Review#1

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

1. General comments

The manuscript provides a description and an analysis of the dissolved oxygen dynamics and budget in the Mediterranean Sea, based on a 3D coupled physical-biogeochemical modeling.

The manuscript makes an evaluation of the model results using oxygen concentration and process estimates from in situ observations. It makes novel contributions with respect to the development of the subsurface oxygen maximum (SOM) in the Mediterranean Sea and investigates the ecosystem metabolisms and physical mechanisms involved in its magnitude and depth in various regions of the sea. It proposes to consider the SOM as an indicator of biological and physical processes and their interactions.

The manuscript is rigorous, very well written and organized and I warmly recommend its publication. I report below comments and questions, in particular on the budget calculation, that should be addressed before publication.

We sincerely thank Reviewer#1 for his/her comments, which gave us the possibility to clarify some aspects of our paper.

We indicate our reply in blue colour and some corrections we propose to implement in the text of the manuscript in italic red. If the proposed changes will be accepted, the updated version of our manuscript will include the corrections stated in this document.

2. Specific comments

2.1 Methodology

Budget. Given that the quantification of the oxygen budget is one of the main results of the manuscript, I suggest that the authors provide additional information on how is the budget performed and clarify the choice of the selected period and areas:

- Is the budget performed "online" or "offline?

The oxygen budget has been performed "offline", i.e., starting from the reanalysis outputs. We will revise this part by specifying at old line 398:

Figure 8 shows the Hovmöller diagrams of the oxygen concentration and its biological and physical derivatives for the areas selected in Fig. 7. Derivative terms are recomputed by using the reanalysis output for a specific year (i.e., 2014).

- Does data assimilation influence the budget (for instance through artificial diffusive fluxes)? Is the budget of dissolved oxygen still closed with assimilation of both biogeochemical and physical data? If not, what are the contributions of the "corrective fluxes" in the budget?

We thank Reviewer#1 for this comment, that allows us to clarify a key point.

The dissolved oxygen budget is closed and the mass conservation of oxygen is respected (i.e., no artificial fluxes are introduced). Physical data assimilation corrects ocean dynamics but the solution of the transport of oxygen respects mass conservation. Biogeochemical assimilation changes only phytoplankton biomass and not oxygen concentration.

In particular, the observations assimilated in the biogeochemical reanalysis are satellite chlorophyll measurements, and not oxygen profiles. In the data assimilation procedure, the content of chlorophyll, carbon, nitrogen, phosphorus, and silicon of four phytoplankton groups (i.e., diatoms, autotrophic nanoflagellates, picophytoplankton and large phytoplankton) is updated at a weekly frequency during the simulation. The processes of production/consumption of oxygen indicated in Eq. 1 of the manuscript are instead dynamically and consistently solved within the model. Therefore, the oxygen budget has not been influenced directly by the data assimilation procedure. In other words, data assimilation did not directly cause creation/destruction of oxygen content in the seawater.

Indeed, a hindcast simulation could have produced to verify the impact of assimilation on oxygen dynamics and budget, however we would like to highlight that the chlorophyll data assimilation proved to be fundamental to better simulate the vertical dynamics of the marine ecosystem and in particular the depth of the deep chlorophyll maximum (Teruzzi et al, 2014; Salon et al, 2019), that is connected also with subsurface oxygen production. In particular, we have recently analysed the variability of dissolved oxygen in the Southern Adriatic Sea by using the same biogeochemical reanalysis and we estimated that the summer SOM dynamics are positively correlated with the chlorophyll concentration in 30-80 m layer hosting the deep chlorophyll maximum (Di Biagio et al., in review). Reanalyses are widely used for investigating not only ocean state and variability but also both physical and biogeochemical processes (e.g., Liu et al., 2017; Ford et al., 2018; Pinardi et al., 2019; de Boisseson et al., 2022; Ozer et al., 2022).

Moreover, the off-line oxygen budget has been computed on monthly means of dissolved oxygen, where this average computation further filtered variations due to the internal dynamical adjustment of the model after assimilation (Cossarini et al., 2019 - Fig. 11 - analyse the time scale of the biogeochemical model adjustment after assimilation).

