Author’s response to the Editor

Dear Editor,

We thank you for the consideration of the manuscript and for the worthwhile suggestions to improve it.

We carefully considered all the reviewers’ comments and in particular we addressed the two major points raised about the manuscript, i.e., the oxygen budget computation and the data assimilation adopted in the simulation (in our reply to Review#1’s comments on Methodology and to Reviewer#2’s general comment).

Moreover, in this revised version of the manuscript we introduced these major modifications:

- we discussed more in detail the choice of the 5 areas analysed in Sec. 3.2.3 (in our reply to Review#1’s comments on Methodology);
- we added the trend evaluation of SOM concentration and depth (following Reviewer#1’s suggestion on lines 575-578 of the submitted version);
- we moved the part concerning the surface oxygen dynamics in the newly introduced Appendix (following Reviewer#1’s comment on lines 332-358 of the submitted version), consequently modifying the numbers of the next figures of the manuscript;
- we revised the metrics used for the comparison between the model results and the observations (following Reviewer#1’s suggestions on Tables 1-2-3);
- we included new version of some figures, modified to address both Review#1’s suggestions (e.g. about unit of measurement, label size etc.) and the authors’ guide of the journal about accessibility of colour figures for readers with colour vision deficiencies.

We indicate the reviewers’ comments in black colour, our replies in blue colour and the corrections we implemented in the text of the manuscript and in the Supplementary Material in italic red.

Author's responses to Reviewer#1's comments on the manuscript:

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.

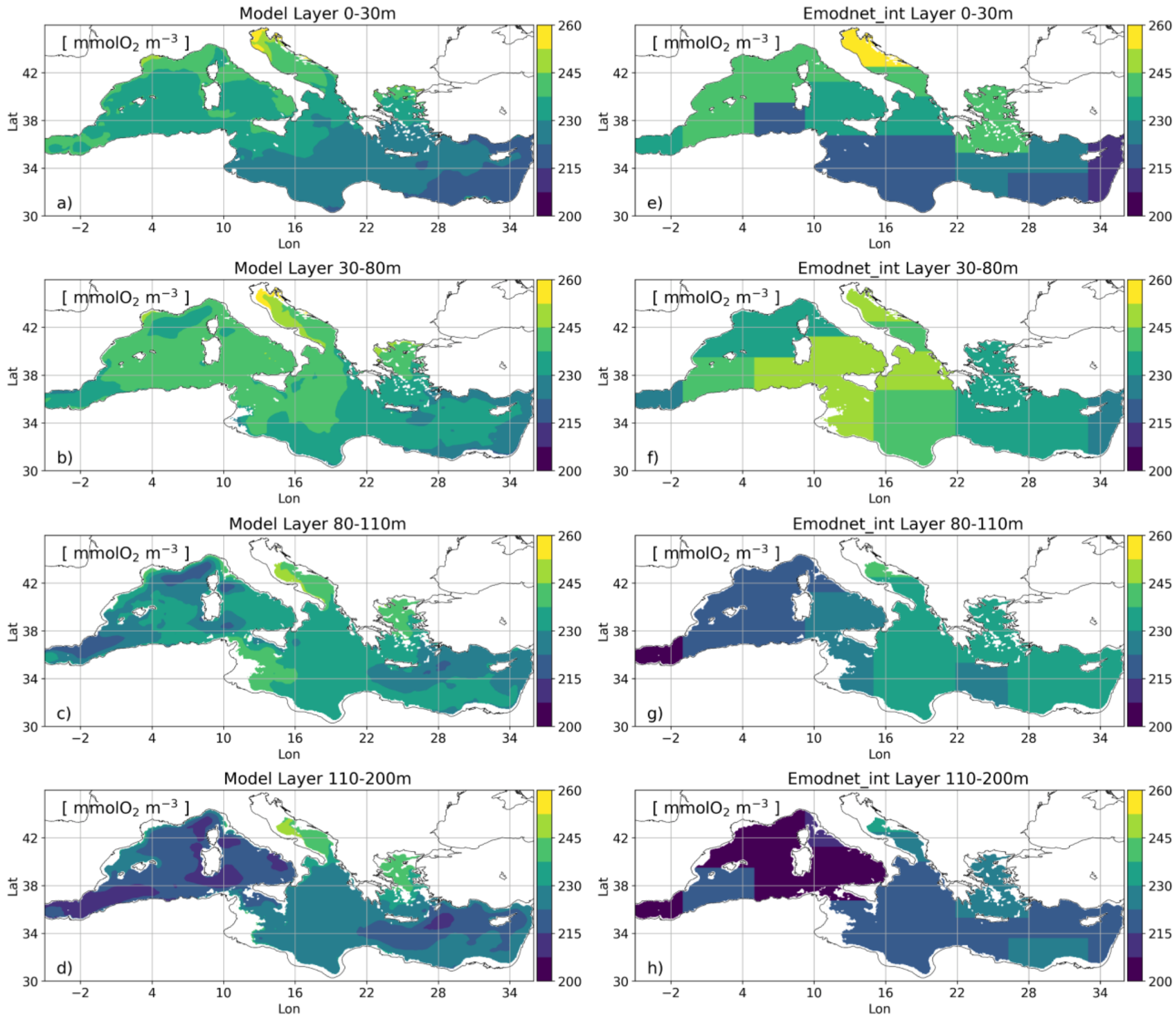
We sincerely thank Reviewer#1 for their comments, which gave us the possibility to clarify and to improve some aspects of our paper.

We indicate our replies in blue colour, while the corrections we implemented in the text of the manuscript and in the Supplementary Material are in italic red.

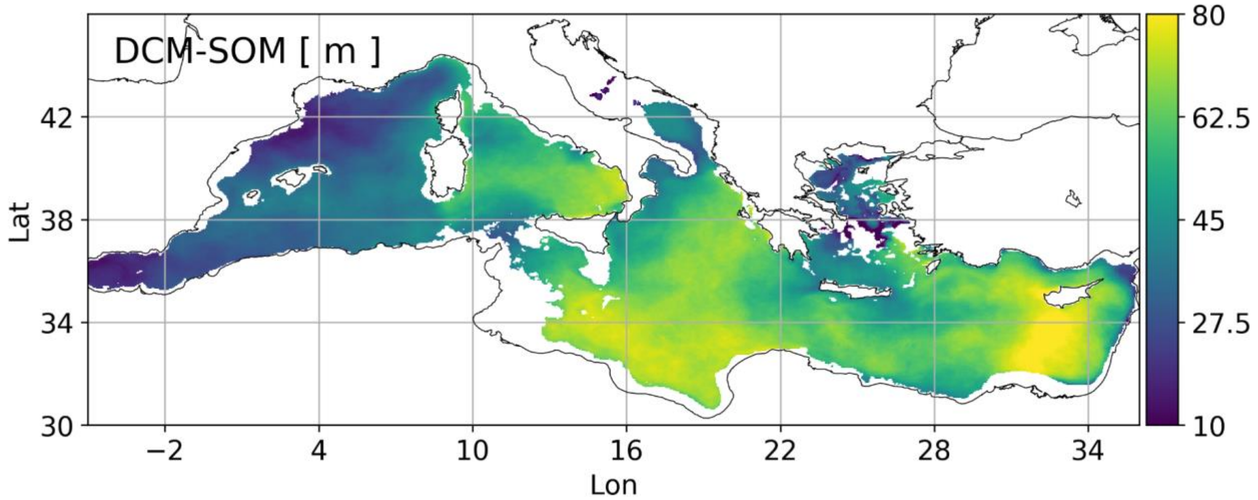
In particular, in this revised version of the manuscript:

- we better explained the oxygen budget computation and discussed more in detail the choice of the 5 areas (in our reply to Review#1's comments on Methodology),
- we added the trend evaluation of SOM concentration and depth (following Reviewer#1's suggestion on lines 575-578 of the submitted version) and moved the part concerning the surface oxygen dynamics in the newly introduced Appendix (following Reviewer#1's comment on lines 332-358 of the submitted version), consequently modifying the numbers of the next figures of the manuscript.

