Biogeosciences Discuss.,
doi:10.5194/bg-2016-411-RC1, 2016

Biogeosciences
Discussions

Interactive comment on "Functional classification of bioturbating macrofauna in marine sediments using time-resolved imaging of particle displacement and multivariate analysis" *by* Stina Lindqvist et al.

Anonymous Referee #1

Received and published: 24 November 2016

General comments:

The paper applies a time-resolved imaging method for the functional classification of eight species of bioturbating macrofauna. The text is generally well written and structured, and contains information useful for marine ecologists or biogeochemical modelers. However, in my view, the scope of the paper is rather specialized. As I explain in more detail below, I think the paper has a few deficiencies: 1. the aim of the study as well as the presentation and meaning of the data is not sufficiently clear; 2. the implications of the findings are insufficiently explained in the broader biogeochemi-

Printer-friendly version

Discussion paper

cal context; 3. the statistical approach used seems to me not fully appropriate as it mixes a subject-independent method (PCA) with a subject-dependent method (choice of specific, possibly not robust, variables *derived* from the raw dataset) to arrive at conclusions.

I fully agree that bioturbating macrofauna play an important role in the biogeochemistry of sediments. Also I can understand that dividing bioturbating macrofauna into functional groups is useful when dealing with complex communities and their impact on sediment biogeochemistry. All this is well explained in the introduction. However, what I do not really understand is the actual *aim* of this study within this context.

If the aim was to classify eight specific species, then the study is rather specialized and perhaps more suitable for a more biological journal. In this context, it is also not clarified why these particular species were selected. If the aim was to demonstrate the utility of the method, then I find the description of the method and the data obtained insufficient to understand (i) what was really done, (ii) why the raw dataset was reduced to the specific variables, and (iii) what the differences between the studied species really mean.

Personally, I prefer to look at raw data first and only then explore how they were processed, analyzed and interpreted by others. However, this study does not make it possible to do this as no measured data is presented. What is presented are only variables *derived* from the raw data based on a specific choice of the authors. This specific choice may be biased towards the aim, possibly influencing data interpretation. However, there is no way to find out.

These days there is plenty of room for including images or videos as supplementary information, and I encourage the authors to do this. Specifically, it would be really useful if they present their raw data as videos showing frames with the luminophores distributions as a function of time. Application of the threshold would be fine, and the replicate images for each species could be combined into one frame. They don't have to be in the full resolution, but at least an impression of "how the distributions looked like" would be useful. Then we can understand the complexity of the dataset as well as fully appreciate the need to reduce it. Additionally, we can better judge whether the approach to reduce the dataset, as chosen by the authors, was appropriate.

In this context, I have some reservations about the statistical analysis employed to arrive at conclusions. It is based on PCA of a set of variables derived from the raw data. However, I am curious why PCA was not used on the raw data itself. The whole point of PCA is to reduce complexity of the dataset about a system into variables "that matter" most in its description. However, a priori reduction of the raw data into derived variables risks that this analysis is biased, because it gives higher statistical weight to some data (in the raw dataset) compared to others. Therefore, I think the authors need to dedicate some space justifying their statistical analysis approach. In my view, the classification of sediment reworking types/modes that the authors present should *emerge* from the PCA analysis of the raw data. Instead, the authors *bundle* the raw data in such a way that the sought after classification of a specific species is either accepted (with some caveats) or rejected.

I am particularly concerned about the maximum penetration depth (MPD) variable. As I understand it from the text, this variable is essentially a measure of rare events. That is, if a particle from the surface is by chance deposited at some depth in the sediment, it can remain there for a long time. This will lead to a constant value of MPD over extended periods of time (as indeed shown in Fig. 2), giving this one (likely rare) event a disproportionately large weight in the final PCA analysis of the (reduced) data.

Also, it is not entirely clear how certain the estimates of the parameters shown in Fig. 3 are. One can always fit a dataset with a model and obtain a process parameter such as r or Db. But the authors do not provide any clues as to the quality of their data fits and uncertainties of the estimated parameters. It would help if a few examples (best and, possibly, worse) of the data together with the fits are shown and the above points are discussed.

Last but not least, the quantities characterizing the 2D redistribution of the particles shown in Fig. 5 are unclear. As I understand it, redistribution of particles is a 2D image. But how is it reduced to a number in cmˆ2? Obviously, there is an enormous data reduction involved. If there is no other metric used to characterize it (e.g., some sort of variance), then again this data points will have disproportionately large weight in the final PCA analysis.

Specific comments/questions:

p.3

l.16: you make a distinction between functional groups and functional modes. In the introduction these subtleties are not clearly explained.

l.21: After reading the introduction, I do not understand the aim of the study, why the specified animals were chosen, and how this choice was related to the classification mentioned in the preceding text.

l.25: what's the point of (n=4) here?

l.32: what do you mean by "gallery-diffusor model could be *evaluated*"?

l.35: I am quite confused about terminology used with respect to functional group vs. behavior, or reworking mode vs. behavior.

p.5

l.29: unclear formulation: for each column? or for two luminophore particles?

l.31: incorrect use of "summarized". perhaps "summed"?

l.35: No results of this evaluation shown. Hard to get a feeling for it, and numbers don't tell much.

p.7

l.15-25: The results are presented in a very inacurate way, making it hard to decide

Printer-friendly version

Discussion paper

how to understand them. Check out the following words that are scattered throughout the paragraph and be more specific about their meaning: occasionally, a significant downward transport, sporadically observed, suggested, specific sampling occasions, mainly observed, clear tendency of similar patterns.

l.26: why do you exclude G. alba and B.lyrifera from this list? Their values are also quite large, and, looking at the STD, probablz not significantlz different from the other three mentioned species.

l.34: what do you mean by indications?

Table 1: since the values do not add up to 100%, I wonder where the rest of the luminophores goes. What about the wall effect, i.e., the transport of luminophores that are transported away from the wall and are thus invisible from the side?

p.8

l.1: Unclear what the "transport rate" shown in Fig. 3 really means. Also the meaning of the unit is unclear.

l.4: Unclear what you really show in Fig. 4. According to methods, 2D redistribution reflects Mt-M0, which is a matrix. But all I see in Fig. 4 points. It is unclear what these points have to do with the 2D character of this metric.

l.5-7: Also these sentences do not make things much clearer.

l.8: I see this only for Glycera and Scalibregma, which is not "in general".

l.15: Why did this occur?

l.21: So this is the measure of bulk sediment transport? Please clarify how/why.

p.10

l.5: Could you not look at this aspect (frequency distributions) in your data? Or is this where the Db and r come from? Not clear how you got them.

l.17-19: unclear what the difference is between diffusive-like mixing mode and diffusive-like transport over long time scales.

p.13

l.21-24: Nice, but you still made an a priori choice of a model when you were choosing the variables derived from your raw data.

p.21

Table 1: Unclear meaning of terminology. What does it mean "Feeding mode = Sub-surface deposit"? And if you say "variable mobility", variable between what? Or "semi-mobile" - what exactly is that?

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-411, 2016.