Seasonal and interannual variability of the pelagic ecosystem and of the organic carbon budget in the Rhodes Gyre (Eastern Mediterranean): influence of winter mixing

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Responses to the Reviewers' comments

Answers to reviewers' comments are reported point by point. The questions and comments of the reviewers are in blue, the answers in black and the modifications that we made in the revised manuscript in red. It should be noted that we have modified the order of the co authors.

Responses to the comments of the anonymous Reviewer 1

First we would like to warmly thank Reviewer 1 for her/his relevant and constructive comments which helped to improve the manuscript.

Review of Manuscript "The manuscript presents the results of a 3D coupled hydrodynamic-biogeochemical model over 2013-2020 period, investigating the seasonal and inter-annual variability of the pelagic ecosystem in Rhodes Gyre, a known site of Levantine Intermediate Water (LIW) formation. Given the lack of other 3D biogeochemical modelling studies in this area, the manuscript presents an important scientific interest. The biogeochemical model results are thoroughly validated and an extensive analysis is provided, adequately discussing different aspects of ecosystem dynamics, with regard to the impact of vertical mixing on phytoplankton growth, organic carbon fluxes and implications for carbon sequestration. Therefore, I recommend the manuscript acceptance for publication, following a minor to moderate revision, according to the comments listed below (in order of appearance)."

Reply: We appreciate the positive assessment of Reviewer 1.

L59 "Other studies have also reported LIW formations in the Gulf of Antalya (Sur et al., 1992; Kubin et al., 2019; Fach et al., 2021), in the southeastern margins of the basin or along the continental margins of the totality of the Levantine Basin (Brenner et al., 1991; Lascaratos et al. 1993; Özsoy et al. 1993)." You could include the Cretan Sea, as a potential site for LIW formation, following Taillandier et al. (2022).

Reply: This has been added as suggested in the revised manuscript.

L192 "The position of the region respecting the set of criteria varying from year to year, we chose the smallest outline in order to cover the most of the gyre during the period of study. Not totally clear how you specify the "smallest outline". Please explain.

Reply: To define the Rhodes Gyre area we tested two methods: the first definition was based on the modeled density anomaly averaged over winters (from January to mid-March) over the period 2014-2020 and set a minimum threshold at 28.8 kg m^{-3}, and the second one was based on the modeled sea surface height (SSH) with the criteria of -0.3 m used by Kubin et al. (2019). Finally, we chose the density-based definition because it seems to be generalizable to all observations and models while the value of the SSH is not accessible for observations and is also not common to all models as they can have a different reference. That said, we observed that the isocontour -0.3 m of SSH is very close to the isocontour 28.8 kg m^{-3} for density (slightly wider, see Figure 1.1 below). We are conscious that the explanation was confusing in the submitted version. We have removed the sentence L192 and modify the sentences to specify only the definition of the study area that we chose:

In this study, we defined the Rhodes Gyre area based on modeled surface density. The winter mean surface density was calculated between January and mid-March, the period generally associated with deep vertical mixing (Malanotte-Rizzoli et al., 2003). The Rhodes Gyre was defined by the area where the density anomaly is above 28.8 kg m^{-3}.

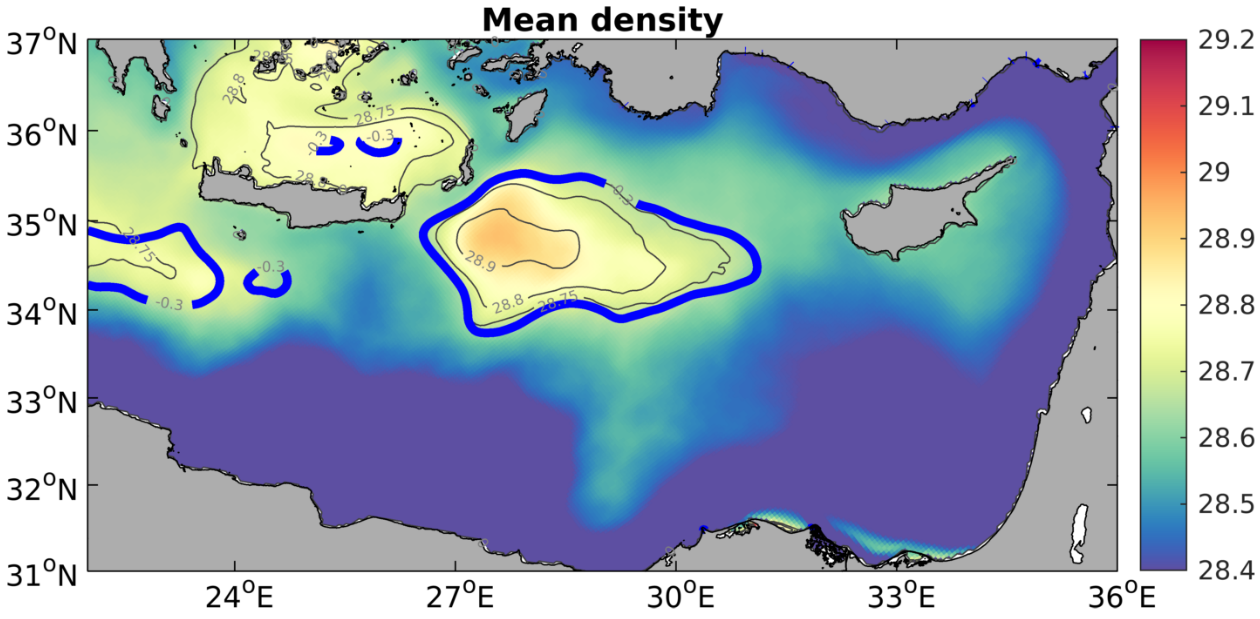


Figure 1.1 Surface density anomaly (kg m^{-3}) averaged for the period January-mid-March between 2014-2021. The blue line represents the SSH contour (-0.3m), black contours represent the densities of 28.75, 28.8, 28.9 kg m^{-3}.

L206 " The physical contribution of the budget is divided into two components: lateral transport and vertical transport, both due to mixing and advection." "In the lateral transport do you consider also horizontal mixing? Because Eq.S3 probably refers only to advection by currents"

Reply: The net lateral transport is calculated with the QUICKEST numerical scheme (Leonard, 1979) and includes the contribution of advection and diffusion processes. This was specified in a new version of the Supplementary Material as follows:

$$F_{OC,lat} = \iint_A OC^* (x, y, z, t)\, v_t(x, y, z, t)\, dA \qquad \text{(S3)}$$

where v_t is the current velocity normal to the limit of the Rhodes Gyre area, A is the area of the vertical section limiting the Rhodes Gyre and extending from the base of the upper layer (150 m) to the surface, and where OC* is a function of OC which results from the use of the QUICKEST advection scheme (Leonard, 1979):

$$OC^* = OC - \frac{Dx}{2} Cr\, GRAD - \frac{1}{3}(1 - Cr^2)\, CURV \qquad \text{(S4)}$$

with Dx is the grid horizontal resolution, CURV and GRAD are given by Leonard (1979) and *Cr* is the Courant number.

L265 "Figure 2 shows the temporal variation of the satellite and modeled surface chlorophyll averaged all over the Levantine Sea" Perhaps you could also show the model/satellite comparison for the Rhodes Gyre area. This would highlight the difference of this area, as compared to the Levantine basin and also validate the simulated inter-annual variability.

Reply: Figure 2 was modified following Reviewer 1 suggestion, as Figure 1.2:

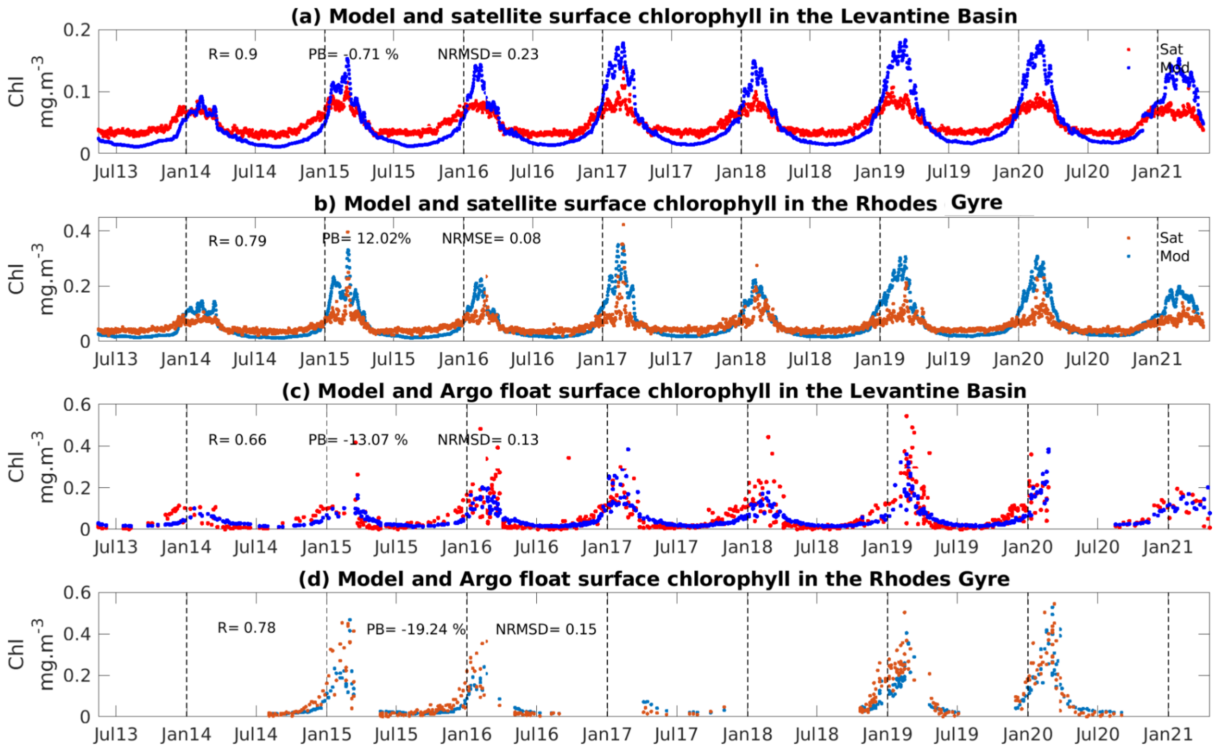


Figure 1.2 (Figure 2 of the new version of the manuscript): Time series of ~~(a)~~ modeled (in blue) and satellite (in red) surface chlorophyll-a concentration (mg m^{-3}) averaged over (a) the Levantine Basin and (b) the Rhodes Gyre. Time series of modeled (in blue) and BGC-Argo float (in red) surface chlorophyll-a concentration (mg m^{-3}) (c) in the Levantine Basin and (d) in the Rhodes Gyre. Coefficient correlation (R), percent bias (PB) and Normalized Root Mean Square Deviation (NRMSD) between model outputs and observations are indicated in (a), (b), (c) and (d).

The statistics have been recalculated for the two distinct areas, the Rhodes Gyre and the surrounding Levantine Sea. We modified the manuscript with the new statistics and to comment on the new figure:

The model captures the seasonal dynamics of the observed satellite chlorophyll over all the Levantine Basin (Fig. 2a) and more particularly in the Rhodes Gyre (Fig. 2b). At the end of fall, the chlorophyll concentration begins to increase progressively and reaches its maximum in February/March, with higher maxima in the Rhodes Gyre compared to the surrounding Levantine Sea, in both the data and the model. The surface concentration is minimal in summer, for both the model and satellite. The model and satellite show differences in magnitude: in the model the winter maximum is generally higher, and the summer minimum values are lower, compared to the satellite data for both regions. The standard deviation (SD) of the model in the Levantine Basin (0.04 mg Chl m^{-3}) and the Rhodes Gyre (0.07 mg Chl m^{-3}) is close to the mean chlorophyll concentration (0.05 mg Chl m^{-3} and 0.06 mg Chl m^{-3}, respectively) which underlines the high variability of this oligotrophic system. The mean surface chlorophyll concentration in the satellite data for the Levantine Basin and the Rhodes Gyre is 0.05 ± 0.02 and 0.06 ± 0.02 mg Chl m^{-3}, respectively. We obtain a highly significant correlation coefficient equal to 0.90 and 0.79 (p-value < 0.01), and low values for the NRMSD (an error of

23% and 8%) and percent bias (-0.71%, 12%), between model outputs and satellite data over the whole study period for the Levantine Basin and the Rhodes Gyre, respectively.

Regarding the comparison with BGC-Argo float data in the Levantine Sea (Fig. 2c) and the Rhodes Gyre (Fig. 2d), the model reproduces correctly both the seasonal cycle and the magnitude of chlorophyll during the different periods of the year. Both model and float data show high variability in late winter/early spring in the Rhodes Gyre in agreement with previous studies (D'Ortenzio and Ribera d'Alcalà, 2009; Salgado-Hernanz et al., 2019; Kotta and Kitsiou, 2019). The statistical metrics show a significant correlation equal to 0.66 (p-value <0.01) between the observed and modeled values in the Levantine Sea. The NRMSD is equal to 13% and the percent bias remains low (-13%). Similar statistical scores were obtained between the model outputs and the float data in the Rhodes Gyre, i.e. correlation (0.78, p-value <0.01) as well as low bias (-19%) and NRMSD (15%). The difference between the comparisons of model results with satellite data and those with BGC-Argo float data could be attributed in part to an underestimation of satellite chlorophyll concentration during winter in the Levantine Sea as suggested by Vidussi et al. (2001) and reported by D'Ortenzio et al. (2021).

