

Interactive
Comment

***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**

Anonymous Referee #2

Received and published: 14 October 2015

General Comments

This study seeks to characterise the seasonal and spatial development of dissolved oxygen (DO) in St Helena Bay and identify shorter-term variability related to the decay of intense phytoplankton blooms. Results of nutrients, chlorophyll a (Chl a), temperature and salinity are used to supplement the findings. Using the nutrient and oxygen data, the authors show evidence of denitrification at some of the inshore stations.

In my opinion, the main handicap of this study is an inadequate dataset used for the stated objectives. The authors are trying to describe a seasonal signal, as well as dis-

C6511

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



cern a seasonal signal from short-term intra-seasonal decay events, using data from a single year. The (roughly) bi-monthly sampling regime is inadequate to capture meaningful data on the sub-seasonal events. The records capturing the short-term events are from one CTD device that was moored on the sea floor at a 50 m station for just over a year. That mooring does provide useful insight, however it provides no information on spatial extent of these short-term events (as I would have hoped from the title). The mooring also raises a concern in that it demonstrates the high degree (and magnitude) of short-term variability, which suggests that the bi-monthly 'seasonal' samples are potentially biased/distorted by such variability. Description of a 'typical' seasonal signal would likely require several years of similar data. In summary, having monitored only one year, I would be very careful of describing a 'seasonal' signal. The authors might look into a larger dataset from this well-sampled area to describe a mean seasonal signal.

I do not see the point of reporting the coefficient of variation results (and Table 1). The authors do not seem to refer back to the results reported in the first paragraph of Section 3.1 (nor Table 1) anywhere in the Discussion and it doesn't add any obvious value.

The conclusion is generally weak, with non-specific generalities such as "The observations of this study contribute to a greater understanding of the scales of oxygen depletion in St Helena Bay and will further contribute to interpretation of historical data sets in the future assessment of long-term change."

Although not vital, some discussion around the levels of hypoxia/suboxia that provide stress to local resources or ecosystems, and the implications of the findings in this regard would add some gravity to the 'so what' of the paper.

After reading this paper, I ask myself whether it adds much that is not already covered in Pitcher et al 2014? The only notable difference to me is the addition of nutrient data (and the denitrification findings).

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Lastly, there is no justification of why this study and its results might be of interest to an international/broader audience not involved in research in St Helena Bay. I feel this is especially important when you publish in a non-local journal. Although there may be lessons/insights here for a broader audience, the authors have not made an attempt in highlighting these or expanding on the implications of their findings. The 'so what' of this paper is lacking.

Specific Comments

Methods

Provide GPS coordinates and depths of stations (in supplementary material if necessary) to make study repeatable.

Figs. 2 and 4 display near-bottom concentrations of nutrients and oxygen. Besides defining 'near-bottom' as < 2 m from the sea floor, there is lacking explanation of 1) whether the displayed values are taken from the CTD probes or water samples, 2) how they were isolated and measured/judged to be within 2 m of the sea floor and 3) whether they consist of a single measurement or represent the average of multiple measurements. More detail is needed to allow the replication of this experiment.

pg 13288, line 5-7: How many samples? Perhaps show calibration in supplementary material.

pg 13288, line 14-24: quoted verbatim from Pitcher et al (2014), could refer readers those methods instead?

pg 13289, line 3-4: This should be in results. Also could show plot in supplementary material.

Results

Specify which part of the water column you're referring to – the measures in Fig. 2 (other than phytoplankton) are near-bottom measurements and do not represent/imply

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



the remainder of the water column. This needs to be made/kept clear throughout the text.

Table 1: CV calculated on temperature (degree C) and any other non-ratio-scales is incorrect.

Fig. 3: Seemingly no November DO measurements for the Olifants River transect? If not, why not?

Explain what the grey dots (seemingly categorised as 'other' in upper left panel) are. Any point in including them?

Fig.4: Indicate whether the fits were significant or not. I would also suggest you provide measures of the strength of relationships. Panel D fits look like relatively poor relationships and I wonder whether they are significant or justified to include?

Provide brief explanation/motivation why surface and near-bottom values have separate fits but not in panel A.

Pg 13289, line 12-13 You mention the use of linear interpolation here. This should be detailed in the methods section and it should be made clear what was interpolated and how, to produce the values in Fig. 2.

pg 13291 line 18-20: This sentence is a contradiction. If it were a high number of points, then they shouldn't be seen as anomalous points. They deviate from the (assumed) linear relationship.

pg 13292 line 3-4: This statement should be supported with evidence (correlation coefficients adjusted for autocorrelation if need be):

pg 13293 line 14-15: Why uncharacteristic? Support with data/references or remove.

Discussion

pg 13296 line 17-20: Provide evidence/references for this statement.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



pg 13297 line 2-3: Denitrification signal not that 'clearly evident' to me in Fig. 4. The author's might consider showing it more clearly by way of plotting Redfield ratios.

pg 13297 line 9-10: Rewrite or add to the end of this sentence, something to the effect of 'support the conclusion that denitrification was taking place in these waters'.

pg 13297 line 25-27: I am not convinced by that statement. The authors will have to explain themselves better (e.g. define what they mean by 'sharp'). Firstly, Pitcher et al. (2014; Fig. 9) show some fairly drastic (to my eyes similar) increases and decreases in DO from the same area. Secondly, this study has not shown evidence of isolating the 'gradual' seasonal decline from the abrupt episodic events. The 'gradualness' in Fig. 2 is purely a function of the monthly/bi-monthly sampling design. The continuous measurements from Fig. 6 show hardly any 'gradual' changes, but mostly surprisingly abrupt shifts in DO. If you assume that the variability in DO is similar at other stations as at the one continuously-monitored station (overlay the picture of Fig. 2 on Fig. 6), then it becomes clear to me that occasional (monthly/bi-monthly) sampling routine is inadequate to capture the DO dynamics. Sampling the stations several days earlier or later could impact the resultant 'seasonal' profile quite substantially.

Conclusion

pg 13298 line 24-26: This sentence implies that upwelling results in suboxic waters and denitrification in autumn. However no evidence has been presented as to whether the suboxic waters (and resultant denitrification) are an annual occurrence or an extreme event during the year of the study.

No results/details are provided about how the findings contribute toward the objective (which is mentioned in Abstract and Introduction) "...to facilitate better interpretation of historical data."

Technical Corrections

There are multiple long sentences in the text that require shortening or are missing

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

commas.

Introduction

pg 13287, line1-4: Awkward sentence, simplify/separate into two:

Results

Fig.4: Fix units on panel C and E y-axes (I assume the latter one should be per cubic m).

Panel D & E: Surface & bottom linear fits (or their formulae) have been confused/interchanged.

Fig. 5: Add units to colour scale bars

Fig. 6: panel B x-axis ticks do not align with others

pg 13289, line 11-14: Split awkward sentence into 2.

Discussion

pg 13296 line 22-25: Rewrite clumsy sentence (and/or split into two):

pg 13298 line 2: Incorrect figure reference, should be 6d, not 6a.

pg 13298 line 7-11: Simplify and clarify clumsy sentence.

References

Pitcher, G. C., Probyn, T. A., du Randt, A., Lucas, A. J., Bernard, S., Evers-King, H., Lamont, T., and Hutchings, L.: Dynamics of oxygen depletion in the nearshore of a coastal embayment of the southern Benguela upwelling system, *J. Geophys. Res.-Oceans*, 119, 2183–2200, doi:10.1002/2013JC009443, 2014.

Interactive comment on Biogeosciences Discuss., 12, 13283, 2015.

BGD

12, C6511–C6516, 2015

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

