Review of the manuscript bg-2019-199

The manuscript bg-2019-199 entitled "**Rapid environmental responses to climate-induced hydrographic changes in the Baltic Sea entrance**" of the authors Laurie M. Charrieau et al., investigates the recent evolution (200 years) of benthic system in the Oresun (Baltic Sea). It consists in a multidisciplinary study based on benthic foraminifera and sedimentological parameters, supported by atmospheric long time series. Along the sedimentary record they identified five different foraminiferal zones, associated to environmental changes. Globally they attributed these environmental changes to changing in the current velocities and to anthropogenic-induced eutrophication.

Overall, the manuscript is well written. It shows nicely the use of foraminiferal-based proxy to reconstruct in detail modern environments. Although the initial part could be shortened, it well introduces the problematics of the paper. The results are consistent with the applied methodologies. However, some concerns can arise relatively to the statistical interpretation and some parts of the discussions that result to be inconsistent.

In light of this, I suggest the present manuscript as suitable for publication in Biogeosciences only after minor revisions.

Hereafter my comments and suggestions.

General comments

The first part of the MS (Introduction and Study area) is well written. However, the information relative to the figures 2 and 3 result to be redundant for the paper. Although the data presented in both figures might represent a good background of the study area, they are not discussed in the article. I may suggest shortening this part and put the figures 2 and 3 as supplementary materials.

A concern reading the paper is how the authors have identified the 5 different foraminiferal zones along the sedimentary record. The frame of the discussion is based on that. As mentioned in materials and methods, they used a constrained Cluster Analysis (CA) using the Morisita's index. The relative dendrogram based on the arithmetic average (with the UPGMA) seems to be consist, however the final attribution into three foraminiferal zones separated in 5 subzones is totally no sense. The choice of the final clusters can be made by two ways: 1) "expert judgement" and 2) statistical significance. I believe in this case the Authors' choice was based on the first one (if it is not the case the Authors should explain that). What we can clearly see from the dendrogram is that there are two main clusters: Cluster 1 including samples from 36 to 4 and Cluster 2 including samples from 1 to 2. Secondary Cluster 1 can be divided in two subclusters: 1a from 36 to 18 and 1b from 17 to 4. Consequently the interpretation of the CA (made by the Authors) is not consistent. I suggested to revise this part proposing a different interpretation of the CA or another alternative statistical analysis. In any case the discussion should be rearranged accordingly.

Normally, the results of abiotic parameters are shown before the biotic parameters because faunal distributions are dependent (or not) on them. In this study is not the case. I suggest to describe before environmental parameters and then the foraminiferal assemblages.

The discussions are sometimes not persuasive. I personally respected the fact that the discussions are very detailed and sharp but sometime the data do not support your statements. In some cases these statements are contradictory. I found that some considerations are too speculative, especially concerning the human interactions. I suggest to reconsider some of them. Look into specific comments.

Specific comments

Line 11: Replace "foraminiferal" with "foraminifera".

Line 23: The largest changes occurred.. in? from? 1950

Line 25: The authors may think to replace Elphidium group to Elphididae.

Line 26: Replace "more sandy" to "sandier".

Line 28: I am not sure in the abstract acronyms or abbreviations are accepted. Please check it.

Line 31-33: I suggest to rephrase this sentence or split in twice.

Line 32: get rid "species" and keep only "foraminiferal assemblage".

Line 43-45: "The region is…Baltic Sea". Please add a reference for this statement.

Line 70: I think you can add more recent references than Sen Gupta 1999.

Line 77: Get rid "analysis".

Line 76-79: The Authors may think to slightly rephrase adding "The objective of this study…"

Line 84: I think the "-" between 1948 and 2013 is too long.

Line 90: Replace ";" with ".".

Line 109: Replace "In" with "At".

Line124: Please specify in the brackets what is CTD.

Line 126-129: "The CTD…bottom water". I do not see the interest of using these data, already published. In any case this part has to be moved to the section study area or results.

