Biogeosciences Discuss., 7, C990–C993, 2010
www.biogeosciences-discuss.net/7/C990/2010/

Biogeosciences Discussions

Interactive comment on "Impacts of exotic mangrove forests and mangrove deforestation on carbon remineralization and ecosystem functioning in marine sediments" *by* A. K. Sweetman et al.

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Received and published: 17 May 2010

General comments

It is an interesting paper that describes the impact of mangrove vegetation before and after deforestation on the fate of added microalgal carbon in sediments. The presence of mangrove roots, both dead and alive, increases benthic metabolism and cycling of added labile organic carbon. Bacteria are the most important processors of algal carbon in live mangrove environments, while macrofauna is more important in mangrove removal and mangrove-free control areas. The findings are relevant for understanding how sediment functioning changes through time after mangrove deforestation and how invasive mangroves may affect carbon dynamics in otherwise mangrove-free areas.

The paper is well written and structured. It addresses a topic that is highly relevant for BG. The idea that mangrove roots can impact the benthic system for years after deforestation is new and exciting. The methods are adequate for this type of study, although not always described clearly (see specific points below). The results are presented clearly, but does not emphasize sufficiently that labeled algae were deposited at the surface and therefore not accessible for subsurface organisms.

My major concerns deals with the use of surface-deposited 13C-labeled labile algal carbon as an agent to examine carbon dynamics in sediments containing live or up to 6 years old remains of mangrove roots. These roots primarily consist of cellulose and lignin located deep into the sediment, while the added algal carbon was deposited on the sediment surface. The authors acknowledged in the discussion that the algal carbon is not fully representative of the bulk sediment organic carbon, but they do not fully consider that the added carbon is deposited at the surface and as such has little chance of affecting deeper layers within the 48 hours of their experiments. This renders the depth profiles of carbon handling by organisms (Fig. 7 and 8) invalid. It tells more about the availability of algal carbon than the organisms' capacity to handle the carbon. I also lack some information how the authors believe that organisms promoted by the presence of mangrove root materials instantly can switch to an alternative carbon source (i.e. added algae). Do these sediments have an unused capacity to handle new carbon sources – or how should we understand the results?

I am also worried about the use of macrofauna to describe the fauna recovered from the sediment. Some of the taxons, such as Harpacticoida and Nematoda, listed in Table 2 usually belong to the meiofauna. The authors should reconsider the use of the term "macrofauna" or remove species belonging to meiofauna from the list. Macrofauna is usually defined as species with adults of a size larger than 1 mm.

Specific comments

Title: The title does not really reflect the contents of the paper. I suggest that it is changed to something like “Impacts of exotic mangroves and mangrove deforestation on near-surface sediment carbon cycling and functioning”

Abstract: No comments

Introduction: No comments

Methods: P. 2637, lines 9-10: I think that it is premature to present the sediment characteristics (Table 1) here. The methods to obtain these data are first described later in the same section. P. 2537, lines 16-26: It is not fully clear here if the sediment cores were obtained from permanently inundated locations. It appears so at Pearl Harbor, but not from Kaneohe Bay. The statement at line 26 “…ensuring that all cores were subjected to the same degree of air exposure” is confusing. P. 2638, line 12: A δ13C signature of 26330$\pm$303‰ is somewhat extreme and cannot be true. δ13C values are usually 1000 times lower! P. 2638, line 13: how much of the labeled Chlorella was added to each core. Give values here preferably in carbon units. It is too late to give this information on p. 2642 (lines10-12). P. 2638, line 27: I wonder why no sampling and analysis were done in order to determine total DIC efflux from the sediment. This would be an obvious parameter to include in a study of carbon cycling. It is particularly relevant when DIC derived from the added microalgae are estimated from DI13C (p. 2642, line 8) because the contribution of microalgal carbon to the total DIC flux could be estimated and evaluated. P. 2640, lines 4-5: Does this mean that POC was not measured and that SMB was used as a proxy instead? P. 2642, line 19: The dissimilarities in tidal elevation were not evident from the previous text (on p. 2637). This must be clarified.

Results: P. 2643, line 23: A macrofaunal abundance of 200000 m-2 is extremely high. This means that there were 20 individuals per cm2. Based on such extreme value, I am convinced that a large fraction of the so called “macrofauna” in reality belongs to the meiofauna (see under general comments). P. 2645, lines 12-17: Here it would be very nice with a total DIC flux in order to see the influence of microalgal C.

Discussion: P. 2647, lines 12-22: Here again it would be nice to have a direct measure of total DIC efflux. P. 2649, lines 1-2: It is obvious that the transfer of algal-C to bacteria was highest in the topmost sediment because it was here the algae were deposited! This is actually acknowledged by the authors for deep-living macrofauna on p. 2650 (lines 26-27) and p. 2651 (lines 13-28), but not here.

Technical corrections

P. 2637, line 5: Rewrite to "The remaining two sites. . .." P. 2637, lines 6-7: Please specify here that it is the 6 yr site. This is not mentioned. P. 2640, lines 15-16: Rewrite to "Bacterial biomass was calculated as PLFA. . .." P. 2643, line 18: Rewrite to ". . .with depth to 5 cm in cores from. . ." P. 2645, line 4: Rewrite to "with greater SOC rates for sediments from the PHM. . ." P. 2645, line 12: Rewrite to ". . .75 to 90% of the processed algal-C was found in the DIC. . ." P. 2656, line 21: This reference has no author. Is that really true? Figs. 7 & 8: The A, B and D subfigures in both figures present the same data as shown in Table 3. Could they be omitted?

Interactive comment on Biogeosciences Discuss., 7, 2631, 2010.