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Discussions

Interactive comment on "The temperature sensitivity of organic matter decay in tidal marshes" *by* M. L. Kirwan et al.

Anonymous Referee #1

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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 long-term 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).

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.

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.

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. . .?"

4) Latitudinal gradient experiment: How many replicate cotton strips did you install at

each site?

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

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.

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.

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.

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 (r2 $\sim$ 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 ($\sim$1 month deployment) and the latitudinal gradient experiment (5-20 d deployments).

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?

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.

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.

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.

14) p. 6027, lines 13-15: There are additional references that give Q10 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).

15) The calculation of the Q10 of oxygen diffusion is interesting and lends a quasi-theoretical basis to the Q10 values for decomposition that you calculated. I do wonder about your calculation of Q10 values, specifically that the Q10 for temperature T is based on the diffusivity rate at temperatures T and T-10. That is one way to calculate the Q10 values (I might have compared T+5 °C and T-5 °C), but then Fig. 4 shows Q10 values for temperatures less than 10 °C. In order to calculate those Q10 values as you describe, you needed to calculate the diffusivity at temperatures ≤ 0 °C. Does the Han and Bartles (1996) equation work in ice?

16) Figure 5 legend: Can you justify your assumption that an increase in atmospheric CO_2 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 CO_2 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).

17) It is worth noting that the responses of plants to a near-doubling of CO_2 is not only due to the effect of CO_2 as a driver of temperature increases. For the sources cited in Figure 5, the CO_2 fertilization effect was likely much greater than any CO_2-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).

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.

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.

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.

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.

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.

END OF REVIEW

Interactive comment on Biogeosciences Discuss., 11, 6019, 2014.