

Interactive comment on “Oxygen budget for the north-western Mediterranean deep convection region” by Caroline Ulses et al.

Anonymous Referee #3

Received and published: 9 September 2020

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

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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.

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 Symphonie (in this particular implementation).

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 precisising 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.

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.

"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.

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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.

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.

3 Minor Comments

L131 $\gamma_{C/DOc} \rightarrow \gamma_{C/DOx}$

L132 mol \rightarrow mole

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

L213 the Root is missing 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.

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

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 ..?

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-277>, 2020.

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