We propose to include a synthetic version of this reply, including also the subsequent points about data assimilation, in the Discussion section of the manuscript:

The oxygen budget has been reconstructed in retrospect by using the reanalysis output. Since data assimilation procedure does not directly affect the oxygen budget, this latter is closed and consistent. Moreover, it has been computed on monthly means of dissolved oxygen, where the average computation further filtered variations due to the internal dynamical adjustment of the model after data assimilation (Cossarini et al., 2019).

Di Biagio V., Martellucci R., Menna M., Teruzzi A., Amadio C., Elena, Mauri E., Cossarini G.: Dissolved oxygen as indicator of multiple drivers of the marine ecosystem: the Southern Adriatic Sea case study, State of the Planet, 7th edition of the Copernicus Marine Service Ocean State Report (OSR 7), in review.

Liu, Y., Meier, H. E. M., and Eilola, K.: Nutrient transports in the Baltic Sea – results from a 30-year physical–biogeochemical reanalysis, Biogeosciences, 14, 2113–2131, https://doi.org/10.5194/bg-14-2113-2017, 2017.

Ford, D., Key, S., McEwan, R., Totterdell, I., & Gehlen, M. (2018). Marine biogeochemical modelling and data assimilation for operational forecasting, reanalysis, and climate research. New Frontiers in Operational Oceanography, 625-652.

Pinardi, N., Cessi, P., Borile, F., & Wolfe, C. L. (2019). The Mediterranean sea overturning circulation. Journal of Physical Oceanography, 49(7), 1699-1721.

de Boisseson, E., Balmaseda, M., Mayer, M., and Zuo, H.: Monitoring and predictions of Marine Heatwave events in the North East Pacific from ocean reanalyses and seasonal forecasts, EGU General Assembly 2022, Vienna, Austria, 23–27 May 2022, EGU22-4079, https://doi.org/10.5194/egusphere-egu22-4079, 2022.

Ozer, T., Gertman, I., Gildor, H., & Herut, B. (2022). Thermohaline Temporal Variability of the SE Mediterranean Coastal Waters (Israel)–Long-Term Trends, Seasonality, and Connectivity. Frontiers in Marine Science.

Cossarini, G., Mariotti, L., Feudale, L., Mignot, A., Salon, S., Taillandier, V., ... & d'Ortenzio, F. (2019). Towards operational 3D-Var assimilation of chlorophyll Biogeochemical-Argo float data into a biogeochemical model of the Mediterranean Sea. Ocean Modelling, 133, 112-128.

- Is there an addition of a nudging term towards observation profiles of dissolved oxygen, such as climatology profiles, in the reanalysis? In that case, is there an estimate of the contribution of the "corrective fluxes" and what is its vertical distribution?

  In the Mediterranean Sea we performed only data assimilation of satellite chlorophyll. As illustrated in Sec. 2.1, a nudging term is actually adopted only in the western boundary of the Atlantic part of the domain (7°W-9°W), where a relaxation towards the climatological field of dissolved oxygen coming from World Ocean Atlas 2018 is prescribed during the whole simulation, in addition to the dynamical forcing by atmospheric fields. However, the Atlantic part of the domain has not been included in the analysis.

- The oxygen budget is carried out at 5 locations and for the year 2014. The choice of the locations and the year is not clear and arises questions: the choice of estimating the budget only for one year is justified, in the discussion, by the small interannual variability, but box E is located in an area where the standard deviation is maximal; box A is located in the Gulf of Lion convection area (due to its particular trophic regime associated with hydrodynamic processes?) but year 2014 seems to be chosen for the analysis because of the absence of deep convection (L 531) (the authors show the maximum monthly mixed layer depth is ~60 m in box A, which is consistent with the findings of Margirier et al. (2020) who identified 2014 as a year with weak winter heat loss and vertical mixing), whereas, in the discussion, it is suggested as "associated with the strong vertical winter mixing" (L 565-566). Is box A located in the Northern Current or in the interior of the gyre? It would help the reading if the authors would clarify the choice of the locations of the 5 boxes and would better specify the hydrodynamic context (for instance, in the thermohaline circulation, the interior of a persistent cyclonic or anticyclonic circulation), before the discussion. A choice of a more "mean year" in terms of winter heat loss and vertical mixing, or the addition, in Supplementary Material, of the results for a strong forcing year and/or a temporal average would make the budget analysis more robust.

