

## ***Interactive comment on “Global change effects on decomposition processes in tidal wetlands: implications from a global survey using standardized litter” by Peter Mueller et al.***

### **Anonymous Referee #3**

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Mueller et al. conducted decomposition experiments using tea bags based on a standardized approach developed by Keuskamp et al. (2013), across different marsh and mangrove sites in order to cover a gradient in temperature, inundation regime, etc. While such cross-ecosystem studies have a high potential, I feel the impact of this dataset in terms of new insights is relatively limited. The dataset can be published but I feel the impact of the conclusions should be toned down somewhat – the manuscript does not really deliver what the title suggests. The dataset should be publishable, but it needs a more critical discussion and should provide the readers with a more complete overview of the caveats and assumptions used in the TBI approach, so that the readers can better assess what can and cannot be deduced from these data.

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My main point is that the TBI index – both the original and the modified protocol suggested here – has plenty of limitations and it remains an operationally defined procedure, with several assumptions that are open to discussion. In addition, we are not looking at mineralization of in situ produced material hence some interactive effects will be missed in this approach; results should not be over-interpreted or generalized.

Specific suggestions -L55: “stabilization was 29% lower”: this does not mean much if you do not define stabilization here, it can be interpreted in different ways. For me this remains a somewhat problematic proxy (see further comments).

-L60-61: data from the eutrophication experiment: would not extrapolate this to ‘high sensitivity to global change’. Eutrophication will also affect the nutrient content of locally produced biomass, this aspect is not taken into account when standardized material is used in the experiments.

-L90-95: an important caveat here is that you only study the decomposition of one type of source material (well, in two versions), but not other sources that contribute to the OM pool e.g. marine or other aquatic inputs into the intertidal system.

-section 2.2: it is important for the readers not familiar with the Keuskamp et al. paper to re-iterate and stress the assumptions on which this approach is based, e.g. that  $k_2$  (decomposition constant of the non-labile fraction) is assumed to be 0, and that  $S$  is assumed to be similar for both types of tea. I still find this major shortcomings- we know the first assumption not to be valid, and I have not seen strong arguments to support the second assumption. The main reason to make these assumptions is to allow to estimate  $k$  and  $S$  using only one time point of measurements instead of having to measure at different points in time. These aspects deserve to be mentioned explicitly and the limitations of the approach should be discussed more critically. -What is the added value of this approach compared to simply measuring the decay of the biomass over a limited number of time steps, and using a more realistic decay model ?

-L212-214: provide the data from Keuskamp et al. as well, we cannot compare or

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assess how much higher your data are.

-discussion L 427-434: This is somewhat problematic also. It demonstrates the disadvantages of using these operationally defined indices; to which extent is this caused by the assumption that S is identical for the two types of substrate ? Secondly, keep in mind that anaerobic decomposition processes are important in tidal wetlands, and can occur at high rates (similar order of magnitude as aerobic decomposition) up to substantial depths.

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