Line 150: I suggest to get rid this sentence because you did not strictly follow Murray 2006 (very general work). In addition you explain just after the sample preparation.

Line 153-154: Why 100-500 μm? In benthic foraminiferal studies the common fraction is >63 μm. Is there any specific reasons? Can the Authors justify that?

Line 154-156: I think you should specify how many samples you finally have (22 right?).

Line 156: Why 300 specimens? Is there any reasons of that?

Line 180-181: This is not clear. From this sentence the readers understand that you are dealing (in the first two centimetres) with living fauna. This is not the case. Please get rid this sentence or correct it.

Line 187-188: I think you should mention here that you calculated the Shannon Diversity as well.

Line 190: You should detail the formula of the Morisita's index as you did for the FAR. In benthic foraminiferal studies this index is not so common.

Line 221: You should add the meaning of NOA.

Line 240-241: Add the percentage of porcelaneous (x), hyalin (x) and agglutinated (x).

Line 243-246: For this part please refer to the comments aforementioned.

Line 347-348: I disagree with this sentence. Low foraminiferal diversity can be due to many reasons and not only salinity. Although in brackish environments (and generally in transitional environments) foraminiferal density is low, this is linked to many factors. Amongst these, for instance the fact that these environments are naturally stressed (rapid changing of physical-chemical parameters) is a major one.

Line 352: Please add the unit for salinity.

Line 352: As far as I know salinity in brackish water is 0.5-30 ‰. If it is so, the fact that you found typical brackish species (tolerant to low salinity) in this interval is definitely in the contrast with this sentence. Please clarify it. I agree with you about low oxygen conditions but affirming that "salinity was about 30" is not so persuasive.

Line 359: Again here saying that low diversity is "usually" link to salinity, needs a better explanation.

Line 360: "However… least 32". Please can you make a reference for this statement?

Line 362-374: I do not see from your data how you can have evidence of pollution in this interval. The only evidence you have is that "TOC was high in this interval but not higher than in the previous zone". So how can you speculate so? Based on foraminiferal diversity and abundance? I think you should have more evidence than that. In the previous interval diversities are even lower. This part is not persuasive at all.

Line 370: I think you can use a better and more recent reference than this.

Line 372-374: From Table 2 *R. subfusiformis* is tolerant to environmental variations not to various kind of pollution. Then what does it means? A lot of foraminifera are tolerant to environmental variations. This is a very general statement and do not support this part of the discussion.

Line 390-393: Sorry but I do not see how you can state this. How has the increase of organic matter been beneficial for foraminifera? All foraminiferal species had a shutdown after 1980. Could the authors explain that or modify this statement?

Line 416-418. This is totally in contrast with the statement in 390-393. You said that there was an increase of nutrients loadings after 1980 and now you state that in the same period measures were taken to reduce nutrients discharges. I think you must clarify all this part concern human impact. Not clear at all.

430: "since after" Since or after?

436: Why open ocean salinity? Elphidium group includes typical brackish species (line 350). This in the contrast with this last statement. Please clarify.

<u>Figures</u>

Figure 1: The contours have a low definition. It is possible to have higher quality picture?

Figure 2-3: Look the aforementioned comments. In addition data from figure 2 have been published before by Laurie M. Charrieau et al. 2018. Figure 3 is not totally clear; it shows seasonal variations of several parameters based on uncertain measurements from 1956. It does not show any variation along the fossil record. I am sorry but I do not see how it can support the study.

Figure 5: Is useful to show both relative and absolute abundance?

Figure 6 The subcluster are not marked. FOR-B and FOR-C are subclusters.

Figure 8-9. Why now did you invert the order of the axes? I think I could be easier for the readers to keep always the same orientation.

Maybe the authors may think to add a synthetic picture with the main parameters used for the reconstruction. I think this could help the readers to better follow the discussions and the final conclusions.

List of references was not checked for completeness.