This is an interesting paper that discusses the effect of temperature on degradation rates in coastal marshes and how this influences capability of marshes to withstand enhanced sea level rise. The apparent current consensus is that peaty coastal marshes are generally resilient to global change as an increase CO2 levels and higher temperatures will enhance primary production, which in turn will increase peat formation enough to cope with enhanced sea level rise. However, the authors rightfully argue that higher temperatures will also increase litter degradation rates and show that this will have a major impact on the sediment accretion rate as it offsets the effects on increased plant productivity in many systems. The paper is very well written and should primarily be seen as a thought-provoking discussion paper on the effects of global change on coastal wetlands.

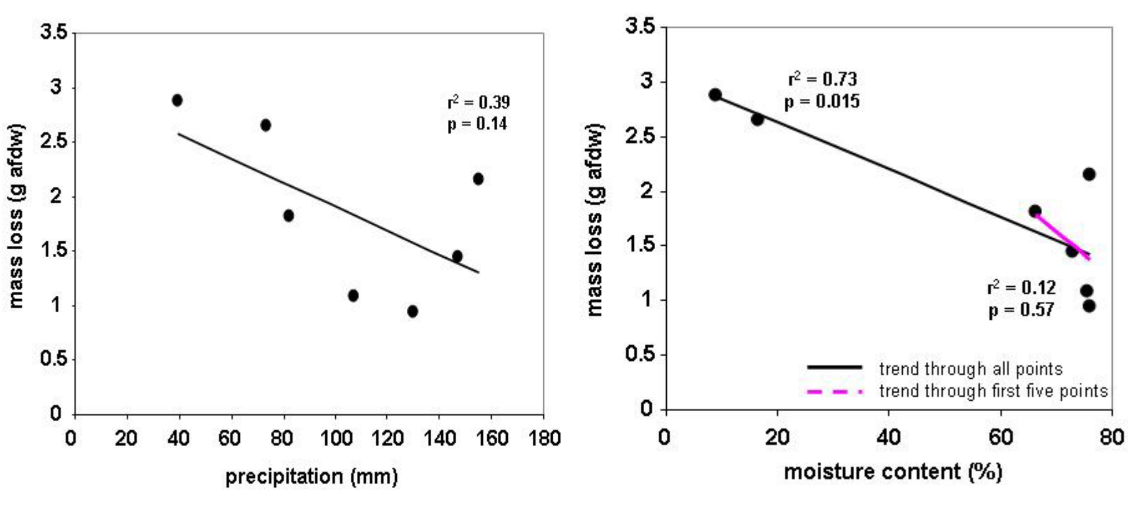
We appreciate Dr. Boschker's review and agree whole-heartedly with his view that our manuscript should primarily be interpreted as a thought-provoking discussion paper. We acknowledge that our simplistic experiment is not likely to improve the understanding of the decay process itself (the terrestrial literature is clearly far more advanced), so our main intent here is to explore how these processes can be applied to coastal ecosystems where they have largely been ignored.

The degradation experiment presented in the paper is relatively limited and in itself doesn't add much the general understanding litter degradation in coastal marshes as similar work has been done already for a long time. There are also some issues that need to be clarified (see below). However, the authors primarily use the experiment to illustrate the well-known effect of temperature on degradation rates (12% per oC or a Q10 of about 2) and how this may influence carbon cycling at coastal marshes and their resilience to global change.

We agree with the reviewer that this is a relatively simple experiment designed to illustrate how a process that is fairly well understood in terrestrial environments might apply in a very different system (i.e. a salt marsh). We strongly disagree, however, that our experiment doesn't add much to the general understanding of litter degradation in coastal marshes. In our manuscript we cite 3 papers from the 1970's and 1980's that indicate decomposition rates tend to be faster in warmer months, but none of these attempt to quantify the relationship between temperature and decomposition rate. We also cite 6 recent (post 2007) and prominent (1 PNAS, 3 Global Change Biology) papers that suggest elevated CO2 and warmer temperatures will lead to enhanced productivity and/or faster organic accumulation rates. Only one of these six papers attempts to address the influence of temperature on decomposition (Charles and Dukes, 2009), indicating that the community either has yet to sufficiently decipher the effect of temperature on marsh decomposition rates, or chooses to ignore it. Our result (a strong relationship between temperature and decay) is fundamentally different than the single study that attempted to quantify it, but found no statistically significant relationship (Charles and Dukes, 2009).