  We thank Reviewer#1 for raising this point. We acknowledge that some sentences of the manuscript should be revised and that our choices of year and locations need more explanations.

With respect to the spatial analysis, we decided to choose some specific locations in order to deepen physical and biological processes in different areas of the Mediterranean Sea, by accounting for both different circulation structures and the general zonal gradient of biological productivity. Given that averaging over large areas can hide the relative importance of specific processes (e.g. the dominance of upwelling at a certain period of the year), only by selecting some restricted areas the analysis of the relative importance of processes can effectively characterize the oxygen dynamics. For sake of brevity and clarity we decided to limit the number of sites to 5 and to select sites that can be representative of the variability within the Mediterranean domain.

In particular, in the choice of the specific locations, since the northwestern Mediterranean Sea is one of the most dynamic areas across the basin, we decided to analyse two different locations there: areas A and B are both included in the Liguro-Provencal Gyre (Menna et al., 2022), but A area is more influenced by the Northern Current, while B area is affected by the Balearic front (Ruiz et al., 2009). Following the zonal direction, we then chose a location in the Centre of the Mediterranean (C area), one in a smaller cyclonic gyre (D area) with respect to the Liguro-Provencal one and, finally, one in a subduction area of the Eastern Levantine Sea (E area). This last location, that corresponds to the area of the North Shikmona Eddy (Menna et al., 2022), is south of the southern margin of the intense red area in Fig. 7d where the standard deviation is maximal and it has been chosen because in 2014 it shows typical dynamics associated with subduction.

Regarding the choice of the year, we propose to include in the Supplementary Material the new Figures S4 and S5 reporting the mean annual values of SOM depth and concentration in the 5 areas in the period 1999-2019. Figures S4 and S5 show that the year 2014 (highlighted by a vertical dashed line in all panels) can be considered a “year with mean values”, considering globally the 5 points. In fact, SOM depth and concentration are generally quite stable during the years, with the exception of E area, in which however SOM depth in 2014 is intermediated with respect to the other years. In particular, in the A area, we would have not obtained high variations in the SOM depth and concentration also choosing a year associated with deep convection. This is because SOM is basically a summer feature only partly influenced by the winter conditions.

We propose to modify lines at old line 393-395by including this explanation:

To highlight the relative importance of the several drivers that are responsible for the SOM variability on the mesoscale/submesoscale, we selected some areas (indicated by the black squares in Fig. 7b) that are approximately 50 km x 50 km wide and representative of different phenomenologies and analysed the seasonal cycle of oxygen during one year of simulation (i.e., 2014). Areas A and B are both included in the Liguro-Provencal Gyre (Menna et al., 2022), but A area is more influenced by the Northern Current, while B area is affected by the Balearic front (Ruiz et al., 2009). Area C is chosen in the central part of the Mediterranean south to the Mid Ionian Jet (Menna et al., 2022), area D is within a smaller cyclonic gyre (i.e., western Cretan Gyre, Pinardi et al., 2015) in the oligotrophic Eastern Mediterranean basin and, finally, area E is located in a subduction area within the continental slope of the Eastern Levantine Sea (E area).

Year 2014 has been chosen because summer SOM depths and concentrations in the selected areas are intermediate within the variability shown in the 1999-2019 period (Figs. S4-S5 in the Supplementary Material).

Nevertheless, we acknowledge that the Reviewer#1’s observation on 2014 from the point of view of the absence of deep convection is correct and we will modify this part by deleting the expression “associated with the strong vertical winter mixing” (L 565-566) and by citing Margirier et al. (2020) findings about A area in 2014.

However, if Reviewer#1 still retains that another year should be explicitly considered and/or the selection of another subduction area in Eastern Levantine could be more appropriate, we will follow his/her suggestions.

