This manuscript could provide for a timely and important discussion on the subject of the relative effect of increases in organic matter decomposition rates as a function of increases in soil temperature under climate change on elevation trajectories of tidal marshes. However, it is not clear exactly how this manuscript is moving this discussion forward. The positive relationship between organic matter decomposition rates and temperature is well established across disciplines. If the purpose of the authors is to point out that this relationship is not being recognized in some of the discourse on organic matter and marsh elevation change, well, that is a valid criticism but a fairly obvious one and should not require this research to suggest (as is done on lines 7-10, page 714). Similarly, it probably isn't appropriate to consider it a 'paradigm' if a few researchers did indeed neglect to consider the role of the temperature/decomposition relationship on marsh elevation trajectories in response to climate change (line 25, p. 710).

Although a positive relationship between organic matter decomposition and temperature is well explored in terrestrial ecosystems, we believe we are among the very first to try and apply these principles to marshes. Since the sensitivity of decay to temperature warming seems to vary between types of ecosystems, and since the only previous attempt to measure it in a marsh showed no significant relationship (Charles and Dukes, 2009), even a simple experiment aimed to confirm that temperature/decay relationships apply to marshes is an important contribution. The novelty of the paper rests in the application of this "well established" relationship to the survival of wetlands, where temperature warming influences not only carbon accumulation, but also the rate of sea level rise, and perhaps the very survival or collapse of an entire ecosystem. The rapidly emerging consensus that elevated CO2 and warmer temperatures will increase the ability of marshes to survive sea level rise through increases in productivity also illustrates the novelty of our work, at least in the wetland community. The 6 references we cite do not simply represent a few examples of studies that neglect decomposition trends; these references are themselves the entire body of literature on how these facets of global change affect the resiliency of marshes to sea level rise, and are located in prominent journals (mostly PNAS and Global Change Biology).

If the authors are attempting to quantify the relative forcing of temperature on marsh productivity versus decomposition, that is an important task, but it doesn't seem that the authors have enough data to accomplish this. It seems that the focus of this paper is that their data show that the effect of temperature on decomposition will offset elevation gains associate with climate change (as the title implies). However, their data is relatively limited, primarily in that it only covers one site and time period. These data are probably not adequate to provide a general summary of this relationship or the variability of this relationship across marsh conditions. It may be more valuable to provide a general discussion on this relationship, using their data as a case study. This would require reworking the manuscript to highlight the literature review components of the paper. For example, the paragraph beginning on line 23 of page 712 could be expanded considerably. In the current draft, many of the more in-depth points of discussion are given as assumptions of the study or as references to other studies. Many of these assumptions and references could make for an enlightening discussion if given more attention. As another example, the sentence on lines 25-28 (p. 712) could be expanded to provide a detailed discussion and literature review on how the temperature/ decomposition relationship would differ across variations in soil water content (and redox status), nutrients, and carbon availability/recalcitrance.

We disagree with the reviewers opinion that our decomposition experiments are too limited to provide insight into the role that temperature plays in determining the balance between productivity and decomposition in a salt marsh. Though the reviewer is correct in pointing out that these experiments were conducted over a short duration and at a single site, the overall rates we measure here (k = 1.5-6.0 yr^{-1}) are similar to the range in short-term decomposition rates reported in a review of 11 marshes from Louisiana to Massachusetts (k=1.0-9.1 yr^{-1}) (Christian, 1984). This similarity suggests that the rates we report are not anomalous, a point that we make in the revised manuscript. Previous work in marshes suggests that other variables (redox potential, inundation frequency, soil water content) are perhaps less important than the reviewer suggests (see our response to the next comment). Nevertheless, we agree that we need to be more cautious in extrapolating the results of a single study, and we like Reviewer #1's suggestion that this work should be used as a model for future work that considers decay over longer time periods and more geographically diverse settings. Therefore we think couching the results as a case-study that makes conceptual projections is appropriate.

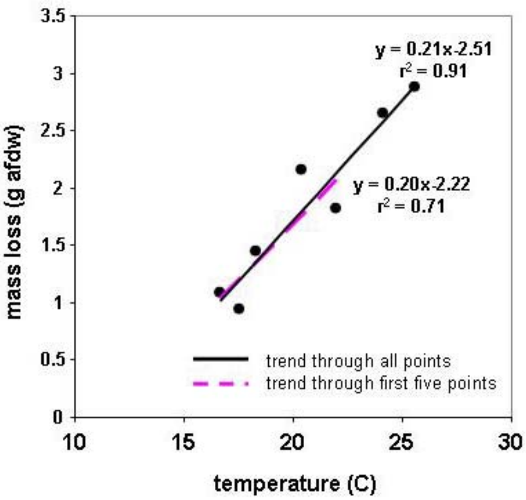
As the reviewer suggests, a more thorough literature review will also be useful. In the revised manuscript, we will address the possibility that our short-term measurements of temperature sensitivity are too high since q10 values in terrestrial ecosystems tend to be highest in the spring and vary widely throughout the year (e.g. Davidson et al., 2006). We will also address the possibility that the temperature sensitivity we measured at the surface is actually lower than what would be expected at depth since recalcitrant carbon pools in terrestrial ecosystems tend to be much more sensitive to warming than labile pools (e.g. Craine et al., 2010). It is important to remember that this type of discussion can only be made by referencing examples from the far more advanced terrestrial literature. The primary usefulness of this manuscript is the application of lessons learned from terrestrial environments to the coastal zone, where changes in decomposition rates have almost entirely been ignored, and unlike terrestrial analogs, have the potential to determine whether the entire ecosystem persists.

Other comments - The decay rates of fresh material are not likely to match the decay rates of soil organic matter (peat) in a variety of states of decomposition, which should be clarified. More importantly, the effects of temperature on peat and root decomposition may differ from the effects on fresh material decomposition, which has important implications for the interpretations of these data.

Our experiments were conducted at the soil surface where fresh material is abundant and the majority of decomposition takes place, rather than at depth where older material in various stages of decay dominates. Therefore, our use of fresh material is appropriate for the experimental setting, but we do see that this deserves to be more explicitly stated in the revised manuscript. As we note in our response above, older recalcitrant material tends to be more sensitive to temperature warming (Craine et al., 2010), suggesting that our use of fresh material may actually result in a conservative estimate of temperature sensitivity.

The redox status of the soils was not described and may have varied considerably due to the fluctuations in water content. Depending on the porosity, it seems that at least the *last two sampling dates were likely aerobic, which could dramatically increase decomposition rates and would be confounded with temperature effects observed.*

We did not measure redox potential because a longer term, more detailed study of decay under various tidal regimes indicates that redox has little impact on decay rates at our study site. Blum (1993) concluded that "The results presented here from [our Phillips Creek study site] and those of others (Valiela et al., 1982, 1984, Bertness 1985, Hackney 1987) indicate that decomposition is not affected by redox conditions or conditions associated with low redox potential." Moreover, because our experiments were all located in a portion of the marsh that receives only about one tide per month, we expect precipitation to be the largest determinant of redox potential and we found that it is not significantly correlated with decay rate (see response to Reviewer #2). Finally, the reported relationship between temperature and mass loss is not substantially altered even if the last two points are removed.



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