

Interactive comment on “Seasonal, sub-seasonal and spatial fluctuations in oxygen-depleted bottom waters in an embayment of an eastern boundary upwelling system: St Helena Bay” by G. C. Pitcher and T. A. Probyn

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This paper reads somewhat like a collation of data that was available for another purpose thus it is not an easy read. Some assertions made are not supported by the data provided. Most of the references used should be updated. The controversy on evaluating the environmental data from St Helena bay is not addressed. Since the decay of a bloom is used to justify the episodic hypoxia/anoxia it is strange that there is no link be-

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tween phytoplankton biomass and macronutrient concentrations in surface water since they are the organisms using the macronutrients.

As indicated by the title we set out primarily to evaluate seasonal, sub-seasonal and spatial variability in DO in the bottom waters of St Helena Bay. A grid of stations was sampled specifically to address the spatial variability and a mooring was maintained specifically to address temporal variability. We do not know why the referee should think that the data appear to have been collected for another purpose [and no reasoning is given by the referee]. There is simply no other sampling strategy that could have been applied to address the objectives of our study. More regular sampling of the grid of stations and more than one mooring would have been useful, but limits will always be imposed by the availability of funding. Our data set represents the most comprehensive of its kind for this region.

The referee indicates that “some assertions are made that are not supported by the data”. We do not consider this statement to be true. Unfortunately the referee does not substantiate this claim with any examples.

The referee also indicates that “most of the references used should be updated”. Again the absence of any reasoning or examples makes it difficult to respond to this criticism. Regarding references specific to St Helena Bay we include Hutchings et al (2012), Jarre et al (2015), Lamont et al (2015), Pitcher et al (2014), etc., and do not believe we are missing more recent work of relevance.

The “controversy in evaluating the environmental data of St Helena Bay” served only to motivate this study. We believe, as stated in the conclusion that our findings will allow better interpretation of historical data sets. We will add to our conclusions further details relating to the risk of using temporally or spatially biased data in the assessment of long-term change [as implied by our findings].

Many studies in upwelling systems have shown the “links between phytoplankton biomass and macronutrient concentrations” to be poor. Our study does not investi-

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gate or report on this relationship.

Specific comments:

p. 13286, line 5. The 'red tide' phrase used here is very outdated. I'm not sure if it was used due to the nature of this journal (i.e. not dealing with microalgae), but this term isn't used readily (i.e. since blooms discolor the water differently according to the phytoplankton species present so it is not always red).

We do not believe that the term "red tide" is "very outdated". The term "red tide" is found in the titles of 45 publications in the journal *Harmful Algae* since 2010.

p. 13288, Line 5. Statistical significance of arbitrary samples taken since they were used to 'correct' the CTD?

CTD and WQM oxygen values were not corrected because their values showed good correspondence with the Winkler measurements – within 12% (as specified in section 2.3). We agree to revise text to "Arbitrary surface and bottom water samples were subject to determination of DO content by means of the Winkler method (Carpenter, 1965) to assess the accuracy of the oxygen sensors". This sentence will be included at the beginning of section 2.3.

p. 13288, line 18. DO sensor range missing. The DO sensor range was from 0-120% of surface saturation.

p. 13289, lines 2-4. Which measurement took precedence over which (i.e. which was used to verify which, sensor or winkler measurement)? Was a correction factor applied? Was this 12

No measurement took precedence. No one measurement was found to be consistently higher or lower than the other. For this reason no correction factors were employed.

p. 13297, line 5-10. The statement of NO₃- deficit being indicative of denitrification is not supported by figures 4 B and C (p 13307). As these figures have no means of

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representing which sample point correlates to which to depict the pathway of NO₃- to NO₂-. There are also multiple points that have high NO₃- concentrations and would not show denitrification.

We disagree [and find the second sentence difficult to follow]. Only the data collected off the Berg River [and more specifically the data collected off the Berg River in autumn] – as labelled in Figures 4B and 4C – are indicative of denitrification [i.e. the data points depicting low nitrate concentrations in low oxygen water, Fig 4B; and the data depicting elevated nitrite concentrations in very low oxygen water, Fig 4C]. Yes there are multiple points with high nitrate concentrations indicating that denitrification is limited to the southern part of the bay in autumn.

p. 13298, Line 5-7. As there is no sample or information on the phytoplankton community, this increase in bottom fluorescence could also have been due to *Prochlorococcus* instead (See: Lavin P, González B, Santibáñez JF, Scanlan DJ, Ulloa O (2010) Novel lineages of *Prochlorococcus* thrive within the oxygen minimum of the eastern tropical South Pacific. *Environ Microbiol Rep* 2:728–738.)

We do not consider this likely. The work of Lavin et al. (2010) to which the reviewer refers makes reference to high and low-light adapted ecotypes of *Prochlorococcus* with the latter occurring in OMZs [particularly "the upper lit part of OMZs"]. However, at 50 m depth in St Helena Bay PAR measurements register no light. Furthermore, as indicated by Lavin et al. (2010) "*Prochlorococcus* dominates in oligotrophic waters and is absent in eutrophic coastal waters". Also, high chlorophyll a values (as measured near-bottom in St Helena Bay [>20 mg m⁻³]) have never been attributed to *Prochlorococcus*. Finally the reviewer does not indicate why "blooms of *Prochlorococcus*" would be associated with "events of anoxia" [their presence in OMZs as described by Lavin et al. (2010) is not considered the cause of OMZs].

p. 13304, Figure 1. add a (bouy station signified by the red block) narrative.

Agree to revise figure caption to read "...was moored at station 6 (depicted by red

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block) on the Berg River. . .”.

p. 13306, Figure 3. A notation missing so hard to correlate to the other figures during the same period. The y-axis units of (D) are incorrect.

We are confused as to what is missing. The “notation” presented in the first panel applies to all panels. Agree to revise y-axis units of Fig. 3d to “ml l-1”.

p. 13307, Figure 4. The y-axis units are incorrect (C)

Agree to revise y-axis units of Fig. 4c to “mmol m-3”.

p. 13309, Figure 6 B-D. The primary y-axis (DO) should start at zero. This will help clarify the presence of anoxic bottom waters.

Agree to insert a dashed line to indicate zero on the primary y-axis.

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