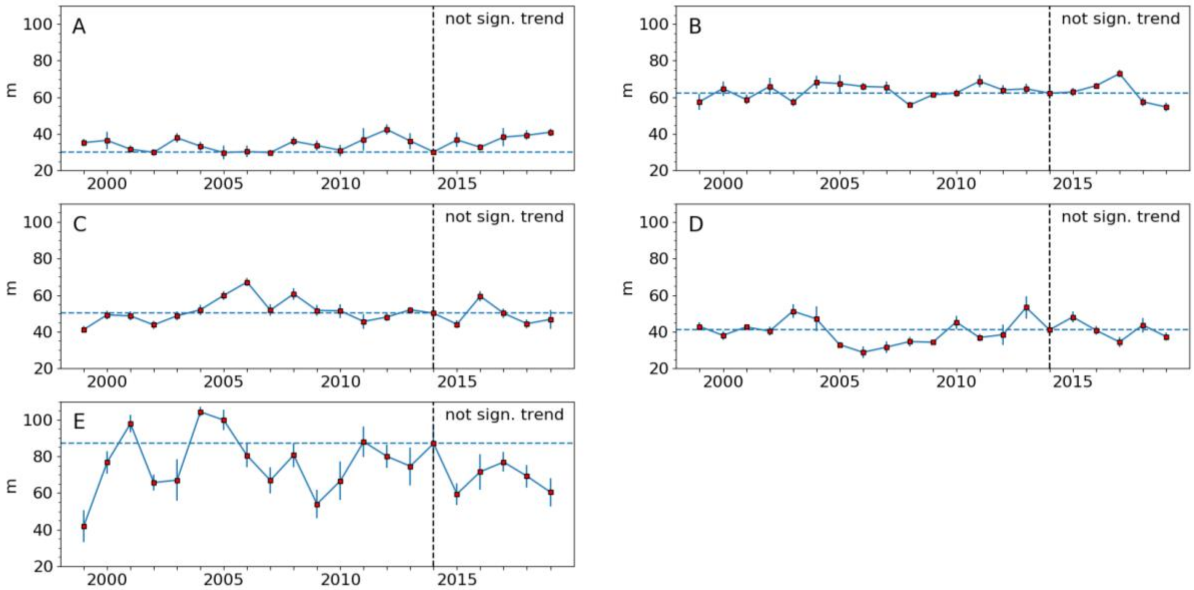

Figure S4: Spatial mean of the 1999-2019 annual summer values of the SOM depth within the A-E areas indicated in Fig. 7b of the manuscript. Vertical bars indicate the spatial standard deviations. Trend significance has been evaluated by Mann-Kendall test (p=0.05).

