### Author responses to Editor comments for the manuscript: "Acidification, deoxygenation, nutrient and biomasses decline in a warming Mediterranean Sea" April, 2022

We thank the Editor for their positive feedback and for providing detailed comments and suggestions, that helped us to improve the manuscript. Editor's comments are in bold, authors' responses are in normal font, italicized where they quote the changes in the manuscript.

I hope this comment does not come too late. I refer to figure 18 panels e-l. It is inappropriate to express changes in pH in percent. It should be percent of the hydrogen ion concentration. Or, perhaps better, show the changes in pH units. See: Fassbender A. J., Orr J. C. & Dickson A. G., 2021. Technical note: interpreting pH changes. Biogeosciences 18:1407-1415.

We thank the Editor for pointing out the error in Figure 18. It has been redrawn following the Editor's suggestion. Here we show the "new" Figure 18 with its relative caption:



7.90 7.93 7.96 7.99 8.02 8.05 8.08 8.11 8.14



Fig. 18 -pH in the layers 0-100m and 200-600m in the PRESENT (2005-2020, a,b,c and d), and relative climate change signal (with respect to the PRESENT, in units of pH) in the MID-FUTURE (2040-2059, e,f,g and h) and FAR-FUTURE (2080-2099, i,j,k and l) in the RCP4.5 (left column) and RCP8.5 (right column) scenarios. The Mediterranean average relative climate change signal in each period (with respect to the PRESENT) is displayed by the top-left colored value (blue or dark orange when negative or positive). Values in the green boxes is the average relative climate change in each period and in each sub-basin shown in Figure 1. Domain grid points where the relative climate change signals are not statistically significant according to a Mann-Whitney test with p < 0.05 are marked by a dot.

### It would be great to have a summary table providing values of key variables with the 2 scenarios at present, mid and far future.

We thank the Editor and Reviewer#1 for the suggestion. A new table has been included in Supplementary materials showing the "unbiased scenario" values (as defined in the Data and Methods section) of Temperature, Salinity, Phosphate, Nitrate, Dissolved Oxygen, Phytoplankton and Zooplankton biomass, Integrated net primary production, Dissolved Inorganic Carbon, and pH in the PRESENT, MID-FUTURE and FAR-FUTURE. As in the rest of the work, the two layers of 0-100m and 200-600m are considered. All the values in bold that are statistically significant different from the mean value in the PRESENT according to a Mann-Whitney test with p<0.05. Here we show the new table added:

		RCP4.5			RCP8.5		
		PRESENT	MID-FUTURE	FAR-FUTURE	PRESENT	MID-FUTURE	FAR-FUTURE
Seawate	er Tempe	rature (°C)					
WMED	0-100	16.26	16.82	17.49	16.43	17.25	19.06
	200-600	13.94	14.89	15.65	14.02	15.26	16.59
EMED	0-100	18.25	18.94	19.79	18.40	19.5	21.72
	200-600	14.55	15.00	15.81	14.6	15.36	16.83
Seawate	er Salinity	v <b>(-)</b>		-			
WMED	0-100	37.40	36.85	36.97	37.41	36.88	36.99
	200-600	38.60	38.81	38.75	38.64	38.88	38.96
EMED	0-100	38.65	38.32	38.47	38.61	38.40	38.76
	200-600	38.94	38.92	38.90	38.94	38.99	39.12
PO4 (mi	mol m <sup>-3</sup> )						
WMED	0-100	0.140	0.135	0.138	0.140	0.131	0.132
	200-600	0.290	0.300	0.280	0.300	0.29	0.28
EMED	0-100	0.035	0.028	0.029	0.030	0.026	0.024

