Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-130-RC1, 2019

Biogeosciences Discussions

Interactive comment on "Identifying areas prone to coastal hypoxia – the role of topography" *by* Elina A. Virtanen et al.

Anonymous Referee #1

Received and published: 13 May 2019

General comments

This study presents a GIS analysis of the ability of selected geomorphological metrics to predict the occurrence of seafloor hypoxia in the coastal sea areas of Finland and Stockholm Archipelago in the northern Baltic Sea. The selection of geomorphological metrics is well justified, and the predictive performance of the metrics is evaluated against subsets of national water quality monitoring data from 808 sites. The selected metrics have been described and published elsewhere, but used in this study in a combination that is itself novel. The key results of the analysis are that Depth-Attenuated Wave Exposure (SWM(d)) is the most important metric, and that all the metrics combined predict hypoxia correctly in >80% of the cases. The main conclusion that sluggish water exchange increases the development of hypoxia in patchy archipelago areas is

not particularly new as correctly pointed out by the authors, but this study nevertheless contributes to the understanding of mechanisms driving hypoxia, and the significance of topography in particular. In general, the manuscript is well written and illustrated, and the topic is suitable for Biogeosciences. However, as usual, there is also room for improvement. Overall, I recommend this study for publication after my comments below have been adequately addressed.

This manuscript is focused on topography, while the other known drivers of hypoxia are less considered, including the other physical drivers. The coastal areas studied in the manuscript generally lie above the Baltic Sea halocline, where the seasonal development of thermocline is an important feature with respect to reduced water exchange and hypoxia. The seasonality of hypoxia is mentioned in the manuscript, but it needs to be emphasized more that the implied coastal hypoxia at large is seasonal by nature, and different from the more permanent hypoxia in areas below halocline. The authors may even consider including the word "seasonal" in the title. The authors further need to discuss the potential effects thermocline has on hypoxia in the shallow sea areas. The authors may even consider exploring, whether the typical depth of thermocline could be included in the analysis in order to improve the predictive ability of metrics.

An important conclusion is that half of the monitoring sites in Stockholm Archipelago and one third of sites in southern Finland experienced severe hypoxia. It should be discussed whether this is an artefact resulting from the locations of monitoring sites or a true difference between these two sea areas.

The authors conclude (e.g. page 15) that hypoxia most often occurs in shallow to moderate water depths, in accordance with previous studies. However, looking at Figure 7, hypoxia seems to be developed in deep channels, which in the previous studies have been concluded to be well ventilated. This discrepancy needs to be discussed further.

Terminology used in the manuscript is partly confusing, and the authors may want to seek help from a colleague with background in seafloor geology/sedimentology in

particular. The term “sinkhole” is widely used in the manuscript, although collapse structures are unlikely in the study area with predominantly siliciclastic sediments.

Specific comments

Abstract, line 12. Add “and vertical mixing” between “water circulation” and “that can”.

Abstract, line 17. Replace “sinkholes” by “local depressions”.

Abstract, line 17. Add “seasonal” between “development of” and “hypoxia”.

Page 3, line 21. Add “Sea” between “Baltic” and “coastal”. Here and elsewhere in the manuscript, note that the term Baltic when used alone refers to the Baltic States.

Page 4, line 1. Replace “archipelago” with “islands”. There is no archipelago really in GoB (except Vaasa).

Page 4, lines 3-4. Specify the type of soft sediments in shallow areas. Organic-rich mud?

Page 4, line 4. Replace “rocky” with “hard clay, till and bedrock”. Rocky is an oversimplification.

Page 10, line 10. Replace “Contrary to” with “In contrast in”.

Page 11, line 7, and elsewhere in the manuscript. The use of word “sink” is very confusing in this context and should be replaced by a more correct term.

Page 14, line 6. The number “20 %” probably does not make much sense here, because the more sites one would sample in the Baltic Sea, the higher would be the number of hypoxic sites. It is probably sufficient to state that hypoxia is known to be widespread in the Baltic Sea.

Page 16, line 10. Replace “canyons” with “channels”.

Page 16, line 16, and elsewhere in the manuscript. Replace “ledge” with “tongue”.

Page 16, line 17-19. What would be the contribution of River Neva to the hydrodynamics and hypoxia in the EGoF?

Page 16, lines 22-26. In case the authors insist that the lack coastal hypoxia in GoB is due to the lack of halocline and permanent hypoxia in central deep areas, the driving mechanisms need to be explained. Probably it is safer to just state that there is less hypoxia in GoB coastal areas because of less islands and stronger wave forcing.

Page 16, lines 27-30. If valid, this conclusion needs to be better substantiated. The authors write many times in the manuscript that the deepest parts (channels) in archipelago areas are usually well oxygenated. How does that oxic deep water then transform to shallow water hypoxia?

The Conclusions section as it is currently written is more about the implications of findings than the actual conclusions of the study.

The electronic supplement to this manuscript only has one figure. This figure is quite informative, and the authors may wish to consider including it as a figure in the actual paper.

That ends my referee comments.

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