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Biogeosciences Discussions

Interactive comment on "Simultaneous quantification of in situ infaunal activity and pore-water metal concentrations: establishment of benthic ecosystem process-function relations" *by* L. R. Teal et al.

Anonymous Referee #1

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General comments

The authors investigate the influence of a natural macro-invertebrate community on the biogeochemistry of sediments in a dynamic marine ecosystem (with tidal influence). They use a short-term in situ experiment to measure bioturbation-induced changes in iron and manganese cycling. By combining simultaneously high resolution imaging of fluorescent tracers (luminophores) and metal concentration profiles (DGT), the intensity of particle mixing due to macrofauna is directly related to small-scale alteration of metal concentration profiles in pore-water. The originality of this work is to propose

a modelling approach permitting to hide the apparent heterogeneity due to the high variability of metal profiles (something reflecting the importance of abiotic environmental factors and multiplication of microenvironments due to the bioturbation of a diverse macrofauna assemblage, as expected in the study site). While the interpretation of each metal profile effectively measured (here, 2 replicates on 3 runs, then 6 profiles for each metal) is obviously difficult, the method proposed here permit to give a "picture" corresponding to a trend of the metal distribution. At the end, these idealized averaged profiles can be easily compared to the luminophore profiles and then the influence of discrete bioturbation events can be interpreted more easily. In the present case, the authors confirm a common observation of bioturbation studies, i.e. that it alters the vertical redox zonation of sediment, with deeper and more pronounced peaks of metal concentrations with increasing infaunal activity. The major interest of this work is to demonstrate the feasibility to consider highly variable processes like bioturbation and biogeochemistry of metals at small temporal and spatial scales directly in situ. However, interpreting their relation remains limited due to the differences of spatial and temporal dynamics of underlying processes, i.e. particle versus pore-water mixing. As highlighted in the discussion, it is thus crucial to improve such technical approach in situ. It would greatly help our understanding of ecosystem functioning at larger scales, something essential for anticipating consequences of environmental changes for instance. As the study of metal biogeochemistry in sediment is particularly challenging, the data presented here, in addition to the originality of the technical approach, are of first interest and this work fits in the scope of Biogeosciences. The methods used are appropriate, the manuscript is generally well written and the findings are discussed in the right context (even if it focuses too much on marine ecosystems). The presentation of data is fine (but see comments below for Fig. 3). The paper should be published providing minor revisions (see below).

Specific comments

Title – it should be shortened a bit if possible.

Introduction – it is well written, clear and concise... but too much focused on marine ecosystem for my point of view. As the authors propose an important improvement in the consideration of bioturbation processes in natural environments, some references to freshwater studies would be welcome (and maybe to soil ecology). Freshwater ecosystems are particularly exposed to rapid environmental changes and suffer a lot from extinction or invasions of species with potential high influence on biogeochemical processes, including cycling of metals and metallic pollutants.

Materials and methods – It is not clear for a non-specialist what is the interest of using DGT probes. For example, it appears only in the title that it is for measuring "porewater metal concentrations". In the results, we find only the term of "flux" and I think that the reader could misunderstand that it corresponds to the flux between the pore-water and the Chelex resin during the time of deployment. Also, the authors do not explain their choice of using DGT instead of another method (why not DET for example?). More largely, there is no clear explanation of the pertinence to study this biogeochemical process (metal cycling) rather than something else. For example, oxygen consumption could also be measured with high resolution and it is more representative of the global sediment functioning. On the other hand, metal cycling is complex and difficult to interpret because it depends on several factors like changes in oxidation/reduction rates, abundance and diversity of metal-oxidizing bacteria, and rates of mineral formation, etc... of course I imagine that the authors have chosen these processes exactly for these reasons but it would be nice to have more details in the introduction and/or materials and methods. A second remark for this part is about the numbers of replicates (2 DGT X 3 deployments X 6 cores of 10 cm). I could imagine the difficulty and the costs of such an experiment but is it theoretically enough to consider the effective variability of a 50-m radius site? As well, it would be interesting to have a deployment with not or very few bioturbation but maybe it only depends on luck to have such an observation that could serve as a "control" reference. Finally, the authors should mention the size of luminophores.

Results – It is not easy for a non-specialist of marine macrofauna to consider the importance and the type of organisms involved in bioturbation processes. Maybe, an additional column in Table 1 with the corresponding group would provide help (Crustaceans, mollusks, worms, etc...). As well, we do not know if it is a typical community for this kind of ecosystems or if it corresponds to an altered community (e.g. presence of invaders, high diversity or not). In the paragraph 3.2, the luminophores profiles are described in relation to potential bioturbation events but several times it refers to epifaunal species that do not appear in the listing of Table 1. Why not including them? Paragraph 3.3 and Fig. 3: there is a mistake between the profiles represented in the figure and the legend. The grey solid lines corresponding to one of the two DGT fixed on the camera are not visible on a, b, e and f. The legend indicates that some profiles from SCUBA divers are missing but there are all appearing. As a consequence, it is difficult to read this part of the manuscript, example "with the exception of the considerable variability in Fe profile shape during deployment 1"... I find that c, d and e show also a high variability... Finally, the last part (relationship Fe/Mn) and Fig. 4 are not essential for a good comprehension of the paper. If some other parts should be developed, this one could be discarded.

Discussion – This a really nice interpretation of the results and it clearly answers to the problematic exposed in the introduction. But here again, I am surprised that it is limited to the context of bioturbation in marine ecosystems. As well, I find that the discussion of metal biogeochemistry itself is not sufficiently developed. There are considerable works in the literature dealing with cycles of Fe and Mn in surficial (marine) sediment and most of the time bioturbation processes are totally overlooked. I understand that the main goal of the study is to propose a novel technical approach to link variable processes at different scales to explain the ecosystem functioning but some details about metal cycling would be appreciated by "pure" geochemists. It would help further "multidisciplinary collaboration" as cited in the text...

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