	200-600	0.018	0.017	0.017	0.017	0.017	0.017	
NO <sub>3</sub> (m	mol m <sup>-3</sup> )							
WMED	0-100	1.03	0.98	1.01	0.99	0.93	0.93	
	200-600	4.63	4.73	4.47	4.75	4.67	4.65	
EMED	0-100	0.23	0.15	0.16	0.19	0.12	0.10	
	200-600	3.14	3.07	3.03	3.01	2.98	2.99	
Dissolve	ed Oxyger	n (mmol m <sup>-3</sup> )						
WMED	0-100	237	233	231	235	230	223	
	200-600	213	207	204	211	205	196	
EMED	0-100	234	232	228	233	229	219	
	200-600	219	217	213	220	217	208	
Phytopl	Phytoplankton biomass (mg m <sup>-3</sup> )							
WMED	0-100	12.94	11.81	11.92	12.72	11.46	10.40	
EMED	0-100	7.47	6.33	6.23	7.00	5.73	4.81	
Zooplankton biomass (mg m <sup>-3</sup> )								
WMED	0-100	14.48	14.02	14.20	14.58	14.05	13.72	
EMED	0-100	11.73	10.92	10.91	11.37	10.50	9.82	
Integra	ted net pr	imary produ	uction (gC m <sup>-2</sup> ye	ar <sup>-1</sup> )				
WMED	0-200	135	134	144	137	139	156	
EMED	0-200	140	136	149	137	139	162	
Dissolve	ed Inorga	nic carbon (j	µmol kg <sup>-1</sup> )					
WMED	0-100	2276	2297	2322	2270	2301	2375	
	200-600	2373	2404	2447	2375	2404	2495	
EMED	0-100	2325	2363	2400	2318	2372	2484	
	200-600	2382	2410	2452	2379	2412	2505	
рН (-)								
WMED	0-100	8.07	8.03	8.00	8.07	8.01	7.88	

	200-600	8.08	8.05	8.00	8.08	8.03	7.92
EMED	0-100	8.09	8.03	7.99	8.09	8.00	7.84
	200-600	8.1	8.06	8.02	8.10	8.05	7.92

Table SP1. "Unbiased scenario" values for seawater temperature, salinity, dissolved phosphate, nitrate and oxygen concentrations, Phytoplankton and Zooplankton biomass at surface, vertically integrated net primary production, Dissolved Inorganic Carbon and pH in the PRESENT (2005-2020), MID-FUTURE (2040-2059) and FAR-FUTURE (2080-2099) time windows. All the values shown in bold format are significantly different from the values of the PRESENT period according to a Mann-Whitney test with p<0.05.

The table is introduced in the "Discussion and conclusion" as follows (lines 841-850):

"Our projections for the biogeochemical tracers and properties at the end of the 21st century shows several signals (Table SP1 for a synthetic overview) that are mostly in agreement with previous studies, at least with those based on the use of the worst-case emission scenarios.

The magnitude of the projected changes has been shown to be, in general, scenario-dependent with the largest deviations from the present climate state observed in the RCP8.5 emission scenario (Table SP1). On the other hand, the analysis of the projections under RCP4.5 emission scenario by the end of the 21st century found in most of the biogeochemical variables (for example dissolved nutrients and biomasses) a tendency to recover the values observed in the present climate (Table SP1)."

## Please also note that I disagree with the focus on RCP8.5. You mentioned in one of your replies that it is extensively used but we are currently not tracking this scenario due to weak but significant climate action. It is now considered to be an unrealistically extreme emission scenario.

We thank the Reviewer#2 and the Editor for raising this point. Initially, the Discussion and Conclusion was generally focused on RCP8.5 due to the fact that available results in the scientific literature of the Mediterranean Sea mainly concern worst-case emission scenarios (e.g. Moullec et al., 2019; Richon et al., 2019; Solidoro et al., 2022). Only recently, we became aware that RCP8.5 has been proposed as an "implausible" scenario by some part of the scientific community (e.g. Hausfather and Peters, 2020). However, in order to better address the request from Reviewer and Editor we extensively modified the manuscript to accentuate the importance of the intermediate scenario and to discuss the differences between the two scenarios.

