Reviewer #1
The manuscript by Meng et al. presents the results of a two-year sampling campaign along a transect in the Celtic Sea. The authors characterise the oxygen and chloprophyll dynamics, with focus on bottom (i.e. below the subsurface chlorophyll maximum, SCM) oxygen. Results are analysed through a simple 0D model of oxygen sources and sinks that allows the authors to attribute oxygen dynamics to dierent physical and biological processes (solubility, respiration, advection/diusion, etc.). Based on the results of this model, the authors estimate what would happen with a 2◦C warmer Celtic Sea. *While perhaps not groundbreaking, this manuscript represents a useful contribution to the characterisation of biogeochemical dynamics in temperate shelf seas, and in particular the data presented here can be valuable as a reference for studies investigating the North Western European shelf.* *The sampling methodology is clearly outlined, while the DO budget model needs some more rigour in stating its assumptions and limitations, which I outlined below.* *The conclusions are supported by the results, though, model assumptions and limitations must be better highlighted also here.* *This is not a reference-heavy paper (nor it should be, in my opinion), nonetheless, all the claims the authors make are adequately supported. Also consider https://doi.org/10.5194/bg-21-2143-2024 for recent studies addressing bottom water deoxygenation in the North Western European shelf.* *The title is adequate, the abstract does convey the scope of the paper, the overall presentation is clear and so is the language and the maths.* *There are still some issues to address, that I will outline below. Overall I'd recommend to accept this paper for publication, subject to minor revisions.*
We thank the reviewer for the positive assessment and constructive suggestions. We have addressed the comments below.
1. The model is indeed a dynamic model, with differential equations describing the [aeral] rate of DO change over the stratified season. This is quite a long timestep (several months). Rigorously one can integrate over such a long timestep without incurring in huge integration errors only if DO changes linearly over the whole stratified season. This, somewhat surprisingly, seems to be a reasonable assumption gauging from the plots in Figure 6. This should however be stated.
Response: We have now stated this assumption explicitly in **Sect. 2.6, 'Total respiration demand for DO' (page 8, lines 185–186)**: 'The seasonal budget assumes that DO inventory

changes approximately linearly over each fitted stratified-period interval, consistent with the trends shown in Fig. 6.'

2. The model assumes the bottom layer depth, h_b, is constant over the stratified season, which is not the case especially in 2014, as the authors state at lines 221222. Rigorously (and also assuming h_b does not change spatially) there should be an additional term to the DO change formulation: $-[O2]\ (\Delta h_b\ \Delta t)$.
Though applying this term over the whole season may still be incorrect if the rate of h_b change is not linear. The authors should consider the feasibility of implementing this additional term. This may not be straightforward if h_b does not change linearly over the yearly cycle. Alternatively, it should be stated that the term relative to h_b change is neglected and assumed to be negligible compared to the other terms, because h_b changes are, at least in one of the observed years, relatively small.

Response: We considered the additional term associated with changes in bottom-layer thickness (dh_b/d_t). The mooring observations show that bottom-layer thickness changes only weakly through most of the stratified period, until approximately mid-October when convective deepening accelerates. Physically this happens because the net heat flux across the sea surface through much of summer does not change much, caused by the increasing solar radiation being offset by increasing back radiation from the warming sea surface. We have not included this term explicitly in the budget and instead assume it is small relative to the other terms over the period used for the main analysis. The switch to rapid convective deepening of the mixed layer in autumn makes the problem very nonlinear, but by this stage most of the oxygen depletion has taken place. This treatment and its limitation are stated in **Sect. 2.6, 'Total respiration demand for DO' (page 8, lines 185–191)**. We also state in **Sect. 3.2, 'Seasonality in the central Celtic Sea' (pages 13–14, lines 270–273)** that the constant-thickness assumption is violated for the late-November 2014 observation and report the comparison obtained after excluding the November data.

