment dates may have varied between the different sites, I hope there was some consistency with respect to marsh phenology since factors that can influence decomposition (besides temperature; for example, radial O2 loss from marsh macrophytes) also vary over the course of the year.*

We have added a sentence to the revised manuscript that states “Cotton strips were deployed during the middle of each site’s growing season, generally in June or July of each year.”

3) p. 6022, lines 25-26: *This sentence was slightly confusing to me. By describing the cut cotton strips as “2 cm wide,” I envisioned that you ended up with some number of 2 cm wide by 30 cm tall cotton strips from each site, where width is the horizontal dimension and the height relates to the vertical distance from the marsh surface to the bottom of the cotton strip. Instead, after reading further, I think that you cut the 30 cm (vertical) height of the strips into 2 cm tall increments. Can you either change your description to read “2 cm tall increments” or else modify the sentence to read something like, “Strips were then cut into 2 cm tall increments so we could examine depth-related variations in tensile strength loss. . .?”*

We have modified the sentence to read, “Strips were then cut into 2 cm increments, and analyzed on a Dillon Quantrol Snapshot tensiometer to measure tensile strength and loss of tensile strength with depth.” The removal of the word “wide” and the addition of the phrase “with depth” should add clarity.

4) *Latitudinal gradient experiment: How many replicate cotton strips did you install at each site?*

We have edited this section to state that we deployed 2 cotton strips along with 2 control cotton strips at each site.

5) p. 6023, line 11: *What was the “non-reactive synthetic membrane” that you used? Nylon? Nitex? Something else?*

Decomposition bags were constructed from Versapor membrane made by Pall Corporation, and we now include that information in the following sentence, “Following Kirwan *et al.* [2013], decomposition bags were 6 x 6 x 1 cm, constructed of a non-reactive synthetic membrane (Versapor©; Pall Corporation, Port Washington, NY) with 5 um pore size, and contained approximately 3.5 g of *S. americanus* roots and rhizomes.”

6) p. 6023, lines 19-20 vs. p. 6025, lines 7-8: *When was the seasonal warming experiment conducted? The Methods section says it went from April 2012 to January 2013, but the Results section talks about temperatures in 2011 and 2012.*

Thank you for catching this important mistake. The dates in the methods section are correct. We have replaced the sentence in the results section, with “In the seasonal warming experiment, average soil temperatures ranged from 26.6°C in the July 2012 deployment to 6.8°C in the December 2012 deployment which encompass the temperature range of the latitudinal experiment (Figure 2a).”

7) p. 6024, line 9-10. This equation describes exponential, not linear, decay. Taking the natural log of $Ct/C0$ gets you a linear relationship with time and therefore allows you to easily calculate the value of k , but the underlying decay is exponential.

We have replaced the word “linear” with “exponential” in this sentence to avoid confusion.

8) As far as I can tell, you do not present the Arrhenius coefficients (activation energies). Were those calculations *only* used to see if the decay vs. temperature relationships were the same for the two different organic matter sources? Since this manuscript is about temperature sensitivity, Arrhenius coefficients are useful in their own right.

We did not directly use the Arrhenius equation for any calculations. Instead, we used principles of Arrhenius kinetics (i.e. that k is an exponential function of T) to justify our choice of an exponential shaped regression curve. To make that more clear, we have edited out the reference to Arrhenius kinetics in the revised manuscript. We do not see a need to explicitly report activation energies since they can easily be determined from the regression equations in Figure 3.

9) p. 6024, lines 18-21: Why not just directly calculate tensile strength loss coefficients (k values) using the equation on p. 6024, line 10? For the litter bags, you used mass of litter at time zero and time final; for the cotton strips you could use tensile strength at time 0 (control strips) and time final. That way you avoid using the relatively weak relationship ($r^2 = 0.4$) between decay coefficients and tensile strength loss (Fig. 2c). You also avoid the complication that the ratio between the linear decay rate (% tensile strength loss per day) and the exponential decay rate (k values, calculated using the equation on p. 6024, line 10) should theoretically vary as a function of time. In other words, the relationship you show in Fig. 2c will vary between your seasonal warming experiment (1 month deployment) and the latitudinal gradient experiment (5-20 d deployments).

