

Interactive comment on “Environmental parameters of shallow water habitats in the SW Baltic Sea” by Markus Franz et al.

Markus Franz et al.

mfranz@geomar.de

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We would like to thank the referee and the handling topical editor for their constructive and useful comments. Below you will find the detailed replies to all comments and in the supplement the revised version of the manuscript with tracked changes (Manuscript_revised.pdf).

Best regards

Markus Franz on behalf of the authors

Response to referee comment 1

General comments

C1

A) In my opinion, the novelty of the study applies, at the most, to the southern Baltic. At least regarding the northern parts of the Baltic (Sweden, Finland, Estonia), corresponding data from shallow, coastal environments can be found in a large number of reports and publications. Albeit those data are often considered trivial and have thus not been highlighted and summarized as systematically as in the manuscript. In any case, the authors should know that these kind of data do exist, use them for comparison and refer to the sources accordingly. Instead of a case study, there would rather be need for a systematic review of data from shallow, coastal areas of the Baltic.

Reply: We fully agree that our study applies mainly to the southwestern Baltic Sea, which is why we tried to focus the manuscript on that area (e.g. title of the manuscript). It was not the intention to give conclusions in a context considering the entire Baltic Sea, but rather give local examples of strong environmental variabilities in shallow waters. This is also the reason why we compare our data only at a local scale. The fact that similar data from other regions of the Baltic exists, but were not found by the authors (as data might be hidden in grey literature or local databases) stresses even more the need for systematic publications of monitoring data. A systematic review of data from shallow, coastal areas of the entire Baltic Sea lies outside the scope of this manuscript.

Changes in the manuscript: A sentence was added to the introduction (page 1, line 28–29; page 2, line 1) addressing the accessibility of environmental data from monitoring programs.

B) Although the manuscript is supposed to be of descriptive nature, a proper synthesis of the findings is lacking. This would be especially important in a study without a specific theoretical framework or hypothesis.

Reply: Please see reply to point C.

C) One of the main points of the study is to highlight the fact that shallow, coastal environments are highly dynamic. However, this is hardly surprising. At least I would not

C2

expect anything else than high diurnal and seasonal variation in most environmental variables from shallow, brackish waters located between the 54th and 55th latitudes. While the authors highlight the dynamism of the surveyed environmental type, they don't summarize its (diurnal and seasonal) dynamics in any kind of way. A spatial comparison of the dynamics and potential phenological differences among the stations would be a major improvement in the manuscript. This would be possible with the existing data.

Reply: The manuscript provides now a spatial comparison of the overall dynamics in temperature, salinity and dissolved oxygen concentration. The stations at the extremes of the spatial coverage (Stations 2 and 13) were chosen as examples for spatial and temporal differences, in order to keep the amount of plots acceptable.

Changes in the manuscript: A sentence was added to the data processing section, describing that data were plotted for two stations (page 7, line 5–6). A new paragraph was added to the data overview section (page 12, line 16–22; page 13, line 1–3) as well as to the conclusions section (page 16, line 6–19) to describe the results and give a synthesis of the findings. Figure 7 was added to depict the recorded data of Stations 2 and 13 (page 14).

D) In order to highlight the dynamic nature of the studied environments, they should be compared with something. First, the authors should refer to corresponding data from other shallow environments (see point A). More importantly, the set-up of the study should have included deeper waters further off the shore. Preferably, the study set-up should have incorporated coastal gradients (inshore-offshore) by each station. Although it is understandable that this kind of an approach would have required completely different resources, highlighting the dynamism of the studied environment without actually comparing it with anything is, in my opinion, a major issue.

Reply: As the reviewer is already indicating, extending the setup at each station to include a depth gradient would exceed the financial and logistic capacities of this monitor-

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ing project. Furthermore, expanding the interpretation of the data further than already done (but consider the new version of the manuscript), would be out of the scope of the targeted publication. The overall aim is to give a detailed description of the dataset, allowing the reader an easy access and further use of the data. Additionally, we do not know of monitoring stations in the study area where temperature, salinity and dissolved oxygen concentration are all together recorded at a 10 min interval. Since the variability of the data is strongly related to the measurement interval, we did not compare our data to measurements taken on a daily or even monthly basis. Only for the data of dissolved inorganic nutrients, such a comparison was possible (see page 16, line 24–26).