L281 "NRMSD (23%)" I guess RMSD is normalized with data STD? Meaning that the error is 23% of the data STD. You could clarify this in the figure caption and/or text

Reply: We apologize for the typographical error in the equation of the normalized RMSD in the submitted manuscript. The equation for the statistical metric is: $NRMSD = \frac{\sqrt{\sum_{k=1}^{K} (Obs - Mod)^2}}{(Max\ obs - Min\ obs)}$ (Otto, 2019). This was corrected in the revised manuscript.

L299 "The model reproduces the general features of the nitrate and phosphate concentration profiles with an increase from the surface to 500-1000 m and a gradual, low decrease below (Fig. 3)." Given that the model is initialized from observation profiles, isn't that expected? Perhaps it would be more meaningful if the comparison focuses on the surface layer, as with Chl. Also it is mentioned (section 2.1.3) that nutrients are initialized from summer CARIMED profiles, while summer nutrients are not included in Fig.3

Reply: First, we apologize for the lack of clarity concerning the initialisation of the biogeochemical simulation. For the initialization of the nutrients we used the observations gathered in the CARIMED database (Alvarez et al., 2019) and averaged them in each sub-region defined in Fig. S2. Generally we used the observations collected during summer periods before 2012, when the data were available, but in the Levantine Sea, where no summer data was available in the dataset we used spring observations before 2012. The manuscript was modified as follows:

We initialized the biogeochemical model with observation profiles during the stratified period averaged over 10 regions of the Mediterranean Sea (indicated in Fig. S2). For inorganic nutrients profiles, we used the CARIMED (CARbon, tracer and ancillary data In the MEDsea) database (Àlvarez et al., 2019; see Sect. 2.2.2), considering only summer data over the period 2011-2012, when data were available. Due to the lack of summer observations for the Levantine region, we used spring observations over the same period.

We agree that it is expected that nutrient concentrations simulated in the deep layers over the period 2013-2020 are close to the nutrient concentrations imposed at the initialization. The assessment of the model results was performed with different in situ data than the ones used for initialization as recommended by Robson (2014) and Hispey et al. (2020). The comparisons with data over the period 2013-2020 show that the model is quite stable in the deep layers over the 9-year analysis period; the slight discrepancies between model results and observations could be explained by spatial variabilities. In the revised manuscript, we followed the suggestion of Reviewer 1, focus on the seasonal variations of the nutrient profiles in the surface and modify Figure 1.3 as follows:

The model reproduces the general features of the nitrate and phosphate concentration profiles with an increase from the surface to 500-1000 m and a gradual, low decrease below, close to those of the profiles imposed at the initialization, showing a stability over the simulation period (Fig. 3). The modeled phosphate ~~and nitrate~~ concentrations in the transitional layer (500-1000), located between the intermediate and deep layers, are in the lower range values of observations. The surface nutrient profiles show low concentrations from the surface to 50 m during winter while in spring and fall the layer depleted in nutrients reaches 100 m, in both observations and model outputs.

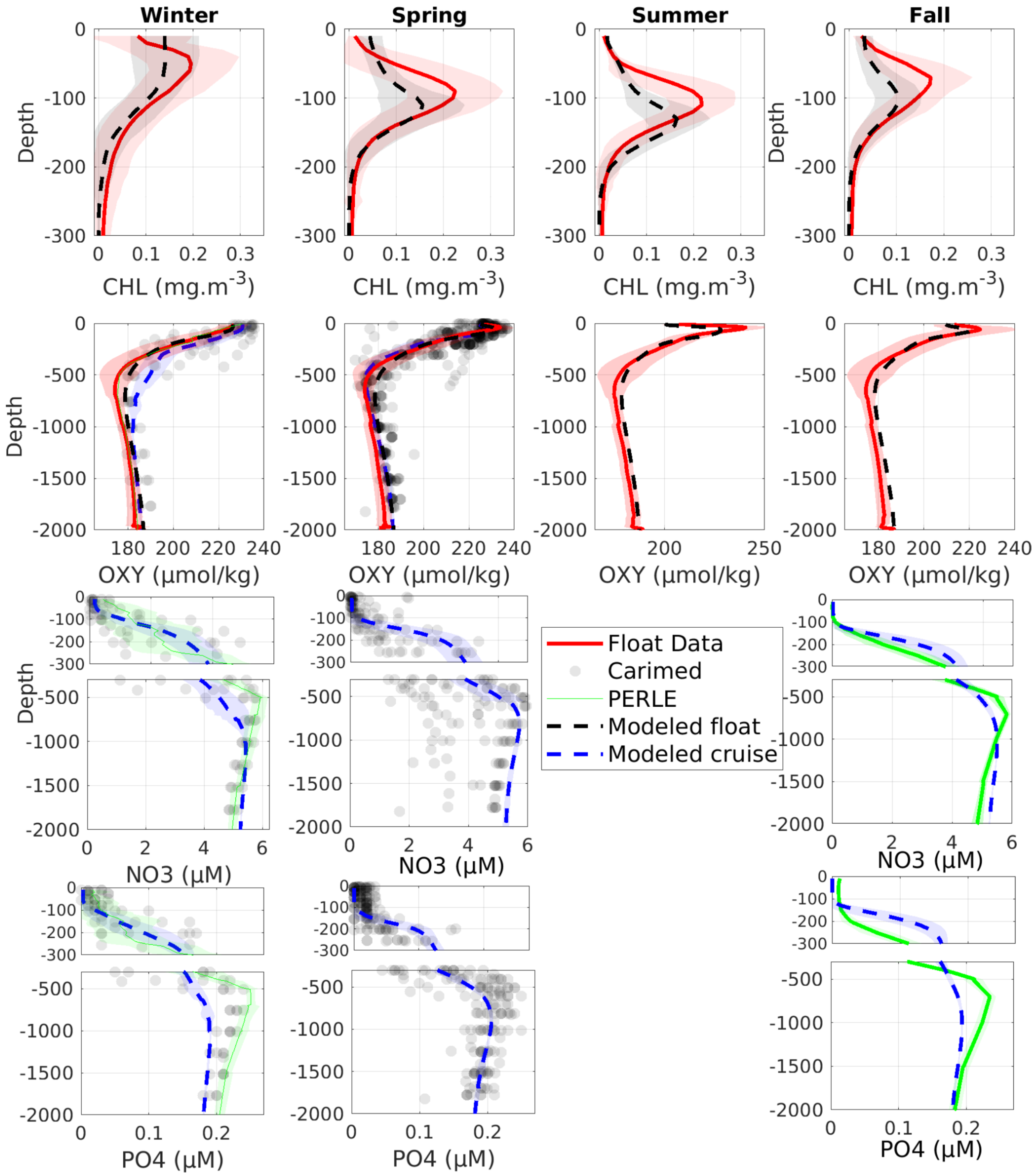


Figure 1.3 (Figure 3 of the new version of the manuscript): Comparison over the Levantine Sea between observed (gray points for CARIMED, green lines for PERLE1 and PERLE2, red line for BGC-Argo float data) and modeled (blue and black lines) profiles of chlorophyll (mg Chl m-3), dissolved oxygen (µmol kg-1), nitrate (µM) and phosphate (µM) concentrations, averaged by season (winter: 21 December to 20 March, spring: 21 March to 20 June, summer: 21 June to 20 September, fall: 21 September to 20 December). Shaded areas represent standard deviation.

L300 “The modeled phosphate and nitrate, concentrations in the transitional layer (500-1000), located between the intermediate and deep layers, are in the lower range values of observations.” This can be seen mostly for phosphate.

Reply: We agree with Reviewer 1, due to the high spatial variability for the nitrate in spring, the modeled nitrate concentrations between 500-1000 m seem to be located in the upper range values of observations. Therefore, we removed “nitrate” from the sentence.

L306 “PERLE 1 and PERLE 2 phosphate observations show high variability, with a SD ~ 0.065 and 0.062 mmol P m -3 respectively. “ The Taylor diagram seems a bit confusing. Blue dots should normally be the data points (correlation=1), not the model.

Reply: Following Reviewer 1 suggestion, in the revised manuscript we positionned the observations on the x axis with a correlation coefficient=1. We would like to point out that we also changed the centered RMSD into RMSD for simplicity.

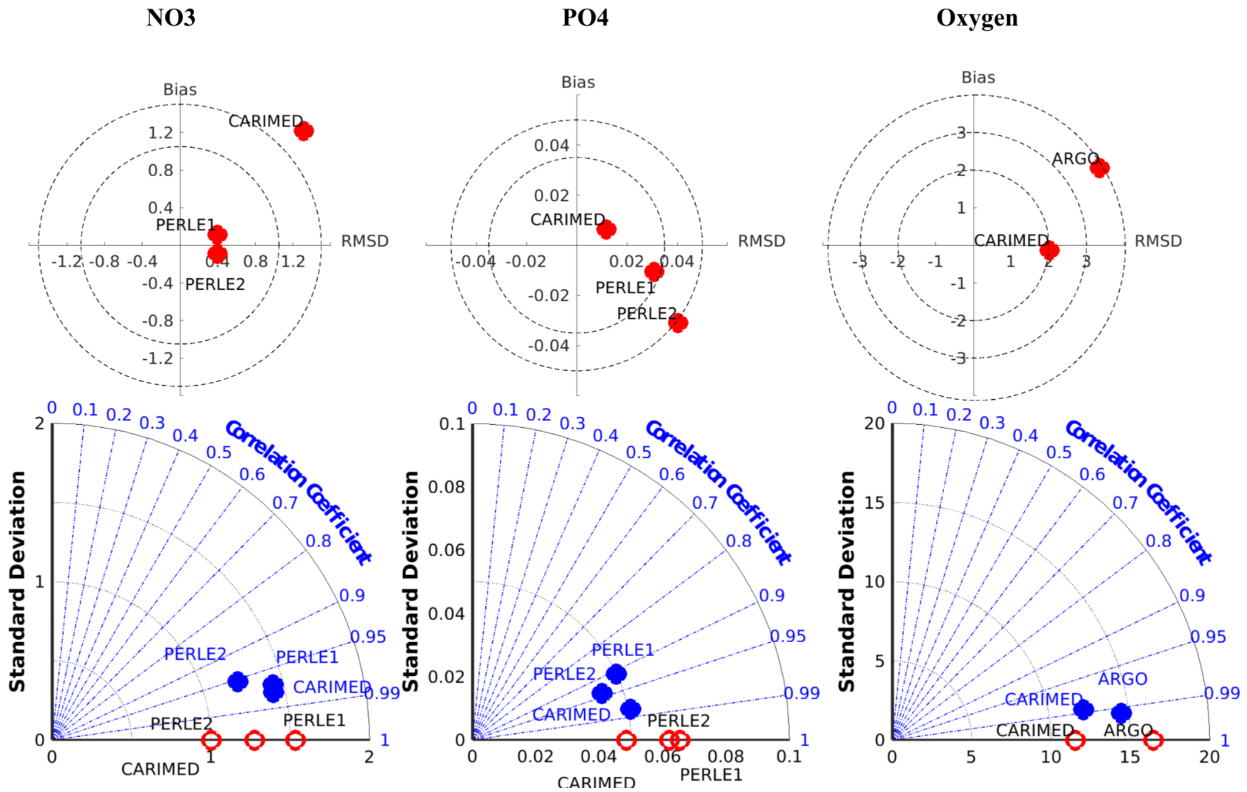


Figure 1.4 (Figure S4 of the new version of the Supplement Material): Statistical parameters of the modeled and observed nitrate (mmol N m^{-3}), phosphate (mmol P m^{-3}) and dissolved oxygen (µmol O$_2$ kg^{-1}) concentrations in the Levantine Basin: Top: target diagram of the bias and the RMSD, bottom: Taylor diagram with the correlation coefficient and the standard deviation. Blue dots indicate model outputs and red circles and dots observations. The angle represents the correlation coefficient.

L370 *"We display both nitrate and phosphate due to their role of limitation on primary production in this region (Moutin and Raimbault, 2002)." Not sure what you mean here. Isn't phosphate the ain limiting nutrient?*

Reply: Most studies in the Levantine Sea advance a limitation of primary production by the availability in phosphate. A few studies (Zohary et al., 2005; Krom et al., 2005; Tanaka et al. 2011) have shown that the nutrient limitation in the Eastern Mediterranean is more complex and mention a co-limitation in nitrate and phosphate impacting the phytoplankton regime in this sea. It would be interesting to see what the model indicates on the nutrient limitation, but it appears for us outside the scope of this paper. We have chosen to show both nutrients to allow further studies and increase the possibility of comparisons to observations and (eventually) to discuss their seasonal evolution. Nevertheless, we removed in the new version of the manuscript those few words that do not lead to a discussion in this paper.

L382 "Increases in plankton and DOC concentrations under the surface layer (0-150 m) are clearly visible during that period (Fig. 6). "Not clear what you mean, as DOC follows a different pattern from phytoplankton (decreases during winter)

Reply: We agree with the comment of Reviewer 1, DOC and plankton follow different temporal evolution in the surface layer (0-100 m) in winter. The sentence here refers to the punctual increases of plankton and DOC concentrations below 150 m during winter as a consequence of vertical entrainment during the deepening of the mixed layer shown in Figure 6. We have realized that the sentence formulation is confusing and therefore we were more precise in the description:

The DOC concentration in the 0-100 m layer further decreases during the winter mixing period, from January to March (Fig. 6e and S7f). One can also notice that the deepening of the mixed layer in winter is also responsible for the transfer of both plankton and DOC under 150 m where their concentrations increase ~~Increases in plankton and DOC concentrations under the surface layer (0-150 m) are clearly visible during that period~~ (Fig. 6).

