

Interactive comment on “Two-dimensional distribution of living benthic foraminifera in anoxic sediment layers of an estuarine mudflat (Loire Estuary, France)” by A. Thibault de Chanvalon et al.

Anonymous Referee #3

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Manuscript by Chanvalon et al presents an interesting, new approach on investigating the distribution of benthic foraminifera in anoxic mudflat sediments. As estuarine sediments can be highly heterogeneous, relating foraminiferal distribution with geochemistry of their microhabitat can be difficult. The approach presented here, however, now allows identification the examination of live foraminifera (CTG-labeled) and the geochemical (dissolved iron distribution) microhabitat simultaneously. In general the introduction, methods and results of the manuscript are relatively well written and the approach used here is very sound. Nevertheless, the discussion is very long and

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in many parts chaotic going far beyond the data (and the approach) presented in the manuscript. Prior to publication the discussion section need to be rewritten. I suggest that the paper will take more of a methodological approach/theme rather than try to hypothesize about range of possible factors, not included in the current study, that may explain foraminiferal distribution in sediment.

Below see more detailed comments/suggestions.

Abstract Adapt depending on a revised, more focused discussion. Lines16-17 would you then also conclude that the distribution of *A. tepida* at depth is not linked to worm burrows?

Introduction In general good but the text could be made more concise. Also in the introduction I would aim to keep to the topic of the paper. For example, now sediment heterogeneity and patchiness are kind of spread over few paragraphs that not only create some repetition but also makes text long. Also few paragraph brakes may make text more digestible (e.g. section on estuarine foraminifera could be a separate paragraph) I also suggest, although intro is already long, that a brief description of pore water iron and phosphorus distribution/behavior and relation to redox chemistry is added, as this may not be obvious to all readers of BG. Finally authors suggest that they will attempt to confront all mechanism listed on lines 22-26 (p. 10316). I would like to point out that authors have not directly measured presence of labile organic matter in their samples, rather that their interpretations are based on presence of dissolved Fe²⁺ from iron oxide reduction. Of course micro-organism that reduce iron will also be associated with organic matter break down but I would also like to argue that if the sediment indeed would contain very high amounts of organic matter the peak in the iron reduction would not take place at around 8 cm depth in sediment (Fig 7) but much shallower. Authors should keep this in mind when interpreting their results especially as they do not have direct measurements on quantity nor quality of organic matter in their sediment samples with corresponding dissolved iron. Minor textual remarks p. 10315 lines 4-7 please rephrase this sentence. It is not clear; especially I do not understand what “it is neces-

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sary to do so” refers to. p. 10315 line19 “. . .of pore water sampling techniques, such as (REFS), with high results. . .” Add such as plus relevant references.

Methods p. 103 line 13 delete “Green at” add “in” p.10319 line 8 add (Figure 2) after “(jaws)”. p.10319 line 10 in which or from which? p.10319 line 11. Delete “Figure 2 shows the sampling outlay:” Change to The device was deployed into the sediment at low tide. The first jaw is made of a 250x. . . p.10319 line 23 “whole” what? Device? p.10320 line 4-5 sentence “Figure 3 compare 1-D and 2-D sampling of foraminifera.” Seems incomplete/does not link with previous sentence.

Results p.10322 line 9. Do you mean you saw polychaetes when you slices the whole cores, or from the grid? It sounds like the core, but rest of the text is about the grid so it is confusing. p. 10324 line 18 why is the colour scale arbitrary? Is it not accurate? p. 10325 lines 1-7 could it be that some burrows contain more or less dissolved Fe²⁺ and P depending on how active these burrows are and how frequently these are flushed? Longer flushing time/inactive burrows would allow dissolved species accumulate I would speculate?

Discussion 4.1 heading: I do not think currently this section really focuses on what the heading suggest. p. 10325 lines 10-22 I recommend rewriting the start of the discussion. Now it reads like an abstract and just seems to add unnecessary length to the paper. p. 10325 lines 23-26 The start of this paragraph should be moved to results. Also I suggest that Figure 8 should be first introduced in the results section. p.10326/27 line29/1-4 I would be careful in suggesting that foraminiferal distribution is not liked to sediment geochemistry. Could it not be the case that dissolved iron and iron redox chemistry is not so relevant for explaining foraminiferal distribution in sediment? We know that some foraminifera denitrify so their distribution may be more closely driven by oxygen and nitrate dynamics. As denitrification is not directly related to iron availability, iron reduction correlation is not seen in this study. Furthermore iron kinetics are generally regarded as “slow processes”, and as reduction of iron takes place from solid phase, where as nitrate is present as a gas in sea water, their dynamics in this

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setting can be different. I would speculate that in heterogenous, tidally influenced environment where oxygen may periodically enter burrows, causing nitrification and relatively sudden changes in oxygen supply and demand, and subsequent changes in denitrification, iron oxidation and reduction occur on slightly different time scales.

4.2 (and all subsections 4.2.1, 4.2.2.) This whole section (and the whole discussion) should be more focused on the topic of this manuscript, which is about the new method for studying foraminifera in two dimensions and correlating this with sediment geochemistry. Now the section discusses sediment heterogeneity very broadly, and the tackling issues like influence of long-term macrofaunal bioturbation, which have clearly not been studied here. This paper shows a snap shot in time. A different type of experiment/study is needed to truly examine macrofaunal impact on sediment heterogeneity, and associated changes in foraminiferal distribution.

4.3. As for previous section I think it is important that the authors will focus their study on data that they have and keep the context of the paper in the new method plus foraminiferal distribution. It is ok to speculate on some issues but you cannot make confirm conclusions about controlling ecological parameters if you do not have data to back this up. p. 10332 line 5-8 if the low iron at 1-3 cm depth would correspond with frequent oxygen supply by bioirrigation would you not also think that there would be more *A. tepida* then? It would be then more preferable habitat for them; in contrast this is where the numbers are lowest? p. 10334 line 6-7 how is the length of the biogenic particles identified by dissolved iron?

Conclusion p.10334 line 22 add “of dissolved iron and phosphorus” after “hotspots” p.10334 25-27 and p. 10335 lines 1-9 please rewrite this part of conclusions so that it will be consistent with revised, more focussed discussion. Also it is not necessary to point out in which section a particular conclusion is discussed.

Figures Manuscript has 11 figures that is quite a lot. Are they all necessary?

Fig 5 Data on *H. germanica* is presented in the figure but not presented in text or

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discussed in any way. I can see it is less common than *A. tepida* but if it is presented in figure it should also be at least mentioned in the text. Furthermore, its abundance seems to increase with depth?

Fig 6 left hand column with Corg data also has on bottom O₂ scale. I would delete the O₂ scale as it is given in the O₂ pore water profile and as O₂ is below detection limit at few mm it is not visible at all on the left hand side diagram. Also why was O₂ not also measured in the bottom water. How did the authors evaluate the sediment water interface?

Fig 8 b somewhere in the figure or the caption it should be mentioned that the DET-1D equivalent mean and extrema is derived from 2D plot. This was quite confusing when trying to link the discussion and figure together.

Fig 10. As the aim of this study was not to explain the impact of macrofauna on distribution of foraminifera, and authors have no real data on this. I would perhaps delete this figure and at the same time make the discussion more focused on the topic of the new method.

Fig 11. Consider leaving out.

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