For example, in the abstract we now state (lines 16-28):

"The analysis shows in both scenarios changes in the dissolved nutrient content of the euphotic and intermediate layers of the basin, net primary production, phytoplankton respiration and carbon stock (including phytoplankton, zooplankton, bacterial biomass and particulate organic matter) [...] The projected changes are stronger in the RCP8.5 (worst-case) scenario and, in particular, in the Eastern Mediterranean due to the limited influence of the exchanges in the Strait of Gibraltar in that part of the basin. On the other hand, the analysis of the projections under RCP4.5 emission scenario shows a tendency to recover the values observed at the beginning of the 21st century for several biogeochemical variables in the second half of the period. This result supports the idea - possibly based on the existence, in a system like the Mediterranean Sea, of a certain buffer capacity and renewal rate - that the implementation of policies of reducing CO<sub>2</sub> emission could be, indeed, effective and could contribute to the foundation of ocean sustainability science and policies."

In the results section we further accentuated the discussion of the differences between the two scenarios. For example, in section 3.3 we write (lines 556-559):

"During the 21st century, a continuous decrease in the oxygen concentration is projected in both scenarios in the Mediterranean Sea (Fig. 8). The simulated reduction of the oxygen values is slower in the RCP4.5 with respect to RCP8.5. For example, under the RCP8.5 emission scenario, the concentration of the dissolved oxygen in the upper layer decreases by approximately 15 mmol m<sup>-3</sup>, which is three times the value observed in the RCP4.5 scenario (Fig. 8)."

or in section 3.4 (line 656-660):

"The evolution of the carbon organic matter standing stock is similar to that observed in the dissolved nutrients, with a substantially stable signal in the first 30 years of the 21st century and a decrease after 2030. Afterwards, while RCP4.5 shows a recovery at the end of the 21st century, the projected decline in RCP8.5 is approximately equal to 5 mgC m<sup>-3</sup>. The same dynamics is observed in the intermediate layer, where the decline after the period 2030-2035 is approximately equal to 0.3 mgC m<sup>-3</sup> for the carbon stock"

Finally, in Discussion and conclusion we state for example (lines 844-860):

"Our projections for the biogeochemical tracers and properties at the end of the 21st century shows several signals (see Table SP1 for a synthetic overview) that are mostly in agreement with previous studies, at least with those based on the use of the worst-case emission scenario.

The magnitude of the projected changes has been shown to be, in general, scenario-dependent with the largest deviations from the present climate state observed in the RCP8.5 emission scenario (Table SP1). On the other hand, the analysis of the projections under RCP4.5 found in most of the biogeochemical variables (for example dissolved nutrients and biomasses) a tendency to recover the values observed in the present climate by the end of the 21st century (Table SP1). As shown in the previous sections, our simulations, by covering also the RCP4.5 scenario, highlights how an intermediate greenhouse emission scenario produces results that are not simply an average between the present condition and the RCP8.5, but (at least for some variables) something quantitatively different. For example, the temporal evolution of pH (Fig.15) is similar in two scenarios in the first 30 years of the 21st century. Conversely, after 2050, pH undergoes a substantial decrease under RCP8.5 while it remains almost stable under RCP4.5 with a final projected variation lower than the half with respect to the worst-case scenario.

This supports the idea - possibly based on the existence of a certain buffer capacity and renewal rate in a system like the Mediterranean Sea - that the implementation of policies of reducing CO<sub>2</sub> emission could be, indeed, effective and could contribute to the foundation of ocean sustainability science and policies."

We are confident that all the changes made to the manuscript will address both Reviewer and Editor's concerns.

Finally, Biogeosciences strongly promotes the full availability of the data sets reported in the papers that it publishes in order to facilitate future data comparison and compilation as well as meta-analysis. This can be achieved by uploading the data sets in an existing database and providing the link(s) in the paper. Alternatively, the data sets can be published, for free, alongside the paper as supplementary information. The ascii (or text) format is preferred for data and any format can be handled for movies, animations etc...Please mention the exact path to your data on dds.cmcc.it, and the doi if there is one.

