Kirwan and Blum are making an important contribution to our ability to predict the future of tidal salt marshes. Consideration of the impacts of increased temperatures on both salt marsh plant production and organic matter decomposition is key to predict marsh sustainability and potential for carbon storage. Conclusions seem appropriate if regionally qualified, but confirmation of veracity of results requires much more information on methods used in the study - this is why I suggested "major revision".

We thank Dr. Chmura for her thoughtful review, and her conclusion that we are making an important contribution to our ability to predict the future of tidal salt marshes. We respond to her request for more information on methodology below.

The work by Kirwan and Blum suggests that additional studies should be undertaken to sort out some of the factors that they have briefly mentioned. For instance, to what extent would temperature of tidal floodwaters affect decomposition and productivity rates? These considerations would be particularly important on cold water bodies, such as the Bay of Fundy and St. Lawrence River estuary. Might we expect a difference with variation in tidal ranges which would pose differences in flooding frequencies? Authors and readers should recognize that the expression of global climate change is regionally variable (Christensen et al., 2007). Global climate models project that along the Virginia coast, the level of warming may be similar during summer and winter. However, on the northern northwest Atlantic the greatest changes are expected to be increased winter temperatures. It would be interesting to determine how such variability affects the balance between productivity and decay. We probably need to use this research as a model for additional studies that consider regional differences in species. For instance, should we expect the same response with Spartina patens, a grass which dominates marshes of New England and eastern Canada, or Atriplex portuloides, which is common in western Europe?

We agree with the conclusion that this research is best used as a model for additional studies. It is certainly true that we have demonstrated the temperature sensitivity of decomposition for only one species (albeit the dominant one in North America), and in one region. Nevertheless, we will add a regional qualification in the revised manuscript since it would be impossible to infer the effects of tidal range, estuarine circulation, and warming seasonality from a single study designed to isolate mean daily air temperature as the variable of interest.

More information is needed about the methods

1. Litter bags a)What were the litter bags made from ? b)What is the mesh size, what were the dimensions of the bags? c)If litter bags were in contact with the sediment surface could fine-grained minerals get through the mesh? If so, how was it removed?

Dimensions of the litter bags were approximately 25 x 25 cm. They lay roughly flat on the sediment surface, but beneath the litter layer. The side of the litter bag not in contact with the sediment was made from Nitex mesh with 0.5 x 0.5 mm openings (Memphis Net and Twine, Memphis, TN) to allow invertebrates recognized as important in decay to enter the bags. The side of the litter bag in contact with the soil was made of bridal organdy fabric, a very fine cloth that prevents most sediment from entering the bag from

the bottom. Because litter covered the mesh topside of the bag, mineral sediment (and presumably most suspended detritus in tidal water) was intercepted by the soil litter layer. Nevertheless, we used loss-on-ignition methods to calculate the amount of inorganic material that collected in the bags (~10% of total weight, see response #4 below for more information on methods), and we express mass loss in terms of ash-free dry weight so that fine-grained mineral sediments are excluded. The y-axis label in Figure 2 neglected to specify that mass loss was expressed as ash-free dry-weight (afdw), a correction we have now made.

2. Temperature (authors bring up the possible effect of tidal floodwaters cooling soils in other studies) a)How was daily temperature measured? b)Was it monitored for all deployment sites? c)What was the water temperature and salinity of the flood water?

Our experiments were designed in a way that minimizes the cooling effect of tidal floodwaters. First, the experiments relied on warming throughout the growing season rather than experimental chamber-based warming, so that both flood water temperatures and soil temperatures would increase throughout the season-long experiment. Additionally, the experiments were located in the upper reaches of the intertidal zone in a single location that receives about one tide per month. Consequently, we measured air temperature only (see Figure 1 caption); we did not measure the temperature or salinity of the flood water.

3. Field deployment: Differences in elevation and period of deployment would mean differences in frequency of tidal flooding of litter bags, thus differences in litter temperature. a)Where in the marsh were bags placed? b)Were all deployment sites at the same elevation? c)How frequently were bags flooded in each of the deployments?

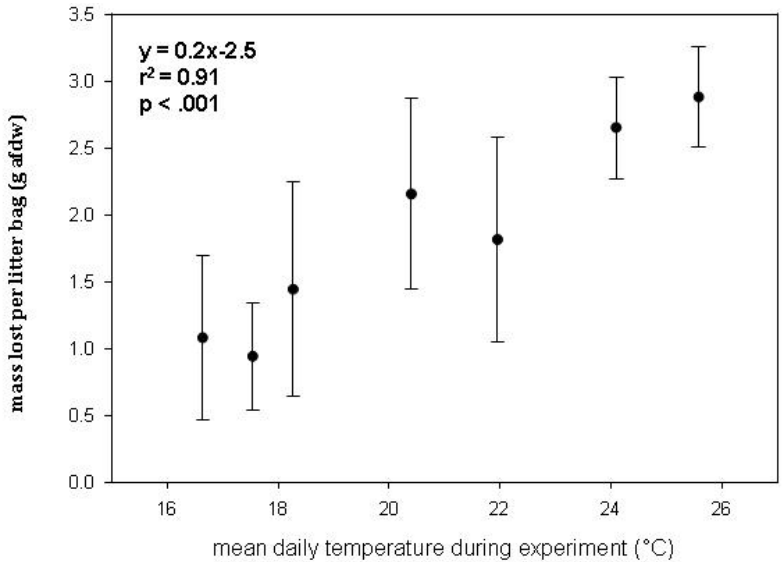
As described above, this location is rarely flooded so that any potential differences between water and air temperature are not likely to be important. Nevertheless, all litterbags were deployed at a single location in the upper reaches of the intertidal zone, and were oriented perpendicular to the topographic slope so that they would be at identical elevations and subject to the same inundation regime (i.e. one tide per month).

