

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

Anonymous Referee #2

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Thank you for inviting me to review “Identifying areas prone to coastal hypoxia -the role of topography” by Virtanen et al, submitted to Biogeosciences. In general, this is a very well written, well-structured and interesting study where the authors use new approaches to quantify the impact of topography on bottom-water [O₂]. I strongly recommend it to be published in Biogeosciences and I hope the approaches presented will be widely used for also other geographical areas. My major objection is the following statements in the beginning of the manuscripts: “We hypothesized that the enclosed nature of seafloors facilitates hypoxia formation.” “We discovered that topographically sheltered seafloors and sinkholes with stagnant water are prone to the development of hypoxia” It is text book knowledge that topography (i.e. sills, deep basins, restricted morphology, skerries etc.) has a large impact on residence time and water circulation,

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hence also on dissolved [O₂] in the bottom water. I honestly don't think this was new knowledge for the authors and hence the main driver of the study. However, and here is where the study becomes more interesting, to determine the degree to which a restricted setting affects the [O₂] (i.e. the quantification) and then model that effect. That is interesting and new. I would like to see the author rephrasing their aim and their hypothesis.

Minor details: It would help the reader to use abbreviations as sparingly as possible and to remind us what the geographical abbreviations stand for in the beginning of the result/discussion section, and preferable use the names of the regions more in the text. I find it slightly difficult to accept the term normoxic and that is defined as > 4.6 mg/l. What is the "normal"/norm for a deep-water in a coastal setting, should we expect fully oxygenated conditions, should that our reference value? It is important to think about in these type of studies. Dead zones is a popular science word which isn't really accurate, dead zones are devoid of higher life but not of all organisms. It should be used within " , if used at all in this type of publications. The conclusions are very short, general and undersell the study. I would suggest the authors to be more detailed and really highlight the specific conclusions from the study. One of them is: topographically prone areas to deoxygenation represent less than 25 % of the investigated seascapes.

The link to SMHI doesn't work (paragraph 2.2). The references are not consistently formatted.

I can't evaluate the modelling approaches, as that is far from my field, and I hope a second reviewer can do that.

I'm looking forward to see the study published.

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