Oxygen budget for the North-Western Mediterranean deep convection region

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Responses to the comments of the anonymous Reviewer 3

First we would like to warmly thank the anonymous Reviewer 3 for his relevant and constructive comments which will help to improve the manuscript.

Answers to the reviewer comments are reported point by point. The questions and comments of Reviewer 3 are in *blue*, the answers in black and the modifications proposed in the revised manuscript in italic *black*.

1 General

The manuscript provides a detailed quantitative assessment of the preponderant contribution of dense water formation at the Gulf of Lion in the oxygenation of Mediterranean intermediate and deep waters, focusing on a particular year (Sep 2012-Sep 2013) and on the basis of high level numerical modelling (ie. coupled 3D, high resolution model). Adding to the fact that the precise quantification of oxygen budget in this context (transport and sink/source terms) is a very timely topic (given the potential reduction of such ventilation events in the coming century), the manuscript is very well written, and succeed in handling the complexity of numerical modelling tools with accurately targeted analyses, providing a clear and accessible result and discussion sections, as well as robust and highly relevant conclusions. I warmly recommend the publication of the manuscript, and only report below a few minor comments or suggestions.

Reply: We appreciate this positive general assessment.

2. Main Comments

Sect. 2.1.1 Given the high importance of this technical aspects for the main conclusion, I would add a sentence on the diffusion and advection scheme used in Symponie (in this particular implementation).

Reply: We agree with Reviewer 3, information on the point was missing in the manuscript. In the revised manuscript, we will specify the schemes of advection and diffusion used for this simulation in a new Sect. 2.1.3 (see response to the next comment) as follows:

"The advection and diffusion of the biogeochemical variables were calculated using the QUICKEST (QUICK with Estimated Streaming Terms) scheme (Leonard, 1979) on the horizontal and with a centred scheme on the vertical. "

We will also add details on diffusion in the model in Sect. 2.1.1 "The hydrodynamic model":

"The vertical diffusion is parameterized with a prognostic equation for the turbulent kinetic energy and a diagnostic equation for the mixing and dissipation lengths, following Gaspar et al. 1990. As explained in Estournel et al. (2016a), the size of the grid is not small enough to explicitly represent convective plumes, which thus need to be parameterized. In our case, to prevent the development of static instabilities at the surface resulting in noise at the scale of the mesh, the heat and water fluxes are distributed over the whole mixed layer whose thickness is given by the depth at which the vertical density gradient becomes negative."

L150-158 The architecture of the different model nesting and interactions, did not appeared entirely obvious to me, at first read. I would suggest a second panel to Fig1. providing a scheme of model interactions, eg. with boxes for each 4 models (NEMO, Symphonie, Basin bio, NW bio) giving temporal and spatial resolution, and mostly, arrows precising the nature of interactions (but i understand it's all offline). This is a mere suggestion to help the reader. According to the author's appreciation, an alternative would be to rework slightly this section to ensure clarity.

Reply: We apologize for the lack of clarity in the description of the downscaling implementation. In the revision, we will rework this section on the description of the coupled model: we will add a new sub-section 2.1.3 dedicated to the description of the particular configuration used for this study. The description of the forcing of the hydrodynamic and biogeochemical models will be transferred from Sect. 2.1.1 and 2.1.2, respectively, to this new Sect. 2.1.3. As suggested, we will make clearer the downscaling strategy at the beginning of this new Sect. 2.1.3 as proposed here:

"*A strategy of downscaling from the Mediterranean basin to the western sub-basin scale was implemented in three steps as described by Kessouri et al (2017). In a first step, the SYMPHONIE hydrodynamic model was initialized and forced at its lateral boundaries with daily analyses of the configuration PSY2V4R4 based on the NEMO ocean model at a resolution of 1/12° over the Atlantic and the Mediterranean by the Mercator-Ocean International operational system (Lellouche et al., 2013). Second, the biogeochemical model was forced at the Mediterranean basin scale by the outputs of the same NEMO simulation. In a third step, the outputs of the two previous simulations were used to initialize and force the Eco3M-S biogeochemical model over the western Mediterranean Sea."*

We are working on Figure 1 to add a second panel in this figure with explanations or a scheme of model interactions.

Fig9, suggestion It seems to me that it would be relevant to add a panel to Fig. 9, indicating the biogeochemical term (VS time and depth). The vertical distribution of this term is adressed several time in the discussion, and would benefit in my opinion from a dedicated figure.

Reply: For the sake of simplicity we would like to avoid adding a new panel in Figure 9. Figure 1 in this response shows the vertical distribution of the biogeochemical term. We think that the current figures are sufficient to illustrate the text and that it would be difficult to include this new figure without more comments.

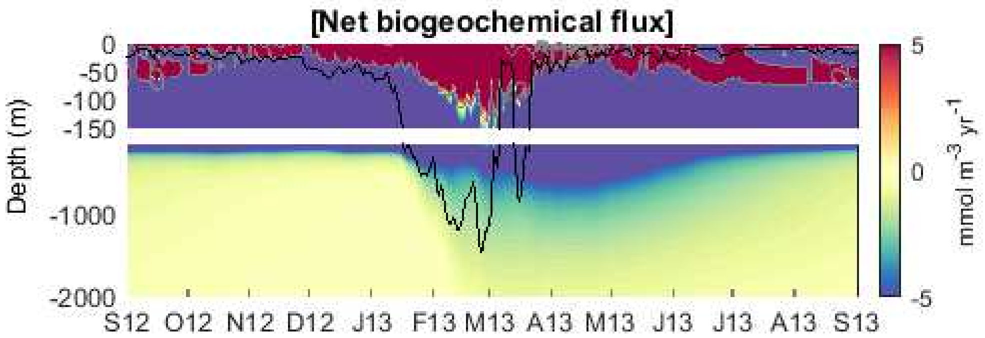

Figure 1: Time evolution of the net biogeochemical production of oxygen (mmol m^{-3}yr^{-1})

"Biological Flux", suggestion As Eq.1 includes nitrification (which appears as an important component of the "biological flux", as discussed in Sect. 6.3), i wonder if it should not be called "biogeochemical flux" rather than "biological flux", in general and through the manuscript.