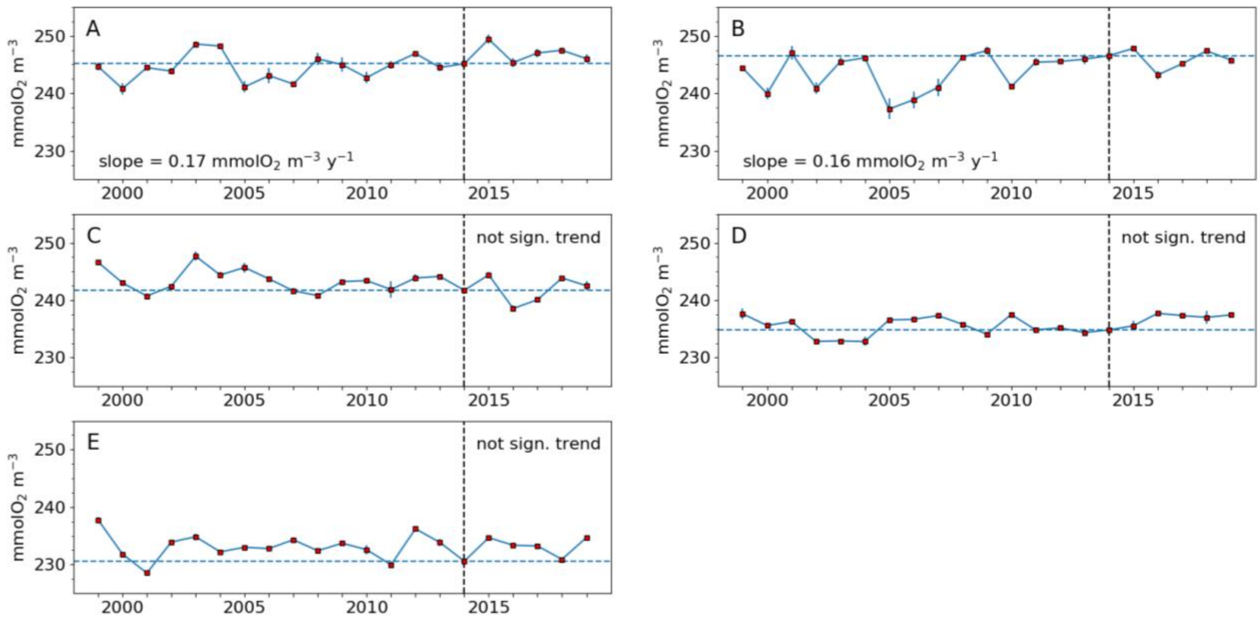

Figure S5: Spatial mean of the 1999-2019 annual summer values of the SOM concentration within the A-E areas indicated in Fig. 7b of the manuscript. Vertical bars indicate the spatial standard deviations. Trend significance has been evaluated by Mann-Kendall test (p=0.05) and the slope computed by Theil-Sen method has been provided in case of significant trend.

Menna, M., Gačić, M., Martellucci, R., Notarstefano, G., Fedele, G., Mauri, E., ... & Poulain, P. M. (2022). Climatic, Decadal, and Interannual Variability in the Upper Layer of the Mediterranean Sea Using Remotely Sensed and In-Situ Data. Remote Sensing, 14(6), 1322.

Ruiz, S., Pascual, A., Garau, B., Faugère, Y., Alvarez, A., & Tintoré, J. (2009). Mesoscale dynamics of the Balearic Front, integrating glider, ship and satellite data. Journal of Marine Systems, 78, S3-S16.

Pinardi, N., Zavatarelli, M., Adani, M., Coppini, G., Fratianni, C., Oddo, P., ... & Bonaduce, A. (2015). Mediterranean Sea large-scale low-frequency ocean variability and water mass formation rates from 1987 to 2007: A retrospective analysis. Progress in Oceanography, 132, 318-332.

Theil, H.: A rank-invariant method of linear and polynomial regres- sion analysis, 3; confidence regions for the parameters of poly- nomial regression equations, Indagat. Math., 1, 467–482, 1950.

Sen, P. K.: Estimates of the regression coefficient based on Kendall's tau, J. Am. Stat. Assoc., 63, 1379–1389, https://doi.org/10.1080/01621459.1968.10480934, 1968.

Data assimilation. Does assimilation of surface chlorophyll data modify the vertical profile of chlorophyll concentration and, if it is the case, does this affect the discussion on the difference in DCM and SOM depths (L 552-574)?

As clarified in two answers before this, the depth of DCM simulated by the model is actually improved by the chlorophyll data assimilation from satellite. However, after the data assimilation, the model undergoes a dynamical adjustment and the oxygen dynamics, based also on phytoplankton, are then computed and integrated at each timestep. Moreover, DCM and SOM depths are considered at monthly frequency, i.e., by furtherly filtering possible oscillations of biogeochemical variations (see previous point on this issue).

As discussed above, we propose to include a paragraph on the impact of assimilation in the reanalysis in the Discussion section.

2.2 Assessment of the model results

Sect. 2.2: I suggest the authors specify the accuracy of the in situ observations of dissolved oxygen concentration (in particular BGC-Argo) and, if possible, of estimates of production and respiration fluxes derived from observations.

In the last years the procedures of post-deployment Quality Control on the BGC-Argo floats have been constantly improved (Bittig at el., 2019 and references therein; Maurer et al., 2021; Thierry and Bittig 2021). As regards dissolved oxygen, Mignot et al. (2019) estimated values of additive bias and root mean square error of dissolved oxygen from 17 BGC-Argo floats (in 2013-2017) equal to 2.9 ± 5.5 µmol/kg and 5.1 ± 0.8 µmol/kg, respectively, with respect to ship CTD-rosette casts where water samples were also collected to measure dissolved oxygen. An uncertainty within 3 µmol/kg has been confirmed also by Maurer et al., 2021.

On the other hand, production and respiration values derived from observations were not always provided together with estimates of corresponding uncertainties. In other cases, instead, such uncertainties cover a wide range of variation.