L386 "the surface nitrate concentration ranging between 0.3 and 1 mmol N m -3, in agreement with the observations of Yilmaz and Tugrul (1998)" Does this refer to winter period? After April this is <0.1mmol/m3. Please clarify in the text

Reply: We agree with Reviewer 1, information on the season was missing in the manuscript. In the revised manuscript, we specified that these values concern the winter period.

L422 "Lateral export is more important for POC, with values exceeding 10 mmol C m -2 d-1 over several months for some summer/fall, when the DOC lateral export shows little variation along the period (Fig. 7f). "Any explanation for this?"

Reply: We would like to point out that we have found an error regarding the legend in Fig. 7f where POC and DOC were inverted. We apologize for this error that was corrected in the revised manuscript in the text and the figure.

In the model, the lateral and vertical fluxes of DOC are significantly correlated (R=0.5 for vertical fluxes, R=0.86 for lateral fluxes) with the lateral and vertical fluxes of water respectively. In particular, the DOC lateral export over several weeks in summer/fall periods, such as in 2016, is associated with water lateral inflow. Moreover the seasonal water mass budget in the surface layer of the Rhodes Gyre area shows an upward inflow at 150 m depth and a lateral outflow towards the surrounding areas when the cyclonic circulation intensifies in fall (Table 1.1). The higher lateral export of DOC related to the one of POC in fall could be explained by the vertical distribution of current velocity, POC and DOC. Indeed the current velocity is generally higher near the surface where the DOC concentration is maximal, compared to 100-150 m where the POC is maximum. We added this potential explanation in the revised manuscript as follows:

Lateral export is more important for ~~POC~~ DOC, with values exceeding 10 mmol C m^{-2} d^{-1} over several months for some summer/fall, when the ~~DOC~~ POC lateral export shows little variation along the period (Fig. 7f). This could be explained by higher current velocities near the surface where the DOC concentration is maximal, compared to 100-150 m where the POC is maximum.

Table 1.1: Annual vertical and lateral flux of dissolved organic carbon (DOC) and water at 150 m for the different years and averaged over the 7-year period, estimated from the model.

	Year		Units	2013-14	2014-15	2015-16	2016-17	2017-18	2018-19	2019-20	Mean
Surface layer **Autumn SON**	**DOC**	Vert. at 150m	$gC.m^{-2}.yr^{-1}$	0.94	0.8	1.8	-1	0.16	1.2	1.8	0.8
		Lateral flux	$gC.m^{-2}.yr^{-1}$	-4	-3.2	-4.5	-0.5	-3.3	-3.7	-5.3	-3.5
	Water	Vert. at 150m	$m.yr^{-1}$	35	25	44	-6	19	34	46	28
		Lateral flux	$m.yr^{-1}$	-34	-24	-43	6	-19	-34	-46	-28

L425 "in summer and autumn, when DOC can be injected into the surface layer." Not clear what you mean here. Please explain.

Reply: Small DOC vertical inputs from the intermediate layer into the surface layer occur in summer and fall, especially in fall 2016. They are associated with intermediate water inflow. Moreover, as mentioned in the reply of the previous comment, the seasonal budgets of water mass and DOC that we have performed for the surface layer of the Rhodes Gyre shows a transport of water and DOC at 150 m depth from the intermediate layer into the surface layer in fall. We modified this sentence to clarify this point:

The vertical export of total OC is reduced from spring onwards and becomes low (< 10 mmol C m^{-2} d^{-1}) in summer and autumn, when DOC can be injected from the intermediate layer into the surface layer due to upwelling events.

L436 "A secondary peak of OC respiration is visible in fall when the maximum POC concentration is the deepest." Not totally clear. Please explain.

Reply: The time evolution of organic carbon content shows two peaks, the first one during spring followed by a second smaller one during fall. We divided the OC stock into DOC and POC stocks and found the same secondary peak in autumn for both components as was previously simulated for the total OC stock. The secondary peak is explained by the general deepening of the ecosystem in summer/fall (Fig. 6 of the manuscript) due to the intensification of solar irradiance. The OC respiration follows the evolution of the OC stock. One can notice that since the depth of the DCM has been shown to be overestimated in the model, this peak might be also overestimated. We modified the text as follow:

A secondary peak of OC respiration is visible in fall when the ~~maximum P~~OC stock increases. This can be explained by the deepening of the ecosystem due to the increase in solar radiation at that period. However, the overestimation of the depth of the DCM shown in Section 3.1.2 suggests that it could be overestimated in the model results.

L499 "The surface values fall in the lower range of observations (41-100 mmol C m-3), which could be partly explained by the locations of the observations, mostly outside the Rhodes Gyre in more stratified and less productive regions." Do you see such pattern (increase of DOC in less productive waters) in the model?

Reply: Figure 1.5a shows the simulated DOC concentrations averaged over the surface layer (0-150 m) over the period 2013-20. Low concentrations are found in cyclonic gyres characterized by higher rates of NPP (Figure 1.5b). In particular in the Levantine Sea, DOC concentration is minimum in the Rhodes Gyre. However, the surface layer DOC concentration is also influenced by the depth of the mixed layer that can be high in anticyclonic gyres, river inputs and/or inflow from the Ionian Sea. Therefore we modified the sentence as follows:

The surface values fall in the lower range of observations (41-100 mmol C m^{-3}), which could be partly explained by the locations of the observations, mostly outside the Rhodes Gyre ~~in more stratified and less productive regions~~ where the concentrations are slightly lower compared to the surrounding Levantine Sea in the model outputs.

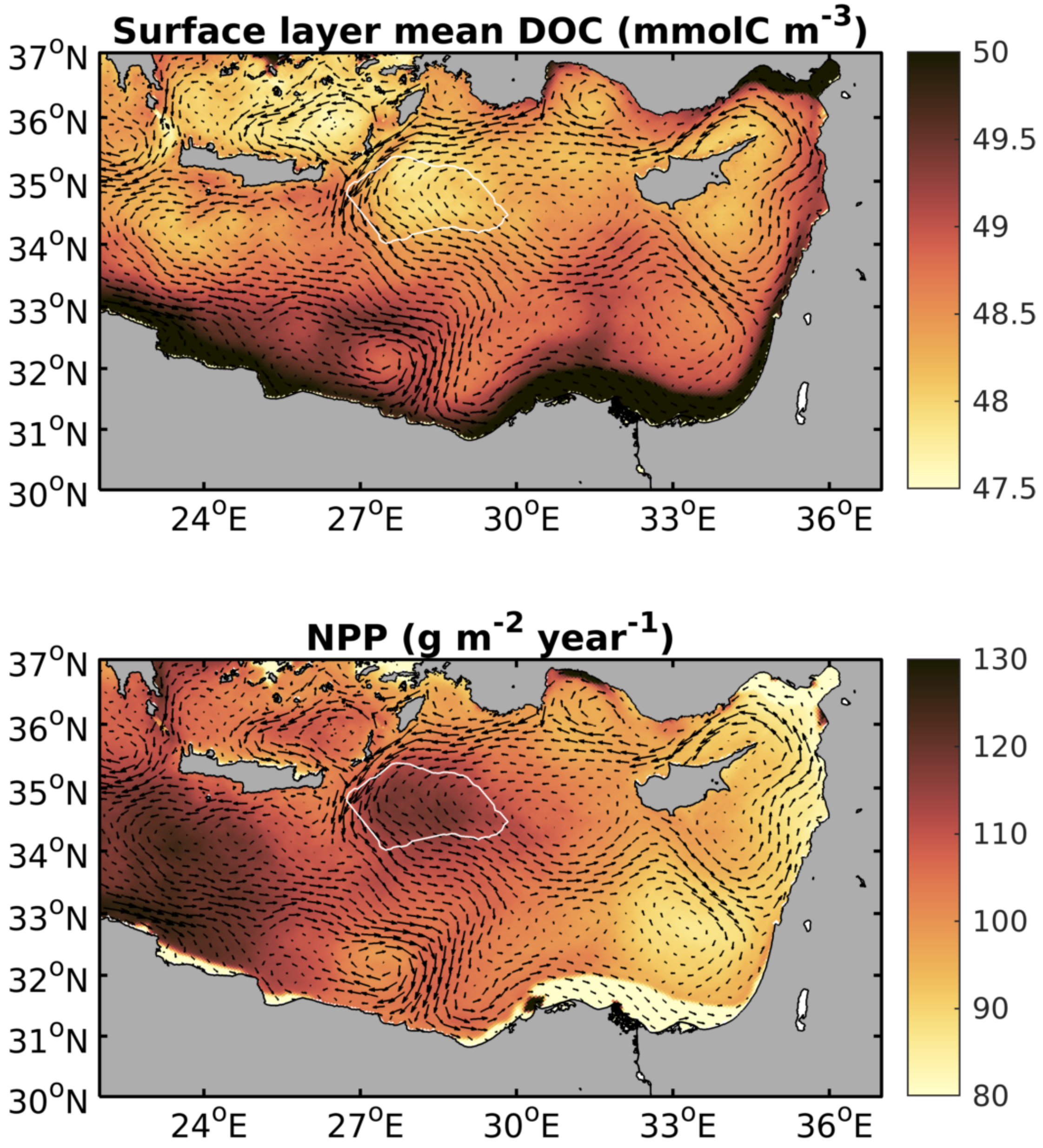


Figure 1.5: Modeled (a) mean surface layer dissolved organic carbon concentration (mmolC/m^3) and (b) net primary production (g/m^2/year), averaged over the period from December 2013 to December 2020. The arrows represent the surface currents averaged over the period of study, the white contour delimits the Rhodes Gyre.

L507 "Regarding the organic carbon biological fluxes, the seven year averaged annual NPP that amounts to 115 ± 15 g" Maybe you could add the mean NPP for the EM for comparison with other studies and also indicating the relatively higher magnitude in the RG area.

Reply: As the Eastern Mediterranean is composed of very various trophic regimes showing diverse characteristics with highly productive regions such as the Adriatic Sea influenced by river inputs and ultra-oligotrophic regime such as the south Ionian and Levantine seas, we found more relevant in the revised manuscript to focus the comparisons of modeled NPP with previous estimates in the Levantine Sea.

However we specify here that we found a value of NPP of 119 ± 14 gC m^{-2} $year^{-1}$ for the EM, that can be compared with estimates based on satellite data of 137 gC m^{-2} $year^{-1}$ by Antoine et al. (1995) corrected by Bosc et al. (2004), 121 gC m^{-2} $year^{-1}$ by Bosc et al. (2004) and estimates based on 3D modeling of 56 gC m^{-2} $year^{-1}$ by Crispi et al. (2002) and 76 gC m^{-2} $year^{-1}$ by Lazzari et al. (2012).

The model results show a higher magnitude of NPP in the RG area compared to the surrounding Levantine open sea: on an annual scale, 115 ± 15 versus 102 ± 17 gC m^{-2} $year^{-1}$, and in winter, 103 ± 20 versus 85 ± 17 gC m^{-2} $year^{-1}$. We indicated it in the revised manuscript. Thus we modified the paragraph as follows:

Regarding the organic carbon biological fluxes, the seven year averaged annual NPP that amounts to 115 ± 15 g C m^{-2} $year^{-1}$ falls in the range of the previous annual estimates for the northern Levantine Sea based on satellite ocean color data (60-152 g C m^{-2} $year^{-1}$, Antoine et al., 1995 ; Bosc et al., 2004; Uitz et al., 2012), or more specifically for the Rhodes Gyre based on modeling studies (92-180 g C m^{-2} $year^{-1}$, Napolitano et al., 2000; Kalaroni et al., 2020; Cossarini et al., 2021) . The higher magnitude of annual and winter NPP values in the Rhodes Gyre area compared to the surrounding Levantine Basin (Fig. S5d), by 13% and 21% respectively, is in line with the findings of Vidussi et al. (2001), Uitz et al. (2012) and Cossarini et al. (2021).

L515 "The mean annual POC export at 150 m depth is estimated in the model at 11.9 ± 3.4 g C m -2 year -1 ." This refers to the Rhodes gyre area? Please clarify in the text.

