Comment on bg-2022-70
Anonymous Referee #1

Referee comment on "Subsurface oxygen maximum in oligotrophic marine ecosystems: mapping the interaction between physical and biogeochemical processes" by Valeria Di Biagio et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-70-RC1, 2022

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.
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”?
- 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?
- 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?
- 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.

**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)?
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.

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?

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.

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

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.

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.

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.
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?

3. Technical comments

**L 43, 47, 78, 244:** “a SOM” instead of “an SOM”.

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

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

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

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

**L 321, Fig. 5, and throughout the manuscript:** Please specify the units: mmol C m\(^{-3}\) d\(^{-1}\) or mmol O\(_2\) m\(^{-3}\) d\(^{-1}\) instead of mmol m\(^{-3}\) d\(^{-1}\).

**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”...). 

**L 349:** Please specify “in winter” and “at the surface” in “(equal to approximately 0.76 and 0.25 mmol m \(^{-3}\) d \(^{-1}\) ...)”
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.

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

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

L 414: (D) areas.

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.

L 433: “and negative values in summer” □ high negative values between May and October

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

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

L 454: reference to “Fig. S1” instead to “Fig. S2”?

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”

L 466: I suggest adding “summer” before “SOM”.

L 475: “Tables 2 and 3” instead of “Table 3”.
L 515: Mignot et al. (2014) described the variability of deep chlorophyll maximum. Are they also describing SOM variability?

L 517: “SOM depth” instead of “SOM”

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

L 567: reference to “Fig. S3” instead to “Fig. S4”?

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

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

References: