Author's reply to the Editor

Dear Valeria Di Biagio et al.,

Thank you for submitting the revised manuscript, which I see has been improved. Your revisions and responses addressed most but not all of the reviewers' concerns. In particular, reviewer 2 still raised a major concern regarding the calculation of the dissolved oxygen budget; further revisions are expected. I am therefore returning the manuscript back to you so that you can make proper changes.
Comments of reviewer #2 are appended below.

Dear Editor,

We thank you for your consideration and feedback on our submitted manuscript.
We have carefully considered all of Reviewer#2's comments, especially those related to the dissolved oxygen budget. In this document, we include our responses in blue and the proposed changes to the text of the manuscript in italicised red; we refer to the number of lines in the new revised manuscript in the pdf tracked-changes version.
In addition, based on some minor suggestions from Reviewer#2, we include a new version of Figure 6, in which we have changed only the title of panels c) and d), and of Figure 1, in which we have deleted one line, and we have changed the order of the figures in the Supplementary Material, according to the order of their citation in the text of the manuscript.

Author's reply to Reviewer#2

Review of "Subsurface oxygen maximum in oligotrophic marine ecosystems: mapping the interaction between physical and biogeochemical processes" by Valeria Di Bagio et al.

General

The authors have addressed my previous comments in their answer and the new version of the manuscript.

My main concern is still about the calculation of the terms of the dissolved oxygen budget, shown in Section 3.2.3 and discussed in the Discussion Section. The authors indicated in their answer that the budget is calculated offline based on monthly model outputs. I suggest the authors further specify the calculation of the dissolved oxygen budget. In addition, I would suggest that in an offline calculation the budget is not fully closed, given that all budget terms are not linear. Besides, I have included additional minor comments on specific sections of the text below.

Overall, this manuscript makes a valuable contribution to the understanding of the dissolved oxygen dynamics, and more generally of the interactions between biological and physical processes, in the Mediterranean Sea and I look forward to seeing the final manuscript published after these final issues are addressed.

We thank Reviewer#2 for the general comments and suggestions on our manuscript.
We add our responses to the individual points below.

In particular, we recognise that further specifications on the dissolved oxygen budget are relevant to the manuscript. We provide them in our response to the main comment below.
In addition, we have carefully reviewed and addressed all other comments reported as minor/technical. In particular, we also include a new version of Figure 6, in which we have changed only the title of panels c) and d), and of Figure 1, in which we have deleted one line, and we have changed the order of the figures in the Supplementary Material to match the order of their citation in the text of the manuscript.
We indicate our responses in blue, and the proposed changes to the text of the manuscript in italicised red. We refer to the number of lines of the new revised manuscript in the pdf tracked-changes version.

Main comment

Section 3.2.3: I understand from the answer of the authors that the assimilation was performed so that the water mass conservation is respected.

We confirm that the data assimilation procedure was performed in such a way that oxygen mass conservation is respected.

Section 2.1 describes how the rate of change of dissolved oxygen is calculated in the reanalysis. But it is still not clear how the budget of dissolved oxygen is calculated. The authors specify in their answer that it is calculated offline based on monthly outputs of the reanalysis. Did the authors use the monthly mean of fluxes (GPP, CR, etc.) or of state variables? For the calculation of the air-sea flux did they use the monthly mean of wind speed, temperature, salinity, etc. or directly the monthly mean of air-sea flux? What about the calculation of the transport terms, are they calculated using the monthly mean velocities and diffusivities? Are all the terms of the budget explicitly calculated or is one of the terms of the budget deduced from the other terms? I suggest the authors specify the calculation of the dissolved oxygen budget in the Materials and methods section or in a Supplementary Material text.
Given that the transport, air-sea flux and biogeochemical terms are not linear terms, I would assume the budget of dissolved oxygen is not fully closed in an offline calculation of the derivatives (especially during dynamic periods). What is the difference between the temporal variation of oxygen inventory and the sum of the derivatives? I would suggest the authors mention at least this potential error, if the value of this difference is not provided.

