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

Anonymous Referee #1

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Review "Drivers of summer of oxygen depletion in the central North Sea" by B.Y. Queste et al.

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1 Review report

The manuscript "Drivers of summer oxygen depletion in the central North Sea" by B.Y. Queste, L. Fernand, T.D. Jickells, K.J. Heywood and A.J. Hind presents a budget estimate of oxygen in the water column near the Doggers Bank, North Sea, based on observations made by an underwater glider. The authors report, in comparison to what is reported in the literature, a fairly high rate of oxygen consumption, and claim that this is indicative to an unknown oxygen sink, which they propose to be linked to depocentres and rapid remineralisation of resuspended organic matter.

The article is well-written and well-structured, and therefore I found it easy and pleasant to read. Not being an expert on oxygen dynamics, I feel not competent to comment on oxygen specific aspects.

There are, however, some issues that the authors need to resolve before this paper could be considered for publication.

One of the main issues I have is with the mathematical basis of the methodology used. Conventionally, a partial derivative is represented by the symbol ∂ rather than δ, and should be changed throughout the manuscript. The processes considered are represented by an advection-diffusion equation, given by equation (1). The advective terms ($u\partial \mathrm{O}_2/\partial x$ and similar terms) have the wrong sign. Furthermore, a physical system described by a partial differential equation, such as equation (1) also needs boundary conditions. In fact, I believe the authors mixed the boundary conditions erroneously in the equation. The term 'ase' represents air-sea exchange, which is typically a boundary condition. The exchange with the seabed is probably also important and is not mentioned. The biological processes, as long as the are relevant in the water column should appear as a term in equation (1).

After stating equation (1), the authors eliminate the terms that are considered insignificant. The diffusive *terms* should be ignored, not the *coefficients* of diffusivity (line 24/8697 and 1/8698). It is probably more correct to say that the terms are negligible because the gradients are small (well-mixed layers). In fact, the coefficients K_H and K_z are termed eddy diffusivity coefficients, which implies that equation (1) is averaged over turbulent time scales. In this case it is fair to assume that $w = 0$. (It is, by the way, the convention to write velocity vectors as in bold face, but the velocity components are set in regular type.) This leaves the simplification of equation (1), *i.e.* equation (2) wrong. A vertical advective term could play a role, of course, but then it represents a process such as upwelling or downwelling, which, as far as I understand is not meant here. As before, the simplified equation (2) also requires boundary conditions to specify the problem, and also here they are included in the differential equation, in particular the term R_{Benthic}. In fact, I think the flux of oxygen through the thermocline, that the authors describe later, is in fact a diffusive flux and should be described by the term $K_z\partial^2 \mathrm{O}_2/\partial z^2$.

Since the analysis the authors carry out is mainly depth-averaged and restricts itself to the BML, it may be beneficial to remove the mathematical formulae altogether rather

than fixing the mistakes, and instead, present a (graphic) box orientated budget model. Such a model would help the authors and the reader to get a picture of what and where the various fluxes, sources and sinks are defined.

If the authors insist on keeping the mathematical model (in corrected form), then it is required to make a connection between the model description and what the glider observes. In its present form, the observations from the glider are plugged into the model equations. Equation (1) and the derived equations probably assume a Eulerian reference frame, although not explicitly stated. It is questionable whether or not the glider observations can be considered to be taken in an Eulerian frame. Also when a simplified box/budget model is included, it should be explicitly stated how this model relates to the observed parameters.

Section 5 “Supply of oxygenated water”, which is an important part of the analysis did not convince me. The authors conclude in this section that, based on the observation, the vertical mixing (!) is the largest potential source source of oxygen input in the BML. In the authors' terminology, mixing would be represented by turbulent diffusive fluxes, not the advective term. Nevertheless, the vertical diffusive term is reasoned away, whereas the advective term is still present in equations (3) and (4).

Figure 6 shows an increase in AOU with two or three dips. These dips are associated with mixing events where oxygen rich water from the SML is injected into the BML layer. If horizontal advection were to be negligible, how can it be that the temperature in the BML drops? (see line 5/8700). The argument the authors put forward in favour of the vertical mixing hypothesis is the observed temporarily increased stratification in the temperature (Figure 7). Although it may be vertical mixing, I think it could be equally likely that the observed time series is affected by spatial variation. In the end of the day, the glider *does* move with respect to the water column.

I do agree with the authors that, if oxygen rich waters enter the BML, the AOU would go down. However, with everything else being equal, I would expect the AOU to increase

gradually at a rate of 2.8 μmoll/dm^3/day after the mixing episode. The graph in Figure 6 would then take the appearance of a staircase. This clearly does not happen and the AOU increases rapidly until (slightly more than?) pre-dip levels. Is this increase solely due to the remineralisation of organic matter? I am suspicious, however, as in all cases (three if you count the last small dip associated with the mixing event during the early hours of 20 Aug.) the disappearance of the dip coincides with the disappearance of the vertical temperature gradient in the BML. Are they causally related? To maintain the interpretation of vertical mixing, the authors should provide more support for it. In my opinion it is equally likely that the glider crosses a patch of water (eddy?) with different a temperature and oxygen concentration. It may not be easy at all to make this distinction, given this dataset, though.

In this light, even the two-day trend of increasing AOU becomes questionable, but this issue could have been resolved if the trend kept increasing, had the glider flown in the opposite direction too.

I think the fundamental problem here is that this dataset, which shows a time varying oxygen concentration, does not allow to observed changes to be decomposed into a local rate of change and a horizontal varying component. It is a real pity, as this contribution could have provided interesting data on the oxygen consumption in the North Sea, but because of the mingling of time and space dimensions in the observations, the conclusions drawn in this manuscript do not have a strong foundation. Perhaps the authors can provide additional proof for their case by calculating the diffusivity coefficients for temperature and oxygen, as an indication that this process is at the heart of the observations.

2 Minor issues

Reading the manuscript I spotted a number of minor issues:

- Abstract: here terms are used “short-lived” and “small scale”. Is it for the target audience clear what order of magnitude these relative qualifications indicate? Perhaps best to make it explicit.
- line 12 p 8694, suggested → suggest (published knowledge)
- line 25 p 8698 “... would require a month.” Like you do in the discussion I would emphasise here that it is just or not more than a month.
- line 18 p 8699 To observe the supply...
- line 13 p 8701 The direction of the tidal currents is not shown. Perhaps better to speak of currents than velocities.
- line 8 p 8704 Figure 3d shows
- 17 p 8704 If this were the case
- 7 p 8710 Van der Molen... (Also some inconsistencies in the reference list in relationship to Dutch surnames starting with “van”.