Reviewer #2

The manuscript presents an analysis of dissolved oxygen data from CTD casts made along a repeat transect in the Celtic Sea during 2014and 2015. The authors construct a simple budget to quantify the processes controlling the summer bottom dissolved oxygen inventory in both years. They extend their analysis of the present-day data to consider the impact of climate change through a 2^{o}C temperature increase on each of the quantified budget terms. They also consider the potential impact of the variability in annual storminess on the bottom dissolved oxygen budget. The findings are a useful contribution to understanding processes influencing ocean deoxygenation in the North West European Shelf.

The authors make good use of relevant literature to parameterise their analysis and to set their work in context of other studies. Whilst the cited literature isn't extensive and the introduction is relatively short, it provides sufficient context for the study and helps to make the manuscript clear to read.

The manuscript is clearly written with a good standard of English. It is well structured, making it easy to follow the analysis of the data and presentation of results. The authors clearly tabulate their results and make good use of suitable figures which are all discussed in the text.

I recommend accepting the paper with minor revisions as described below.

We thank the reviewer for the positive assessment and constructive suggestions. We have addressed the comments below.

1. Line 71: Further information about the DO sensor calibration is needed – the current text is brief, particularly as this is the main data set being analysed.

• Provide details of the oxygen sensors on the CTD rosette. How were the data processed to correct for sensor lag?

• Please provide some further information about the nature of the calibration e.g. number of data points, was a simple offset applied, or a regression? How many data points and over what temperature/salinity range? Were the Winkler DO samples collected on the same CTD casts as the data being presented?

• Is the +/- 3 uM, the uncertainty in the discrete DO samples, or in the calibrated DO sensor data?

• Presumably it is the CTD DO data which are analysed, rather than the Winkler DO data? Make this clear if so.

• Were the samples used to calibrate the DO sensor collected on the same CTD profiles being analysed?

Response: We have expanded the oxygen-method description in **Sect. 2.1, "The Celtic Sea and data availability" (page 3, lines 75–80)**. The revised text identifies the CTD-mounted Sea-Bird SBE 43 sensors and corrected for sensor response and cruise-specific temporal alignment. Calibration samples were collected from Niskin bottles on all CTD casts and analyzed by Winkler titration. It also states that cruise-specific bottle–sensor regressions were applied and that suspect or outlying samples were excluded where identified during cruise-specific quality control. The analytical dataset comprises the processed CTD-sensor profiles, with the Winkler measurements used for calibration. The earlier unsupported blanket ±3 µM uncertainty is clarified as the worst-case uncertainty from the cruise regressions.

2. Line 85: there is mention of the subsurface chlorophyll maximum but there is no mention of how chlorophyll is measured. Is it from a fluorometer? If so, how is this calibrated to derive mg m^{-3} from fluorescence units?

Response: We have added the chlorophyll measurement and calibration details in **Sect. 2.1, "The Celtic Sea and data availability" (page 3, lines 82–86)**. The manuscript now states that CTD chlorophyll fluorescence was measured with CTD-mounted Chelsea Instruments Aquatracka MKIII fluorometers and cruise-specific regressions against Niskin-bottle chlorophyll-a measurements were used to convert fluorescence to chlorophyll-a concentration (mg m^{-3}). We have also included the worst-case uncertainty in the chlorophyll calibrations.

3. Line 86: Whilst the depth over which 'bottom water' is calculated is explained later in the paper, it would be useful to provide the information in the methods section so that the reader has this context early on in the paper.

Response: We have added the definitions in **Sect. 2.2, "DO budget" (page 4, lines 99–101)**. Bottom-water DO concentration is defined as the mean over the lower 40 m of the water column, whereas the inventory used for the budget is integrated from the manually identified base of the seasonal thermocline to the seabed.

4. Line 106: Given the impact of the choice of K_z value on the value calculated for diapycnal mixing and the large impact it has on the resulting calculation of oxygen consumption rate, could the authors consider a sensitivity analysis to quantify this. Given the oxygen budget is relatively simple, some discussion of the uncertainties this may introduce/assumptions made and their impact would be valuable.

Response: We have added a sensitivity analysis using the full range of $K_z = 4 \times 10^{-5}$ to 2×10^{-4} m^2 s^{-1} (stated in **Sect. 2.3, 'Diapycnal DO flux from the SCM' page 6, lines 123–130**) The sensitivity results in **Sect. 3.3.1, 'Vertical mixing' (pages 15–16, lines 308–314).** This range represents the extremes of the likely envelope of mixing values, so we have not

incorporated it into our uncertainty estimates. We also note that our use of the representative value of this range is consistent with in situ measurement of Kz reported by Williams et al., 2022.