We thank Reviewer#2 for raising these points and acknowledge that the calculation method for the oxygen budget was not clear enough.

The objective of the analysis in Section 3.2.3 was to compare the physical and biological contributions to the overall temporal evolution of dissolved oxygen at the seasonal scale, focusing on summer SOM. To characterize these two contributions (Fig. 7), we present our results on the monthly time scale, as commonly used (e.g., Gonzàles et a., 2008, Lemee et al., 2012, cited in the manuscript), but calculations are performed with higher frequency data and then averaged to the monthly time scale, as described below.

In the dissolved oxygen budget, the biological part of the derivative was calculated retrospectively using the model variables (i.e., $\mathbf{P}_c^{(j)}$, $\mathbf{Z}_c^{(j)}$, $\mathbf{B}_c$, **A** and $\mathbf{R}_{eq}$, with j=1,2,3,4) and the parameters and functions given in the Supplementary Material (Table ST1 and Equations S1-4, respectively) according to Equation 3.
The physical part of the derivative was instead estimated as the difference between the total derivative of the dissolved oxygen and the biological derivative.

We recognize that any ex post calculation of the oxygen budget is subject to uncertainties, that were left mainly to the physical component. For this reason, our analysis of the physical component (Section 3.2.3) remained at a qualitative level and we used vertical diffusive fluxes and velocities to describe the potential phenomenology associated with transport (Fig. 7).

To better explain this part in the manuscript and also to address Reviewer#2's comment on L 598 below (on page 6 of this document), we have modified lines 404-407 as follows:

Derivative terms are recomputed in retrospect on a monthly basis, starting from the weekly reanalysis output in a specific year, i.e., 2014. In particular, the central column of Fig. 7 displays the biological component of Eq. 1, computed by following Eq. 3, and the right column displays the sum of the other terms in Eq. 1, computed as the difference between the total derivative and the biological derivative.

Also, in the Discussion section, we have changed line 564 as follows:

Moreover, the budget of dissolved oxygen has been computed at the monthly frequency,

Finally, in the Discussion section, we added a sentence to acknowledge that uncertainty are possibly present in our reconstruction by modifying lines 566-568 as follows:

Although the retrospective computation of the budget entails uncertainties, the physics-related processes were clearly dominant in the oxygen dynamics with respect to biological processes (Fig. 7), except for the most biologically active areas (northwestern Mediterranean areas, A and B in Fig. 6).

For the sake of completeness, for both the biological and the total derivative we have computed in sequence:
1) the derivatives in each grid point (x,y,z) in the water column under the 5 selected areas (Fig. 6), in 2014 and at the weekly frequency, since data assimilation in the model was performed at this time scale and model outputs can be considered robust at this time frequency (i.e., possible shifts due to data assimilation are filtered out);
2) the monthly mean values of these derivatives;
3) the spatial horizontal average of the monthly means of the derivatives for each vertical level, in each of the 5 areas (x,y).

However, we have not thought to include this level of detail in the new revised manuscript, unless Reviewer#2 suggests it.

Minor/technical comments

L12: “validation associated with community production and respiration”: Considering the discussion L 701-729, “comparisons with estimates from in situ observations” appears to me more appropriate.

We agree. We have changed this expression as indicated by Reviewer#2 in the new revised manuscript.