We also report here the new versions of some figures, modified to address both the Review#1's suggestions (e.g. about unit of measurement, label size etc.) and the authors' guide of the journal about accessibility of colour figures for readers with colour vision deficiencies (i.e., we modified the colormap used in old Figs. 3-7-9, now Figs. 3-6-8). In particular, the new versions that we propose for current Figs. 3 and 8 are reported below, whereas the others are reported in correspondence of the relative comments:



(New version of Fig. 3)



(New version of old Fig. 9, now Fig. 8)

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 have revised this part by specifying at lines 400-401:

Figure 7 shows the Hovmöller diagrams of the oxygen concentration and its biological and physical derivatives for the areas selected in Fig. 6. 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, we would like to remark that the biogeochemical reanalysis assimilates satellite chlorophyll observations and not oxygen vertical 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 is not influenced directly by the data assimilation procedure. In other words, data assimilation does not directly introduce sources or sinks of oxygen content in the seawater.

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). We are aware that a hindcast simulation could have been produced to verify the impact of the physical and biogeochemical assimilation on oxygen dynamics and budget. However, we would 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 a 30-80 m layer hosting the deep chlorophyll maximum (Di Biagio et al., in review).

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 (see Cossarini et al., 2019 - Fig. 11 - who analysed the time scales of the biogeochemical model adjustment after assimilation).

To conclude, we have added to the manuscript a synthetic version of this reply, including also the subsequent points about data assimilation, in the Discussion section (lines 541-546):

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). Since data assimilation procedure does not directly affect the oxygen budget, the latter is closed and consistent. Moreover, the budget has been computed on monthly means of dissolved oxygen, 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). We found that…

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.

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.

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 first version of the manuscript should have been revised and that our choices of year and specific areas needed more explanations.

With respect to the spatial analysis, we decided to choose some specific areas in order to delve into physical and biological processes at mesoscale (and sub-mesoscale) in different regions of the Mediterranean Sea, characterized by both different circulation structures and the general zonal gradient of biological productivity. Given that averaging over large areas (e.g., subbasins) can hide the relative importance of specific processes at smaller scales (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. Finally, we decided to limit the number of locations to 5 for sake of brevity and clarity in the figures and in the text, however we think that the selected areas sufficiently embrace the relevant processes/phenomenologies representative of the variability within the Mediterranean domain.

In particular, in the choice of the specific areas, since the northwestern Mediterranean Sea is one of the most dynamic regions 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 cyclonic gyre (D area) which is smaller than 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 yellow area in Fig. 6d where the standard deviation is maximal and it has been chosen because in 2014 it shows a dynamics typically associated with subduction.

Regarding the choice of the year, we have included in the Supplementary Material the new Figure S4 reporting the mean annual values of SOM concentration and depth in the 5 areas in the period 1999-2019. Figure S4 shows 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 locations. In fact, SOM concentration and depth are generally quite stable during the years, with the exception of E area, when the SOM depth in 2014 is, however, intermediate 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 modified lines at old lines 393-395 (now lines 388-397) by 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. 6b) that are approximately 50 km x 50 km wide and representative of different phenomenologies, and then we 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.

Year 2014 has been chosen because summer SOM depths and concentrations in the selected areas are intermediate when compared to the variability shown in the 1999-2019 period (Fig. S4 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, therefore we have modified this part by deleting the expression "associated with the strong vertical winter mixing" (lines 565-566 in the submitted manuscripts) and by citing Margirier et al. (2020) in Discussion at lines 536-538:

We selected 2014 since it was characterized by a lack of strong forcing signals (e.g., deep convection, Margirier et al., 2020), and it can be considered exemplary of the mean regimes of the Mediterranean SOM (Fig. 7 and Fig. S4 in the Supplementary material).

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 their suggestions.

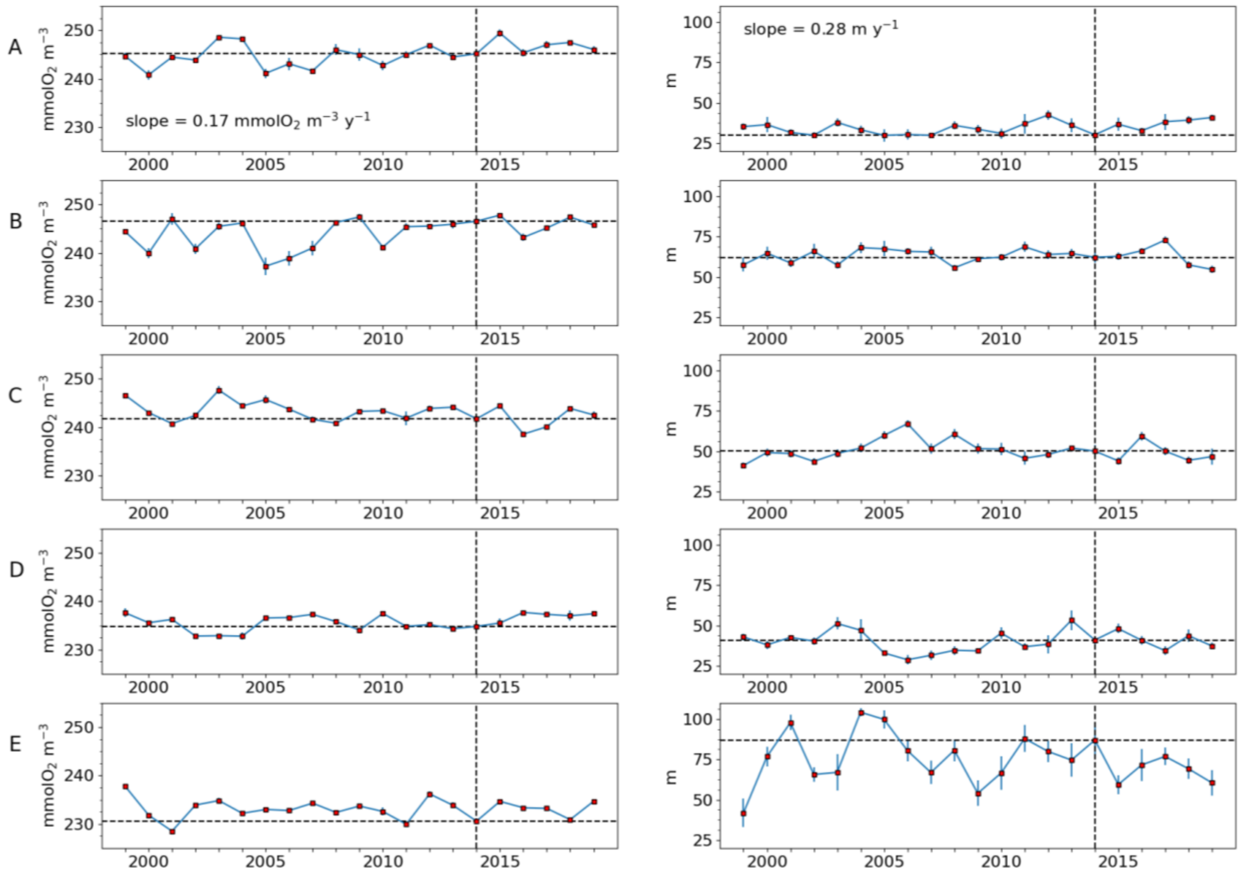


Figure S4: Spatial mean of the 1999-2019 annual summer values of the SOM concentration (first column) and depth (second column) within the A-E areas (rows) indicated in Fig. 6b of the manuscript. For each year, the vertical bar indicates the spatial standard deviation. Trend significance has been evaluated by Mann-Kendall test (p=0.05) and the slope computed by Theil-Sen method has been provided in the plot in case of significant trend. Horizontal and vertical dashed lines refer to the year 2014 extensively discussed in the text.

