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***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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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

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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 discern 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.

We disagree with most of the above observations relating to the inadequacy of the data sets. We use a high resolution data set from the mooring to establish temporal variability and a grid of stations [sampled 8 times over the course of a year] to establish spatial variability. Further the two data sets when used in conjunction are complementary in that the grid of stations provides a spatial extension to what is observed at the mooring station, and the mooring data provides a temporal perspective to the grid data.

We are certainly not using “the (roughly) bi-monthly sampling regime to capture sub-seasonal events” or “mooring data to provide information on spatial extent”, as claimed by the referee. We have no hesitation in describing a seasonal trend from our data set [and there are numerous studies that have described seasonality from a single year of data]. Yes it depicts only the seasonality of that particular year – we make no claim of a “typical” or “mean” seasonal signal [which might not exist if the system is

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subject to unequivocal, unidirectional change in response to “climate change”]. We are confident that our data set represents the most comprehensive of its kind for this region in attempting to describe and understand seasonal, sub-seasonal and spatial fluctuations in DO concentrations in St Helena Bay.

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.

Reporting of the “coefficient of variation results” provides a measure of alongshore bay variability [particularly for DO] and also serves for better comparison of variability between the 8 variables. We would prefer to retain Table 1 and make reference to it in the discussion. Reference is made to this data in the abstract.

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.”

We agree that our conclusion could be strengthened. This may be achieved with greater reference to specific findings and the consequent risks of using temporally or spatially biased data in the 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.

Although beyond the scope of our study we may add specifically the likely impacts on rock lobster [based on the additional information provided by our findings].

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

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(and the denitrification findings).

We are able to identify other notable findings [in addition to those presented by the nutrient data and the first indication of denitrification in St Helena Bay]. Possibly the most notable was the observation of sub-seasonal events of anoxia in the bottom waters of St Helena Bay. Pitcher et al (2014) distinguished two forms of oxygen-deficient waters in St Helena Bay: [1] seasonal hypoxia confined to the bottom layer and [2] episodic anoxia through the entire water column in shallow [ $<10\text{m}$ ] nearshore environments [linked to the decay of red tides]. Our present findings further show events of anoxia in bottom waters to be linked to the decay of red tides. These are in fact the first reports of anoxia [defined as  $<0.02\text{ ml l}^{-1}$ ] outside of the nearshore environment of St Helena Bay. Our study also provides the first real attempt to examine the spatial distribution of low oxygen waters within the bay [outside of reports of across-shelf distributions]. We provide the first evidence of lower DO in the southern region of the bay.

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.

We are surprised that the referee does not consider the scales and mechanisms of formation of hypoxic and/or anoxic waters in a major embayment of an eastern boundary upwelling system [EBUS] of international interest. EBUS are considered to be strongly affected by the stressors of ocean acidification and deoxygenation making them hotspots for change. At the very least our results provide the basis for comparison with other embayments of EBUSs from which much can be learnt.

Specific Comments

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## Methods

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

We will include the station coordinates as supplementary material [owing to the high number of stations].

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.

Our measurements are obtained from the CTD DO sensor. We do indicate in the methods section that water samples for Winkler determinations were only arbitrary [for assessing the accuracy of the DO sensor on the CTD]. Our values were derived from a single CTD reading at a depth within 2 m of the seafloor. A weight below the CTD indicated deployment within 2 m of the bottom. We will revise the text to ensure that the reader is aware that the DO data comes from the CTD.

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

We can include the comparison of the CTD and WQM oxygen sensors and the Winkler measurements in the supplementary material.

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

We consider that information relating to the WQM should be immediately available to the reader. Further our description does differ somewhat from Pitcher et al (2014) in that information on the WETLabs fluorometer is included.

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

As indicated above we can include the comparison of the CTD and WQM oxygen sensors and the Winkler measurements in the 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 the remainder of the water column. This needs to be made/kept clear throughout the text.

We have checked all sub-sections of the results to ensure that the origin of the data (near-bottom, surface, through the water column) is clearly indicated. We define surface as 1 m depth and near-bottom as within 2 m of the seafloor (in the methods section).

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

We will replace CV with SD.

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

Correct. Poor weather prevented sampling the Olifants River transect during November.

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

All data are presented in each panel; with the colour dots depicting the values of that particular month [with different colours depicting different transects]. This representation assists in depicting the change specific to that month. The grey dots can be

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removed if necessary.

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.

We will include all the statistics relating to the relationships presented in Fig. 4. Panel D fit indicates a strong relationship – we suspect that the referee is referring to Panel F rather than D. Although the fit presented in F is poor we believe that this is of some interest. More important, is that the figure does indicate that NH<sub>4</sub> concentrations tend to be highest nearshore. We did not include separate fits to the surface and bottom data in Panel A because these relationships would have been poor. We will include a single fit to all data in all panels.