This is a very interesting suggestion that caused us to reconsider our method for estimating decomposition sensitivity to temperature the latitudinal experiment. Unfortunately, we can find no evidence that tensile strength actually decreases exponentially through time. In fact, one relatively recent study in marshes found that it decayed linearly (Slocum and Mendelsohn, 2009; figure pasted below). An earlier study suggested a more complex response, but not an exponential one (Hill et al., 1988). The reviewer’s suggestion to bypass the relatively weak calibration experiment is tempting, but unfortunately we see no reason that it would lead to

results that are more directly comparable to the seasonal warming experiment.

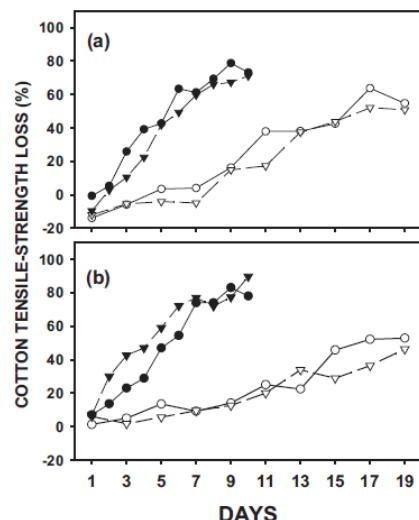


Figure 2: Effect of soil type (triangles = salt-marsh soil, circles = oligohaline-marsh soil) and temperature (white = 20°C, black = 35°C) on decomposition rates (% cotton tensile-strength loss) of (a) artist canvas (Fredrix brand 12-ounce duck, style number 548) and (b) Shirley soil burial test fabric in sods placed in darkened growth chambers. Shown are means of five sods per treatment combination.

10) p. 6025, line 4: If your cotton strips were 30 cm long (I assume that’s their depth, although the Methods section doesn’t specify whether the 30 cm is a vertical or horizontal dimension), why did you measure/report tensile strength loss down to only 20 cm?

Material below a depth of 20cm was so degraded that it was difficult to extract from the soil, and frequently disintegrated or broke in the process. As a result, we did not have enough data from these depths to speculate on their sensitivity to temperature.

11) p. 6025, lines 2-5, “Although we measured soil temperature at one depth. . .” I am a little uncomfortable with your statement that you had similar decay vs. temperature relationships at different depths in the soil. Without some knowledge of what the temperatures are at depth, you are really just guessing that temperatures are the same at 4 cm

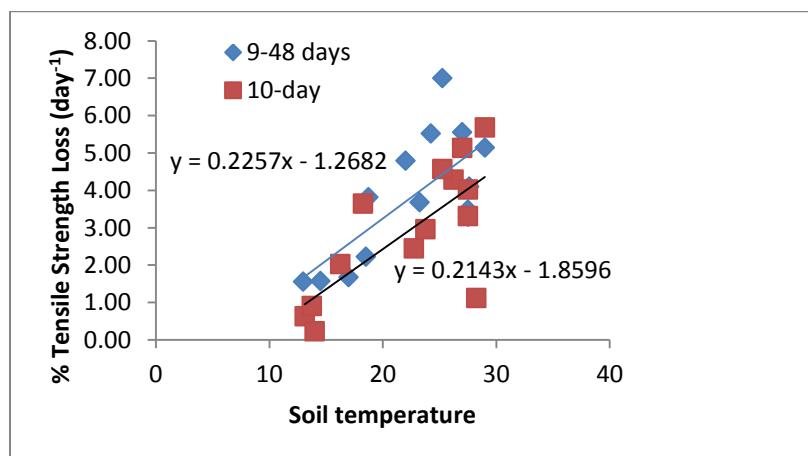
as they are at 18-20 cm (that is the implicit assumption in Fig. 1b). Depending on your site and when in the year you sampled, it is possible that some sites were warmer at the surface than at depth, whereas others were cooler. That would affect the curves shown in Fig. 1b.