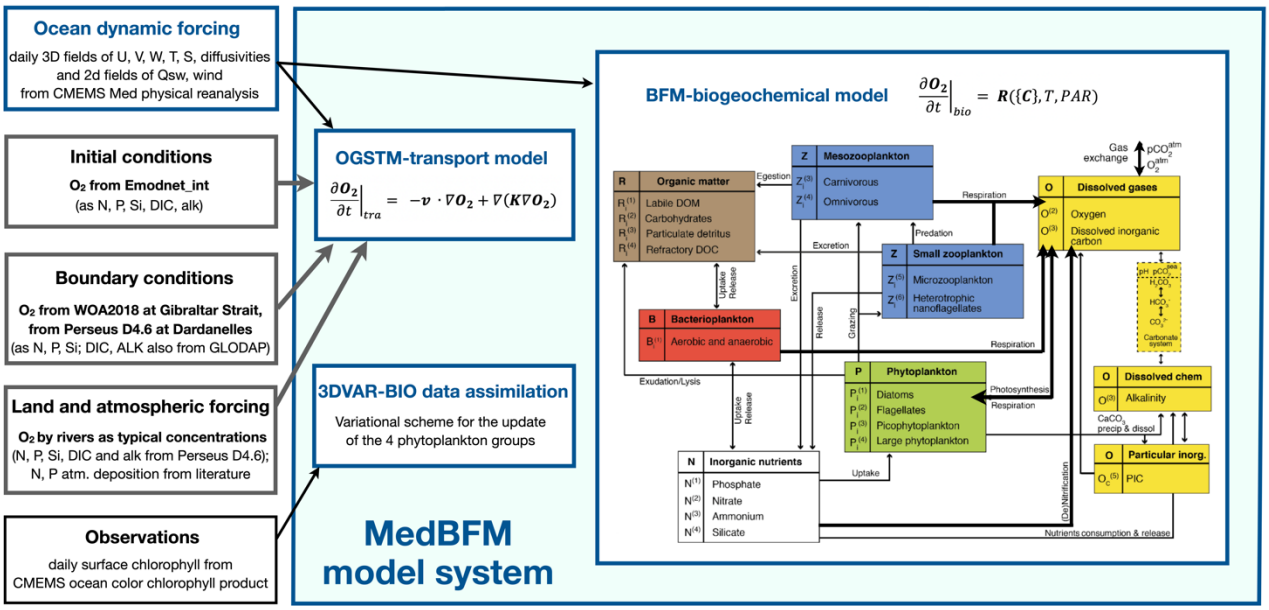
L 76 and reference section: Please take into account the recent update of the reference for Reale et al.

We thank Reviewer#2 for this indication. We have updated the reference in the text and bibliography of the new revised manuscript.

Figure 1: “i=C,N,P,S” : I suggest the authors change “S” to “Si”. Shouldn’t Chl be added for i associated with phytoplankton? Is i=C,N,P for bacteria and zooplankton?

We thank Reviewer#2 for the suggestion. We recognise the index “i” should be better specified. We have deleted the phrase “i=C,N,P,S” in Figure 1 and added this sentence in the caption (lines 133-134):

In the figure, i=C,N,P, Si, Chl is for phytoplankton and i=C,N,P for bacteria, zooplankton and organic matter (but i=C,N,P, Si is for $R^{(4)}$).



(New version of Figure 1)

L 230-231: Are the Alboran and Aegean sub-basins already aggregated with other sub-basins? If

not, I suggest the authors add the metrics estimates for those sub-basins in Figure 4 and Tables 1 and 3.