3. in eq. (6), by computing the respiration demand as a difference between the total DO change and the rest of the terms, all the model uncertainties, unaccounted for processes, etc. are transferred to the respiration demand term. This is a limitation of the model, both when computing the DO budget and the expected change in a warmer sea. This limitation should be adequately discussed.

Response: We now state explicitly in **Sect. 2.6, 'Total respiration demand for DO' (page 8, lines 179–183)** and clarify that the residual represents an effective respiration/remineralization demand but also that we have attempted to quantify uncertainties in the budget processes (so R_{O2} uncertainty will incorporate these) and we are confident that we have included the major oxygen terms for the temperate shelf environment. We further discuss the sensitivity of this estimate in **Sect. 3.3.4, 'Residual and overall budget' (page 17, lines 345–348)** and note in **Sect. 4.2.3, 'Changes in respiration rates' (page 19, lines 418–421)** that the warming calculation inherits the uncertainties and unrepresented processes contained in the residual estimate.

4. equations (4) and (5): it is not clear how the authors estimate the vaules of the horizontal gradients $\Delta[O_2]/\Delta y$ and $\Delta^2[O_2]/\Delta y^2$. Please specify.

Response: We have clarified the estimation of both horizontal DO gradients in **Sect. 2.5, 'Horizontal transport and dispersion' (pages 7–8, lines 160–174)**. The first-order gradient, $\Delta[O_2]/\Delta y$, was calculated from observed bottom-water DO concentrations along the CTD transect between CCS and stations toward the shelf edge. The second-order gradient, $\Delta^2[O_2]/\Delta y^2$, was represented by the curvature of a Gaussian cross-shelf DO distribution fitted to resemble the observed DO maximum at CCS and used in a one-dimensional diffusion model.

Other specific comments:

Line 65: Define BML.

Response: We now define BML at its first occurrence as 'bottom mixed layer (BML)' in **Sect. 2.1, 'The Celtic Sea and data availability' (page 2, lines 63)**.

Lines 108 –109: The sentence 'The effects of episodic … are considered separately below' feels useless, as what it refers to is introduced immediately after. It can be safely removed.

Response: We have removed the redundant sentence. The revised text now proceeds directly to the treatment of episodic strong-wind mixing in **Sect. 2.3, 'Diapycnal DO flux from the SCM' (page 6, lines 132–145)**.

Figures 3, 4, 5: In the DO plots, the authors should replace the rainbow/jet colormap with another one that is perceptually uniform and colour-blind friendly. On top of the usual considerations about the rainbow colormap drawbacks (e.g. https://doi.org/10.1038/s41467-020-19160-7), I find having the highest values of DO in red to be somewhat misleading.

Response: We have replaced the palette used for DO in Figs. 3–5 with the cividis colormap; high DO values are now shown in yellow. The revised figures appear in **Sect. 3.1 (Fig. 3: page 9, lines 202–204; Fig. 4: page 11, lines 230–231)** and **Sect. 3.2 (Fig. 5: page 12, lines 243–245)**.

Lines 174–175: The sentence 'For a given areal DO … concentration decrease' expresses a trivial truth. I'm not sure why the authors included it. If this is useful to explain some of the observations, this should be clearly highlighted, else I would remove the sentence. NB this sentence does not seem to qualify the preceding one, that bottom DO increases in the coast–shelf-edge direction, because areal DO loss from bottom water will not be constant along the transect.

Response: We have removed this sentence. The revised discussion of the cross-shelf DO pattern is in **Sect. 3.1, 'Cross-shelf patterns in DO' (pages 9–10, lines 205–229)**.

Line 180: Replace 'DO will not be linked' with 'DO doesn't seem to be linked'.

Response: We have revised the wording in **Sect. 3.1, 'Cross-shelf patterns in DO' (page 10, lines 226)** so that the text now states that the local bottom-water DO maximum 'does not seem to be linked' to the obvious seabed bank.

