Overview:

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 different physical and biological processes (solubility, respiration, advection/diffusion, 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.

Specific comments:

My main criticism is about the DO budget model: The authors present a simple model for the bottom layer DO budget that allow them to estimate the relative contribution of different processes (diffusion from the SCM, benthic demand, horizontal transport and respiration in the water column) to the observed DO change over the stratified season. This model is intended to be as simple as possible, thus makes a number of assumptions that are not always be adequately highlighted. I am not suggesting the authors should make their model more complex, as this would make the interpretation of results less straightforward and would ultimately be out of the scope of this manuscript. Still, all assumptions and limitations should be clearly stated, both when the model is presented and, especially, when results are interpreted.

The ones I see are:

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.

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 221–222. Rigorously (and also assuming h_b does not chanage spatially) there should be an additional term to the DO change formulation:

$$-[O_2]\,\frac{\Delta h_b}{\Delta t}$$

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.

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, unaccouned 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.

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.

Other specific comments:

line 65: Define BML.

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

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.

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.

line 180: replace "DO will not be linked" with "DO doesn't seem to be linked"

line 221: replace "regression will be influenced" with "regression is influenced"

lines 220–225: Here the authors compute the aeral 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. and it may be worth checking what happens to the rate of DO decline if the November 2014 data are excluded. are they comparable to 2015?

line 236: add a proper citation for ERA5.

line 325–327: Chanages in primary production patterns are also an important factor, both for direct oxigen 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 some output from an atmospheric climate projection to estimate the sign and magnitude of the expected change.

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

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

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.