(Figure S4 proposed as new figure in the Supplementary Material)

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 regression 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 further filtering possible higher frequency oscillations of biogeochemical variables (see previous point on this issue).

As discussed above, we have included 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, with error values in the range 0.08-1.69 mmolO_2 m^{-3} d^{-1} for CR and 0.11-1.08 mmolO_2 m^{-3} d^{-1} for NCP. In the other subbasins (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} d^{-1} for GPP, 0.09-2.02 mmolO_2 m^{-3} d^{-1} for CR and 0.06-1.69 mmolO_2 m^{-3} d^{-1} for NCP; from Gazeau et al. (2021) only one value of the two is accompanied with standard error for NCP and equal to 0.09 mmolO_2 m^{-3} d^{-1}.

Therefore, in the previously submitted 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 previously reported uncertainty estimations were indeed an index of 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 modified the Tables 2-3 consistently as follows:

COAST (0-30 m)	GPP [mmolO_2 m^{-3} d^{-1}]		CR [mmolO_2 m^{-3} d^{-1}]		NCP [mmolO_2 m^{-3} d^{-1}]	
	OBS mean (N) (min / max)	**MODEL** mean (min / max)	**OBS** mean (N) (min / max)	**MODEL** mean (min / max)	**OBS** mean (N) (min / max)	**MODEL** mean (min / max)
WIN	1.21 (4) (0.55 / 2.14)	0.57 (0.09 / 2.21)	0.61 (4) (0.25 / 1.03)	0.29 (0.15 / 0.89)	0.60 (4) (-0.29 / 1.38)	0.27 (-0.10 / 1.32)
SPR	3.40 (4) (1.37 / 5.86)	1.24 (0.22 / 3.22)	0.74 (4) (0.61 / 0.96)	0.82 (0.26 / 2.15)	2.67 (4) (0.73 / 5.25)	0.43 (-0.25 / 1.90)
SUM	2.57 (19) (0.02 / 5.81)	1.08 (0.55 / 2.48)	2.81 (19) (0.27 / 7.10)	1.00 (0.58 / 1.84)	-0.19 (20) (-6.01 / 4.41)	0.09 (-0.12 / 0.76)
OPEN SEA	CR [mmolO_2 m^{-3} d^{-1}]					
	SURF (0-20 m)		MID (30-50 m)		DEEP (90-130 m)	
	OBS mean (N) (min / max)	**MODEL** mean (min / max)	**OBS** mean (N) (min / max)	**MODEL** mean (min / max)	**OBS** mean (N) (min / max)	**MODEL** mean (min / max)
WIN	1.24 (7) (0.01 / 3.11)	0.23 (0.11 / 0.70)	0.87 (5) (0.39 / 2.05)	0.16 (0.08 / 0.43)	1.07 (8) (0.01 / 2.94)	0.04 (0.02 / 0.10)
SPR	2.08 (3) (1.42 / 2.68)	0.86 (0.13 / 1.85)	1.60 (3) (1.16 / 2.09)	0.46 (0.09 / 0.99)	1.87 (4) (1.11 / 3.75)	0.06 (0.03 / 0.24)
SUM	1.71 (4) (0.01 / 5.53)	0.89 (0.60 / 1.30)	1.54 (4) (0.24 / 3.84)	0.78 (0.48 / 1.15)	1.02 (4) (0.26 / 2.11)	0.26 (0.08 / 0.58)

(New version of Table 2)

OPEN SEA	SUB	GPP [mmolO$_2$ m^{-3} d^{-1}]		CR [mmolO$_2$ m^{-3} d^{-1}]		NCP [mmolO$_2$ m^{-3} d^{-1}]	
		OBS mean (N) (min / max) or SE	**MODEL mean (min / max)**	**OBS mean (N) (min / max) or SE**	**MODEL mean (min / max)**	**OBS mean (N) (min / max) or SE**	**MODEL mean (min / max)**
SURF < 30m	**swm**	0.92 (1) SE = 0.39	0.90 (0.52 / 1.53)	0.63 (1) SE = 0.38	0.84 (0.55 / 1.17)	-2.40 (3) (-18.55/11.06)	0.05 (-0.09 / 0.44)
	tyr	1.47 (3) (0.58 / 3.21)	0.69 (0.44 / 1.23)	2.89 (3) (0.56 / 5.11)	0.66 (0.47 / 1.04)	-0.95 (5) (-4.5 / 1.39)	0.04 (-0.07 / 0.21)
	aeg	1.84 (2) (1.75 / 1.92)	0.68 (0.41 / 1.11)	3.81 (2) (2.88 / 4.74)	0.62 (0.30 / 0.92)	-1.98 (2) (-2.82 / -1.13)	0.06 (-0.05 / 0.51)
	ion	1.90 (10) (0.58 / 3.63)	0.60 (0.35 / 1.24)	2.34 (10) (0.05 / 6.46)	0.56 (0.34 / 0.98)	-0.18 (12) (-2.83 / 2.69)	0.04 (-0.06 / 0.26)
	lev	0.70 (4) (0.12 / 1.84)	0.55 (0.33 / 0.90)	1.11 (4) (0.38 / 2.27)	0.52 (0.33 / 0.79)	-0.41 (4) (-0.78 / -0.18)	0.03 (-0.08 / 0.21)
MID 30m – 80m	**swm**	x	0.85 (0.07 / 1.33)	x	0.79 (0.23 / 1.09)	-2.23 (1) (SE = 0.08)	0.06 (-0.17 / 0.34)
	tyr	1.56 (1) SE = n.d.	0.62 (0.16 / 0.95)	1.84 (1) SE = 0.35	0.60 (0.24 / 0.84)	-0.28 (1) SE = 0.44	0.02 (-0.11 / 0.25)
	aeg	0.44 (1) SE = 0.82	0.56 (0.09 / 0.86)	2.95 (1) SE = 0.82	0.52 (0.12 / 0.76)	-2.51 (1) SE = 0.80	0.04 (-0.08 / 0.23)
	ion	2.08 (2) (1.51 / 2.65)	0.53 (0.22 / 1.43)	2.10 (2) (0.22 / 3.97)	0.52 (0.18 / 1.11)	-0.02 (2) (-1.32 / 1.29)	0.01 (-0.13 / 0.41)
	lev	1.48 (3) (0.24 / 2.93)	0.57 (0.29 / 1.27)	2.14 (3) (1.46 / 2.5)	0.55 (0.26 / 0.98)	-0.67 (3) (-2.26 / 1.47)	0.02 (-0.13 / 0.41)
DEEP 80m – 110m	**swm**	x	0.32 (0.01 / 0.95)	x	0.40 (0.10 / 0.80)	x	-0.08 (-0.24 / 0.18)
	tyr	x	0.38 (0.01 / 0.78)	x	0.38 (0.07 / 0.67)	x	0.00 (-0.13 / 0.23)
	aeg	x	0.29 (0.02 /0.68)	x	0.28 (0.05 / 0.59)	x	0.02 (-0.09 / 0.15)
	ion	1.27 (4) (0.07 / 3.58)	0.38 (0.03 / 0.88)	2.71 (4) (0.9 / 6.15)	0.37 (0.05 / 0.67)	-1.44 (4) (-2.75 / 0.15)	0.01 (-0.10 / 0.26)
	lev	0.16 (1) SE = 0.39	0.49 (0.14 / 1.07)	0.74 (1) SE = 0.39	0.47 (0.12 / 0.84)	-0.58 (1) SE = 0.17	0.02 (-0.11 / 0.32)

(New version of Table 3)

We also clarified the chosen metrics in the captions of Tables. In particular, we modified lines 272-273 in caption of Table 2 as:

The number N of available observations for each vertical layer is indicated in parentheses, and the uncertainties associated with the observations are estimated from the minimum-maximum range of values.

and lines 283-285 in caption of Table 3 as:

Uncertainties associated with the observations are estimated from the minimum-maximum range of values, except for the case of one single observation (N=1), for which the reference uncertainty from the literature (as standard error, SE) is indicated when available (otherwise, indicated as not determined, n.d.).

and we also deleted the last sentence of the caption, that in the previous version referred to the min/max range of the NCP observation in the southwestern basin at the surface layer, since in the new version of the table it is explicitly indicated.