For example, in NWM (Table 2), only Gonzales et al. (2008) reports standard errors on the single observations, in the range 0.08-1.69 $mmolO_2/m^3/day$ for CR and 0.11-1.08 $mmolO_2/m^3/day$ for NCP. In the other sub-basins (Table 3, May-June period), for values coming from Regaudie-de-Gioux et al. (2009) and Lagaria et al. (2011) the standard errors on the single observations are in the range 0.15-1.43 $mmolO_2/m^3/day$ for GPP, 0.09-2.02 $mmolO_2/m^3/day$ for CR and 0.06-1.69 $mmolO_2/m^3/day$ for NCP; from Gazeau et al. (2021) only one value of two is accompanied with standard error for NCP and equal to 0.09 $mmolO_2/m^3/day$.

Therefore, in the old version of the manuscript we computed the uncertainties associated with the production and respiration fluxes as the standard deviation of the observations (and when only one observation was available, the original uncertainty reported in the reference was indicated). However, given that the reported uncertainty estimations then represent the variability of the observations within the selected layers, we think that the min/max range could be more appropriate to inform on the variability of the observations. For this reason, we will modify the Tables 2-3 consistently.

We will better clarify the chosen metrics in the text/captions of Tables 2-3.

Bittig HC, Maurer TL, Plant JN, Schmechtig C, Wong APS, Claustre H, Trull TW, Udaya Bhaskar TVS, Boss E, Dall'Olmo G, Organelli E, Poteau A, Johnson KS, Hanstein C, Leymarie E, Le Reste S, Riser SC, Rupan AR, Taillandier V, Thierry V and Xing X (2019) A BGC-Argo Guide: Planning, Deployment, Data Handling and Usage. Front. Mar. Sci. 6:502. doi: 10.3389/fmars.2019.00502

Maurer T.L,. Plant J.N. and Johnson K.S. (2021) Delayed-Mode Quality Control of Oxygen, Nitrate, and pH Data on SOCCOM Biogeochemical Profiling Floats. Front. Mar. Sci. 8:683207. doi: 10.3389/fmars.2021.683207

Thierry, V., & Bittig, H. (2021). Argo quality control manual for dissolved oxygen concentration. http://dx.doi.org/10.13155/46542

Mignot, A., D'Ortenzio, F., Taillandier, V., Cossarini, G., & Salon, S. (2019).Quantifying observational errors in Biogeochemical-Argo oxygen, nitrate, and chlorophyll a concentrations. Geophysical Research Letters, 46, 4330–4337. https://doi.org/10.1029/2018GL080541

L 210-214: For the comparison with BGC-Argo observations, are the modeled dissolved oxygen concentrations extracted at the same locations as observations or averaged over the same sub-basin?

We thank Reviewer#1 for this comment. In the comparison with BGC-Argo observations, the concentrations of dissolved oxygen are actually extracted at the same locations as observations. Then the skill performance metrics are computed on the basis of observation-model misfits. Finally, the overall metrics are obtained by aggregating the partial results on the basis of the sub-basin. We will specify this information in the new version of the manuscript. The procedure is explained in Salon et al., 2019.

L 238, Table 1: Cossarini et al. (2021) showed a good reproduction of the temporal evolution of the oxygen profile in the northwestern Mediterranean along the trajectory of float 6901470 (their Fig. 7B). I suggest that the authors also provide temporal correlation between BGC-Argo observed and modeled SOM depths and concentrations. Since the authors assess the impact of biological and physical processes on the onset of the SOM (L 66), it could have been worthy to consider May and June in the period over which the comparisons model/observations are carried out.

We thank Reviewer#1 for these suggestions. We will follow both of them.

L 250, Table 1: Please add the standard deviation associated with the mean values in Table 1.

We agree and we will add it.

Sect. 3.1.2: The effort to compile the GPP, CR and NCP estimates deduced from in situ observations and to compare the modeled biogeochemical fluxes with those estimates is highly appreciable.

Many thanks.

Tables 2 and 3: The authors don't show the same parameter to characterize data variability: standard deviation for observations and, min and max for reanalysis outputs. The number of data in both sets is different but I suggest they give the same parameter(s) for both data sets to simplify the comparisons (L 287 for instance) or further justify this difference.