5. Figure 5: Is the kriging introducing artefacts due to the length of the gaps between some of the surveys? e.g. the low dissolved oxygen at approximately 25m in late August 2014 which is not coincident with a survey and follows immediately after the high oxygen concentration at the same depth earlier in August.

Response: We used linear interpolation rather than kriging to try to minimize extraneous peaks in the contouring and have checked the CTD data. The issue arises because there are 2 very close CTD profiles in late August. The first has a maximum in DO at about 35 metres and the second has a marked minimum at the same depth. The switch between these is clear (yellow-to-blue in the revised Fig. 5), and then the patch of low DO after the CTDs is caused by the linear interpolation towards the next CTD profile in early November. So, strictly the data support the contouring – it is an issue related to the mix of fine and coarse temporal sampling. We could "fix" this by simply removing one of the CTD profiles, but it would be an arbitrary choice, and arguably not honest. **Fig. 5 in Sect. 3.2 (page 12, lines 243)**

Other minor comments:

Line 45: 'In the semi-enclosed North Sea, bottom-water DO saturation commonly drops to 75–80% of saturation values in summer'. This is the case for stratified waters in the North Sea but monitoring data show that bottom dissolved oxygen concentrations in the well mixed southern North Sea remain at 100%. Please clarify this statement.

Response: We have clarified this statement in **Sect. 1, "Introduction" (page 2, lines 41–44)**. The revised text specifies that the 75–80% summer saturation applies to seasonally stratified North Sea waters, whereas the well-mixed southern North Sea remains close to 100% saturation.

Line 62: Figure 1 isn't currently labelled with a and b but the text refers to Figure 1a

Response: We have corrected the citation in **Sect. 2.1, "The Celtic Sea and data availability" (page 2, lines 60)**, which now refers to Fig. 1 rather than Fig. 1a.

Line 65: 'During the stratified period, typically from spring to late autumn, the BML are separated from surface waters by a thermocline'–'the BML is separated…

Response: We have corrected the subject–verb agreement in **Sect. 2.1 (page 2, lines 63):** "the bottom mixed layer (BML) is separated from surface waters by a thermocline."

Line 81: There is a one sentence paragraph which should be combined with the text in the following paragraph.

Response: We have combined the material into the continuous CTD methods text in **Sect. 2.1 (page 3, lines 75–86)**.

Figure 2:

• Split the legend text into what refers to Fig. 2a and Fig. 2b. Explain what Δz and Δ[O_2] are.
• The oxygen profile plot and the legend text show DO with units of uM but the analysis in the paper uses units of mmol m^{-3}. Change the units on Figure 2 ad the legend to match the text in the paper.

Response: The Fig. 2 caption now describes panels (a) and (b) separately and defines Δz and Δ[O_2] in **Sect. 2.2 (page 5, lines 110–115)**. The caption reports DO in mmol m^{-3}. The units in the figure have been changed.

Line 125: '…using measurements from muddy and sandy sediments, comparable to those at CCS, collected during…'. It would be good to make it clear that these sediment sites are in the same region as your study e.g. '..collected in the area of interest and comparable to those at CCS...'

Response: We have clarified the regional context in **Sect. 2.4, "Benthic DO demand" (page 6, lines 147–148):** "using measurements collected during May (post-spring bloom) and August at muddy and sandy sediment sites in the Celtic Sea, with sediment characteristics comparable to those at CCS. "

Line 138: 'The horizontal diffusive flux, F_{diff} , was calculated using a horizontal eddy diffusivity K_H in the range 20 to 100 $m^2 s^{-1}$'…. Was a single value used, if so what? Or were a range of values used - it is unclear here.

Response: We have clarified that the full range was used. **Sect. 3.3.2, "Horizontal transports" (page 16, lines 325–329):** " using K_H = 20 and 100 $m^2 s^{-1}$, which covers a range consistent with values for sub-mesoscale processes on continental shelves", reports the resulting diffusive-loss range of −2.6 to −10.8 mmol $m^{-2} d^{-1}$ and the mean of −6.7 ± 4.1 mmol $m^{-2} d^{-1}$.

Figure 3:

• The y axes both show the location of CS2 but this is not shown in Figure 1. Amend Figure 1 to show CS2 or remove from the y axis of these plots if not discussed.

• The legend shows DO with unit of mmol m^{-3} but the scale bar units in Fig 3b shows uM - change to mmol m^{-3}.