We fully support the Biogeosciences policy. We have uploaded, in netcdf format, the dataset including the variables discussed in the manuscript in the data portal dds.cmcc.it where they are now available under open access license. Following the Editor's request we modified the "Data availability" statement as follows:

"Both physical and biogeochemical data produced in the numerical experiments are available at <a href="https://dds.cmcc.it/#/dataset/medsea-cmip5-projections-physics">https://dds.cmcc.it/#/dataset/medsea-cmip5-projections-physics</a> and <a href="https://dds.cmcc.it/#/dataset/medsea-cmip5-projections-physics">https:/

### Author responses to Reviewer#1 comments for the manuscript: "Acidification, deoxygenation, nutrient and biomasses decline in a warming Mediterranean Sea" April, 2022

The authors projected the climate change-related impacts in the marine ecosystems of the Mediterranean Sea in the middle and at the end of the 21st century using an offline coupling model combining the physical model MFS16 and the transport-reaction model OGSTM-BFM, under emission scenarios RCP4.5 and RCP8.5, focusing on the middle and the end of 21<sup>st</sup> century. Projected changes are presented for temperature, salinity, dissolved nutrients and oxygen, net primary production, respiration, organic matter, plankton and bacterial biomass, particulate organic matter, and biogeochemical parameters (DIC, pH).

### The paper provides interesting projections in a changing Mediterranean Sea that is already under multiple pressures.

We thank the Reviewer #1 for their positive feedback and for providing detailed comments and suggestions, which helped us to improve the manuscript. Reviewer's comments are in bold, authors' responses are in normal font, italicized where they quote the changes in the manuscript.

### Major comments:

1) P3, L75: No, not "all" the modelling studies focused on high emissions scenarios. For example, there is Benedetti et al. (2018) who used A2, A1B and B1, and Goyet et al. (2016) who used B1 and A1F1.

- Benedetti, F., Guilhaumon, F., Adloff, F. and Ayata, S.-D. (2018), Investigating uncertainties in zooplankton composition shifts under climate change scenarios in the Mediterranean Sea. Ecography, 41: 345-360. https://doi.org/10.1111/ecog.02434
- Goyet, C., Hassoun, A., Gemayel, E., Touratier, F., Abboud-Abi Saab, M. and Guglielmi, V., 2016. Thermodynamic forecasts of the mediterranean sea acidification. Mediterranean Marine Science, 17(2), pp.508-518. Thermodynamic Forecasts of the Mediterranean Sea Acidification | GOYET | Mediterranean Marine Science (ekt.gr).

Goyet et al. (2016) is the only modelling study that it is projecting carbonate system parameters in the Mediterranean Sea so far, and the one used in MedECC (2020; cited by the authors to tackle OA projections in the Mediterranean). Yet, it is not mentioned at all in this work. Please check this study out and try to compare your results with theirs.

We thank the Reviewer for pointing out this error in the sentence. We have included and discussed the suggested references in the manuscript. In the introduction we state (lines 85-117):

"An assessment of the effects of climate change on the biogeochemistry and marine ecosystem dynamics of the Mediterranean Sea has been considered in a number of previous studies based on different emission scenarios".

"Benedetti et al. (2018), using environmental niche models and considering six physical simulations based on different emission scenarios (A2, A2-F, A2-RF, A2-ARF, A1B-ARF, B1-ARF; Adloff et al., 2015), projected, in response to climate change, a loss of copepods diversity throughout most of the surface layer of the Mediterranean Sea."

Goyet et al. (2016) is discussed in the "Discussions and Conclusions" section where it is stated that (lines 909-916):

"The overall accumulation of  $CO_2$  in the basin resulted in an acidification of the Mediterranean water with a decrease in pH of approximately 0.23 units in the worst-case scenario, which is slightly lower than the 0.3 projected on a global scale (Kwiatkowski et al., 2020) and lower than the value provided in Goyet et al. (2016), who projected, using thermodynamic equations of the  $CO_2$ /carbonate system chemical equilibrium in seawater, a variation of 0.45 pH units in the basin under the worst SRES case scenario (and 0.25 pH units in the most optimistic SRES scenario). However, this last estimate probably tends to overestimate the future acidification of the basin, as it does not consider the decrease in the exchanges and the penetration of  $CO_2$  across the ocean-atmosphere interface due to the warming of the water column (MedECC, 2020)."