There are some issues with the degradation experiment. As it is an in-situ litter bag study, one can not assume that the effects seen are primarily due to temperature. Although the authors state that 'the mean daily temperature : : :. best explains the increase in degradation rate' (p711, l10), they should more clearly and extensively *discuss why they do not consider other environmental variables. For instance, there was also a strong decrease in litter moisture content with temperature (fig 1), which may have influenced litter degradation either directly (decrease in rate due to water stress) or because oxygen conditions improved in the litter layer (increase in rate).*

Since field-based experiments can never isolate a single variable of interest (i.e. mean daily temperature), the reviewer is correct in noting that other variables could potentially be important. From a statistical perspective, mean daily temperature was strongly correlated with mass loss ($r=0.95$, $p< 0.001$) and precipitation was not ($r=0.62$, $p=0.14$) (see figure below). Although a correlation between mass loss and litter moisture was statistically significant ($r=0.85$, $p=.015$), concluding a causal relationship is problematic because the trend is entirely driven by the last two points, and the estimates of litter moisture themselves are of limited value. There is no significant correlation between litter moisture and mass loss through the first five points (see figure), which stands in strong contrast to the robustness of the relationship with temperature (our response to Reviewer #3 demonstrates that removing the last 2 points makes no difference in the trend). The estimates of litter moisture are also problematic since they represent a single measurement at the end of each experiment and therefore represent an instantaneous snapshot of moisture conditions that would be very sensitive to the duration of time since the last rainfall or tidal inundation. Finally, more sophisticated work at this site (Blum, 1993) and elsewhere (Valiela et al., 1982, 1984, Bertness 1985, Hackney 1987) suggest that redox potential has little impact on decomposition rates. These observations, coupled with the knowledge that temperature is a strong driver of organic decay in other ecosystems, led us to the conclusion that temperature was indeed the appropriate environmental variable to focus on. Nevertheless, the reviewer makes a good point; the revised manuscript will include a caveat that other variables could co-vary with temperature, a discussion of litter moisture, and a statement that precipitation was not significantly correlated with mass loss. This improvement will focus on the discussion above, and incorporate the reported r and p values. If the editor believes it is warranted, we will also include the graph of mass loss vs. litter moisture and the graph of mass loss vs. precipitation in the revised manuscript.



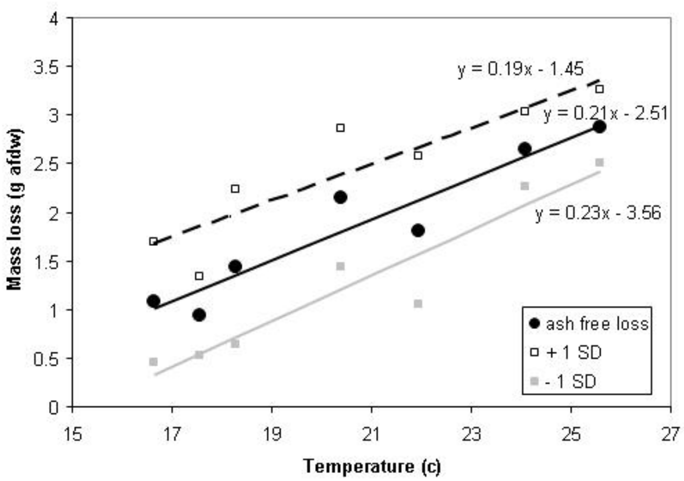
In addition, bacterial and fungal biovolumes were also followed throughout the experiment. The authors simply state that biovolume did not increase with temperature, but this is not what is shown by the actual data in fig 2b. There seems to be a bimodal response with first an increase in biovolume with increasing temperature followed by a decrease at higher temperatures. Could the observed drop in biomass at higher temperatures be caused by water stress? Please discuss.