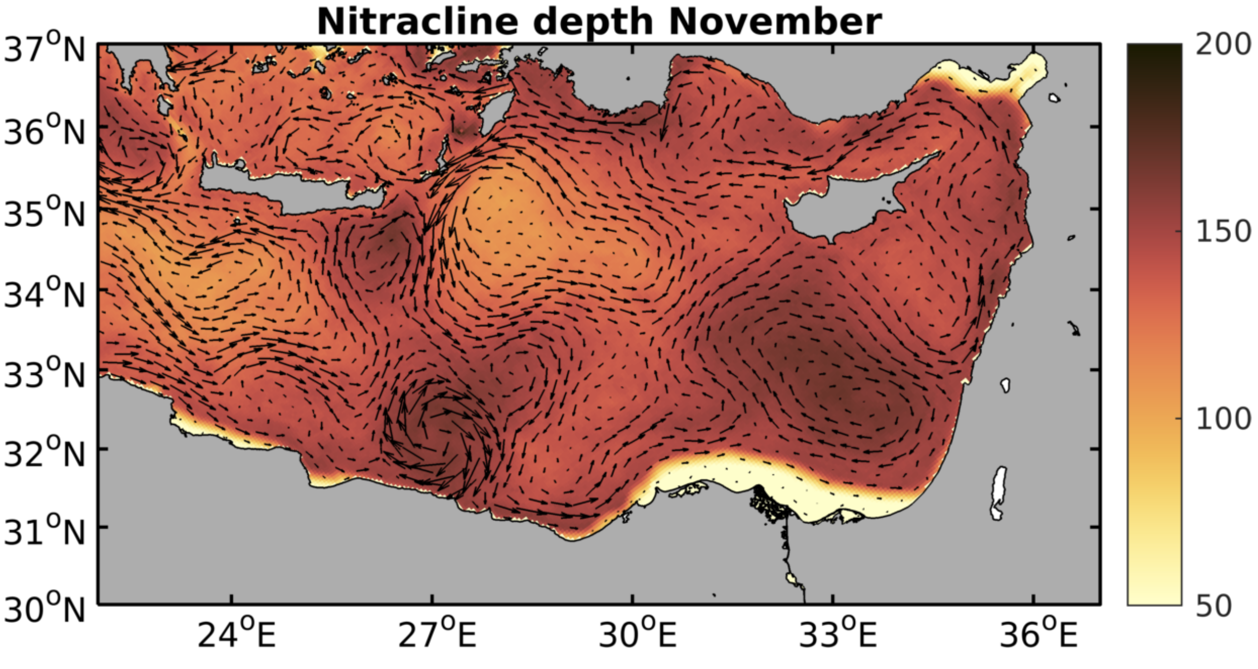
Reply: This has been added as suggested in the revised manuscript.

L540 "The intensification of the cyclonic circulation in fall favors the shallowing of the nutriclines." I think part of this shallowing might be also related to the solar radiation seasonal variability and the deepening of the DCM. Although this is partly illustrated based on the interannual variability, given that the study focuses on the Rhodes Gyre, it would be nice to show (maybe with a figure in the supplement) for comparison the difference from another area or an average over the Levantine where such shallowing of the nutriclines, related with the circulation does not occur.

Reply: We agree that it should be mentioned that this shallowing is first due to the solar radiation seasonal variability, and then that it could be reinforced by the circulation. To follow a recommendation of Reviewer 2 to lighten the discussion section we removed this sentence from Sect. 4.1 but to take into account this comment we changed the text in Sect. 3.3:

During fall, nutrient concentrations gradually increase in the surface layer with the weakening of the stratification and the gradual rise of the nutricline (defined here as isoline 1 mmol N m^{-3} for nitracline and 0.05 mmol P m^{-3} for phosphacline) up to the surface (Fig. 6a, 6b and S7) induced by the reduction of solar insolation and the shallowing of the DCM, possibly reinforced by the intensification of the cyclonic circulation.

Figure 1.6 shows the spatial variability of November nitracline depth and the difference of nitracline depth between July and October for the period 2014-2020. The depth of nitracline is shallower in cyclonic regions in November (Fig. 1.6, top) and the difference of depth nitracline in fall, i.e. the shallowing of nitracline, appears high in the Rhodes Gyre region, compared to the surrounding open sea (Fig. 1.6, down). Since there is already an important number of figures in the Supplementary Material, we prefer not to add this figure in it, but if Reviewer 1 still suggests, we will follow her/his recommendation.



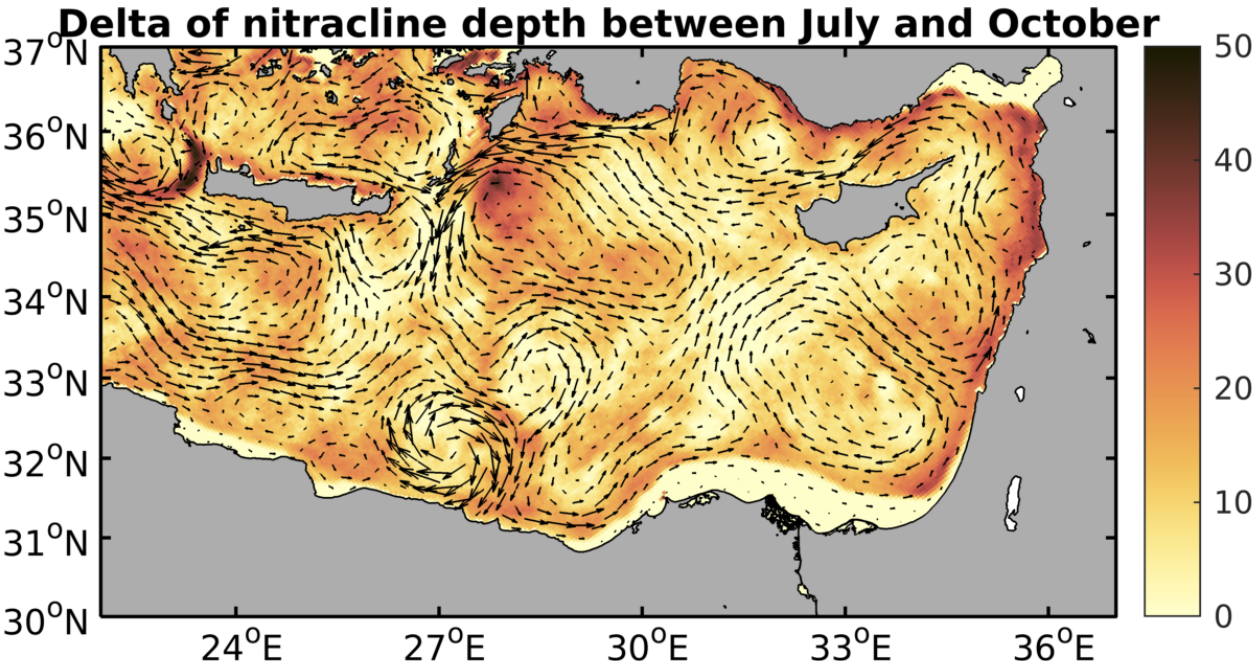


Figure 1.6: Modeled November nitracline depth (m) (top) and delta nitracline depth (m) between July and October (down), for the period of 2014-2020. Current velocities simulated at 15 m depth and averaged over September - October - November 2014-20 are superimposed.

L575 "On the other hand, the model PP relies at 30% on the uptake of nitrate, and at 70% on the uptake of ammonium (not shown). The former is significantly correlated with HL (R> 0.88, Fig. 10g) and MLD, whereas no correlation can be found between the latter and HL (R< 0.69, Fig. 10h) or winter mixing." This might be a bit misleading, as PP is generally well correlated with mixing/heat loss. I would suggest to rephrase

Reply: We agree with this analysis. Contrary to what we wrote, Fig. 10 shows that the correlation of annual NPP with winter heat loss (HL) is not significant if 2013-14 is not considered. This result is in agreement with the lack of correlation of ammonium uptake (main contributor to NPP) with winter heat loss. Therefore, we rewrote this section as follows:

Although winter and spring NPP is higher under cold winters (not shown), annual NPP is not significantly correlated with winter heat loss if 2013-14 is not considered (Fig. 10e). The modeled NPP depends on the nitrate and ammonium uptake supporting, respectively, 30% and 70% of the NPP. Nitrate uptake is significantly correlated with HL (R= 0.88, p-value < 0.01, Fig. 10g) and MLD, whereas the ammonium uptake shows no significant correlation with HL (R= 0.69, p-value < 0.01, Fig. 10h).

L601 "For example, the dates of maximum MLD and chlorophyll are in March during both the mild winter 2013-14 and the severe winter 2014-15 (Table S1)." Not clear. Table S1 indicates 12Feb for Chl (2013-2014) and 20 Feb for MLD (2014-2015). Please clarify.

Reply: We apologize for the lack of clarity in line 601. In the model results, we found that the date of the maximum chlorophyll concentrations was between early February and early March and, contrary to the NW Mediterranean deep convection region, doesn't depend on the winter severity. For example, it was found on 11 and 12 February during both the mild winter 2015-16 and the severe winter 2016-17. This sentence, that was moved in Sect. 3.3 following a suggestion of Reviewer 2, was modified as follows:

For example, it was found mid February during both the mild winter 2015-16 and the severe winter 2016-17 (Table S1).

Besides we removed the line with the date of the MLD maximum since we did not present it in the revised manuscript to shorten and simplify the text.

Technical corrections

L74 "Ediger and Yilmaz (1996) highlighted interannual variability"

the interannual variability

Reply: This was corrected as suggested in the revised manuscript.

L115 "we use a 3D hydrodynamic-biogeochemical coupled modeling"

we use 3-D ..coupled model simulations..

Reply: This sentence was corrected in the revised manuscript to take into account this suggestion, as well as a comment of Reviewer 2, as follow:

For that, we analyzed a simulation of a 3-D hydrodynamic-biogeochemical coupled model implemented over the Mediterranean Sea over the period from December 2013 - April 2021, and we focused on the Rhodes Gyre.

L146 "As for the Gibraltar Strait, a narrowing was conducted with a 1.3 km grid for a better representation of the exchange area between the Mediterranean Sea and Atlantic Ocean." Better rephrase e.g. ..Strait, the model resolution was further increased…

Reply: This was corrected as suggested in the revised manuscript.

L148 "and closer levels ranging near the surface." better rephrase e.g. and increased resolution near the surface

Reply: This was corrected as suggested in the revised manuscript.

L151 "The SYMPHONIE simulation runs" Better rephrase e.g ..simulation is performed from..

Reply: This was corrected as suggested in the revised manuscript.

L154 "monthly discharges were based on the study of Poulos et al. (1997)," you mean monthly climatology?

Reply: Indeed we meant monthly climatology. This was corrected as suggested in the revised manuscript.

L156 "We used the daily 3D current velocity, temperature, salinity and vertical diffusivity outputs of the hydrodynamic simulations as forcing fields for the biogeochemical model run". Is salinity somehow involved in biogeochemical processes?

Reply: Salinity is involved in the calculations of air-sea oxygen fluxes through the oxygen solubility (Ulses et al., 2021) and in the carbonate system dynamics (Ulses et al., Biogeosciences Discussion, in revision). However these processes are not presented and discussed in this article.

L175 "concentrations of nutrients were imposed at subbasin scale" Not sure what you mean by subbasin

Reply: Ludwig et al. (2010) estimated the river nutrient inputs for the main rivers and 10 sub-regions (Alboran, South-Western, North-Western, Tyrrhenian, Adriatic, Ionian, Central, Aegean, North-Levantine, South-Levantine) of the Mediterranean Sea. In our simulation, we imposed the concentration of nutrients in the rivers of the same 10 sub-regions based on this study. We changed the sentence to clarify this point:

At the river mouths, concentrations of nutrients were imposed ~~at subbasin scale~~ using the results ~~dataset~~ of Ludwig et al. (2010) who estimated the nutrient river discharge for the main rivers and 10 sub-regions of the Mediterranean Sea (Alboran, South-Western, North-Western, Tyrrhenian, Adriatic, Ionian, Central, Aegean, North-Levantine, South-Levantine).

L211 "The internal variation of organic carbon inventory, biological term and lateral physical term were calculated online," Please rephrase "online" e.g. calculated from model output

Reply: "online" meant that we computed the terms of the budget during the simulation at each time step and not after the simulation had run using instantaneous or mean model outputs. The online calculation allows an exact calculation of the various fluxes. To clarify this we slightly modified the sentence:

The variation of organic carbon inventory, the biological term and lateral physical term were calculated online, i.e. during the simulation, while the vertical term was calculated as the residual based on values of all other terms.

L259 "The hydrodynamical model was evaluated and validated ""evaluated" and "validated" appear quite similar

Reply: We removed "and validated" from the sentence in the revised version of the manuscript.

L292 "could be attributed in part to an underestimation in winter of chlorophyll concentration in satellite data in the Levantine Sea" rephrase to be more concise e.g. an underestimation of satellite chl concentration during winter in the Levantine...

Reply: This was corrected in the revised manuscript.

L318 " In winter, the surface oxygen concentration is maximal coinciding with the peak of surface chlorophyll." Rephrase e.g As with Chl-a, the surface oxygen concentration is maximum during winter..

Reply: This was corrected in the revised manuscript.

L386 "Phytoplankton accumulation" Not sure if accumulation is the wright word. Maybe phytoplankton growth?

Reply: This was corrected in the revised manuscript.

L409 "the time series of the variation of the organic carbon inventory, of biogeochemical fluxes and of vertical and horizontal exchanges at the limits of the two boxes." Rephrase e.g. the variability of the organic carbon inventory, the biogeochemical fluxes and the vertical and horizontal exchanges at the limits of the two boxes.

Reply: This was corrected in the revised manuscript.

L488 "We notice however an underestimation in the magnitude of the modeled maximum chlorophyll and dissolved oxygen concentration when comparing with both the BGC-Argo float and cruise data." You refer to the sub-surface maximum? Please clarify in the text

Reply: In the revised manuscript, we added "subsurface" maximum for better clarification.

L561 "We found a significant correlation between nutrient injection and mean winter HL (heat loss) or mean winter MLD (higher than 0.85)" repeated above. Please rephrase or merge.

Reply: To avoid repetitions we removed the previous sentence.

L700 "High interannual variability of annual" The high…

Reply: This was corrected in the revised manuscript.

FigS2 "Red dots represent the river mouths." is repeated in the Fig.caption

Reply: This was corrected in the revised manuscript.