2) P3, L78-79: The authors mentioned that Moullec et al. (2019), under RCP8.5 emission scenario, found an increase in both phytoplankton biomass and net primary production by the end of the 21<sup>st</sup> century. However, this pattern is not homogenous in the Mediterranean since Moullec et al. (2019) have also highlighted a difference between the Eastern and Western basins with an increase in the first and a decrease in the second. Please edit accordingly.

Agreed. The paragraph has been reformulated as follows (lines 95-97):

"On the other hand, Moullec et al. (2019), under RCP8.5 emission scenario, found an increase/decrease in both phytoplankton biomass and net primary production by the end of the 21st century in the Eastern/Western Mediterranean Sea."

P4, L111-113: In addition to the BOUM mesoscale experiments working on relating eddies with biogeochemical changes (BG - Influence of anticyclonic eddies on the Biogeochemistry from the Oligotrophic to the Ultraoligotrophic Mediterranean (BOUM cruise) (copernicus.org)), there are actually many modelling studies, for example:

- Ramirez-Romero E, Jordà G, Amores A, Kay S, Segura-Noguera M, Macias DM, Maynou F, Sabatés A and Catalán IA (2020) Assessment of the Skill of Coupled Physical–Biogeochemical Models in the NW Mediterranean. Front. Mar. Sci. 7:497. doi: 10.3389/fmars.2020.00497
- Guyennon, A., Baklouti, M., Diaz, F., Palmieri, J., Beuvier, J., Lebaupin-Brossier, C., Arsouze, T., Béranger, K., Dutay, J.-C., and Moutin, T.: New insights into the organic carbon export in the Mediterranean Sea from 3-D modeling, Biogeosciences, 12, 7025–7046, https://doi.org/10.5194/bg-12-7025-2015, 2015.

• Herrmann, M., Somot, S., Sevault, F., Estournel, C., and Déqué, M. (2008), Modeling the deep convection in the northwestern Mediterranean Sea using an eddy-permitting and an eddy-resolving model: Case study of winter 1986–1987, J. Geophys. Res., 113, C04011, doi:10.1029/2006JC003991.

#### Therefore, I would suggest to re-write this paragraph.

We agree with the Reviewer that, before our work, there have already been other observational/modeling efforts to resolve the eddies dynamics in the Mediterranean Sea and its impacts on the biogeochemistry. However, our work has two specific differences with respect to the available literature: i) the works listed by the Reviewer, although being eddy-resolving, focus either on limited areas of the Mediterranean Sea or on a single variable/physical process; (ii) they are hindcasts and do not provide projections for the biogeochemical variables under different emission scenarios. In order to address the Reviewer's comment, we reformulated the paragraph as follows (lines 132-139):

"These considerations emphasize the importance of providing eddy-resolving future projections of the Mediterranean Sea biogeochemistry, under different emission scenarios. In fact, although observational and modeling studies have been carried out in the recent period to assess the importance of the mesoscale dynamics in the physical and biogeochemical state of limited areas of the Mediterranean Sea (e.g. Hermann et al., 2008; Moutin and Prieur, 2012; Guyennon et al., 2015; Ramirez-Romero et al., 2020), long-term eddy-resolving biogeochemical projections under different emission scenarios, to the best of the authors' knowledge, have not been analyzed so far in the region. Such projections might be used in future studies specifically focused on the analysis of climate change impact on specific organisms, habitats and/or local areas."

# P8, L263-266: To characterize the spatial distribution and the variability of anomalies, the authors considered their horizontally averages in each sub-basin in the Western Mediterranean (WMED=(ALB+SWM+NWM+TYR)/4) and in only two sub-basins of the Eastern Mediterranean (EMED=(ION+LEV)/2). Why did you exclude the Adriatic and the Aegean Sub-basins here?

We thank the Reviewer for pointing this issue, because we realized that this explanation was missing in the manuscript. Here we followed the approach already adopted in the other works (e.g. Lazzari et al., 2012; 2016; Di Biagio et al., 2019; Reale et al., 2020 a,b) where the characteristics of both Adriatic and Aegean Sea (e.g. the paramount importance of riverine inputs or Dardanelles straits in the biogeochemical dynamics of both basins) are considered such peculiar to make both basins separate with respect to the Eastern Mediterranean. For this reason, they are not included in our averages.