Our assumption is not that temperature at depth equals temperature near the surface (which we agree is not valid), but rather that a similar latitudinal gradient in temperature exists near the surface and at depth (i.e. soils in New Brunswick are cooler than South Carolina regardless of whether they are at 0 cm or 20cm depth). Nevertheless, we agree with the reviewer that there is too much uncertainty associated with temperature at depth to make the claim that decomposition patterns near the surface were similar to those at depth. In response, we have removed the trends at various depths from Figure 1b and also removed their discussion from the manuscript.

12) p. 6025, lines 15-18: As mentioned in an earlier comment, the slope of Fig. 2c should change as a function of the time scale of the measurement, and thus should not be used as a universal “calibration” for experiments of different duration.

We disagree that the slope of Fig 2c should change as a function of time because we have been careful to normalize both decomposition indices by time. Cotton strip tensile strength loss (y-axis) has been expressed as % loss per day on the basis of the linear response described in Comment 9. Decomp bag mass loss (x-axis) has been expressed as an exponential decay coefficient (k , day $^{-1}$) following standard methods. Moreover, the calibration experiment (27-35 days) and latitudinal experiment (most strips were deployed between 10-26 days) were conducted over reasonably similar timescales.

During our review of the timescales involved in the latitudinal cotton strip deployments, we discovered that we incorrectly reported the range of deployment durations. While most of the experiments were in the 10-20 day range as noted in the text, the Canadian sites were actually deployed for much longer durations (89, 60, and 48 days in 2008, 2009, and 2010 respectively). To demonstrate that this longer timescale does not affect the temperature sensitivities we reported in the manuscript, we present here the results of an additional experiment that is not referenced in the manuscript. In 2010, we deployed cotton strips in both the convention manner where duration depends on soil temperature (9-48 days as described in text), and another experiment where strips were deployed for 10 days at each site, regardless of initial soil temperature (not included in the manuscript). The two experiments demonstrate that the temperature sensitivity of tensile strength loss is nearly identical (figure below), and further supports our assumption that tensile strength loss is linear and independent of time scale.



13) p. 6025, lines 15-18, again: I'm not sure I understand the logic of using the "calibration equation" to convert the tensile strength loss numbers to k values. Figure 2c is plotting a decay rate of cotton strips vs. the decay rate of native organic matter for a single site in Maryland. By applying the regression equation to the cotton strip data from the 14 latitudinal sites, you are essentially predicting how belowground biomass from Maryland would decay in all the other sites. But why is this valuable information? As you mention in the text, you already have a common organic matter source (i.e., cotton strips) that has been deployed at all the sites. If you are actually interested in knowing the exponential decay rate of the cotton strips, just use the equation on p. 6024, line 10 and then you will have k values for the loss of tensile strength for cotton.

Our logic for the calibration experiment was to convert the latitudinal tensile strength measurements into a measure of mass loss that could be directly comparable to the seasonal warming experiment results. Our somewhat weak, but highly significant correlation, indicates that these two proxies of decay are indeed related, although perhaps not as strongly as we would hope. Nevertheless, the fact that Q_{10} value from the latitudinal experiment is very similar to the Q_{10} value from the seasonal warming experiment suggests both approaches have merit.

14) p. 6027, lines 13-15: There are additional references that give Q_{10} values for tidal marsh metabolic processes and/or greenhouse gas emissions. To name a few, see Morris and Whiting (1986. *Estuaries*. 9:9-19) for salt marsh CO_2 emissions, Neubauer (2013. *Estuaries and Coasts*. 36:491-507) for tidal freshwater marsh CO_2 and CH_4 emissions, and Megonigal and Schlesinger (2002. *Global Biogeochemical Cycles*. 16:1088) for CH_4 production, oxidation, and emissions. Admittedly, some of these are freshwater and not brackish/saline wetland studies, but so is the Inglett et al. paper you cited (and further, the Inglett paper is from a non-tidal system).