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Responses to the comments of the anonymous Reviewer 2

First we would like to warmly thank the anonymous Reviewer 2 for her/his relevant and constructive comments which helped to improve the manuscript.

This manuscript titled 'Seasonal and interannual variability of the pelagic ecosystem and of the organic carbon budget in the Rhodes Gyre (Eastern Mediterranean): influence of winter mixing' by Habib et al. presents a model description of the biogeochemistry and organic carbon budget of the Rhodes gyres. This gyre has a peculiar importance for the Mediterranean Sea since it is a major forming region of the Levantine Intermediate Water (LIW). The article is well written and thoroughly detailed. The research focus is original because of the lack of modeling studies focused on the eastern basin (and in particular on the Rhodes gyres). Overall, I recommend this article for publication after a few comments and questions detailed below are addressed.

Reply: We appreciate this overall positive assessment.

The introduction gives rich background information, but the key research questions addressed by this study are not explicitly stated. I think the last paragraph of the intro (lines 112-116) could be reformulated to highlight the research gap that is addressed here and to make a short outline of the paper in a couple of sentences.

Reply: We have added two sentences on the gaps in the studies cited above (the detailed description of the ecosystem that biogeochemical floats do not allow, and the interannual variability that the intermittent bloom trophic status of the area suggests).

However, this study is limited to parameters measured by biogeochemical floats, which does not allow for a more detailed exploration of biogeochemical and ecosystem dynamics. Furthermore, the intermittent trophic status of the area suggests significant interannual variability that remains poorly understood. In order to fill these gaps, ~~T~~the present study aims to gain insight into carbon dynamics through the examination of seasonal and interannual variabilities of the biogeochemical and physical fluxes of organic carbon, under particulate and dissolved forms, in the Rhodes Gyre and the estimate of an annual budget of organic carbon in the area over a multi-annual period.

Finally, we have added a short outline of the paper:

For that, we analyzed a simulation of a 3-D hydrodynamic-biogeochemical coupled model implemented over the Mediterranean Sea, over the period from December 2013 to April 2021, and we focused on the Rhodes Gyre. The paper is organized as follows: first we describe in Sect. 2 the numerical models and the various data sets used to evaluate the model.

Then, in Sect. 3 we present the assessment of the coupled model, the seasonal and interannual physical and biogeochemical variability and an annual budget of organic carbon. Results are discussed in Sect. 4 and conclusions are given in Sect. 5.

Methods are described in detail, yet, a few key information are missing. In particular, the time resolution of the model and the outputs are not described.

Reply: The time step is 20 minutes for advection and diffusion of biogeochemical variables and 2 hours for biogeochemical reactions. The model outputs are stored every 24 hours for 2D variables of fluxes and every 5 days for 3D variables. The outputs of the budget terms are stored every 3 hours and 20 minutes. As suggested by Reviewer 2, we completed the paragraph with the time step of the biogeochemical model:

The time step is 20 min for advection and diffusion of biogeochemical variables and 2 h for biogeochemical reactions.

Also, there is no mention of a model spin-up or of model drift (or absence of drift) in the biogeochemical variables.

Reply: As was mentioned in the manuscript, the biogeochemical model runs for the period from 15 August 2011 to 2 May 2021. In this study, we started analyzing the model's outputs from the end of 2013 (this has been added in Section 2.1.3) in order for it to reach a relatively stable nominal state. We considered the first two years as two years of spin-up for the model. We also checked for possible drifts in the biogeochemical variables. We show in Figure 2.1, the time evolution of two biogeochemical variables, the nitrate and the phytoplankton (the one of total organic carbon is discussed in a following reply), in the surface layer (0-150 m), the blue line represents the trend calculated with the first year 2013-14 whereas the red line is the trend excluding the first year. As was mentioned in the manuscript, winter 2013-14 is considered as an exceptional warm year and therefore might influence the trends. The trend (blue line) shows a gradual increase in the nitrate (+0.0061 mol N m^{-2} yr^{-1}) and phytoplankton (+0.0005 C mol m^{-2} yr^{-1}) for all the period, however when removing the first year (red line), the increased trend is not clearly visible. The red line slopes show values 2 and 5 times lower than what was previously observed (+0.002, +0.0001 mol m^{-2} yr^{-1} respectively). It should be noted that very little data exist and are available for the period 2013–2021 to compare our results and draw a firm behavior of the biogeochemical variables with time.

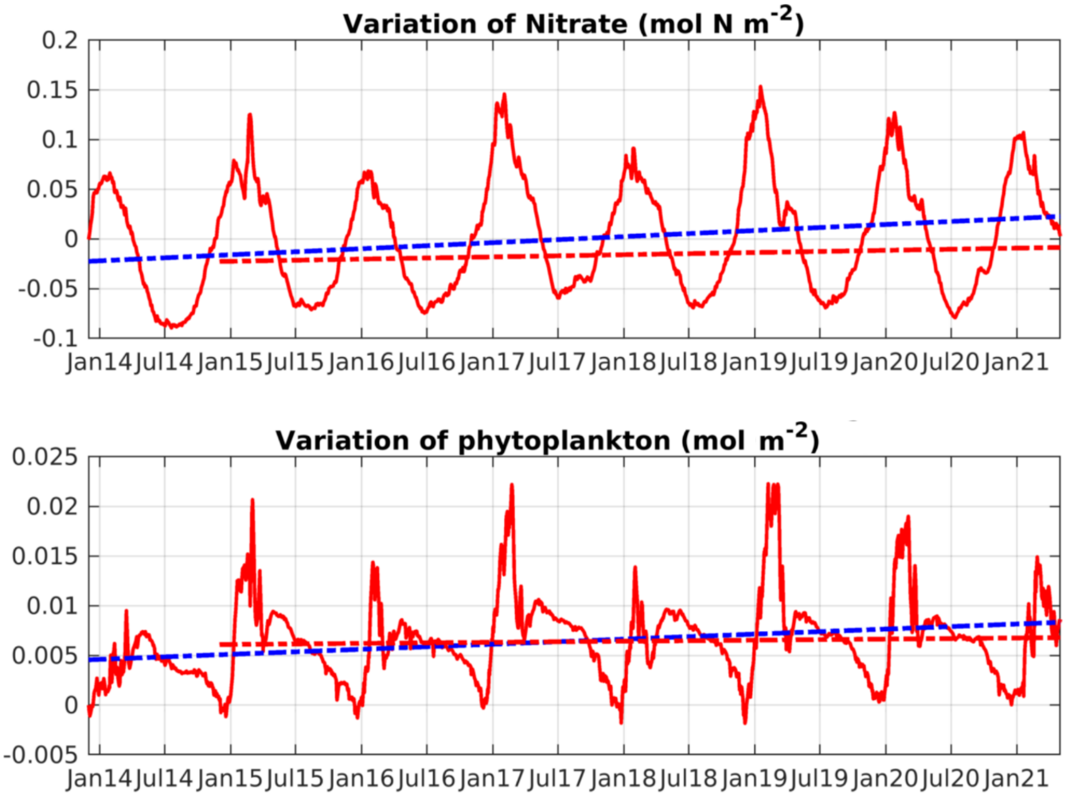


Figure 2.1: Time evolution of the variation of the nitrate (mol N m^{-2}) and phytoplankton (mol m^{-2}) inventory averaged over the Rhodes Gyre surface layer (0-150 m). The blue line represents the trend from December 2013-May 2021, the red line, the trend excluding the first year (December 2014-May 2021).

Some information is missing in 2.1.1 about the key features and the resolution of the model. Also, it is unclear from the Methods that the model is ran over the entire Mediterranean, which introduces some confusion in the results (see my comment below).

Reply: Concerning the area represented by the model, as suggested in a following comment, a precision on the model domain has been added in the introduction by modifying a sentence in Introduction:

“we analyzed a simulation of a 3-D hydrodynamic-biogeochemical coupled model implemented over the Mediterranean Sea over the period from December 2013 - April 2021, and we focused on the Rhodes Gyre.”

Besides, we also added in the Material and methods section that the biogeochemical model uses the same grid as the hydrodynamic model (described just before: “The model domain covers the Mediterranean Sea as well as the Marmara Sea and reaches 8° West in the Gulf of Cadiz…”).

The paragraph in lines 141-150 relates to the model description and could go in 2.1.1.

Reply: We have chosen for the clarity of the article, to have two sections (2.1.1 and 2.1.2) that describe the hydrodynamic model and the biogeochemical model, respectively, (its state variables as well as the basic references that allow to go into more details of the equations) and then a third section (2.1.3) that gives the characteristics of the simulation carried out with this coupled model (the region, the grid, the initial and boundary conditions). Lines 141-150 are specific to the simulation and are therefore in Section 2.1.3.

Lines 156-158: did you do a spin-up of the model? Did you check for drift in the biogeochemical variables?

Reply : We answered above to these questions.

If I understand correctly, you used a model for the global Med basin and zoomed on the Rhodes gyres which you identified using the criteria you describe in 2.1.4?

In the introduction, the need for dedicated models of the Rhodes Gyres is highlighted (lines 99-103): "On the other hand, only one 1-D coupled hydrodynamic-biogeochemical model has been carried out in the Rhodes Gyre (Napolitano et al. 2000), while most 3D modeling studies investigated the whole Mediterranean Sea (Lazzari et al., 2012; Macias et al., 2014; Guyennon et al., 2016; Richon et al., 2017, 2018; Karaloni et al., 2020; Cossarini et al., 2021) or eastern Mediterranean Sea (Petihakis et al., 2009) without focusing on the LIW formation region of the Rhodes Gyre." These sentences seem to imply that you are about to use a dedicated model of the Rhodes gyre. It's fine to use a model of the global basin, but I think it would be less misleading if the introduction and methods section mentioned explicitly your model domain. Maybe you could add a sentence at the end of the introduction saying that you are using a model for the global basin, as done in previous studies, but the originality is that you are using precise criteria for describing and analyzing the Rhodes gyre?

Reply: The originality of our study is the focus on the Rhodes Gyre whereas all the mentioned modeling studies described all the Mediterranean Sea and briefly mentioned our region of interest. As mentioned before, we clarified this point, on one hand at the end of the introduction, and on the other hand in Section 2.1.3 of the methods. We also added a sentence at the beginning of Section 2.1.4 "Definition of the study area…".

In this paper, the analysis of the simulation of the whole Mediterranean basin is restricted to the Rhodes Gyre.

The results are overall well described.

On Figure 7a, it looks like there is a drift in the OC inventory? Is that so? If yes, please discuss the reasons for it.

Reply: The annual budget of organic carbon shows positive and negative annual variations of the OC inventory in the surface layer (Table 1), that are not linked to the magnitude of winter heat loss or vertical mixing. As Reviewer 2 has noticed, we find a global increase trend in the OC inventory equal to 0.44 mol C m^{-2} $year^{-1}$ in the surface layer over the period 2013-2020 (Table 1) (a decrease is also found without considering the warm year 2013-14), as mentioned in Sect. 3.5 (L463-464 of the submitted manuscript). This is in general agreement with the observations by Ozer et al. (2022). In their study, these authors found a general long positive trend for the depth-integrated chlorophyll a measured offshore Haifa, to the east of the Levantine Basin, between 2002 and 2021, superimposed by interannual variations. They suggested that the long-term warming and salinification result in an increased buoyancy and a shallowing of the LIW (up to 110 m) enabling a higher level of nutrients to become available to the photic zone from below, supporting the observed rise of the integrated chlorophyll a.

We want to emphasize here that the increase in organic matter content (Fig. 7a) is not so clear for winter values, which remain low for all cold winters. The possible trend would therefore rather concern the stratified period and would be independent of the winter conditions that are the focus of our paper. This was discussed in Section 4.2 when we observed that NPP and ammonium uptake are poorly correlated with winter heat loss and could be influenced by trends in temperature or nutricline depth. Furthermore, we believe that our time series is too short to determine a long-term trend. We added a discussion on that point at the end of Section 4.1.

Finally, though the model results show positive and negative annual variations of organic carbon inventory, an increasing trend in the OC inventory of 0.44 mol C m^{-2} $year^{-1}$ is found over the period 2013-20. This is in general agreement with the observations by Ozer et al. (2022). In their study, these authors found a general long positive trend superimposed by interannual variations, for the depth-integrated chlorophyll measured offshore Haifa, to the east of the Levantine Basin, between 2002 and 2021. They suggested that the long-term warming and salinification result in an increased buoyancy and a shallowing of the LIW (up to 110 m) enabling a higher level of nutrients to become available to the photic zone from below, supporting the observed rise of the integrated chlorophyll a. Considering the lack of data in the study area to assess this point in the model and the high interannual variability, an extension of the simulation over a longer period would be needed to detect a possible drift in the model.

Although figure 7 is described in great details, I think the opposite trends seen between 7c and 7d could be mentioned and discussed. I find interesting that the NCP and total transport seem to compensate for each other during the cold winters (see the peaks at ~+60mmol/m2/d for NCP in 2015 that are outbalanced by the ~-60mmol/m2/d for total transport).

Reply: We agree with Reviewer 2. In the revised manuscript, we mentionned this in sections 3.4, 3.5, 4.3 and 4.4 to take into account this suggestion.

In Section 3.4:

Thus the increase in total OC transport during cold winters seems to be counterbalanced by an increase in NCP. For instance in winter 2014-15 peaks reaching 60 mmol C m^{-2} d^{-1} are visible for both NCP and OC total transport.