We modified the expression “where observations display high standard deviations” as:

where observations display a large range of values

at lines 296-297 and the expression “with large uncertainties that make the estimate compatible with equilibrium” as:

with a range of values that makes the estimate compatible with equilibrium

at lines 301-302 and replaced the term “uncertainties” by “range of values” also at line 303.

Moreover, we modified lines 483-485 as:

and the dark-light method used in the observations considered in this work has proved to lead to GPP estimates that are on average 2-7 times higher than those yielded from methods based on ^{14}C or active fluorescence on the global scale (Regaudie et al., 2014). Moreover, while in situ CR estimations…

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

Regaudie-de-Gioux, A., Lasternas, S., Agustí, S., & Duarte, C. M. (2014). Comparing marine primary production estimates through different methods and development of conversion equations. Frontiers in Marine Science, 1, 19, https://doi.org/10.3389/fmars.2014.00019

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 and time as observations (i.e., “match-ups”). 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 for each subbasin. The procedure is explained in Salon et al. (2019). We have specified this information in the new version of the manuscript (lines 214-217):

In this case, the concentrations of modelled dissolved oxygen were extracted at the same locations and time as the BGC-Argo observations, the skill performance metrics were computed on the basis of observation-model misfits and then the overall metrics were obtained by aggregating the partial results for each subbasin (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 this comment.

We carefully considered it, and, since we focussed our paper on the summer SOM, we remain confident that performing the model/observations comparison only in the summer season can be more suitable to better characterise this feature, also considering that SOM is still in a transition and progression phase during spring months in several areas (see new Figure 7). Moreover, the evaluation of the temporal correlation between BGC-Argo measurements and model values of dissolved oxygen during the year have already been provided in Teruzzi et al. (2021). Nevertheless, if Reviewer#1 still suggests extending these metrics in time, we will follow their recommendations.

Teruzzi, A., Di Biagio, V., Feudale, L., Bolzon, G., Lazzari, P., Salon, S., Coidessa, G., & Cossarini, G. Mediterranean Sea Biogeochemical Reanalysis (CMEMS MED-Biogeochemistry, MedBFM3 system) (Version 1) [Data set]. Copernicus Monitoring Environment Marine Service (CMEMS). https://doi.org/10.25423/CMCC/MEDSEA_MULTIYEAR_BGC_006_008_MEDBFM3, 2021.

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

We thank Reviewer#1 for this suggestion. Estimated uncertainties are indeed often compared to the variability of the variable under consideration (also in Cossarini et al., 2021). We added the standard deviation referred to the summer metrics (please see the previous reply) in Table 1, here reported in the new version:

	SOM concentration [mmolO_2 m^{-3}]			SOM depth [m]		
	REF mean ± std	BIAS	RMSD	REF mean ± std	BIAS	RMSD
swm	252.1 ± 9.9	-5.2	11.0	44 ± 9	11	18
nwm	245.4 ± 9.6	1.8	9.3	39 ± 15	1	11
tyr	257.3 ± 8.6	-9.7	13.0	42 ± 10	2	9
adr	264.0 ± 2.6	-14.0	14.1	22 ± 3	3	5
ion	253.1 ± 4.1	-13.4	13.9	46 ± 17	4	16
lev	249.9 ± 4.1	-15.0	15.4	49 ± 12	12	17

(New version of Table 1)

Moreover, we added the standard deviations as further metrics in the caption of the table at lines 255-257:

Table 1: SOM concentration and depth in the Mediterranean aggregated subbasins during the July-August-September (JAS) period in January 2013-December 2019: mean and standard deviation of BGC-Argo float observations, BIAS and RMSD of the model with respect to observations.

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 provided consistent parameters for the uncertainties in the new version of the manuscript, 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 having spotted it. It is actually NPP and we corrected 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 shown the 1999-2019 trend evaluation in the SOM depth and concentration in the A-E areas in Fig. S4 reported before (and added in the Supplementary Material): according to Mann-Kendall test ($p=0.05$), trends are not significant in the selected locations, except in the case of A area (0.17 mmolO_2 m^{-3} y^{-1} and 0.28 m y^{-1} for SOM concentration and depth, respectively, Fig. S4). Moreover, we found only sparse and patchy patterns of positive/negative significant trends in the SOM features across the Mediterranean Sea, as shown in panels e-f of the new version of Figure 6 that we added in the manuscript:

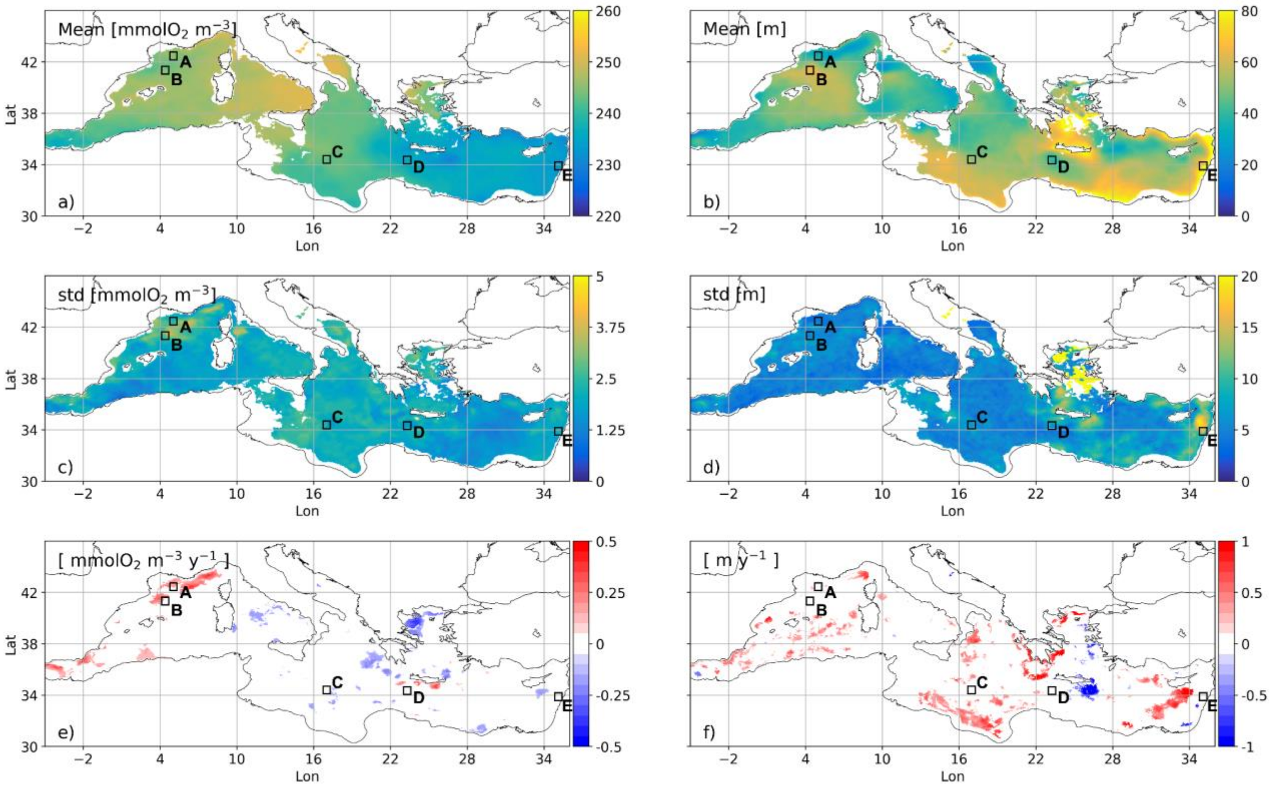


Figure 6: Mean concentration (a) and depth (b) for the modelled SOM in summer, computed by averaging the annual maximum vertical values of the mean oxygen concentration in the JAS months and their associated depths, respectively, in the period of 1999-2019; standard deviation of the annual maximum values (c) and their depths (d), in the same period; trends evaluated in concentration (e) and depth (f) by the Theil-Sen method and provided only if significant as obtained from Mann-Kendall test (p=0.05), in the same period. Areas A-E delimited by black squares are hereafter defined as follows: A = Gulf of Lion, B = Balearic front, C = central Ionian, D = western Cretan gyre, E = eastern Levantine (area). Coastal areas (i.e., with depths higher than 200 m) are masked.

