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Point by Point response to reviewers.

Peterborough, 02/06/2023

Dear Editor,

We thank you and the reviewers for evaluating our manuscript. Here, we have explained the modifications made to our manuscript according to the reviewers' suggestions, and we provide point-by-point responses (in bold) for each comment.
A revised version of the manuscript labelled "Marked Up Manuscript" is attached. In this version, we have marked in yellow and light blue parts of the text that have been changed according to the comments of reviewer #1 and reviewer #2 respectively.

Reviewer 1

The authors have assembled a nice review of high frequency variation in dissolved oxygen (DO) and the potential causes and consequences.

We thank the reviewer for the appreciation in the work.

I suggest that the manuscript would be improved if the focus was on DO variation in just coastal environments rather than including both coastal and open ocean examples. The inclusion of both leads to some confusion; for example, on lines 68-69 the authors state (from references) that short-term DO variation is greater than seasonal. While this may be true for shallow coastal environments it is not universal in continental slope or open ocean waters. Note on lines 77-78 the authors quote a gradual 2% reduction in the ocean (global average) DO with possible further decrease of 7%. The shallow systems they highlight in Figure 1 may have had reductions in DO more than these global averages. The point is with the exception of Figure 3 (which could be eliminated) all of their examples of high frequency variation in DO are from coastal systems and many of these are shallow systems where sedimentary processes can drive water column DO. Thus, if they focus on just coastal systems the paper would not have open ocean examples that do not apply to shallow coastal systems.

We have welcomed the reviewer's comment and focus our contribution only in shallow water productive systems where the magnitude of the oxygen fluctuations is more significant. We remove unnecessary reference to open ocean systems and focus our attention on productive coastal aquatic ecosystems where the variability of oxygen is marked.

line 113: Water residence time and oxygen input through 2-layered gravitational flow will also impact oxygen fluctuations. In general, I suggest that the authors could have included more

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physical mechanisms that support or negate high frequency DO fluctuations. Is DO variation highest in shallow waters that do not have a density stratification? How does water residence time influence DO variation? Will there be more short-term variation in DO if > 1% surface irradiance reaches the bottom? These physical drivers might be used to be able to predict coastal environments that are likely to have large short-term variations in DO.

We thank the reviewer for this stimulating comment and we agree that the framework of modelling oxygen dynamics in shallow water is far more complex than currently available historic datasets allow to build. We address this comment in section 4.1 where we describe the oxygen dynamics in the pelagic compartment of shallow coastal environments and their relative impact in the nutrient daily cycling. We therefore highlight the need for a more precise framework to disentangle the effects of the complex interaction among biotic and abiotic factors on oxygen availability in the water.

Lines 354-360. I suggest that it is not necessary to point out spatial variability for this example, when most all of the phenomenon discussed have inherent spatial scales of variability. The focus of this paper is high frequency temporal variability.

We have welcomed the reviewer's comment and remove the focus on the spatial variability as an intrinsic characteristic of the temporal variability.

One topic to include in the management section is that oxygen criteria (minimum oxygen concentration tolerated by commercially important species) developed by environmental regulatory agencies should include not just "average" DO conditions but also the minimum values measured with high frequency sampling as shown in their Figure 1.

We have welcomed this comment and amended the final section (according also with the indication of the reviewer 2) where we discuss the needs to incorporate the oxygen oscillation at daily scale in the assessment of thresholds for animal conservation and coastal management.

Reviewer 2

I think the authors are potentially on target for an impactful paper, but I think the narrative needs to be better focused and simplified. My impression is that the authors tried to describe all of the relevant time and space scales of oxygen depletion in their 'oxyscape' concept, but many of these time and space scales have been given extensive treatment in the literature already (e.g., seasonal, vertical). The abstract points to the absence of an appreciation for oxygen variability, and while the scale of this variability is not narrowed upfront, there is some indication that high-frequency variation is the most under-analyzed. Three of the five figures describe diel variability, and I agree that this is a time-scale that is under appreciated and could be the focus on a sharp, short review. The paper wanders in and out of this diel-cycle timescale. My recommendation is to limit the scope of this perspective to high-frequency (daily, subdaily)

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variability, its biogeochemical impacts, and its organismal impacts. This focus would make the paper more compelling, and prevent the authors from having to superficially describe seasonal and spatial scale-dynamics that are much better covered in other publications. I think this focus could be supported by more literature around high-frequency cycles, and its impacts on biota.

We thank the reviewer for the guidance and the suggestion that we have implemented and followed.

As examples of where this paper can be shortened, sections 5.1 and 5.2 are too superficial to be helpful in what is essentially a targeted perspective piece. These biogeochemical cycles are well known at the cales described – it would be more effective to focus on the diel-cycling aspects that follow, and focusing the effort there. I think figure 3 is not very helpful for providing new insights into the oxyscape.

We have welcomed the reviewer's suggestion and we removed the section 5.2 while we completely rewrite the section 5.1 (now 4.1 and 4.2) to focus only on the diel scale of oxygen variation on the water column in shallow productive aquatic environments. We therefore change figure 3 (whose data has been incorporated in the supplementary data) that now depicts the daily oscillation of the oxygen and the relative oscillations of the nutrients taking as example the water columns from the Venice lagoon, whose water column is influenced by the photosynthetic activity of a mosaic of primary producers ranging from microphytobenthos biofilm, seagrasses meadows and seaweed prairies. We have also eliminated the seasonal variation by incorporating and making more concise sections 3 and 4 with focus on sub daily oxygen change in productive aquatic ecosystems.

The paragraph starting on line 387: This paragraph seems off target. It seems that the relevant point is that human-built structures could be put in place to support macro-primary producters that would generate oxygen in surface waters, perhaps supersaturating during the day, as opposed to algae that could sink and support deoxyegnation. The discussion also drifts into topics of habitat for macrofauna and blue carbon only superficially.

We did implement the comment by revising the section making it more concise and focusing on the importance on the diel scale in oxygen fluctuations. We remove all the section that discuss the oxygen production and seaweed utilisation to increase the oxygen in the water as our focus is not to discuss or suggest solutions to oxygen loss but to attract the attention on the need of high frequency time scale logging to study the correct fluctuations of the oxygen and its relevance for the aquatic biota.

There is another element of the paper that seems to suggest that the 'oxyscape' is poorly considered in coastal management. This coudln't be further from the truth in North America, where the largest estuarine/marine areas have very targeted management programs for oxygem (Long Island Sound, Gulf of Mexico, Chespaekae Bay). Perhaps I missed somethign here. Consider for example, the text: "While attention has been given to ocean warming and

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acidification, oxygen dynamics have been overlooked in assessing marine habitat or species' sensitivity to climate changes and anthropogenic disturbances."

We indeed amend this section by acknowledging the existent efforts to monitor and manage oxygen in coastal waters, although this effort is circumscribed in a few geographic regions. For example the UK in its Marine Strategy does not have a clear report on deoxygenation (see for example: https://moat.cefas.co.uk/ocean-processes-and-climate). The EU Marine Strategy Framework Directive does not have a clear focus on how to address the problem of oxygen loss, relies on oxygen models and measures from satellites which do not always capture the real oxygen dynamics. The lack of strategies reflects the difficulties to record at the moment the regional fluctuations of oxygen and how to integrate this in a national strategy. Also, the HELCOM Science Agenda from the Baltic Sea, although the great amount of data on oxygen, often fails to align modelled data in shallow coastal waters with oxygen data retrieved by high frequency sensors. We therefore converge our message to the need for a characterisation of oxygen fluctuation at sub daily resolution to be able to inform conservation and management actions.