At the end of Section 3.5:

Finally, the excess of biological production during cold winters is almost entirely compensated by an excess in total OC export.

In Section 4.3:

This could explain the compensation between the excess in NCP and OC export during cold winters.

In Section 4.4:

The high interannual variability of annual NCP (SD of 22%) in the Rhodes Gyre appears to be primarily linked to the intensity of winter atmospheric HL and vertical mixing (significant correlation > 0.88 between annual NCP and winter HL, Fig. 10f), which indicates an enhanced autotrophic metabolism during cold years, that is almost counterbalanced by an enhanced OC export.

The paragraph on lines 437-445 could go after line 426 in order to group the results regarding the surface.

Reply: We agree with Reviewer 2, therefore in the revised version we grouped the results regarding the surface as suggested.

The discussion is overall well written and detailed, but some minor rearrangement could help making it easier to read. The introductory paragraph on lines 476-489 can probably be discarded. This would help streamline the article.

Reply: We removed the introductory paragraph of the Section Discussion in the revised manuscript. We have left the first lines of Section 4.1 as we have added elements of discussion as recommended by Reviewer 3.

Lines 515-522: You explain that your modelled estimates of POC export are different to those measured and those from other models. Can you give a short explanation of what may cause these discrepancies and the potential implication for your results?

Reply: The observations to which we compare the model are, apart from that of Moutin and Raimbault (2002), all located in other regions of the Mediterranean. Some are stronger than ours and others are weaker. We believe that it is not possible with these too rare and short-lived observations to conclude that the model is biased and to give implications for our results. We modified this paragraph in the revised manuscript as follows:

The mean annual POC export at 150 m depth in the Rhodes Gyre is estimated in the model at 11.9 ± 3.4 g C m^{-2} $year^{-1}$. The POC export data in the Mediterranean are almost all located in other regions (Gulf of Lion, Gogou et al. (2014); Adriatic Sea, Boldrin et al. (2002); Ionian Sea, Gogou et al. (2002)) and show values measured or extrapolated at 100-150 m between 3 and 23 g C m^{-2} $year^{-1}$. Only Moutin and Raimbault (2002) reported POC export for the Rhodes Gyre but limited to May-June 1996. These values are twice our mean values for the same months. It does not seem possible with these too rare and punctual observations, to conclude on a possible bias of the model.

Lines 575-580: Do you think the NH4 uptake could be linked with atmospheric deposition? Could the influence of atmospheric deposition of NH4 explain the absence of correlation between NH4 uptake and HL? (i.e. maybe NH4 uptake actually correlates with deposition).

Reply: The ammonium atmospheric deposition was applied as a constant value over the whole simulation period. We added this missing information in Section 2.1.3 of the revised manuscript.

To answer this question, we have run a new simulation in which we removed the ammonium atmospheric deposition. The uptake of ammonium is reduced by 1% and the correlation between winter heat loss and ammonium uptake is decreased by 0.2%, if ammonium deposition is neglected and thus remains insignificant.

There is a lot of information on Figure 11, but I'm wondering if all (or any) is necessary to the article. This figure focuses on 3 case studies over you time series and the long text associated is actually very descriptive. Maybe you could put this figure and the associated text (lines 582-607) in supplement and only keep in the main article a few sentences highlighting the key informations brought by those case studies.

Reply: We realize that Figure 11 shows a lot of information that might not be commented in the text, therefore we removed NPP and we modified Figure 11 to only show 2 case studies representing the general behavior of cold and mild winter years, 2013-14 and 2014-15 (see Fig. 2.2 with the modifications in Figure 11 of the manuscript). In the revised manuscript, the description of Figure 11 was reduced and (moved in result Section 3.3 to follow a following comment), and Figure 11 move in Supplementary Material:

The interplay between vertical mixing, deep nutrient injection and increase in surface phytoplankton shows interannual variability, as illustrates Fig. S8 for the mild winter 2023-14 and the cold winter 2014-15. When the mixed layer punctually reaches the nutriclines in early winter or throughout a mild winter as in 2013-14 (Fig. S8a), surface nutrients and chlorophyll increase gradually and nearly synchronously. When the winter is severe as in 2014-15 (Fig. S8b), the surface nutrient response is each time a rapid increase (<1 day see for example early January and early February 2015), while the chlorophyll response depends on the depth of the MLD. In the case it is shallower than the euphotic layer, chlorophyll increases gradually (~12 days in January 2015). In the case the MLD exceeds the euphotic layer as in February 2015, chlorophyll development is delayed due to dilution of phytoplankton cells in the deep ML and light limitation for phytoplankton growth.

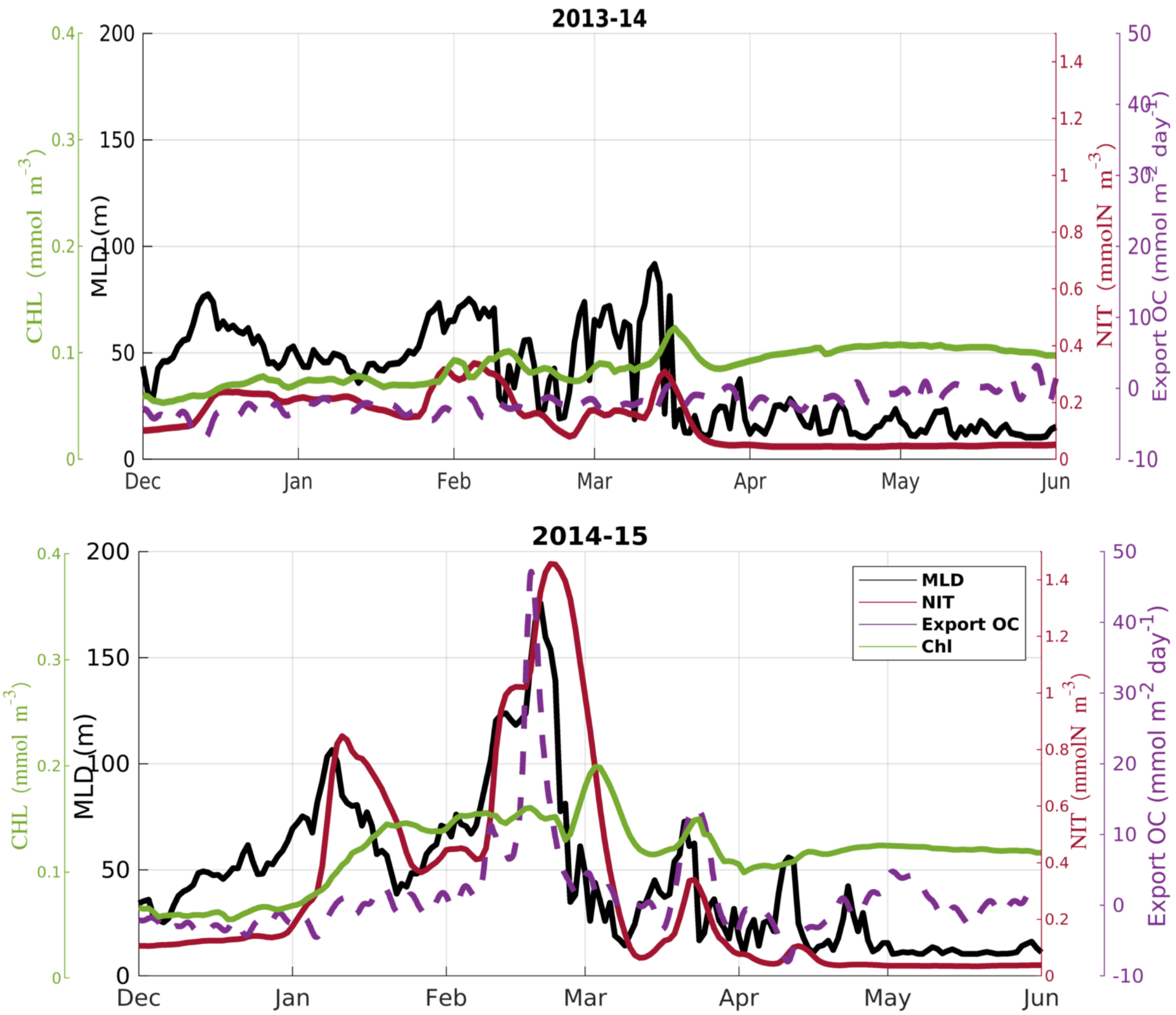


Figure 2.2 (modified Figure S8 in the supplementary material): Annual cycle of the Mixed Layer Depth (MLD, m) (black), surface chlorophyll (CHL, mg m^{-3}) (green) and nitrate (NIT, mmol N m^{-3}) (red) concentration~~ concentration, net primary production (NPP, mmol C m^{-2} day^{-1}) (blue) in the upper layer (0-150 m)~~, and the organic carbon export (mmol C m^{-2} day^{-1}) at 150 m depth (purple), for years 2013-14, and 2014-15~~, 2016-17~~.

Lines 609-629: this long text describing deep convection in other regions than the Rhodes gyre feels of place and can probably be discarded since they do not bring additional information.

Reply: We agree that this text is too theoretical for this paper and so we removed it until Line 633. Some ideas from lines 625-633 were introduced after the description of the export in the Rhodes Gyre in order to compare the processes in the deep and intermediate convection regions.

Similarly on lines 639-646: this paragraph feels like a description of the figures 7 and 10 and should therefore go in the results.

Reply: We have chosen to separate the results and the discussion. Figure 7, which presents the time series produced by the model, is clearly a result. The discussion concerns the interpretation of these results and as such, Figure 10, which illustrates the dependence of different biogeochemical fluxes on winter heat loss, has been introduced into the discussion to analyze the curves of Figure 7. We referred to Figure 7 to discuss the particularity of the year 2013-14 and how its consideration can alter the analysis. It seems to us therefore that this structure should not be modified.

Figure 12 focuses on a specific event over the time series and introduces some confusion. I think this example and the text associated could go in supplement.

Reply: We realize that the description of these physical dispersion processes is outside the scope of this paper. They could be the subject of a specific article. We have preferred to delete this section and Figure 12 in the revised version of the manuscript. We added some words to indicate that the lateral export corresponds to the subduction that follows the formation of the intermediate water:

The clear relation between winter severity and both annual OC vertical export at 150 m and lateral OC flux from the Rhodes Gyre to the Levantine Basin in the intermediate layer allows us to identify the responsibility of physical processes of LIW formation followed by subduction from the Rhodes Gyre to the Levantine basin.

Overall, maybe the discussion sections 4.3 and 4.4 can be merged and significantly streamlined. As it is presented, the text feels long and some paragraphs are very descriptive. Plus, the case studies of Figures 11 and 12 make it difficult to read the messages of the authors clearly. I think all text related *with description of the figures should go either in the results section, or in supplement (for the text associated to Figs 11 and 12). The discussion should be limited to the key messages of the authors placing their results in the context of their research questions and the current state of knowledge regarding the carbon cycle in the Rhodes gyres.*

Reply: We hope that deleting the lengthy text that begins Section 4.3 as well as the lengthy discussion that accompanies Figure 12 (as mentioned in a previous reply, description of Fig. 11 and S8 was moved in the result section) to make for a much clearer message. Besides, as recommended we have merged sections 4.3 and 4.4 and deleted some parts of the previous section 4.4 such as the results on DIC budget in the northwestern deep convection region. We thank the reviewer for this advice.

Other minor points:

- Line 44: What do you mean by "after further transformations", please reformulate.

Reply: This was corrected in the revised manuscript:

Various processes are involved in the ocean carbon sink: chemical processes driving the air-sea exchanges according to CO_2 solubility linked to sea surface temperature and salinity, biogeochemical processes in which dissolved inorganic carbon is first converted into organic carbon through photosynthesis, and then transferred to great depths, possibly after ~~further transformations~~ remineralization, and diffusive and advective physical processes.

- Line 91: 'understanding its formation', remove 'of'

Reply: This was corrected in the revised manuscript.

- Line 108: add a comma after 'project'

Reply: This was corrected in the revised manuscript.

- Lines 119-120: Either add references for the model, or discard these sentences because the model is described in detail right after.

Reply: The sentences were discarded in the revised manuscript.

-Line 215: "Because the number of observations in the Rhodes Gyre area are limited,..."

Reply: This was corrected in the revised manuscript.

- Line 271, the equation does not display correctly.

Reply: We apologize for the error in the equation of the normalized RMSD. In the revision, we corrected it.

- You could add a circle around the Rhodes gyre area on Figure 4b, similarly to the one on figure 1.

Reply: We added the Rhodes gyre area on Figure 4b.

-Figure 6 precise that these are modelled results. I can't see the green line in 6c. No mention of the C and M periods in the text.

Reply: "Modeled" has been added in the text and in the figure caption and we increased the thickness of the green line in Fig. 6c (see the Fig. 2.3 with the modification in Figure 6 of the manuscript). We added a mention to the C in Section 3.3. We would prefer to keep both C and M on this figure to make the reading easier since in Section 3.3 (last paragraph) we discuss the interannual variability by distinguishing the cold and mild winters and because the cold years are highlighted (with a blue bar) later in Figures 7 and 8. We have also rescaled the plots to begin in December 2013.