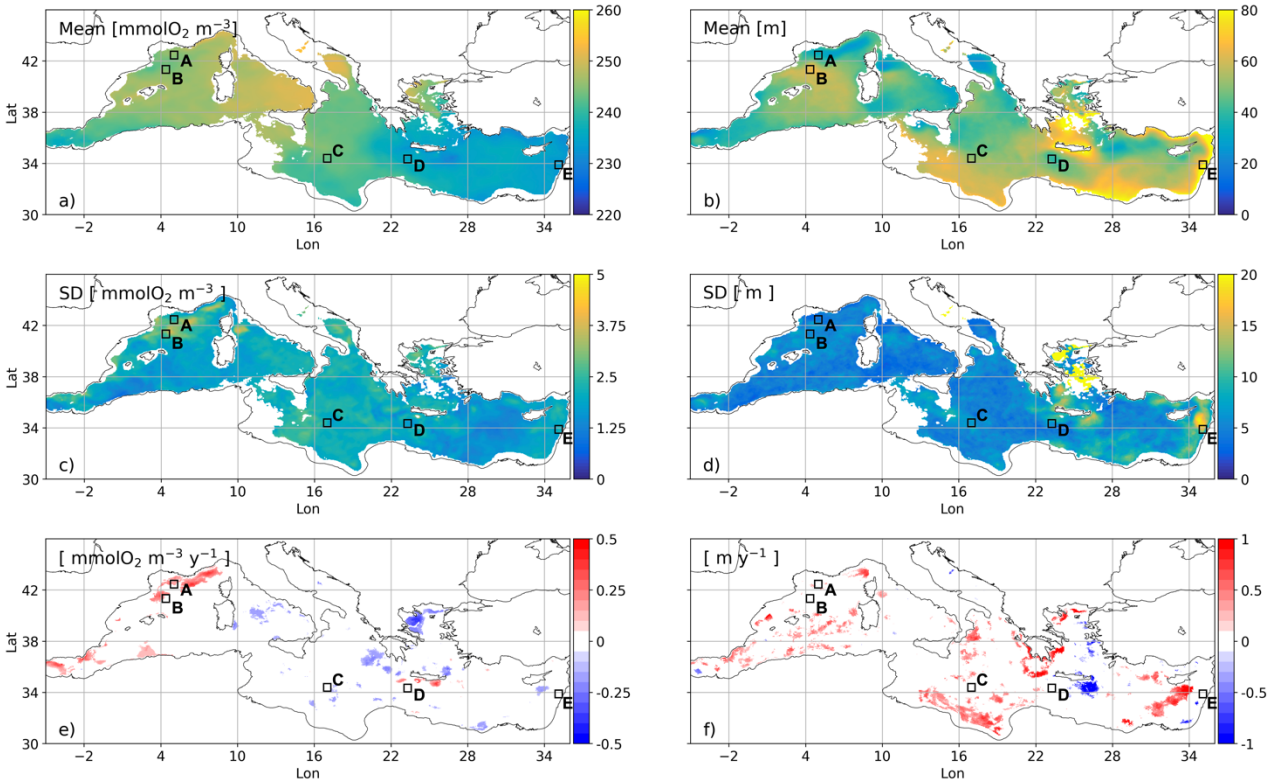
The aggregated sub-basins are indicated in the caption of Figure 2 and are: swm=swm1+swm2, tyr=tyr1+tyr2, adr=adr1+adr2, ion=ion1+ion2+ion3, and lev=lev1+lev2+lev3+lev4, i.e., Alboran and Aegean Sea are not aggregated, but are considered individually as "alb" and "aeg" (as indicated in Figure 2).

The inclusion of the sub-basins (aggregated or not) in Figure 4 and in the tables reporting the metrics depends on the availability of observations in the different sub-basins.
In Table 3, the Aegean Sea ("aeg") is indeed included because observations (of primary production, community respiration and net community production) are available in this area, whereas this is not the case for the Alboran Sea. Similarly, metrics for the Alboran Sea and Aegean Sea are not reported in Table 1 because BGC-Argo observations of dissolved oxygen were not available in these sub-basins.
On the other hand, dissolved oxygen observations in the Alboran Sea and the Aegean Sea were available in the EMODnet_int dataset and metrics of these sub-basins were included in the reference of the Mediterranean biogeochemical reanalysis (Cossarini et al., 2021).
Nevertheless, we decided to report in Figure 4 only the sub-basins where BGC-Argo observations were also available, in order to highlight the high agreement between observations from floats and in situ data, and to include information on the Alboran Sea and the Aegean Sea in lines 237-238 of the manuscript:

"RMSD estimations in the Alboran Sea and Aegean Sea with respect to EMODnet_int (Cossarini et al., 2021) are in agreement with the recognised upper limits of 15 and 25 mmolO_2 m^{-3} in the 0-30 m and 150-300 m depth layers, respectively. "

Table 1, Figure 6: I suggest the authors change "std" to "SD"

We have changed the term as suggested in Table 1 and Figure 6.
The new version of Figure 6 is also reported here:



(new version of Figure 6)

Supplementary figures: I suggest the authors number the figures in chronological order of their citation in the main text.

We agree. In the text of the manuscript, we have changed the reference number of the figures included in the Supplementary Material (sorted as Figs. S1, S2, S3 and S4) and we have changed the order of the figures in the Supplementary Material accordingly.

