

## ***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.***

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Anonymous Referee #1

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

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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).

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## Specific comments

Title – it should be shortened a bit if possible.

REPLY: Shortened to: “Establishing benthic ecosystem process-function relations in situ”

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.

REPLY: We understand the reviewers point of view and appreciate the importance of pollutants in freshwater and terrestrial soil environments. Although we feel our results do not necessarily straightforwardly translate to these systems (different environments and processes), it is true that the methodology is directly transferable . We have added an additional freshwater reference but also argue that our reference lists covers literature from riverine, to estuarine, mangrove forests, coastal and deep-sea, temporal to arctic environments. We are also limited with how many additional references we can still add in.

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?).

REPLY: In view of this and other comments made we have added an extra section to

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the introduction to explain why we chose to measure trace metals and use DGT. We added some further sentences to the DGT section in the methods section to explain why we use DGT as a method and not DET. The use of DGT probes are a standard and established methodology in biogeochemistry and we do not see the need extensive explanation in the present contribution, however we have added some additional detail about how DGT works to the methods alongside the key references, including previous work by the present authors, at appropriate places within the manuscript for readers who wish further detail on these techniques.

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.

REPLY: We agree with the reviewer that we did not address properly in the introduction why we opted for metal cycling rather than e.g. oxygen consumption. We have added text in the introduction to address this (see final paragraph). The DGT method also allows us to take time-integrated measurements of Fe/Mn profiles which we can relate to the full infaunal activity occurring during the time of deployment. Furthermore, the reviewer is correct of course that metal cycling is rather complex and difficult to interpret. However we would argue that many of the processes influencing metal cycling will remain fairly consistent throughout the deployment and between deployments within the same area. Having said that, our conclusions as to why we struggle to establish a solid link between process and function in this study is that other processes not measured are also influencing this link, demonstrating how key it is to measure more processes

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simultaneously. We have reworked some of the discussion to address in more detail the limitations and difficulties of interpreting DGT profiles and hope through these adjustments to have satisfied the reviewer.

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.

REPLY: The referee makes an interesting observation here. Most DGT studies use considerably less, if any, levels of replication. In this study, we used 15 replicates to characterize metal concentrations within a single habitat. The referee has, however, misunderstood the objective of our sampling design. Our aim was not to characterize a 50m radius site, rather it was to account for the variability in observations within a specific habitat site. Previous work in the literature have made conclusions based on either single or low replication deployments within a limited area (often 1-2m diameter, relating to the footprint of a benthic lander). Here, we seek to improve the level of replication and the amount of variation we incorporate into our evaluation. In this way, we can be sure that any patterns we observe are conservative, real, and not overly influenced by small-scale processes. In terms of a control site, it would certainly be a valuable addition, but as the reviewer rightly points out, it would need to be a “lucky” deployment. That said, it is possible to compare findings from this study with findings in Teal et al 2009 from sites of lower activity. In effect the three sites that can then be compared (although care must be taken due to the differences in sediment) show a gradient in infaunal activity and also in the Fe/Mn patterns observed. We have added an additional sentence to the discussion to highlight this.

Finally, the authors should mention the size of luminophores.

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REPLY: We thank the referee for spotting this omission – we have added the size distribution of the 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).

REPLY: This is a valid point and we have added an additional column indicating the Phylum as the referee suggests. In addition we added columns with the bioturbation potential (see new table and legend). This should provide non-expert readers with enough information to get a better view of the type of community.

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?

REPLY: Table 1 refers to samples that were taken to give an indicative listing of infaunal species composition and the epifaunal species named were not contained within the sample (as can be expected due the sampling method). The time-lapse sequences revealed an important role of epifaunal species, which we have incorporated, which would otherwise be missed by infaunal sampling alone. The objective of the sampling was to provide a listing of numerically and biomass dominant (infaunal) species that occur at the site to aid the interpretation of the time-lapse imagery. We cannot provide this for epifauna.

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.

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REPLY: We thank the reviewer for pointing out this mistake. It should indeed be SPI DGT profiles that are missing rather than SCUBA diver deployed ones. This has been corrected in the figure legend and should now be clearer

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.

REPLY: This is correct and the text has been amended. Although Figure 4 may not be viewed as essential for the understanding of this paper it does provide a nice comparison with the g-SPI results in Teal et al. 2009 (which is encouraged in relation to the reviewers previous point about having a control deployment).

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.

REPLY: Our primary focus was to investigate whether sediment bioturbation and the redistribution of porewater metals were linked, as often is assumed, rather than the detail and specifics of the chemistry. However, we acknowledge that we could have added a more detail on metal cycling, and have added some key references and additional text in the Discussion as the referee has suggested.

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