We agree and we will provide consistent parameters for the uncertainties, as discussed in our reply to Reviewer#1's comment on Section 2.2.

L 298-300: Is NCP or NPP compared with satellite and literature estimates in Cossarini et al. (2021) ? If it is NPP please replace NCP by NPP.

We are sorry for this oversight and we thank Reviewer#1 for spotting it. It is actually NPP and we will correct it.

2.3 Mean values and trends.

L 575-578: In this study, the authors present the mean values of SOM depths and concentrations over a 20-year period. Do they find trends in concentration and depth of the summer SOM over this period 1999-2019?

We thank Reviewer#1 for raising this issue.

We have already evaluated possible trends in the SOM depth and concentration in the selected A-E areas (Figs. S4 and S5 shown before) by using the Theil-Sen method. We found that trends are not significant (Mann-Kendall test, $p=0.05$) except in the case of SOM concentration in NWM, where we think that values in the first years could have led to the trend.

We will evaluate trends in the SOM depth and concentration in the whole Mediterranean basin. If we will find patterns of significant trends, we will include the corresponding maps as two new panels in Fig. 7 in the new version of the manuscript. In any case, we will include a comment in the Discussion section and/or in the Supplementary Material below the Figs. S4 and S5.

3. Technical comments

L 43, 47, 78, 244: "a SOM" instead of "an SOM".

We thank Reviewer#1 for this comment. The text was revised by a professional English Editing Service that indicated "an SOM" as the preferred form. Additionally, "an SOM" was also used e.g. in Martz et al., 2008 (reference in the manuscript), or Possenti et al., 2021. We thus think to maintain this form, but if Reviewer#1 still prefers it, we will adopt the form "a SOM".

Possenti L, Humphreys MP, Bakker DCE, Cobas-García M, Fernand L, Lee GA, Pallottino F, Loucaides S, Mowlem MC and Kaiser J (2021) Air-Sea Gas Fluxes and Remineralization From a Novel Combination of pH and O2 Sensors on a Glider. Front. Mar. Sci. 8:696772. doi:10.3389/fmars.2021.696772

L 73: I suggest adding "was modelled" before "at the surface (Cossarini et al., 2021) during the last two decades".

We thank Reviewer#1 for this comment. We acknowledge that the sentence was not very clear. We think to rewrite that part as:

and a negative oxygen trend at the surface was estimated from a biogeochemical reanalysis (Cossarini et al., 2021) covering the last two decades.

L 141: RHS acronym is not defined and is used only once.

We agree. We will replace the "RHS" term by the extended form "right hand side".

Caption of Figure 3: Please specify the period over which the model outputs are averaged.

We will add it.

Caption of Table 3, L 274: Gazeau et al. (2021) instead of (2020), model outputs.

We thank Reviewer#1 for noticing these oversights. We will correct them.

L 321, Fig. 5, and throughout the manuscript: Please specify the units: mmol C m-3 d-1 or mmol O2 m-3 d-1 instead of mmol m-3 d-1.

We agree. We will specify them.

L 333: "where surface oxygen follows the cycle of oxygen saturation" Since the evolution of surface oxygen and oxygen saturation is different in winter/early spring as the authors mention later I would reformulate this sentence (for instance by adding "generally" and/or "except in winter"…).

We agree. We will reformulate the sentence at old line 332-334 as:

At the surface, the excess oxygen net production (Fig. 5), combined with air-sea interactions (Eqs. 1 and 2) and mixing/stratification processes yields the seasonal cycle displayed in Fig. 6, where surface oxygen generally follows the cycle of oxygen saturation […]

L 349 : Please specify "in winter" and "at the surface" in "(equal to approximately 0.76 and 0.25 mmol m -3 d-1 …)"

We agree. We will better specify the sentence.

L 332-358: The paragraph doesn't appear to fit well with the title of the section. I suggest writing this paragraph in another result section (for instance, oxygen dynamics at the surface) or merge it with the discussion L 505. Moreover, the sentence L 349-351 ("positive NCP values […] appear less relevant with respect to the effect of cooling (which increases the oxygen solubility)") is not clear. I suggest rephrasing it or adding an estimate of the air-sea flux induced by the cooling.