Caption of Figure 6: I suggest changing “depths higher than 200 m “ to “depths shallower than 200 m”.

We agree. We have changed the expression as suggested. Also, we decided to add the same sentence:

Coastal areas (i.e., with depths shallower than 200 m) are masked.

also in the caption of Figure 8 (lines 627-628).

L 598: I suggest adding the frequency of the reanalysis output.

We have changed the sentence (in lines 405-406 of the new revised manuscript) as follows:

Derivative terms are recomputed in retrospect on a monthly basis starting from the weekly reanalysis output in a specific year, i.e., 2014.

L 609-610: I suggest breaking the sentence into two sentences.

We have changed this sentence (in lines 417-418 of the new revised manuscript) as follows:

The highest values of biological oxygen production in the first 25 m are found in February and March (Fig. 7, central column). Moreover, oxygen consumption under 150 m is observed in all areas (same Figure and column). However, …

L 779: I suggest changing "output" to "monthly outputs"?

The sentence in which we used this phrase (i.e., "reanalysis output") was intended to emphasise that, from the spatial point of view, the budget computation had been done on the 5 areas, as the SOM showed mesoscale variability (lines 560-563):

"… the high spatial heterogeneity of the SOM(Fig. 6a-b) appeared to be linked to the Mediterranean mesoscale variability (e.g., Bonaduce et al., 2021). Thus, the oxygen budget has been reconstructed in retrospect by using the reanalysis output inside 5 areas (Fig. 6) selected as representatives of different circulation structures and biological regimes (Fig. 7)"

We have included the specification of the monthly basis (i.e., temporal specification) for the budget calculation immediately below (in lines 564-566, as indicated in this response on page 3):

"Moreover, the budget of dissolved oxygen has been computed at the monthly frequency, where the average operation further filtered high frequency signals due to the internal dynamical adjustment of the model after data assimilation (Cossarini et al., 2019)."

and therefore we would prefer not to add the temporal specification also in the previous line, unless Reviewer#2 still thinks that this part should be revised.

L 780: "the latter is closed". If all the 3D terms of the budget of dissolved oxygen are calculated offline based on monthly mean of state variables, I would suggest it should not be fully closed, given that the various budget terms are not linear. Please see my main comment on the budget calculation.

The sentence referred to the fact that data assimilation does not introduce extra source/sink terms into the oxygen dynamics. Thus, the calculation of the terms of the budget as presented in Equations 1 and 3 is consistent, except for the uncertainties due to the frequency of the calculation (described on page 3 of this document).

The sentence in lines 563-564 has been changed as follows:

Since data assimilation procedure does not directly affect the oxygen dynamics, our computation of the budget includes all terms and is consistent.

L 824-827: There is the study by Pujo-Pay et al. (2011), showing the difference between the DCM and SOM depths (their Figure 2), that the authors could consider citing.

Pujo-Pay, M., Conan, P., Oriol, L., Cornet-Barthaux, V., Falco, C., Ghiglione, J. F., Goyet, C., Moutin, T., & Prieur, L. (2011). Integrated survey of elemental stoichiometry (C, N, P) from the western to eastern Mediterranean Sea. Biogeosciences, 8(4), 883–899. https://doi.org/10.5194/bg-8-883-2011

We thank Reviewer#2 for this valuable suggestion. We have added this part to lines 598-599 of the new revised manuscript:

An increasing difference between DCM and SOM depths across an eastward transect in the Mediterranean Sea has been observed also by Pujo-Pay et al. 2011. Moreover, analogous…

and we have added the reference in the bibliography of the manuscript.

L 832: “practically coincides”: I suggest the authors provide a mean value of the difference (~ 20m?).

We agree. We have changed the sentence in lines 604-605 of the new revised manuscript as follows:

In particular, the northwestern Mediterranean areas (cases A and B, Fig. 7) are very productive and in summer the vertical level of their biological oxygen production sustaining SOM is on average 25 m shallower than the DCM (Fig. S4).