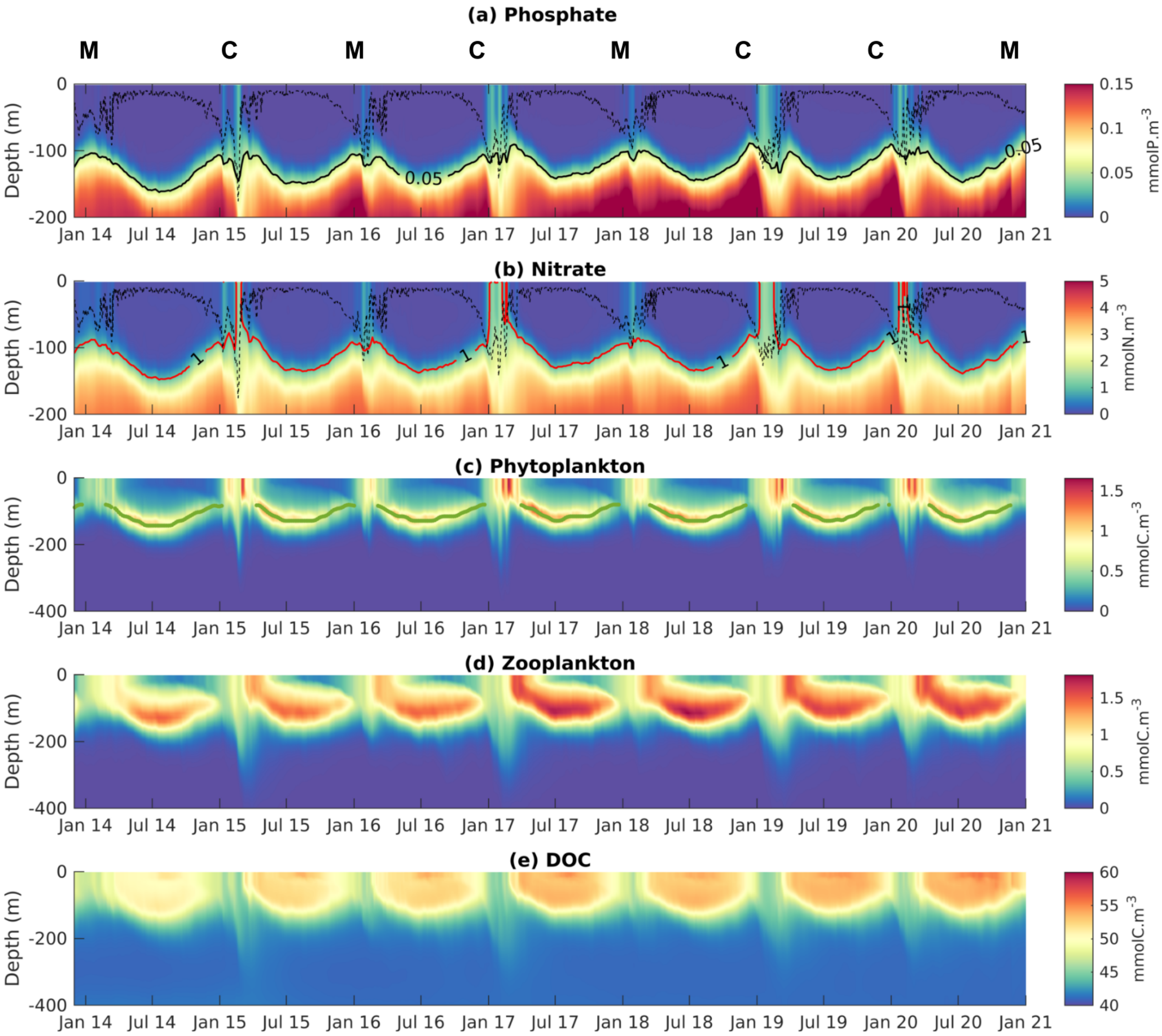


Figure 2.3 (Figure 6 in the manuscript): Hovmöller diagrams of modeled (a) phosphate (mmol P m^{-3}), (b) nitrate (mmol N m^{-3}), (c) phytoplankton (mmol C m^{-3}), (d) zooplankton (mmol C m^{-3}) and (d) dissolved organic carbon (mmol C m^{-3}) concentrations averaged over the Rhodes Gyre, from December 2013 to January 2021. The black dotted line in (a) and (b) indicates the mixed layer depth. The red line represents the depth of the nitracline in (b), and the black one of the phosphacline in (a). The green dotted line in (c) indicates the deep chlorophyll maximum. C refers to cold winter years and M to mild winter years.

- Line 356 replace "smaller" with "lower".

Reply: This was corrected in the revised manuscript.

- Line 377: "when the mixed layer depth increases" instead of "intensifies"

Reply: This was corrected in the revised manuscript.

- Lines 635-636: I am not sure I understand this sentence. Please rephrase.

Reply: In the revised manuscript, we removed the introductory part of Section 4.3 and therefore removed the sentence in question.

*- Line 714-715: Is there other references for DIC budgets than this unpublished work?**

Reply: We removed the reference to this unpublished work. We kept the other references.

Responses to the comments of Maurizio Ribera d'Alcalà

First we would like to warmly thank Maurizio Ribera d'Alacalà, the Reviewer 3, for his relevant and constructive comments which helped to improve the manuscript.

The paper presents the results of a modelistic study in the Easterm Mediterranean sea with a specific focus on the Rodhes gyre, a permanent cyclonic structure playing an important role in the Mediterranean intermediate water formation (LIW) and displaying higher level of phytoplankton biomass during late winter-early spring in respect to the rest of the basin, which is well known for its extreme oligotrophy. The main aim of the study is to quantify the contribution, in terms of carbon fluxes in different forms, of the gyre activity to the Eastern Mediterranean basin biogeochemistry, and to connect their variability to physical forcing and the linked nutrient fluxes from different sources, with a key role played by the vertical transport. Most of the relevant processes and fluxes are included in the model whose simulations are build on the GCM Symphonie, already calibrated for the whole basin, whose daily averaged outputs are used for assessing the transport of tracers which react according to the formulations of Eco3-M model. The latter is a classical biogeochemical flux model validated in many previous studies (see refs in the text). Results are compared with different data sets, both from in situ measurements and from satellite observations. The match between data and model outputs is definitely good (see below for further comments) especially considering the differences in time and space resolutions among the compared data.

As the authors rightly comment (l.491) the resolution of in situ data does not provide a convincing test for a model performance, unless some macropatterns are missed. To some extent one would be tempted to rely more on the calibrated model outputs than on the interpolations/extrapolations of in situ data to assess global or regional fluxes. This is, indeed, the main contribution of the study, which flanks other recent modelistic studies carried out on the basin (cited in the text), though with the specific focus on the Rhodes gyre. The main conclusions of the study are: 1. that there is a net lateral export of fixed carbon from the gyre toward to the Eastern basin, in other words the gyre 'feeds' the basin, with the export being more significant than the utilization within the structure; 2. that the heat fluxes that drive the mixed layer dynamics correlate well with the nutrient fluxes and the main biological responses, suggesting that they are the dominant driver of biotic response.

While the latter is quite expected for the bottom-up structure of the model and for what is already well established, the former is the first quantitative assessment of the contribution of the gyre activity to the biogeochemistry of the basin and is an interesting result considering the closeness of gyres in respect to their boundaries.

The paper is quite long, likewise the discussion which is more focused on the comparison of the model outputs with other estimates than on the few observed discrepancies between the model and the observations.

From a conceptual point of view there is a sort of circularity in these modelistic studies. The results of model simulations are consistent with the basic oceanographic processes, that are known since decades, which are at the base of their formulations. e.g., the sequence nutrient transport to the photic zone-mixed layer dynamics-phytoplankton growth. Since they do not include all the possible processes, and with the caveat of the difficulty to compare the results with observations carried out often at drastically different scales, e.g., Bio-ARGO data with model cells, what would deserve further analyses should be the mismatches. Instead, the focus is on the ability of the model to get as close as possible to the reality, which is certainly useful for operational purposes but not for a better understanding of the role of the many processes which are not included in the formulations. I am aware that the conclusion could be that they play a minor role but this is not tested. I consider this paper worth to be published because in all sections there is a comprehensive discussion of the existing information on the processes that the model simulates and because it provides useful results on the biogeochemistry of the Eastern Mediterranean.

Reply: We appreciate this positive general assessment.

My suggestion to the authors for this study or for its follow-ups is to analyze and discuss the following aspects:

- The delimitation of the Rhodes gyre domain is based on hydrographical and geometric properties. Did they consider to delimit the domain using dynamic data, i.e., velocity fields?

Reply: We did not think of this interesting possibility. The velocity field which is rather related to the sea surface height gradient is indeed commonly used to detect eddies and could certainly be applied to the Rhodes Gyre. We retain this possibility for other applications.

- How do they interpret two evident mismatches, the vertical winter chl.a profile (fig.3 top left plot) and the systematic difference in the DCM depth (Fig.3 upper plots), and the overestimate of the model of the Chl.a maxima (Fig.2a).

Reply: We agree with Reviewer 3 that the model clearly shows some discrepancies in chlorophyll magnitude and vertical distribution that have not been discussed. Regarding the winter, the model produces on average a mixed chlorophyll profile while the ARGO floats show a DCM around 50 m. To better understand this difference, we take the example of float 6901764 (Fig. 4) for which we see during the winters of 2015-16 and 2016-17 an alternation of mixed profiles and typical DCM-like profiles. If we consider the model in Fig. 4, we see that these alternations also exist but are less marked than in the observations (in particular when the Chl maximum deepens, the surface concentration does not decrease as much as in the observations) which probably explains the difference in the average profiles. Different hypotheses can be formulated to explain these alternations, which may correspond to small-scale spatial variability (of the order of a few kilometers for the dominant sub-mesoscale in winter) and/or temporal variability (wind intermittency). The study of this variability is clearly outside the scope of this paper. Nevertheless, it would be very interesting to study them in particular in order to understand if the model suffers rather from a lack of spatial resolution to reproduce the submesoscale or if the defect concerns rather the representation of the too slow phytoplankton response to the variability of physical forcing.

For the other seasons the systematic overestimation of the modeled DCM depth compared to float data, especially during summer and fall, could also be related to rates of biogeochemical processes in the model such as the remineralization, decomposition of particulate organic matter into dissolved organic matter, bacterial processes or settling velocity of detritus or micro-phytoplankton. This default could also be linked to the optical module that calculates the photosynthetically active radiation (PAR) available to support photosynthesis. Currently, water and phytoplankton concentrations are taken into account in the calculation, but not the concentrations of CDOM and detritus. Thus, sensitivity tests to biogeochemical rates and PAR calculation have to be performed to further improve the modeling of the chlorophyll profiles in our model.

As for the modeled surface chlorophyll a, Fig. 2 shows that during the mixing period, the model overestimates the maxima compared to the satellite data whereas it represents their magnitudes well when compared to the float data. We mentioned in the submitted manuscript, a potential explanation: the underestimation of satellite chlorophyll concentration during winter as suggested by Vidussi et al. (2001) and reported by D'Ortenzio et al. (2021). In their study, D'Ortenzio et al. (2021) compared surface chlorophyll concentration from satellite data, float data and in situ sampling during the PERLE2 cruise and reported lower values for the satellite compared to the float and cruise data during winter in the Rhodes Gyre.

Nevertheless, our validation shows that the main biogeochemical characteristics of the gyre such as the occurrence, shape and variability of the deep chlorophyll maximum (DCM), low nutrient concentration at the surface are well simulated. Thus, we believe that our modeling tool is at the state of the art and is fairly good to be used for the analysis of organic carbon in the Rhodes Gyre.

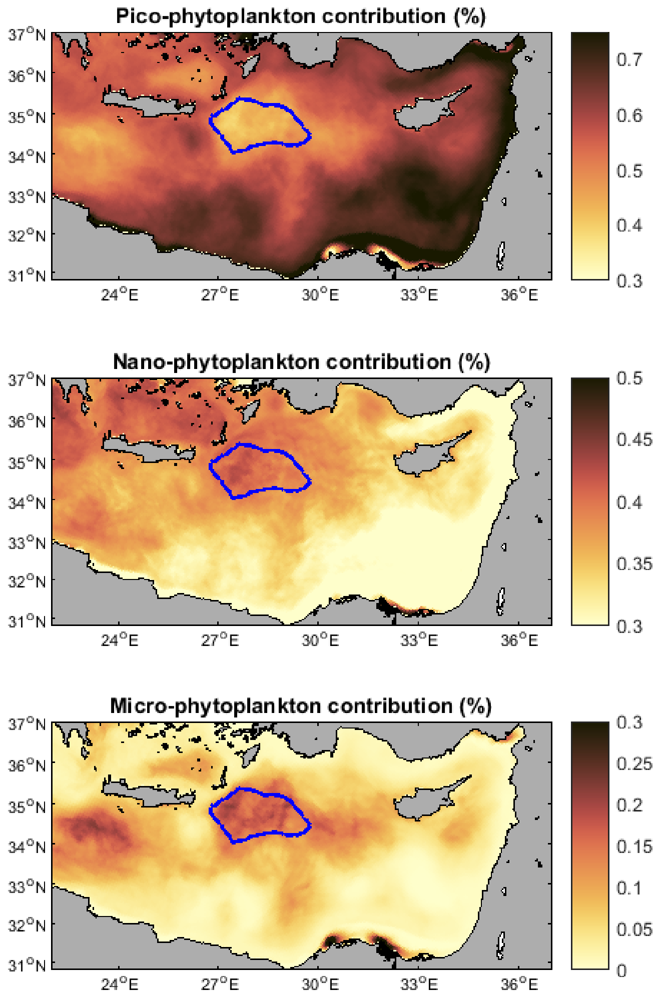
Concerning the question about the misrepresented DCM we add a comment in Section 4.1 (Model skill assessment):

Concerning the overestimation of the maximum depths, further studies will be necessary to improve the model parameterizations (optical model, POC degradation processes, particle sinking). Concerning the too strong homogenisation of the winter profiles, we notice that in winter, mixed chlorophyll and DCM-like profiles alternate indicating small scale (few kilometers) or temporal variability related to meteorological conditions. The study of the physical processes driving this variability and their impact on phytoplankton deserves a dedicated study of physical and biogeochemical Argo profiles and probably a higher spatial resolution modeling.