We thank Reviewer#1 for this comment. We will separate Sec. 3.2.1 in two parts and we will rephrase the sentence at lines 349-351.

L 378: Please add the reference to Fig. 7b.

We agree. We will specify it.

L 378, 420-421, and throughout the manuscript: I suggest reformulating without parentheses.

We agree. We will rephrase the two sentences.

L 414: (D) areas.

We thank Reviewer#1 for noticing this oversight. We will correct it.

L 414-415: "the Gulf of Lion is the unique case in which the values of the subsurface oxygen derivative in summer are comparable with late winter-early spring surface values": this is difficult to see in Fig. 8 because the colors are saturated in winter.

We agree. We will rephrase the sentence.

L 433: "and negative values in summer" ⇒ high negative values between May and October

We agree. We will rephrase the sentence.

L 443: I suggest removing "intense" to characterize the production in May.

We agree. We will remove the word.

L 445, 571: I suggest replacing "coastal" by "continental slope"

We agree. We will rephrase the sentences by using (at old line 445):

in the continental slope area of the eastern Levantine (E)

and (old line 571)

in the subduction areas within the continental slope of the Levantine Sea

L 454: reference to "Fig. S1" instead to "Fig. S2" ?

We thank Reviewer#1 for noticing this oversight. We will correct it.

L 458: I would replace "on the onset of the subsurface oxygen maximum" by "on the intensity and depth of the summer subsurface oxygen maximum"

We agree. We will modify this expression.

L 466: I suggest adding "summer" before "SOM".

We thank Reviewer#1 for this comment. However, since in Table 1 we will also add the comparison between the BGC-Argo floats and the model in May and June (following Reviewer#1's suggestion referred to Table 1 and L238), we propose to add the expression "late spring-summer" before "SOM".

L 475: "Tables 2 and 3" instead of "Table 3".

We thank Reviewer#1 for this comment. Actually here we refer to the late spring-early summer GPP and CR, therefore only to Table 3. Nevertheless, we recognise that we did not specify it, so we will specify the considered months in the sentence.

L 515: Mignot et al. (2014) described the variability of deep chlorophyll maximum. Are they also describing SOM variability?

We thank Reviewer#1 for this observation and for noticing this oversight. We will replace this reference by Yasunaka et al., 2021 (already cited in the manuscript).

L 517: "SOM depth" instead of "SOM"

We agree. We will specify it.

L 565: I suggest removing "(cases A and B, Fig. 8)" if only 2014 is still considered.

We thank Reviewer#1 for the remark. Since A and B areas are actually more productive than the others areas considered, we prefer to rephrase this sentence by deleting the consideration on the strong vertical mixing as follows:

In particular, the northwestern Mediterranean areas (cases A and B, Fig. 8) are very productive and in summer the vertical level of their biological production practically coincides with the DCM (Fig. S3).

L 567: reference to "Fig. S3" instead to "Fig. S4"?

We thank Reviewer#1 for noticing this oversight. We will correct it.

Fig. S1: Please indicate the months instead of the number of days since 1st January in the x-axis.

We thank Reviewer#1 for this observation. We will indicate the months in the x-axis.

Fig. S2: Since NCP is also negative, I suggest extending the range of the plot to negative values and using an ‘anomaly’ colormap.

We apologise for this oversight: here we were reporting NPP, not NCP. We will correct the caption of the figure.

Fig. S3: I suggest enlarging the labels of the axes and color bars, and indicating the SOM depth.

We agree. We will modify the figure in the new version of the manuscript.

References:

Margirier, F., P. Testor, E. Heslop, K. Mallil, A. Bosse, L. Houpert, L. Mortier, M.-N. Bouin, L. Coppola, F. D’Ortenzio, X. Durrieu de Madron, B. Mourre, L. Prieur, P. Raimbault et V. Taillandier: Abrupt warming and salinification of intermediate waters interplays with decline of deep convection in the Northwestern Mediterranean Sea. In : Scientific Reports 10.1 (déc. 2020), p. 20923. doi : 10.1038/s41598-020-77859-5, 2020.

Citation: https://doi.org/10.5194/bg-2022-70-RC1