(New version of old Fig. 7, now Fig. 6)

In particular, significant trends are found only in the 8 and 13% of the open-sea basin for SOM concentration and depth, respectively (Fig. 6e-f). The areas with positive and negative trends in the SOM concentration (Fig. 6e) have similar spatial coverage and their mean values are approximately equal to 0.19 and -0.18 mmolO_2 m^{-3} y^{-1}. Trends in the SOM depth (Fig. 6f) are instead mostly positive (12% coverage), with mean value approximately equal to 0.49 m y^{-1}, whereas the very sparse (1% coverage) negative trends reach a mean value of -0.71 m y^{-1}.

Overall, the sparse areas/patches of significant trends of the SOM concentration and depth (Fig.6e-f), which do not show a substantial overlapping, together with their low interannual standard deviation displayed in most of the basin (Fig.6c-d), identify the SOM as a relatively stable feature across the Mediterranean Sea during the simulated period.

We modified the text of the manuscript in lines 356-361 as:

Considering the whole basin throughout the simulation period, in Fig. 6, we mapped the mean concentration and mean depth of the SOM at each horizontal point, by averaging (in the 1999-2019 period) the annual maximum vertical values of the mean oxygen concentration and their associated depths in the July-August-September (JAS) months (Fig. 6a-b). Moreover, we evaluated both interannual variability and possible trends (by Theil-Sen method; Theil, 1950, and Sen, 1968) of SOM concentration and depth in the same period (Fig. 6c-d and Fig. 6e-f, respectively).

We also modified lines 364-366 as:

Interannual seasonal variability (Fig. 6c) accounts for a percentage lower than 2% and trends are significant (according to Mann-Kendall test, p=0.05) only in the 8% of the open-sea basin, with positive and negative values covering a similar portion (Fig. 6e) and mean values approximately equal to 0.19 and -0.18 mmolO_2 m^{-3} y^{-1}.

and we added at lines 384-387 :

In addition, just patchy areas covering 13% of the open-sea basin display significant values of trend in the SOM depth (Fig. 6f); they are mostly positive (12% coverage), with mean value approximately equal to 0.49 m y^{-1}, whereas very sparse (1% coverage) negative values correspond to a mean value of -0.71 m y^{-1}.

We modified lines 533-536 in the Discussion section as:

Moreover, since the interannual variability of summer SOM concentrations and depths (Fig. 6c-d) appears lower than the spatial variability and only sparse and patchy areas display significant trends (Fig. 6e-f), we proposed to analyse one single year rather than a temporal average, which would have masked the spatial variability of the processes.

Finally, to better connect the comments on the temporal variability of the SOM to the next part, concerning the spatial variability and the choice of the five areas, we modified lines 538-543 as:

The multiyear analysis showed that the interannual variability in the depth and concentration is quite low (Fig. 6c-d), demonstrating that the SOM is a stationary feature of the oligotrophic Mediterranean Sea. On the other hand, 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).*

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 rewrote that part (lines 72-73) 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 replaced 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 added it. The new version of the caption (lines 237-238) is:

Figure 3: Mean maps of modelled oxygen concentrations (a-d) and EMODnet_int observations (e-h) averaged in selected vertical layers (0-30 m, 30-80 m, 80-110 m, and 110-200 m) in the 16 Mediterranean subbasins in the 1999-2019 time period.

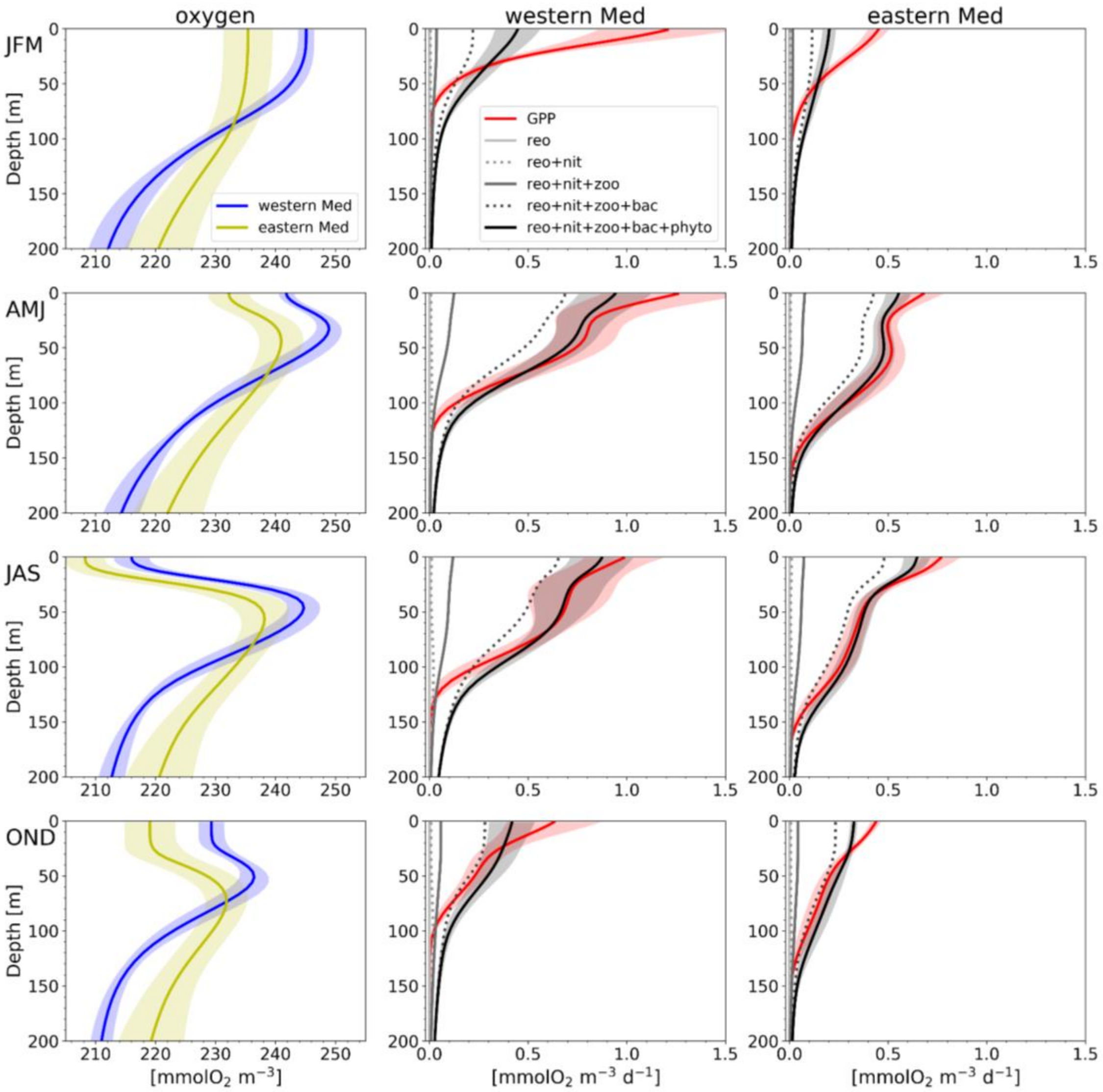
Caption of Table 3, L 274: Gazeau et al. (2021) instead of (2020), model outputs.

