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Biogeosciences Discussions

Interactive comment on "Degradation capability of the coastal environment adjacent to the Itata River in central Chile ($36.\vec{5}^{\circ}$ S)" *by* S. Pantoja et al.

Anonymous Referee #2

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Review of 'Degradation capability of the coastal environment...' Pantoja et al.

This paper examines the degradation of organic matter in the Itata River using some standard oceanographic measurements (Chl, temperature, etc.) and hydrolytic enzyme activity. The main conclusions were that surprisingly winter activities were higher in the river and estuary than in the summer and that aminopeptidase activity was comparable to cellulase activity in the sediments. Most previous work has suggested that aminopeptidase activity should be higher than cellulase activity. Lastly they constructed a carbon budget based on these degradation rates and primary production and concluded that there was not enough primary production activity to support the degradation rates that were measured and therefore a large amount of the degradation that occurs in this system is likely of dissolved organic matter that is transported into the estuary

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and coastal region via the river.

While I do not have any real problem with this final conclusion about the significance of DOM and the idea that DOM could support a great deal of the degradation that they observe, I don't find the arguments very compelling. I say this essentially because I don't think they understand what exactly they are measuring. All of the rates were measured with model substrates at 10 μM concentrations. The only concentration of substrate that they report is for total protein and the concentrations were 10-50 less that this. That means that they were essential measuring a potential amount of activity and degradation, not the actual amount that was occurring. They mention this in discussing the enzyme measurements but seem to have forgotten it when it came to interpreting these results. If they had wanted to measure the actual degradation rates, a better approach would have been to add the fluorescent substrates at tracer levels, around 10x less than the ambient concentrations of substrate as Ammerman did back in the 80s and 90s.

So, if they didn't measure actual rates, how could they use their data in a way that we could better interpret the patterns observed? One thing that would help a great deal is to report the enzyme activities normalized to biomass somehow, such as bacterial numbers, particulate carbon or even chlorophyll. That would enable the reader to differentiate between changes in activity that were due to a physiological vs. an ecological response. For example, a physiological response might be where the microbes increase enzyme activity in response to increased or decreased substrate availability. On the other hand, at the ecological level, you might observe an increase in activity due to the fact that there are just more microbes present. As the data are presented and interpreted presently, we cannot determine which of these factors might be most important. It seems that the authors are mostly assuming that the differences they observe are due to physiological responses but my experience is that the biomass likely has more to do with changes in activity.

My last major criticism is that it is not possible to construct a carbon budget based on

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potential activity measurements with model substrates as is done in Figure 7. If the rates are potential rates they don't have any value in constructing a carbon budget and drawing conclusions about heterotrophy vs. autotrophy. Even if the rates could be used in this way, I don't think hydrolysis tells us much about this balance because you do not know the fate of the carbon that is being hydrolyzed, i.e., is it respired or transported offshore or incorporated into biomass that ends up in the sediments? If either of the latter two options occur, you would be using hydrolysis as an indicator of heterotrophy but the organic carbon is not completely remineralized.

Other issues: Introduction first paragraph–the gap in knowledge of understanding transfers of carbon between the terrestrial and marine environments is an important one, but I don't feel that this paper resolves the issues very well.

Methods: p1341, line 5. The authors should clarify what nutrient measurements were made spectrophotometrically.

1342, line 5-10–were these concentrations of substrate saturating to each of these enzyme activities? Also, were any time-course measurements made to insure that measurements were made in the linear portion of the activity measurements?

1342, line 15. As mentioned above, these rates are not 'actual' rates because you can not assume that there was 10 μM of substrate for them to work on and you do not know that the model substrates actually behave the same way the in situ substrates do.

1342; sediment rates–it would be useful to the reader to be able to compare water column and sediment rates, i.e., the pelagic rates could be integrated.

1348, bottom of page. I agree that organic matter supply may not be the only thing impacting enzyme activity and as I mention above, I don't even think the organic matter supply is the most important one. One question comes to mind that is relevant here is whether these enzymes are inducible or not. I suspect that they are but it should be discussed because it is important to this conclusion.

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Fig. 2: It's really hard to see much on these figures including the labels. I would suggest putting fewer panels on each page and making them much bigger.

Fig. 5: The legend must be wrong because it is labeled MCA-glu and MUF-glu and the MCA-glu is not discussed in the methods.

Interactive comment on Biogeosciences Discuss., 8, 1335, 2011.