In order to address the Reviewer's comment, we modified the sentence as follows (lines 307-313):

"Horizontal spatial averages are computed considering the sub-basins defined in Fig. 1, the whole Mediterranean basin and two macro-areas: Western Mediterranean (WMED which includes ALB, SWM, NWM, TYR) and the Eastern Mediterranean (EMED which includes ION and LEV). The Adriatic and Aegean Seas are usually not considered part of the Eastern Mediterranean due to the importance of local forcing, such as riverine loads, in shaping the variability of the biogeochemical dynamics in those two sub-basins. Because of that, following the approach already adopted in previous works (Lazzari et al., 2012; 2016; Di Biagio et al., 2019; Reale et al., 2020 a,b) they are not considered in the spatial averages related to WMED and EMED." P9-10, L306-315 & Fig. 2: The authors mentioned that mean simulated values in the first 0-200 m are quite realistic in all the variables, and that biases started to show at 600 m depth. However, the vertical profiles show such discrepancies between CTRL average profiles and observational data (EMODnet) even in shallower depths, i.e. less than 50 m for phosphate in the WMed., surface waters for nitrate in the WMed., greater than 200 m for oxygen, and so much general biases in pH. Could you please elaborate more on this?

We thank the Reviewer for raising this point and remarking this lack in the manuscript. We agree with the Reviewer that the model clearly shows some underestimation/overestimation in some of the simulated biogeochemical variables that have not been thoroughly discussed. On the other hand, our validation shows that the main biogeochemical characteristics of the basin such as the occurrence, shape and spatial variability of the deep chlorophyll-*a* maximum (DCM), nutricline deepening between Western and Eastern basin, low nutrient concentration at the surface, vertical profiles of DIC, spatial distribution of total alkalinity and pH are well simulated.

Additionally, as we explained better in section 2.4, we employed a simulation protocol aimed at removing the internal model biases and drifts that allowed a more robust estimation of the scenario trends. Inevitably, the adoption of a very long spin-up simulation generated some inconsistencies with regard to reference data for validation.

Thus, without presuming to oversell our results, we believe that our modeling tool is fairly good to be used for scenario simulations, also considering the levels of validation of other scenario simulations published in literature (see for example Richon et al., 2019 and Solidoro et al., 2022).

However, we further elaborated the paragraph by listing the major biases in variables and layers. More specifically we rewrote the paragraph as follows (lines 357-375):

"Figure 2 also shows the average vertical profiles, computed for the entire, the Western and Eastern Mediterranean basins, of Chl-a (c), PO4 (d), NO3 (e), dissolved oxygen (f), DIC (g), pH (h) and total alkalinity (i) in the CTRL compared with the recent CMEMS reanalysis (only for Chl-a and pH, Cossarini et al., 2021) and EMODnet datasets (European Marine Observation and Data Network; Buga et al., 2018). In spite of the tendency to overestimate the Chl-a values, the model captures the DCM location, the west-east trophic gradient in the basin, and also the nutricline depths deepening between Western and Eastern basin and the low nutrient surface concentrations. Mean simulated values in the first 0-200 m are quite realistic for almost all the biogeochemical tracers and properties, with correlation values between observations and modelled data greater than 0.93. At the same time, the CTRL overestimates the PO4 concentration between 100 and 300 m of about 50%, and the dissolved oxygen concentration of about 15% below 200 m and underestimates, below 200 m, the NO3 concentration of about 20% and the pH of about 1% between 100 and 300 m. It is worthwhile to point out the low resolution of the observations below 200 m that could make our comparison less robust. In general, the biases in the initial conditions are originated by the spin-up simulation that allows to remove the largest part of model drifts. As explained in section 2.4, these biases, which are still present in both the CTRL and scenario simulations, do not affect the calculation of the climate change signals, and are generally lower than the changes observed in the scenarios at the end of the century. To summarize, although the model shows some deficiencies in simulating the vertical distribution of some biogeochemical tracers and properties, the main features of the system are reliably simulated and thus, MFS16-OGSTM-BFM is robust enough to be used to investigate the evolution of the Mediterranean biogeochemistry under different emission scenarios."

