

Review of the ms “Organic carbon rich sediments: benthic foraminifera as bioindicators of depositional environments” by Elena Lo Giudice Cappelli et al.

The review is based on the version of the manuscript received in April 2019.

The aim of the present study is “To investigate the relationships between sedimentary OC in six west Shetland voes and the associated changes in benthic foraminiferal assemblages...” in order “...to: 1) Fingerprint the source (terrestrial vs. marine) and quality (refractory vs. labile) of organic matter and the form (organic vs. inorganic) of sedimentary carbon. 2) Establish benthic foraminiferal biogeography in Shetland’s voes from recent surficial sediments. 3) Investigate the use of benthic foraminifera as bio-indicators of OC content in coastal sediments and their potential for palaeo-OC reconstruction purposes”. This is a very topical theme, an interesting approach, and the manuscript should be of interest to the readers of Biogeosciences.

The manuscript is generally well organized and well written, all figures and tables are necessary, and adequate literature is cited. However, my concern is the weakly described quantitative relationship between the foraminiferal assemblages and the associated geochemical parameters. This relationship is supposed to serve as the baseline for using foraminifera as indicators for OC enrichment (see aims) and, hence, ought to be more clearly addressed. Methodological weaknesses (see examples below) which potentially affect the relationships/correlations should be identified and discussed.

Page 1, lines 13-14: “..... evaluate the use of modern benthic foraminifera as bio-indicators of carbon content in six voes (fjords) on the west coast of Shetland.” I guess the authors do not mean any kind of carbon? Please specify. The same applies other places in the manuscript.

Page 1, lines 14-16: “Benthic foraminifera are sensitive.....” Please make it clear to the reader if these statements are based on previous studies or results of the present study.

Page 3, line 20: “...sub-sampling the top layer of each grab,...” What was the thickness of the “top layer”? How do the authors know if the sampled top layers in the grabs were intact and had not lost some of the fines from the sediment-water interface, i.e. that the samples were comparable? Since no replicates were collected, how do the authors know how representative the OC and OM data were for each site?

Page 3, lines 22-23: “...foraminiferal counts are ‘total’ (live + dead) because the sampling technique may lead to underrepresentation of ‘live’ foraminifera.” This needs some explanation.

Page 3, lines 26-27: “An earlier field survey of Shetland voes carried out in August 2009 measured bottom water temperature (BWT), salinity (BWS) and oxygen (O₂) at the same locations as this study (Fig. 2)”. If this implies that the present foraminiferal data collected in 2015 were only compared with

hydrographic data from 2009, it should be addressed in the discussion; particularly the statements postulating “low” or “poor” oxygen concentrations in Olna Firth, should be modified throughout the ms.

Page 5, lines 14-17: “Both size fractions were analysed. Depending on sample volume, we subdivided each sample into a number of splits using a standard splitter and, when possible, picked at least 300 specimens ...”. The samples were dry-sieved and dry-split? Please clarify. It is not clear why the samples were sieved into two size fractions? How did the authors ensure that the proportion between the two size fractions was the same in the counted splits as in the original sample? This is essential and needs to be explained.

Page 5, line 21: total assemblages (live + dead) were analysed. Please explain how you distinguished *in situ* tests from tests transported into the sites. This is particularly relevant in the more high energetic environments and deserves some comments.

Page 5, lines 23-26: “Ten taxa” This belongs to results.

Page 6, lines 3-5: = results.

Page 6, lines 11-12: “...despite having very different geomorphologies (unrestricted vs. restricted) and circulation patterns (high vs. low energy) (Fig. 3).” This belongs to discussion.

Page 7, line 3: “...at sites closed to land ...” close to land

Page 7, section 3.1.4: Most of this belongs to discussion. How meaningful is the average stable isotope values of the different lochs? Would you not expect that the average values depend on how many samples are collected and analysed from different parts of the land-sea transect?

Page 8, line 7: “In Vaila Sound, an unrestricted geomorphology (Fig. 1), ...” It is not obvious, based on Fig. 1, that Vaila Sound has an unrestricted geomorphology; please explain, and perhaps modify Fig. 1.

Page 8: Section 4.1 may be shortened, particularly since the data are not used in the further discussion.

Page 9, lines 24-32: These are results.

Page 10, lines 2 and 26: I cannot find the Supplementary Fig. 1 and Fig. 2.

Page 10, lines 4-5: “In general, foraminiferal assemblages do reflect the geomorphology of the six voes (restricted vs. unrestricted basins) and the seaward gradient in OM and OC distribution (Figs. 4 and 5).” The links between the foraminiferal assemblages and the distribution of OM and OC are neither easily seen from Figs 4 and 5, nor from the descriptions in the following sections. If the authors can show that the statement above actually holds, they should provide some clearer justifications.

Page 13, Conclusions, lines 28 and 32: The usefulness of benthic foraminifera as bio-indicators for OC is mentioned in the abstract, in the aims of the study, and in the conclusions but it is not addressed in the discussion. Hence, the importance of foraminifera as bio-indicators for OC in the present study should either be tuned down, or it should be thoroughly addressed in the discussion with concrete, quantitative, examples illustrating how they can be used.

Page 17, Fig. 2 caption. Please add that the CTD-data are from August 2009, whereas the sediment samples for the present study were collected in August 2015.

To summarize, this is a generally well written manuscript on a timely topic which should be of interest to the readers of Biogeosciences. The figures and tables are all needed and well-presented but some should be adjusted to show the postulated relationships between the benthic foraminiferal assemblages and the associated geochemical data. If possible, it would be helpful for the reader if Fig. 1 is modified so it indicates the difference between unrestricted and restricted geomorphologies of the voes.

I recommend publication of this manuscript following minor revision.