• I would suggest adding values along the scale bars for both temperature difference and bottom DO so the reader can more easily see what value a colour represents - currently the reader needs to work out the intervals which particularly hinders interpretation of the DO plot given the wide range of colours across the scale.
Response: The figure has been amended as suggested in **Sect. 2.1 (Fig. 1: page 4, lines 88–91)** and **Sect. 3.1 (Fig. 3: page 9, lines 202–204)**. Site CS2 is now shown and labelled in Fig. 1 and retained on the y axes of Fig. 3.
Figure 5: Figure 3 shows a CTD cast at CCS in March 2014 (as well as other sites along the transect). Is this included in Figure 5 plots? I can't see a dotted line on the left of each plot.
Response: Yes, the March 2014 CCS profile is included. We have thinned the axes to try to make the posted data points clearer in **Sect. 3.2 (Fig. 5: page 12, lines 243–245)**.
Line 203: 'In both summers DO concentrations declined…' clarify that bottom DO concentrations declined…
Response: We have clarified the sentence in **Sect. 3.2, "Seasonality in the central Celtic Sea" (page 12, lines 249–250)**: "In both summers bottom water DO concentrations declined over the stratified period …".
Line 219: Did you have a method to automatically detect the base of the thermocline based on temperature or density? I couldn't see it in the methods.
Response: We have clarified the method in **Sect. 2.2, "DO budget" (page 4, lines 97–99)**. The manuscript states that the base of the thermocline was identified manually from each CTD temperature profile as the transition from the strong vertical temperature gradient within the thermocline to relatively homogeneous bottom water.
Line 230: '…sbottom.. '– delete the 's'
Response: We have removed the extra character in **Sect. 3.3, "DO budget in the bottom mixed layer" (page 14, lines 276–280)**.
Line 248: '…background diffusivity) suggests a… ' – delete the ')'
Response: We have removed the extra closing parenthesis in **Sect. 3.3.1, "Vertical mixing" (page 14, lines 297)**.
Line 270: 'Horizontal eddy diffusion was assessed using a simple 1D model with DO concentration at CCS given an initial Gaussian distribution similar to the CTD observations, with a local maximum in DO concentration set to 10 mmol m^{-3} above the background. ' This is method information and I think should appear in section 2.5

Response: This also links to a comment from reviewer 1. The material has been moved to the Methods section and clarified in **Sect. 2.5, 'Horizontal transport and dispersion' (pages 7, lines 168–173)**. The Methods now specify the 10 mmol m^{-3} peak above a 250 mmol m^{-3} background, the 25 km Gaussian width. The K_H range, and how the decline at the Gaussian centre was used in **Sect. 3.3.2, 'Horizontal transports' (page 16, lines 328–329)** now reports the resulting horizontal-transport estimates.

Line 310: 'Thus, the November bottom water DO inventory will be bias low.' Suggest changing this to '…..will be biased to lower values.'

Response: We have adopted the suggested wording in **Sect. 4.1 (page 18, lines 371):** "the November bottom-water DO inventory will be biased to lower values."

Line 311: '…would instead by 34 mmol m^{-2} d^{-1},'– change 'by' to 'be'

Response: We have corrected the typographical error in **Sect. 4.1 (page 18, lines 372)**: 'the slope of Fig. 6c would instead be 34 mmol m^{-2} d^{-1}'.

Line 357: I think the variable Q10 should be explained to the reader.

Response: We have clarified the use of the Q_{10} approach in **Sect. 4.2.3, "Changes in respiration rates" (page 19–20, lines 418–424)**, including providing the equation underlying the approach.

Line 372: What source of wind data are you using for the 1960-2020 analysis? Is it ERA5 reanalysis? In the methods section you describe using this for the 2014 and 2015 analysis, but it is not mentioned in this analysis. Please clarify and also add the source to the legend for Figure 8.

Response: We have clarified in **Sect. 4.2.4, "Variability in wind events" (pages 20–21, lines 448–455)**, that the 1960–2020 analysis uses the same ERA5 reanalysis source and CCS location as the 2014–2015 analysis. The **Fig. 8** caption also identifies ECMWF ERA5 in **Sect. 4.2.4 (page 21, lines 458–460)**, and the ERA5 DOI is provided in the Data availability statement **(page 23, lines 524–526)**.

Line 392: '…Changes in DO because of reduced solubility in a warmer…' – suggest changing 'because of' to 'due to'.

Response: We have made the suggested wording change in **Sect. 5, "Synthesis and conclusions" (page 22, lines 490)**: "Changes in DO due to reduced solubility in a warmer ocean are significant …".