Specific comments

Page 2, rows 12-15: Benthic communities will face not only low or high extremes but high variation in environmental conditions. In shallow areas, both sorts of extremes are very common.

Reply: The chosen formulation was a bit ambiguous. We were referring to the special case of coastal upwelling, not which conditions organisms are facing in general. The strong variability of environmental conditions in shallow water areas is mentioned in the following sentence.

Changes in the manuscript: The mentioned sentence was edited (page 2, line 17-18).

Page 2, rows 16-18: At least for the northern Baltic, there is already plenty of reports and scientific publications where this type of data can be found. However, those data may be considered too trivial to be summarized in their own right. In other words, the systematic summary presented in this manuscript can be considered useful (but not without references to and comparisons with data from other shallow, coastal areas of the Baltic).

Reply: Please see reply to point A and D.

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Page 2, rows 23-25: As the purpose of the study is to show that shallow, nearshore habitats are particularly dynamic compared with deeper, off-shore habitats, depth gradients should absolutely be included in the sampling set-up (see also general comments).

Reply: Please see reply to point D.

Page 4, rows 4-9: Wading to the sampling stations can have had large effects on many of the measured variables because of sediment resuspension. The authors should be able to show that such effects did not take place. In my opinion, wading should absolutely be avoided, preferring other ways of approaching the stations.

Reply: The water samples have been taken from a water depth of 1 m. Approaching the stations in chest waders seems to us the least invasive method. Using a boat, besides practical and logistical issues, would clearly resuspend more sediment by its outboard motor compared to a person in waders. It is clear that we cannot exclude any effect by wading to the stations, but we think water movement by waves should keep this effect marginal.

Page 4, row 16: In nearshore environments such as those sampled, high amounts of organic carbon and even copper may be present. Such compounds can influence results from the cadmium reduction method. In practice, high levels of carbon and copper can lead to underestimates of total nitrogen. It would be good if the authors could show that carbon and copper levels were low enough not to influence the analysis.

Reply: The interference of copper and organic carbon to the cadmium reduction method are well described in the literature and therefore taken into account by the applied standard procedures, cited in table 2. Consequently, no measurements for copper and organic carbon concentrations were performed.

Page 5, rows 14-15: The cleaning intervals seem unnecessarily long. During the productive season, considerable fouling can happen in a matter of a few weeks. The

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authors should explain in greater detail how they made sure sensor drift (caused by fouling) did not affect the recorded data.

Reply: Even under increased cleaning effort in summer and autumn, fouling could not be avoided completely. Therefore quality control procedures and in addition the visual inspection of the data were applied to identify measurements resulting from drifting sensors.

Changes in the manuscript: A sentence was added to clarify that fouling was an issue in some cases and data was therefore flagged accordingly (page 5, line 15–16).

Page 6, rows 25-26: See comment for rows 14-15 on page 5.

Reply: See reply to previous comment.

Changes in the manuscript: A more detailed description was added explaining the procedure of visual data inspection (page 6, line 24–25; page 7, line 1–2). In addition a sentence was added regarding the selection of threshold values for quality control procedures (page 6, line 22–23). Thresholds were chosen to be higher than the accuracy of the respective logger, to avoid misclassification of data that varied just as a result of the accuracy of the logger.

Page 15, rows 3-4: It can be very much expected that shallow waters located between the 54th and 55th latitudes are very dynamic, on both diurnal and seasonal scales. By contrast, it would be interesting to know more about spatial variation in phenology on both scales. This kind of information disappears when all temporal variation is lumped into boxplots. When temporal variation is summarized like this, there's a high likelihood that the results from the different stations look uniform merely by chance. Besides, statistical inference seems not to have been used to assess whether the stations differed from or were similar with each other in terms of temporal variation. See also general comments.

Reply: An exemplary spatial comparison of the dynamics in temperature, salinity and

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dissolved oxygen concentration was added, but see reply to point C. The statistical analysis of the data would represent a major interpretation of the data, which is not intended for this type of publication.

Please also note the supplement to this comment:

<https://www.earth-syst-sci-data-discuss.net/essd-2018-159/essd-2018-159-AC1-supplement.pdf>

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2018-159>, 2019.