- The model produces grazers maxima at the level of DCM, which is not what is generally observed, especially considering that DCM phyto belong to the small size classes. Could they discuss this results analyzing which components of phyto and zoo are in those maxima?

Reply: In the model results, regarding the phytoplankton, we found that, in summer, the maximum of pico- and nano-phytoplankton is, respectively, 25 m and 15 m above the depth of the DCM, while the micro-phytoplankton maximum is 15 m below the DCM. While the nano- and micro-phytoplanktons growths are concentrated in the lower part of the surface layer, the pico-phytoplankton develops over the whole surface layer and shows a less pronounced maximum than the larger phytoplanktons. Over the surface layer (0-200 m) of the Rhodes Gyre, the pico-, nano- and micro-phytoplanktons represent, respectively, 46%, 33% and 21% of the total phytoplankton biomass in average over the year. This distribution varies little throughout the year. Regarding the zooplanktons, the three size-classes develop over the whole surface layer and their maximum concentrations in summer are located between 15 m and 50 m above the DCM (15 m, 30 m and 50 m for micro-, meso- and nano-zooplankton, respectively). The zooplankton concentration and grazing rates are maximum ~15 m above the DCM.

In previous modeling studies on the northwestern Mediterranean Sea based on the same coupled model, comparisons of modeled phytoplankton composition and zooplankton concentration with the data at the DYFAMED site were performed and showed that the model satisfactorily reproduced the main features in magnitude and variability, except the seasonal variation of meso-plankton that appears too smooth in the model (Auger et al., 2014; Ulses et al., 2016). In the open Levantine Sea and in particular in the Rhodes Gyre region, there are very few observation and modeling studies on consistent composition and vertical distribution of phytoplankton and zooplankton. For the present simulation the composition of phytoplankton over the Levantine Sea shows a domination of small phytoplanktons on average over the year and in winter (58%, 30% and 12% of the total phytoplankton for, respectively, pico-, nano- and micro-phytoplanktons). Based on in situ measurements, Vidussi et al. (2001) found that pico-, nano- and micro-phytoplanktons contribute in winter, respectively, to 27%, 62% and 12% of the total biomass. Besides, these authors showed a spatial variability of phytoplankton composition linked to meso-scale structures, with a higher contribution of micro-phytoplankton (26%) and lower contribution of pico and nano-planktons (15% and 59%) in cyclonic gyres compared to the other regions. The spatial variability in the composition of phytoplankton is well reproduced in the model (Fig. 3.1) and we obtain a similar contribution for micro-phytoplankton but we found a higher contribution for pico-phytoplankton and the opposite for nano-plankton. Furthermore, the modeled contribution of pico-phytoplankton in the South Aegean Sea in March and September (between 44% and 52%) is in the lower range of the observations of Ignatiades et al. (2002) (57% and 72%) and model results of Petihakis et al. (2009) (47% and 67%).


Pico-phytoplankton contribution (%)

Nano-phytoplankton contribution (%)

Micro-phytoplankton contribution (%)

Figure 3.1: Modeled contribution (%) of the three phytoplankton size-classes. Note that the range of the colorbar varies for the three sub-figures.

Our model's result on the annual cycle of total zooplankton profile is in line with the previous 3D modeling study of Macias et al. (2014) showing a maximum of zooplankton concentration near the DCM in spring at its development. Highest abundances of mesozooplankton between 50 and 135 m in the Levantine Sea were observed in previous studies (Koppelman et al., 2004; Nowaczyk et al., 2011) and suggested grazing near the DCM. Christaki et al. (2011) reported in the western Levantine Sea high abundances of total ciliates near the DCM, while the maximum abundance for hetero-nanoflagellates was found 50-70 m above the DCM. Tanaka et al. (2007) observed a strong variability for hetero-nanoflagellates and total ciliates between two stations located inside and outside the Cyprus anticyclonic eddy, in the eastern Levantine, near the surface (0-20 m), with higher values inside the eddy. At the same eddy region, Christaki et al. (2011) found a slight deep maximum in ciliates abundance and quite homogeneous vertical distribution of heterotrophic nanoflagellates. One can also mention the observations by Tanaka and Rassoulzadegan (2002) at the DYFAMED station in the NW Mediterranean Sea, where intermediate to deep convection occurs, showing a deep maximum near the DCM for ciliates biomass in August 1999.

In this study over the Levantine Sea, which constitutes a first step of a longer term work, we consider and present the dynamics of phytoplankton and zooplankton as a whole. The analysis of the spatial and temporal variability of phytoplankton and zooplankton composition, in response to the interannual variability in temperature, circulation and meso-scale structures appears to us beyond the scope of this study. We think that adding a discussion on the composition will not be appropriate if we haven't presented them in the result section. We propose to add few words in Section 4.1:

Comparisons with complementary biological (in particular composition and biomass of phytoplankton and zooplankton) and biogeochemical observations carried out during the PERLE cruises whose analyses are in progress will be used in near future studies to continue the evaluation.

Nevertheless since we haven't yet compared our model's results on zooplankton (concentration, composition) specifically in the study area, the Rhodes Gyre, if Reviewer 3 recommend it, we propose to remove the text and figure on zooplankton from Section 3 and add this limitation in the discussion 4.1 section.

- Table 1 reports interesting result on the interannual variability in carbon fluxes. These and the implications of the biogeochemistry of the basin are not discussed at all. Neither is discussed the weight of the gyre in the carbon fluxes of the whole basin.

Reply: In the model, the area of the Rhodes Gyre (27 000 km^2) represents 5% of the Levantine basin area (540 000 km^2), it contributes to 11% of the organic carbon production (~ 826 ± 19 Gmol C yr^{-1}) in the surface layer of the Levantine basin. The annual consumption in the intermediate layer amounts to 233 ± 85 Gmol C yr^{-1}. It represents 3% of the biogeochemical consumption estimated by the model in the whole Levantine Basin. Regarding the vertical carbon export in the Rhodes Gyre, it represents 5% of the total OC vertical export in the whole basin. In this article, we prefer to limit the results to the Rhodes Gyre; therefore the implications of these fluxes and the weight of the Rhodes Gyre in the carbon fluxes of the whole basin were not discussed but will be detailed in a future paper.

- Both GPP and NPP display high values, for GPP maxima, in summer. Considering the water column structure and the position of the DCM, this should be mostly recycled production in the upper part of the water colums with, as a side effect, an accumulation of DOC (Figs. 7a, 7b and 11). Information on the players and comments on the mechanisms could be helpful.

Reply: In the model results, in summer the rate of GPP is maximum at the lower part of the surface layer, near the DCM (Figure 3.2). The rates of uptake of nitrate and ammonium, reflecting new and recycled production, respectively, also show maximum values, respectively at the depth of the DCM and 15 m above the DCM in summer (Fig. 3.2). The uptake of ammonium takes place in the whole surface layer, while the uptake of nitrate is restricted to the lower part of the surface layer. The uptake of ammonium in the surface layer represents 78% of the total nitrogen uptake.

The DOC accumulation in the upper part of the surface layer in spring and summer is mostly explained by the biogeochemical processes with DOC production processes (messy feeding (12%), exudation (66%), decomposition of POC (17%) and bacteria mortality (5%)) that exceeds over DOC consumption processes (bacteria growth (17%) and respiration (83%)) at that period (Fig. 3.2).

We added the following sentence in Section 3.4:

The modeled gross primary production (GPP) generally follows the cycle of the solar insolation (not shown) with minimum values in December and maximum values at the end of June. A secondary peak is visible between February and April (Fig. 7b). Its vertical distribution (not shown) is close to the one of phytoplankton (Fig. 6c) and mostly relies on recycled production (ammonium uptake represents 78% of total nitrogen uptake).

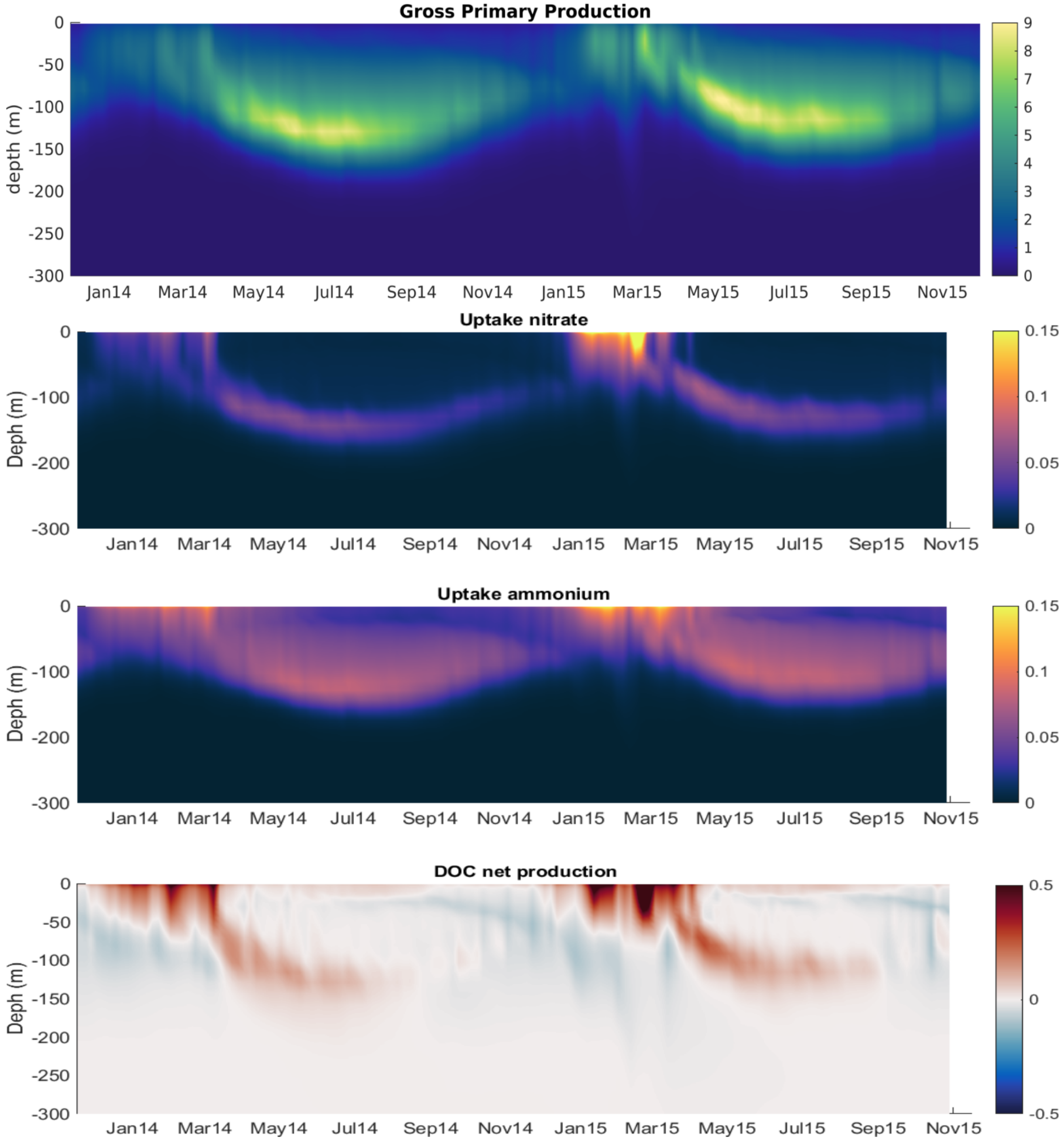


Figure 3.2: **Hovmöller diagrams of modeled gross primary production (mmol C m^{-3} d^{-1}), uptake of nitrate (mmol N m^{-3} d^{-1}), uptake of ammonium (mmol N m^{-3} d^{-1}) and DOC net production (mmol C m^{-3} d^{-1}), averaged over the Rhodes Gyre, from December 2013 to December 2015.**

Below minor suggestions/remarks

l.52 ...with surface temperature reaching 25 °C ..this value is an underestimate if the max considers all seasons (see El-Geziry, Acta Oceanol. Sin., 2021, Vol. 40, No. 3, P. 1–7, https://doi.org/10.1007/s13131-021-1709-2)

Reply: We thank the reviewer. The value 25 °C used in the manuscript represents the climatological temperatures which vary between 16 and 25.5 °C based on Manca et al. (2004). This value is underestimated compared to the more recent suggested reference. The value has been changed and the new reference has been added to the revised manuscript.

l.90 Palmiéri et al., 2015 wrong date or missing

Reply: This was corrected as suggested in the revised manuscript.

l.140 Kessouri, 2015 either wrong date or missing see also l.967

Reply: This was corrected as suggested in the revised manuscript.

l.713 & l.714 are they different? see ref. on l.1241 Otherwise Ulss et al. subm. is missing in the list

Reply: Since this article is not yet published, its reference was removed from the revised manuscript.

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