Line 221: Replace 'regression will be influenced' with 'regression is influenced'.

Response: We have made this correction in **Sect. 3.2, 'Seasonality in the central Celtic Sea' (page 13, lines 267)**: 'the 2014 regression is influenced by the November…'.

Lines 220–225: Here the authors compute the areal rate of DO loss in the two consecutive summers, then correctly note the two values are different because by November 2014 the surface layer had deepened considerably compared to the rest of the stratified season, i.e. the $\Delta h_b/\Delta t$ term of the model is no longer negligible (see comment above about the DO budget model). That the model assumptions are violated here should be clearly stated. It may also be worth checking what happens to the rate of DO decline if the November 2014 data are excluded. Are they comparable to 2015?

Response: We now state explicitly in **Sect. 3.2, 'Seasonality in the central Celtic Sea' (pages 13–14, lines 270–273)** that the rapid change in bottom-layer thickness violates the constant-thickness assumption for the late-November 2014 point. Because the available pre-November 2014 inventory observations are insufficient for a robust alternative seasonal regression. We instead clarify that the 2015 rate is more relevant to the budget because it is not affected by autumnal thinning of the sub-thermocline layer. We have also noted the 2014 depletion rate without the November data (-34 ± 8 mmol m^{-2} d^{-1} (n=6; r^2=0.84)), which is not significantly different from the 2015 rate.

Line 236: Add a proper citation for ERA5.

Response: We have added the Copernicus Climate Change Service ERA5 dataset citation and DOI (https://doi.org/10.24381/cds.adbb2d47) in **Sect. 2.3 (page 6, lines 142)** and in the **Data availability statement (page 23, lines 525–526)**.

Lines 325–327: Changes in primary production patterns are also an important factor, both for direct oxygen production by phytoplankton, and especially for the production of organic matter that contributes to community respiration, hence oxygen consumption. Furthermore, the expected change in DO linked to changes in system productivity is expected to be quite patchy, driving decline as well as increase in DO, with high spatial (both horizontal and vertical) and temporal (seasonal) variability. As for the wind, this also is a

factor, as it influences water-column mixing, but one would need output from an atmospheric climate projection to estimate the sign and magnitude of the expected change.

Response: We propose adding a subsection on changes in primary production at the end of the Discussion (**Sect. 4.2.5, 'Changes in primary production' pages 21–22, lines 465–485**), focused on the uncertainties in predictions of future production and how it might affect dissolved oxygen.

Line 355: I would rename this section 'Changes in respiration rates', as rigorously primary production, which is not accounted for here, is also a metabolic rate.

Response: We have renamed **Sect. 4.2.3 'Changes in respiration rates' (page 19, line 417)**.

Line 421: The fact that the model the authors employ does not account for changes in netPP is not an advantage; it is a limitation.

Response: We propose the **Sect. 4.2.5 'Changes in primary production' (pages 21–22, lines 465–485)** to discuss drivers and implications of changes in primary production. The final paragraph of the paper has been simplified by having the netPP material removed.

Line 423: The sentence 'However, if we are confident in our estimate …' may leave the reader wondering, are you indeed confident in your estimate? Please rephrase.

Response: We suggest removing this statement and rewriting the final paragraph to have a stronger focus on the need to better understand how the physical drivers of primary production in a warmer world might operate. This is related to an additional point, not directly queried by the reviewer but certainly arising from further thinking about the reviewer's comments: faster respiration rates would require more organic fuel to lead to a great reduction in oxygen and there is some doubt there is enough labile material available to support this **(Sect. 4.2.3, 'Changes in respiration rates' page 20, lines 436–440**; **Sect. 5, 'Synthesis and conclusions' (pages 22–23, lines 489–492 and 520–522).** So, solubility-changes in DO are guaranteed in a warmer ocean, but how much changes in respiration rates affect oxygen will depend on the supply of organic material to bottom waters.