Reply: In the revised manuscript, we will replace the term “biological” by “biogeochemical” in the text and in Figures 6 and 8.

L467 Something disturbs me between the sentence 463-466 and the next sentence 466-467. The first states "at the annual scale downward export below the euphotic zone ranges from 22.2 to 27.6 mol m−2 yr−1 ". The second states ,essentially, "During the convection, downward export below the euphotic zone ranges from 14.3 to 18.7 mol m−2 yr−1 ". Does the second sentence characterizes the part of the annual flux that takes place during the convection event ? Why a yr−1 unit then ? Please clarify.

Reply: We apologize for this error: the values correspond to the amount of oxygen in the surface layer that is exported below this layer during the convection event. We will correct this error in the revised manuscript by replacing these values with the values of downward export flux in mmol m^{-2} day^{-1} (as we would like to include the results of two new tests to reply to the first comment of Reviewer 2, values of export flux will change compared those in the submitted version);

“The downward export below the euphotic zone over the deep convection period ranges from 223 to 302 mmol m^{-2} day^{-1} (mean value: 265 ±30 mmol m^{-2} day^{-1}).”

lateral export term It appears important to me the fact that the lateral export term in the upper layer is high, and significant in regards to atm. fluxes and local BGC net oxygen production. This indicate that the deep convection event acts as a conveyor of oxygen produced in the surface layer of surrounding areas to the deep mediterranean, and not only as a conveyor of "local oxygen". In my opinion this point should be better highlighted in the conclusions. Eventually, this aspect could be sustained with an additional panel to Fig 9, showing the vertical distribution (along time) of the lateral fluxes, but this last point is really a mere suggestion left open to the author’s appreciation.

Reply: We agree that the lateral inputs of oxygen from the surrounding areas are significant when compared to the biogeochemical production or consumption term of the budget. As suggested by the reviewer, to underline this point, we will add a discussion in the Sect. 6.2 (The role of the NW deep convection area in the ventilation of the western Mediterranean Sea) and will modify the third point of the conclusion:

In Sect. 6.2:

“Lateral O_2 inputs in the surface layer occurred mainly from February to September with two peak periods, early March, a calm period between two convective events and during restratification in April. These imports are mainly related to eddies produced by the baroclinic instability that is triggered at the periphery of the convection zone when strong wind ceases (Killworth, 1976; Testor et al., 2018). These inputs from the peripheral zone contribute to the vertical export of oxygen to the aphotic layer. Firstly in the short term, the oxygen imported between two convection events is exported at depth by the following events. At longer time scales (May-September), the convection zone is also fed by the peripheral zones and in turn produces a vertical export to the aphotic layer. These exchanges are of lower intensity and concern shallower layers but are not negligible when integrated over the year. ”

In the conclusion:

“The NW Mediterranean deep convection area acts as a conveyor of atmospheric oxygen, as well as oxygen produced locally and in the surrounding areas in the surface layer towards the intermediate and deep layer of the whole western Mediterranean Sea.“

Again, for the sake of simplicity, we would like to avoid adding a new panel in Fig. 9, considering the complexity of the processes involved in the lateral transport and the spatial heterogeneity along the boundary of the convection zone. We think that the study of the physical processes involved and the vertical and horizontal redistribution produced is beyond the scope of this paper but would justify further studies.

3. *Minor Comments*

L131 γC/DOc → γC/DOx

Reply: This will be corrected as suggested in the revised manuscript.

L132 mol → mole

Reply: This will be corrected as suggested in the revised manuscript.

L212 yk o , should be described in the previous line, with yk m . It is currently not explained.

Reply: This will be corrected as suggested in the revised manuscript.

L213 the Root is mising in the definition of NRMSE. Also when used in the text, it is given in percentage, so maybe indicate a "100x" and "%" as is done for PB in the same line.

Reply: We apologize for this error. This will be addressed as suggested in the revision.

L220 for readibility please favor, after the coma, "as well as modelled time evolution ... during the winter that are close to the observations".

Reply: This will be corrected as suggested in the revision.

L224 "[The model is able to reproduce] the deep chlorophyll maximum". Can the authors be a bit more specific , eg. the depth of the DCM, or its location, or timing or dynamics, or ..?

Reply: In the revised manuscript, we will specify what the model reproduced in the deep chlorophyll maximum as suggested:

"These studies showed that the model is able to accurately reproduce [...] the dynamics and depth of the deep chlorophyll maximum during the stratified, oligotrophic period."

Finally, we would also like to point out that we have found an error regarding the trajectory and the name of the float for which the temporal evolution of the oxygen content is shown in Figure 5b. We apologize for this error that will be corrected in the revised manuscript.

References:

Killworth, P.: The mixing and spreading phase of Medoc 1969. Progress in Oceanography, 7, 59–90, 1976.

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