We thank Reviewer#1 for having noticed these oversights. We corrected them.

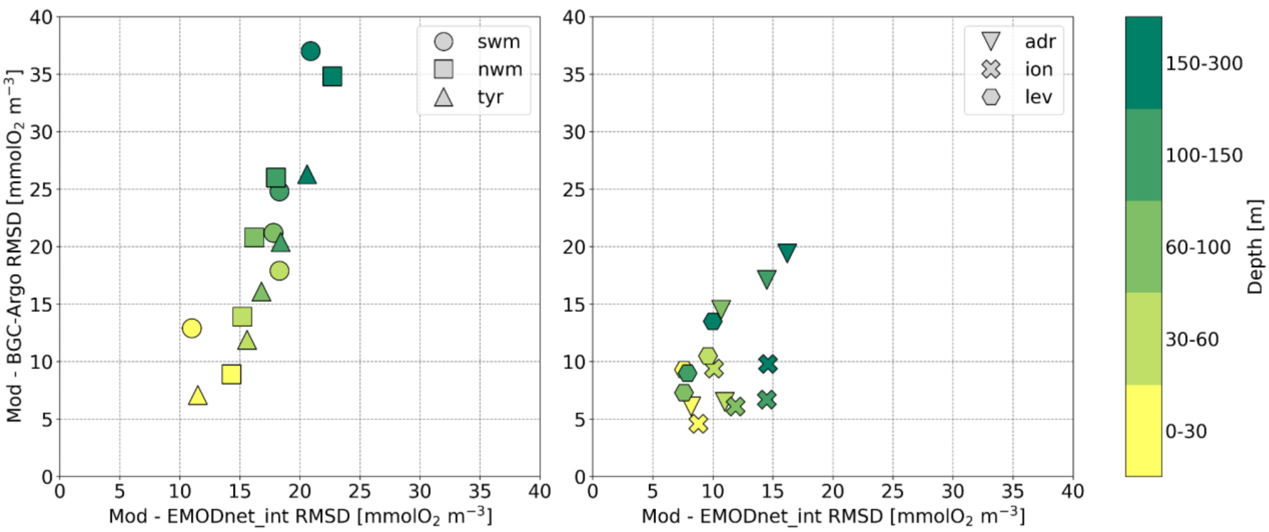
L 321, Fig. 5, and throughout the manuscript: Please specify the units: mmol C m-3 d-1 or mmol O2 m-3 d-1 instead of mmol m-3 d-1.

We agree. We specified them.

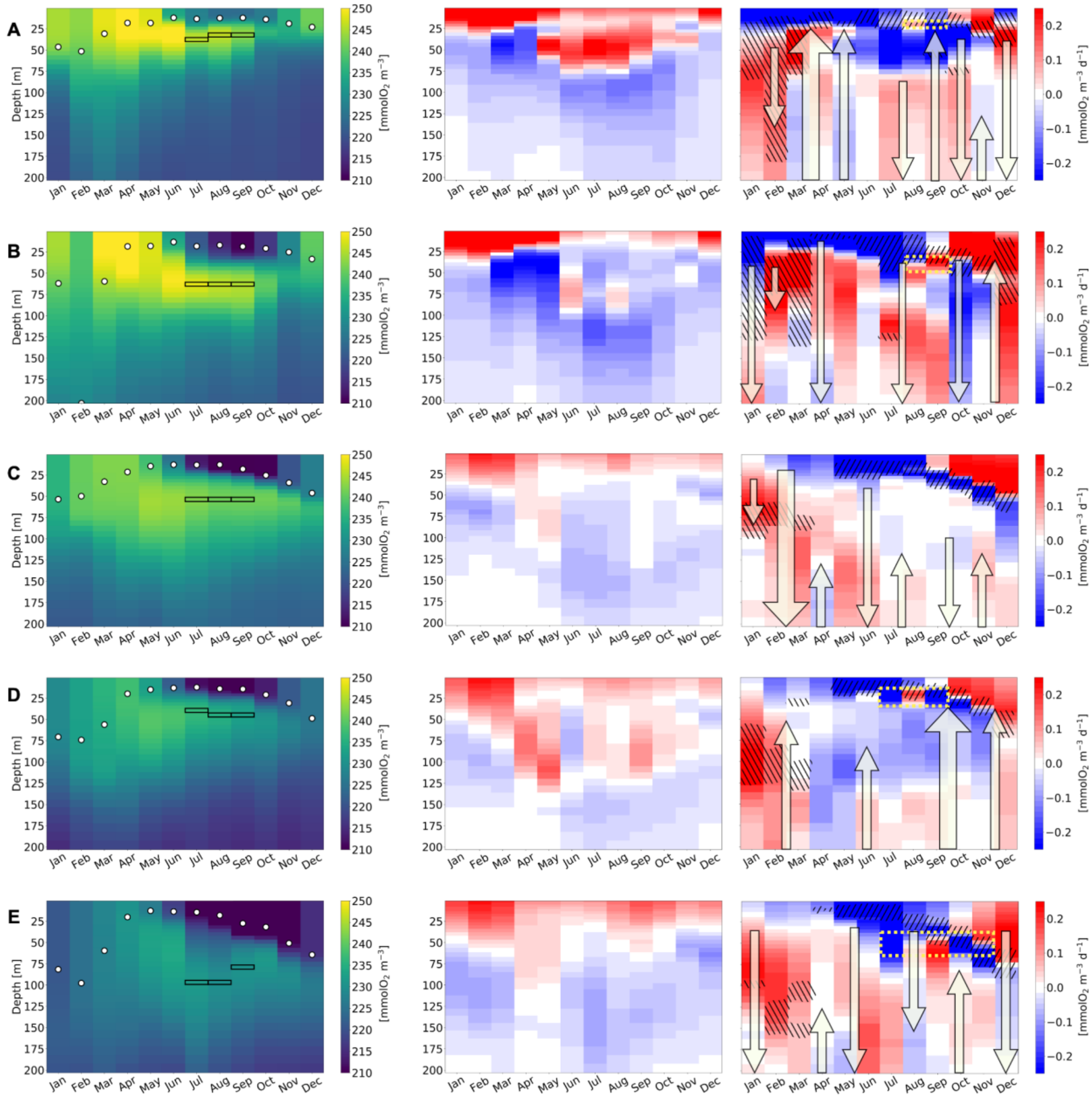
We report here the new version of Fig. 5, along with the Figs. 4 and 7, in which we also modified the unit of measurement (as well as in other figures elsewhere reported in this reply):



(New version of Fig. 5)



(New version of Fig. 4)



(New version of old Fig. 8, now Fig. 7)

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"…).

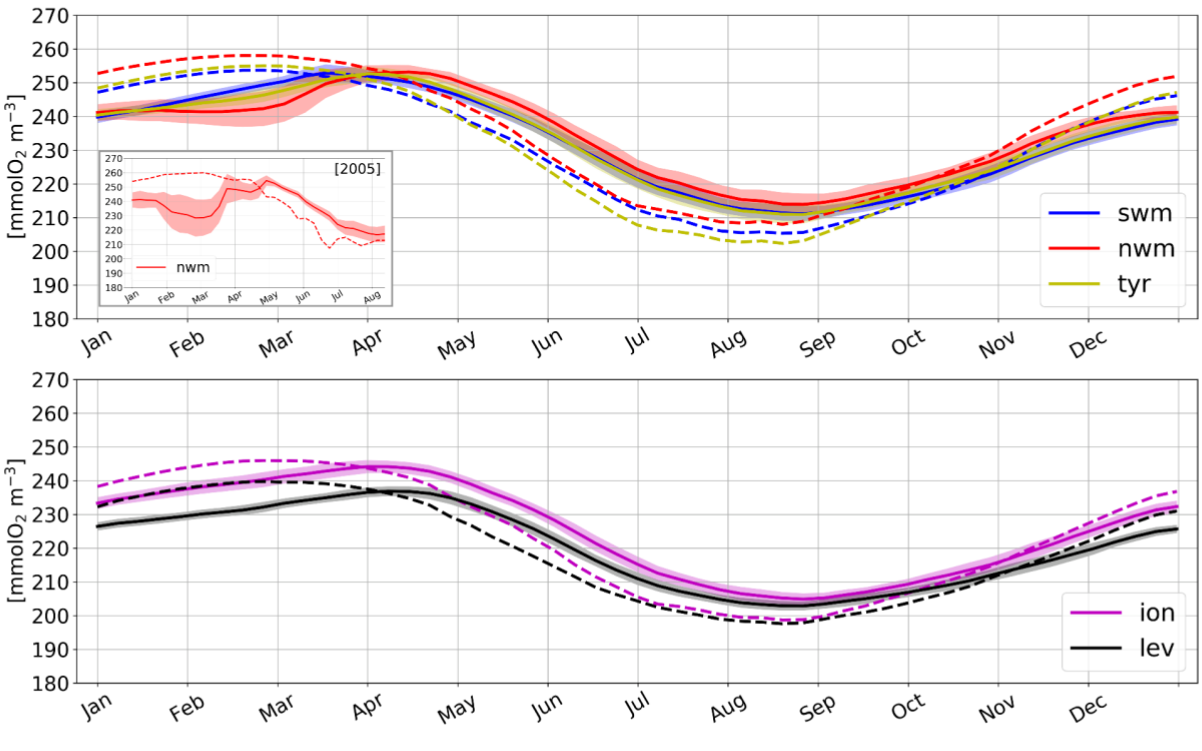
We agree. We reformulated the sentence as written in the reply to Reviewer#1's comment to lines 332-358.

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 reformulated the sentence as written in the reply to Reviewer#1's comment to lines 332-358.

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. In order to improve the readability of the manuscript and provide a clearer argumentation, we decided to move this part in the newly introduced Appendix A. Consequently, old Fig. 6 here reported was renamed Fig. A1:



(New version of old Fig. 6, currently Fig. A1, in Appendix A)

and the numbers of the subsequent figures of the manuscript have been modified accordingly.

We also report here the text of Appendix A (lines 625-640), in which we revised the sentence at lines 349-351 of the submitted manuscript as suggested, and included also modifications associated with the two previous Reviewer#1's comments:

Figure A1 shows the seasonal cycle of the model-derived surface oxygen concentration, that accounts for both biological processes (Fig. 5) and physical ones, i.e., air-sea interactions (Eqs. 1 and 2) and mixing/stratification processes.

The surface oxygen generally follows the cycle of oxygen saturation, with higher values in the western subbasins (the seasonal range is approximately equal to 210-255 mmolO_2 m^{-3}) compared to the eastern subbasins (200-245 mmolO_2 m^{-3}) due to the increasing west-east temperature gradient across the Mediterranean Sea (Escudier et al., 2021).

Undersaturated values in winter and fall indicate that the oxygen production identified by the positive NCP values in Fig. 5 (equal to approximately 0.76 and 0.25 mmolO_2 m^{-3} d^{-1} at surface in the western and eastern Med in winter) is actually less relevant with respect to the effect of cooling (which increases the oxygen solubility) and mixing with deeper waters and/or convection events, which are well-known drivers for the undersaturated surface waters in these seasons (e.g., Copin-Montégut and Bégovic, 2002; Coppola et al., 2018). In particular, the winter deviation from the oxygen saturation and the interannual variability in the nwm subbasin are much higher than those exhibited by the other subbasins (Fig. A1), given the impact of some deep convection events that occurred in the area in some years (e.g., 2005, 2013; Smith et al., 2008; Waldman et al., 2016). In particular, the decrease in surface oxygen can reach 16% with respect to the mean climatological value (box on top of Fig. A1).

On the other hand, in both the western and eastern subbasins, we observe supersaturated waters at the surface in the spring-early summer (Fig. A1), despite NCP estimations being lower than the corresponding winter values (Fig. 5).

Moreover, we modified the last sentence in Sec. 3.2.1 (lines 338-339) 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. A1 (Appendix A).

and the first sentence of Sec. 3.2.2 (lines 353-354) as:

Besides the well-known case of surface oxygen dynamics (Appendix A), also the SOM feature reflects the interplay of biological and physical processes because, as shown below, it is an emerging property driven by multiple factors.

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

We agree. We specified it at line 367 as:

On average, the SOM is located close to a depth of 40 m in the western Med and 50 m in the eastern Med (Fig. 6b).

and deleted it at the end of the next sentence.

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

We agree. We rephrased the first sentence as written in the reply of the previous comment and the second expression as:

oxygen production is dominant in February-May and consumption in June-December in all areas (not shown).

at line 423. Moreover, for the same reason we modified lines 436-437 as:

Vertical diffusive fluxes are generally downwards in January-March and upwards in May-October.

L 414: (D) areas.

We thank Reviewer#1 for having noticed this oversight. We corrected 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 rephrased this part as:

the Gulf of Lion is the only case in which the values of the subsurface oxygen derivative in summer are comparable with late winter-early spring surface values (values higher than 0.2 mmolO_2 m^{-3} d^{-1} in Fig. 7).

at lines 416-418.

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

We thank Reviewer#1 for this comment. Actually, Fig. 7 (i.e., old Fig. 8) shows that values of physical derivative at surface are not always negative in September and October (please see e.g. C-D areas); moreover, in A area they are largely negative (<-0.2 mmolO_2 m^{-3} d^{-1}) only up to June. Therefore we would prefer to maintain a more general description of such derivative values and, thus, not to modify the sentence.

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

We agree. We removed the word.

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

We agree. We rephrased the sentences by using (at line 448-449):

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

and (line 580)

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 having noticed this oversight. We corrected 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 modified this expression.

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

We agree. We added it.

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

We thank Reviewer#1 for having noticed this oversight. We corrected it.

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 having noticed this oversight. We replaced this reference by Yasunaka et al., 2021 (already cited in the manuscript).

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

We thank Reviewer#1 for having noticed this oversight. In the previous version of the manuscript we had indicated old Fig. 7b (now Fig. 6b, showing the SOM depth), but actually here we was referring both to the SOM concentration and depth, thus we modified the sentence at lines 521-522 as:

Our analyses (Figs. 6a-b) show that the SOM has a large spatial heterogeneity across the Mediterranean

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 preferred to rephrase this sentence by deleting the consideration on the strong vertical mixing as follows (lines 575-576):