4. Lab processing a)"Roughly" half of the contents of each litter bag - were the postincubation litter bag contents first weighed and results from the split samples normalized to the mass of the entire sample? b) I don't understand how mass loss from a litter bag is determined by combusting the remaining sample in a muffle furnace - this seems to be just one step in the process and more explanation is needed here. c)Fungal and bacterial volume assessment need more explanation. For instance: 1)How were fungal hyphae and bacteria removed from the incubated litter? The supporting reference (Hobbie et al.) utilized filtered water samples.) 2) Is there support for the application of the method to fungal hyphae? (Hobbie et al. make no mention of fungus.)

4. a) Yes, samples were weighed before they were split. The mass allocated to various analyses was tracked, and changes in mass are expressed relative to the entire sample. b) Thank you for catching this ambiguous sentence. We will add more detail to the revised manuscript to describe the following methods: Prior to filling litter bags, we air

dried all material to establish a consistent initial weight, oven dried a subsample at 75°C, and then combusted it in a muffle furnace to determine its initial ash weight (i.e. inorganic and other residual material), and ash-free dry weight (dry weight – ash weight). At the end of each decomposition experiment, we dried the contents of each litter bag and calculated total mass loss (initial dry weight – final dry weight). We then combusted the remaining material in each sample to calculate the ash-free dry weight loss, a measure that isolates the loss of organic material (initial ash free dry weight – final ash free dry weight) and excludes the accumulation of any inorganic material.

While reviewing these calculations, we found a mistake in the spreadsheet formulation. In the new results, the slope between mass loss and temperature is identical to before, but with overall lower estimates of mass loss (see updated figure below). When expressed as exponential decay coefficients (k), our measurements yield k = 1.5-6.0 yr^{-1}, in good agreement with the range of decay coefficients (k = 1.0-9.1 yr^{-1}) calculated from a compilation of short-term measurements from 11 marshes throughout the United States (Christian, 1984).



c) Fungal and bacterial microbes were not removed from the litter. Instead, we used Acridine Orange (AO) to stain them in-situ. AO adheres to DNA, allowing both bacteria and fungi to be identified using the methods of Hobbie et al. (1977), as modified by Rublee and Dornseif (1978) for sediments. The approach used by Jones and Mollison (1948) was used to macerate the litter and appropriate dilutions of the macerated litter prepared (to achieve between 20-200 cells per microscope field or 10-50 hyphal intersections per field against an ocular grid) prior to staining. These are standard methods in the microbial ecology community, and are described in more detail in Blum et al. (1988) and Blum and Mills (1991), including their application to fungal hyphae. We will add these references to the revised manuscript.

In the introduction and discussion (pg 713) authors use results from CO2 enrichment experiments in their arguments. However, is it appropriate to assume that results from CO2 enrichment of C-3 species can be extrapolated to C-4 species, which are more efficient in uptake of CO2? Discussion of CO2 fertilization might be relevant if the manuscript addressed competition between C-3 and C-4 species, but it does not seem to.

The introduction simply states that some facets of global change (i.e. elevated CO2) have been observed to enhance marsh productivity and soil accumulation rates, and does not rely on any extrapolations between species. Our discussion includes a single paragraph on how the results of our decomposition experiment might be interpreted in the context of elevated CO2 (i.e. the effect of elevated CO2 on temperature warming and organic decay). We do not actually extrapolate the productivity response of C_3 plants to C_4 plants (the C_4 productivity response comes directly from Kirwan et al. 2009 and the C3 response comes directly from Langley et al., 2009), so the concern about CO2 uptake efficiency is not relevant. However, for the sake of illustration, we do estimate the amount of enhanced decomposition that might occur in C_3 marshes under the temperatures associated with elevated CO2 (pg 714, line 5). Implicit in this argument is that the temperature sensitivity of organic matter decay is similar between C_3 and C_4 plants. Since the literature on decomposition sensitivity to climate change is still in its infancy, the appropriateness of this assumption is unknown. Consequently, these comparisons should be considered very preliminary, and we will certainly add a caveat to the revised manuscript that explicitly states this assumption.

pg 709 ln 11 C3 marshes = marshes dominated by C3 vegetation; also note variable ways C3 is written.
Thank you, we will use the format "C_3" consistently in the revised manuscript.

ln 25 and elsewhere - I don't think the conclusion that marshes may survive faster rates of sea level rise merits use of the term "paradigm".

Would "emerging paradigm" or "emerging conclusion" be more acceptable? We cite at least 6 papers since 2007 (including several from PNAS and Global Change Biology) that support "the paradigm that global change will lead to wetlands that are more resilient to sea level rise."

Pg 710 ln 14 "Bags were buried in contact with the sediment surface, but underneath any accumulated plant litter." Do authors mean "buried beneath plant litter and in contact with the sediment surface"?

Yes, that is correct.

pg 714 ln 714 mean annual GLOBAL temperature

Yes, we will make that clarification in the revised manuscript.

References not cited in original text:

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