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.

We believe that it is clear that it is the variables presented in Fig. 2 that are determined from linear interpolation. We will include a sentence detailing our approach of linear interpolation to determining a value for each variable along the 50 m depth contour.

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.

Will be revised to read “. . .several anomalous points. . .”.

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

As mentioned the necessary statistics will be added to the relationships presented in

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Fig.4.

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

We use the word “uncharacteristic” because October is typically known as a month of strong upwelling (and there are many references to support this observation). However, our paper does not seek to measure upwelling/downwelling and our observation of “uncharacteristic downwelling” is not central to the point being made. The word “uncharacteristic” could simply be removed. However, we believe that retaining the word assists in orientating the reader in terms of seasons/months of upwelling/downwelling.

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

Reference to Codispoti et al. (2005) will be added.

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.

Our evidence for denitrification is based on [1] a set of data points from bottom samples collected on the Berg River transect [mostly during autumn] that show low NO<sub>3</sub> concentrations in waters with low DO [thereby indicating a nitrate deficit, Fig. 4B]; and [2] the same set of samples that show high concentrations of NO<sub>2</sub> [an intermediate in the denitrification process, Fig. 4C]. These data points are clearly depicted as “outliers”. Plotted by way of Redfield ratios would similarly show these same data points to deviate from the assumed ratio.

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'.

We will revise to read “. . .: hence the elevated NO<sub>2</sub> concentrations off the Berg River in autumn (April) are indicative of denitrification.”

pg 13297 line 25-27: I am not convinced by that statement. The authors will have to

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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.

Pitcher et al (2014) Fig. 9 does show abrupt changes in DO but these observations were from the nearshore (<20 m; i.e. not from the same area as in the present study). The abrupt changes in DO seen in the nearshore are driven by the upwelling/downwelling cycle [also apparent in the temperature data; upwelling introducing cold, DO depleted water into the nearshore region; downwelling introducing warm water with high DO]. Our present observations of temporal changes in DO in St Helena Bay are based primarily on the data obtained from the mooring (Fig. 6) – not the data obtained from the monthly/bi-monthly sampling of the grid of stations (Fig. 2). The grid of stations was sampled primarily to provide insight into spatial variability [although this sampling did in fact reflect the seasonal signal]. The mooring in the present study [at 50 m depth] was not subject to short-term upwelling/downwelling cycles [as indicated by very gradual changes in temperature and salinity – except for the winter mixing]. Rather changes in DO concentrations were driven predominantly by biological processes. Outside of the period of winter-ventilation, DO concentrations tended to be highest in summer and lowest in autumn. But superimposed on this “seasonal” signal is what we have termed “sub-seasonal” fluctuations driven by episodic deposition of organic matter [as indicated by corresponding increases in bottom fluorescence]. We acknowledge that our description of a “gradual” seasonal decline is not accurate [owing

to the strength of the sub-seasonal signal] and undertake to revise this description.

## 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.

We will revise this sentence. Although “concentrations of DO are consistently lower during the upwelling season” we do not wish to imply that this is directly a function of “upwelling”. As indicated the “suboxic environment present in autumn” is primarily a function of biological processes.

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.”

As mentioned 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].

## Technical Corrections

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

See corrections below.

## Introduction

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

Could be revised to read: In addition to measurements of DO in relation to various physical properties, the development of hypoxia and/or anoxia was followed in relation

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to changes in phytoplankton biomass. Macronutrients were also measured to assess local biochemical shifts in response to varying DO concentrations.

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

Correct. Both should read “mmol m<sup>-3</sup>”.

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

Correct. As mentioned we will revise with a single fit to both surface and bottom data.

Fig. 5: Add units to colour scale bars

Agree to insert “mg m<sup>-3</sup>” and “ml l<sup>-1</sup>” into figure caption.

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

Correct needs to be changed.

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

Could be revised to read: Alongshore bay characteristics were assessed through comparison of near-bottom variables (except for Chl a) along the 50 m depth contour as determined from linear interpolation (Fig. 2). A measure of relative variability along the contour was provided for each variable through determination of a coefficient of variation (V; Table 1).

Discussion

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

Could be revised to read: Denitrification is a function of available NO<sub>3</sub> as the electron acceptor, but is also inhibited by the presence of oxygen, occurring only during suboxic conditions (i.e. DO concentrations <0.1-0.2 mL L<sup>-1</sup>; Codispoti et al., 2005).

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

Correct should be revised to read “Fig. 6d”.

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

Could be revised to read: The spikes in bottom fluorescence in St Helena Bay are likely to indicate events of phytoplankton mortality and rapid vertical flux of particulate matter to the sediment.

#### 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.

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