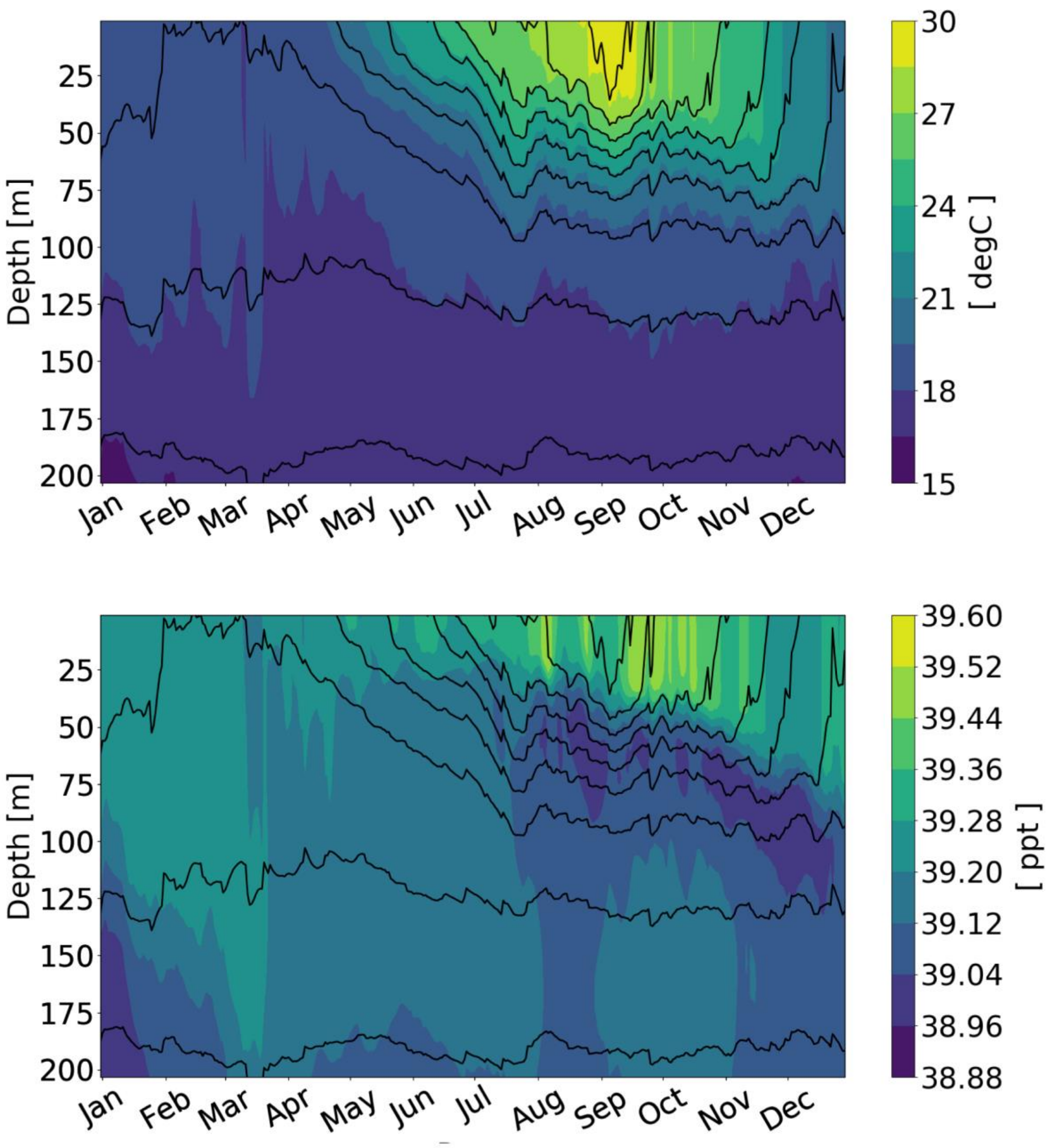
In particular, the northwestern Mediterranean areas (cases A and B, Fig. 7) are very productive and in summer the vertical level of their biological production practically coincides with the DCM (Fig. S3).

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

We thank Reviewer#1 for having noticed this oversight. We corrected 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 indicated the months in the x-axis in the new version of the figure here reported and modified in the Supplementary Material:



(New version of Fig. S1)

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 corrected the caption of Fig. S2 in the Supplementary Material as:

Hovmöller plot of net primary production …

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

We agree. We modified the figure and its caption in the new version of the Supplementary Material as reported here:

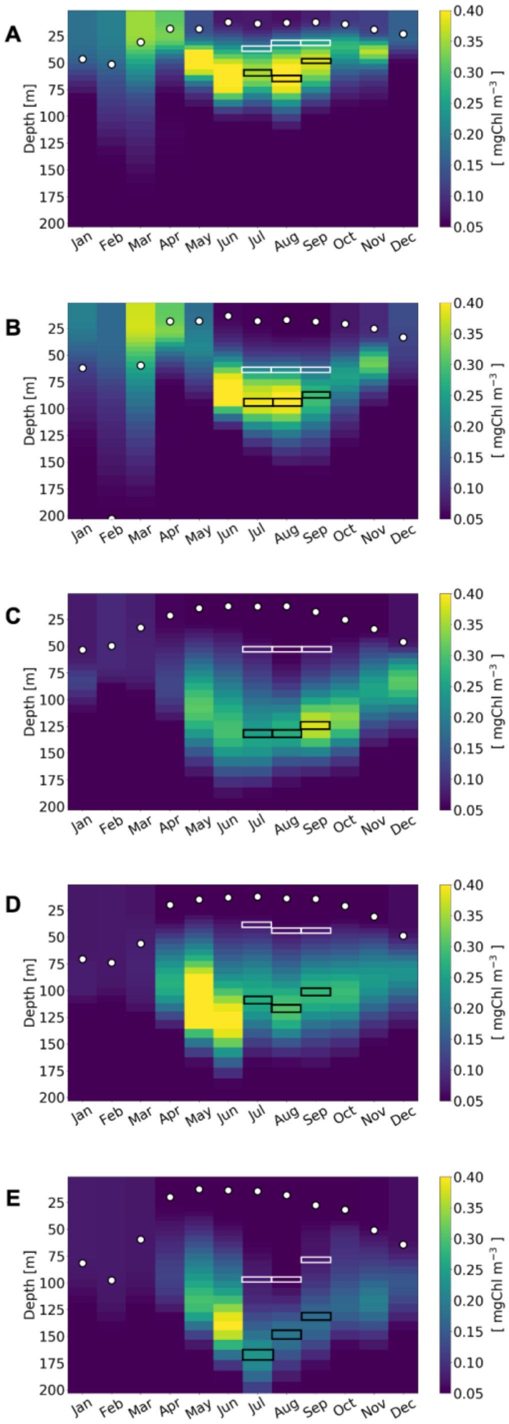


Figure S3: Hovmöller plot of mean model-derived monthly chlorophyll concentration in the Mediterranean areas indicated in Fig. 6 in 2014. White circles indicate the depth of the mixed layer, black and white rectangles the depth of DCM and SOM in the summer period (JAS months), respectively.

(New version of Fig. S3)

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

Author responses to Reviewer#2's comments on the manuscript:

The authors analyzed an existing coupled physical-biogeochemical model and mapped the subsurface oxygen maximum concentration and depth in the Mediterranean Sea. They proposed SOM to be a suitable feature in oligotrophic seas to evaluate and monitor the ecosystem state.

This manuscript is very well written and structured, and its topic is interesting enough. However, I have one major concern. The model applied data assimilation for biological variables. How would this artificial factor impact vertical structures of biological variables and their budget analysis? Does their present conclusion/result still stand? This concern has to be addressed to insure that the analysis is meaningful and make their story convincing.
Citation: https://doi.org/10.5194/bg-2022-70-RC2

We are very thankful for the Reviewer#2's overall comment on the manuscript, which gave us the opportunity to discuss an important aspect and to improve our article.

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, we would like to remark that the biogeochemical reanalysis assimilates satellite chlorophyll observations and not oxygen vertical 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 is not influenced directly by the data assimilation procedure. In other words, data assimilation does not directly introduce sources or sinks of oxygen content in the seawater.

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). We are aware that a hindcast simulation could have been produced to verify the impact of the physical and biogeochemical assimilation on oxygen dynamics and budget. However, we would 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 a 30-80 m layer hosting the deep chlorophyll maximum (Di Biagio et al., in review).

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 (see Cossarini et al., 2019 - Fig. 11 - who analysed the time scales of the biogeochemical model adjustment after assimilation).

To conclude, we have added to the manuscript a synthetic version of this reply in the Discussion section (lines 541-546):

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). Since data assimilation procedure does not directly affect the oxygen budget, the latter is closed and consistent. Moreover, the budget has been computed on monthly means of dissolved oxygen, 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). We found that…

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.

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