We appreciate the reviewer's list of additional references. In particular, the Morris and Whiting approach makes for an excellent comparison because it isolates heterotrophic respiration. We now include their range of Q_{10} values (1.5-1.8) in the sentence in question, and note that they are very similar to the range we measured. We have also included a comparison to the findings of Neubauer 2013, but note in the revised manuscript that the Q_{10} values are likely much higher than ours because Neubauer measured ecosystem respiration (which includes autotrophic respiration from above and belowground portions of plants) whereas ours are most comparable to heterotrophic soil respiration. We excluded the Megonigal and Schlesinger reference because the methane sensitivity it reports is not directly relevant to our study of mass loss, since methane is only a small portion of heterotrophic activity in brackish marsh ecosystems. Our revised sentences now make clear that our decomposition measurements best correspond to heterotrophic respiration. They now read, "The temperature sensitivity of decomposition that we report ($Q_{10}=1.3-1.5$) is also less than the sensitivity reported for ecosystem respiration of a freshwater marsh ($Q_{10}=3.0-3.6$) [Neubauer, 2013], presumably because our estimate corresponds to heterotrophic respiration, whereas ecosystem fluxes include autotrophic and heterotrophic respiration from plants and soil. In contrast, the sensitivity we report ($Q_{10}= 1.3 - 1.5$) is very similar to the range of estimated temperature sensitivities of CO_2 emissions from freshwater wetland soils ($Q_{10}=1.3-2.5$) [Inglett et al., 2012] and salt marsh soils ($Q_{10}=1.5-1.8$) [Morris and Whiting, 1986]."

15) The calculation of the Q_{10} of oxygen diffusion is interesting and lends a quasitheoretical basis to the Q_{10} values for decomposition that you calculated. I do wonder about your calculation of Q_{10} values, specifically that the Q_{10} for temperature T is based on the diffusivity rate at temperatures T and $T-10$. That is one way to calculate the Q_{10} values (I might have compared $T+5^{\circ}C$ and $T-5^{\circ}C$), but then Fig. 4 shows Q_{10} values for temperatures less than $10^{\circ}C$. In order to calculate those Q_{10} values as you describe, you needed to calculate the diffusivity at temperatures $< 0^{\circ}C$. Does the Han and Bartles (1996) equation work in ice?

Good point. We have removed the calculated Q_{10} values for $T < 10$ from the figure. Restricting the analysis to $T > 10$ degrees does not change the observed pattern. In fact, restricting the analysis to $T > 10$ leads to closer correspondence between Q_{10} of O_2 diffusion (1.25-1.4) and the

Q10 of our decomposition experiments (1.3-1.5). And to be clear, we only use the T and T-10 approach to calculate the Q10 values in this particular figure.

16) *Figure 5 legend: Can you justify your assumption that an increase in atmospheric CO₂ from 380 to 720 ppm will produce a 3 °C rise in temperature? As a starting point, you may want to consider that the latest IPCC report said that climate sensitivity for a doubling of CO₂ is, with medium confidence, in the range of 1.5-4.5 °C (it has a lower probability of being higher or lower than that range).*

We have added a reference to the IPCC Fifth Assessment Report following our statement that we assume an increase in CO₂ from 380 to 720 ppm is roughly equivalent to a 3°C warming. 3°C is, of course, exactly the midpoint of the range of sensitivities indicated by the reviewer, and also the mean value for equilibrium climate response to doubling CO₂ listed in the IPCC 5AR Table 9.6 and technical summary. More importantly, we clarify that our comparison is based on the assumption that elevated CO₂ experiments would correspond to warming of approximately 3°C. We have deleted the particular CO₂ concentrations since ambient concentrations have changed throughout the various experiments, and because it implies a more precise comparison than can be made. The sentence now reads, “Red bar represents decomposition response to 3°C warming, which we assume is **roughly equivalent** to the near doubling of [CO₂] considered in CO₂ fertilization experiments (Stocker et al., 2013).”

