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Discussions

Interactive comment on "Drivers of summer oxygen depletion in the central North Sea" *by* B. Y. Queste et al.

Anonymous Referee #3

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The manuscript presents new and interesting insights into the short-term oxygen dynamics through the deployment of a single Seaglider. The manuscript aims to provide an oxygen budget of the water column in a particular location of the North Sea. The study finds that there is a high rate of oxygen consumption compared with previous work and propose this is linked to 'depocentres' and reminineralisation of organic matter.

The story is portrayed clearly but the quality of the presented results could do with some fine-tuning. I am a little torn about the conclusions drawn by the authors given the very short time series of data – 3 days. More below.

Unfortunately I am not an expert on oxygen dynamics and so most of my comments relate to specific areas of the manuscript and the technical merit of the paper. I really

hope someone with a broader O2 knowledge will be able to review the paper as well since Sections 6 and 7 could not be adequately reviewed by me. I would request that a oxygen expert review this paper to assess its scientific impact and uniqueness (value adding aspect) given the short time series the manuscript is based on – see first major point for more on this.

General/major points:

1. My biggest concern relates to the fact that the time series of observations is very short (3 days). The conclusions made about short-term variability of oxygen depletion in the N Sea are from just this one short time series... ie. a longer time series of high resolution measurements showing reproducibility of such events would be much more convincing to understand their stated impact and importance. In other words, can we better quantify the importance of this observed 3 day time series on the monthly to seasonal to annual O2 dynamics and rates? In addition, I expect that a thorough understanding of the presented oxygen scenario require an idea of what the O2 conditions where like before and after this time series was collected. Defining a new unknown O2 sink from just this time series alone may be taking the dataset too far but I am willing to convinced otherwise?

2. The use of the term 'depocenters' seems a bit loose throughout the manuscript and is also not specific enough – more detail needs to be provided to suggest it is a depocenter and what may be leading to it being clasified as an area subject to depocenters. Depocenters are normally formed when the water column shear stress/bottom shear stress is very low that allows for deposition mostly in dips in the topography. Is the tidal and mixing (advective and vertical) not too high to clasify these as depocenters? I am normally familiar with this term being used in report writing (to generalise areas and remain unspecific) and not in scientific publications, which needs to be much more specific and detailed on the characteristics and features.

3. The presentation of chl-a glider data needs some improvements. Firstly, using only

the manufactures calculations to derive chl-a is by my experience innacurate, especially because regionally specific fluorescence to chl-a ratios exist and should be taken into account. Bottle samples should be collected in situ with the glider profiles in order to filter, acetone and then read on a fluorometer calibrated with pure chl-a standard. These bottle 'gold standard' chl-a values are then regressed against the Seaglider derived ones and corrected appropbrietly. Were bottle samples collected during the SG mission?

The quenching of the chl-a fluorence data should be corrected as best possible when presented in a scientific publication. There are numerous ways to correct for quenching, including using the available backscatter data to correct it – very handy to have that available and should be utilised (see Berenfeld and Boss, 2003; Sackman et al., 2008; Swart et al., 2014).

Lastly, Section 6.2 refers a lot of the chl-a data and DCM but all these references are to Fig 3d. One cannot clearly see the variations of this chl-a with the section alone – esp. when you start referring to diurnal variability and links with the winds enhancing chl-a through nutirent supply... these statements are too definitive given the way the data is currently displayed. In order to really see the variations a 1D time series of integrated chl-a through the water column should be displayed. If this doesn't reveal any variability then the DCM should be somehow isolated and plotted as a 1D time series. Hence, coming back to the quenching – if you provide an integrated time series, then the data has to have the quenching addressed first as this will bias the time series.

4. Wind and tide data: I did not find enough details on the wind and tide data you used, especially for the wind. What is the temporal and spatial resolution of the winds used? How did you co-locate this with the glider locations and what are some of the assumptions?

Also, there is likely some sort of lag between the observed wind and the mixing events... can be 2-18 hours depending on the region, the depth of MLD and extent

of stratification. Perhaps there were no correlations with winds if you have not applied a lag to the mixing events? MLD should be displayed on Figure 3 sections. This should tell you straight away if there may be a link between the wind and mixing. Perhaps a Brunt-Vaisala Frequency section may reveal something? Overall I don't think the authors have done a good enough job at relating winds and mixing with the available data to make the statements they use in the text.

5. Figure use and quality:

Figure 3: some panels could be removed, namely density if you rather plot the density contours on one or more of the other plots instead, which would be useful. Figures will hopefully be bigger since the ones on BGS Discussions are tiny. Add MLD to plots. Maybe I am wrong but I do not think its the norm to display the 650nm scatter data in raw Beta units as this is hard to interpret or compare to other work. The data should be converted to conventional backscatter?

Figure 4 and 5 are hardly used and discussed. Fig 4a-d are not even refered to in the text as far as I could see. They must either be more used in the manuscript or left out or included into other figures where possible. Figure 4 in fact could be discussed more and reveals more about whats hapenning than revealed by the authors in the text. Figure 5 really does not reveal much and can be removed.

It seems Figure 8 was included in a hurry. There are no y-axis labels and units. There is no wind speed vector magnitude indicator. I would recommend representing wind speed here as wind stress to understand its force on the sea surface, as is the norm. I don't believe adding the bathymetry plot (c) reveals anything and should be dropped. These small fluctuations in the bathy (72-75m) in my opinion don't have bearing on anything proposed in the manuscript or does it say something about the presence of depocenters? Fig 8 could do with the same vertical line indicators on it as displayed in Figs 6 and 7 that indicates the higher 'mixing' events so reader can relate time of events.

Specific/minor comments:

- P8695, line 24: The 16min casts is an average. Please provide a range rather as this is very specific.

- There is generally an over use of the semi colon - ; I think rather start a new sentence or restructure the sentence in many cases.

- P8700, line 3: change to 'freshening of 0.3 in the SML'

- P8700, line 22: is '3m' a typo. If not its unclear what you are showing here. Max gradient of pycnocline is over about 10m and found 30m below sfc.

Interactive comment on Biogeosciences Discuss., 12, 8691, 2015.