Yes, this is likely true. The community could be water stressed and that could explain the drop in biomass at higher temperatures. Fungal communities (which dominate the biovolume in these experiments) are highly sensitive to moisture content (Newell et al., 1996). However, if the community were stressed by water availability, we would expect a slower rate of decay, when in fact we observed a decay rate that continued to increase.

Finally, there is no error estimate given for the 12% increase in degradation rate per oC, whereas the variability in the data in fig 2a suggests that it may be substantial.

This is a good suggestion. In response, we have estimated error for this relationship based on differences in trend lines through data points one standard deviation above and below the mean for each experiment (see figure below). The slope of the regression between mass loss (g ash free dry weight) and temperature (degrees C) is 0.19, 0.21, and 0.23, for maximum estimates of decay, mean estimates of decay, and minimum estimates of decay. Using 0.21 g °C $^{-1}$ as the mean estimate of decay sensitivity to temperature yields an increase of 19.4% per degree of warming (relative to an initial decay of 1.08 g at 16.6 °C). Minimum (0.19 g °C $^{-1}$) and maximum (0.23 g °C $^{-1}$) estimates of decay yield 17.59 and 21.30% per degree of warming. Consequently, we will report the sensitivity of decay as 19 ± 2 % per degree warming.

[Please note that these calculations were done on an updated version of our results that corrected an earlier spreadsheet error. As we explain in our response to Reviewer #1, the slope between mass loss and temperature is identical to before, but with overall lower estimates of mass loss. Therefore, when sensitivity to decay is expressed relative to the initial mass loss, the percent increase is significantly larger (19% instead of 12%).]



The authors should stress that the work is primarily applicable to peaty marshes as for instance found on US east coast. In many parts of the world, marsh sediment levels are mainly dictated by the deposition of inorganic sediment and to a much lesser extend by organic carbon cycling. This is only indicated rather indirectly.

Nearly the entire discussion section of our manuscript deals with how carbon accumulation rates will change in response to temperature warming, and consequently is not dependant on the relative importance of peat vs. mineral accretion processes. The reviewer's concern would only be relevant when assessing the ability of a marsh to survive sea level rise. Indeed, there are many marshes (both in the US and abroad) where mineral sedimentation rates are high enough that potential changes in organic decay rates are not likely relevant to their survival. We tried to make this point clear early in the introduction (Pg. 709, Line 2), but will add a caveat to other sentences in the revised manuscript. As an example, we will modify Pg. 715, Line 21 to read, "This suggests that the net effect of temperature warming (or by analogy, elevated CO_2) is to make marshes more vulnerable to sea level rise, especially in regions where marsh accretion is dependent on peat accretion."

Other comments:
P708, L23: 'also bury organic carbon'
Yes, we will make this change in the revised manuscript.

P712, L23: the word 'initial' is puzzling here. Surely they can not suggest that this is one of the first studies on the effect of temperature on litter degradation. Or do they mean preliminary or short term?
The sentence in question refers to the effect of temperature on litter degradation in ***coastal wetlands***. It reads: "In this initial attempt to measure the sensitivity of decomposition rates to temperature warming in a coastal wetland…" While there are many experimental studies from other ecosystems (e.g. terrestrial forest soils, peatlands, lakes), we believe we are the first to measure a significant relationship between temperature and litter decay in a salt marsh, and are aware of only one other study that attempted to do so (Charles and Dukes, 2009). Nevertheless, to be on the safe side, we will replace the word "initial" with "preliminary".

References not cited in original text:
Bertness, M. D., 1985. Fiddler crab regulation of Spartina alterniflora production in a New England salt marsh. Ecology 66: 1042-1055.

Hackney, C. T., 1987. Factors affecting accumulation or loss of macroorganic matter in salt marsh sediments. Ecology 68: 1109-1113.

Newell, S.Y., Arsuffi, T.L., Palm, L.A., 1996. Misting and nitrogen fertilization of shoots of a saltmarsh grass: effects upon fungal decay of leaf blades. Oecologia 108, 495–502.

Valiela, I., Howes, B., Howarth, R., Giblin, A., Foreman, K., Teal, J., Hobbie. J., 1982. Regulation of primary production and decomposition in a salt marsh ecosystem. In:

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