17) *It is worth noting that the responses of plants to a near-doubling of CO₂ is not only due to the effect of CO₂ as a driver of temperature increases. For the sources cited in Figure 5, the CO₂ fertilization effect was likely much greater than any CO₂-caused warming effect, so these data points (the dark green bars) are not really a valid comparison with the data points that are based only on changing temperatures (the light green and red bars).*

The response of C3 plants to near doubling of CO₂ (dark green bars) is actually **entirely** due to CO₂ fertilization since the experimental chambers used in the Figure 5 sources do not facilitate warming. Likewise, the expected response of C4 plants to climate change is due entirely to temperature warming since C4 plants show negligible responses to CO₂ fertilization. As noted in the response above, these components of global change are roughly equivalent to each other, and therefore the comparison is valid. Nevertheless, the reviewer’s comment provides an opportunity to make the intent of our simple comparisons more clear. In response, we have altered the first sentence of the caption to stress that we are comparing responses to “individual components of global change.” We have also added a sentence to the figure caption stating that “These are simplistic comparisons and do not address potential interactions, such as warming impact on *S. Americanus* productivity, or the influence of CO₂ fertilization on decomposition rate”. We have added a phrase to a similar sentence in the main text to specify that our comparison does not consider “interactions between multiple components of global change [Langley and Megonigal, 2010].”

18) *Figure 5: I think this figure could do a better job of showing the uncertainty in reported salt marsh decay rates. The figure shows several different ways of looking at how marsh productivity will respond to warming, but only one value of the decay response. Your own data in this manuscript produce Q10 values of 1.2 and 1.5 so the decay response should, at a minimum, have an error bar. Besides your own data, you mention other decay estimates in the text (e.g., Inglett et al.; Kirwan and Blum 2011, etc.) and I provided a couple other references earlier. If you consider the full body of decomposition responses to temperature, you may reach different conclusions (or at least less strong conclusions) about the relative responses of primary production and decomposition to warming.*

We have taken the reviewer’s suggestion to include an error bar on our decomposition estimate. However, we do not include estimates from Kirwan and Blum (2011) because that preliminary experiment was conducted with above-ground plant material placed on the soil surface at a single site, and therefore not directly comparable to our results here. Inglett et al. and other references

provided by the reviewer were excluded from this figure because they are gas flux measurements, and therefore not directly comparable to the net changes in mass presented in the productivity references, and in our decomposition experiment. Nevertheless, all of these references are now discussed in the revised manuscript (see Response to Comment 14).

Technical corrections:

19) 23) p. 6021, line 29 (also p. 6027, line 27). Do you mean “physiochemical” (meaning, relating to physiological chemistry) or “physicochemical” (meaning, relating to physics and chemistry)? The latter seems more appropriate.

We have changed the word to “biogeochemical” in both occurrences.

20) line 6022, line 12. *Italicize names of plant species. Check throughout manuscript as there are other places where scientific names were not properly formatted.*

We have gone through the manuscript and italicized all plant species names.

21) p. 6028, lines 15-16. *The Megonigal et al. reference is from 2004. The Literature Cited section also needs to be corrected in this respect.*

For some reason, this article has been cited with different years by different people. Our use of 2003 is consistent with Megonigal’s citation to his own work, and with information directly from the publisher (screen shot below shows the year as 2003).



22) Figure 1: *The figure legend mentions solid and dashed lines but all the lines on my review copy are solid. The 0-6 cm line is notably thicker than the others, but I cannot see any dashed lines.*

Thank you for catching this typesetting error. The dashed lines became solid lines during the conversion to PDF format. In response to comment #11, we have now removed all lines except for the thick solid line.