### P11, L326: Could you explain in the text the depth classification adopted in this study: 0-100 m and 200-600 m?

Agreed. The sentence was modified as follows (lines 389-392):

"Mean temperature and salinity evolution between 0-100 m and 200-600 m in the 2005-2099 period under the RCP4.5 and RCP8.5 scenarios in the whole Mediterranean Sea and in the Western and Eastern basins are shown in Fig. 3. As for the biogeochemical variables, these depths have been chosen as they are representative of the location of MAW and LIW, respectively".

### P12, L342-358: Is it possible to check if those differences are significant or not?

Following also the Reviewer#2's suggestion we extensively redrew the figures including also an assessment of the statistical significance of the observed difference using the Mann-Whitney test with p<0.05. Just as an example here we report the "new" Figure 5 with the relative caption. With this new layout of our figures we think that the reader may capture much better both the intensity of the change and where this change is significant, masking out all the areas where it is not significant.





46°N

40°N

35°N

PRESENT (b) 0-100m RCP8.5

PRESENT (a) 0-100m RCP4.5

46°N

40°N

35°N

Fig. 5 - Phosphate concentration (in mmol  $m^3$ ) in the layers 0-100 m and 200-600 m in the PRESENT (2005-2020, a,b,c and d), and relative climate change signal (with respect to the PRESENT) in the MID-FUTURE (2040-2059, e,f,g and h) and FAR-FUTURE (2080-2099, i,j,k and l) in the RCP4.5 (left column) and RCP8.5 (right column) emission scenario. The Mediterranean average relative climate change signal in each period (with respect to the PRESENT) is displayed by the top-left colored value (blue or dark orange when negative or positive). Values in the green boxes are the average relative climate change in each period and in each sub-basin shown in Figure 1. Domain grid points where the relative climate change signals are not statistically significant according to a Mann-Whitney test with p<0.05 are marked by a dot.

### P20, L476: Please explain briefly the role of "damping effect" in controlling oxygen values at the Gibraltar Strait?

We thank the Reviewer for raising this point. Here, with damping effect we refer to the fact that the inflow of biogeochemical properties at the Gibraltar Strait plays a fundamental role in driving the biogeochemical dynamics of the sub-basins near the Strait such as the Alboran Sea, partially limiting, in the case of the oxygen, the reduction in the solubility at the surface in response to the warming of the water column. To better address the Reviewer's comment we revised the sentence as follows (lines 564-568):

"The projected decreases in both scenarios are usually lower in the Alboran Sea and South Western Mediterranean with respect to the rest of the basin, as a consequence of the damping effect driven by the oxygen values imposed at the Atlantic boundary. In fact, the advection of dissolved oxygen associated with Atlantic Water partially limits the reduction in the oxygen solubility at the surface as a consequence of the warming of the water column in the sub-basins near the Strait of Gibraltar, such as the Alboran Sea."

### P24, L565-566: I guess you are talking here about the "projected" change not the "observed" change. In any case, I would suggest to better re-write this sentence.

Agreed. The sentence was reformulated as follows (lines 663-665):

"In the RCP4.5 simulation for all these biogeochemical tracers, in general a recovery in the biomass at the end of the 21st century is found and the projected changes are approximately 50% with respect to the RCP8.5 scenario where no recovery is observed"

P29, Fig.16: Captions on the plots should be corrected to distinguish between its different components (a-f), as well as between the locations (Med., WMed., EMed.).

We thank the Reviewer for spotting the error in the figure. The figure was modified accordingly.

In section 2, authors refer to alkalinity (ALK) in the text (i.e. L188). Do you mean by this term, the number of moles of hydrogen ions equivalent to the excess of proton acceptors (bases formed by weak acids) over proton donors (acids) in a kilogram of sample? Mostly yes, and this term should be labeled total alkalinity (DOE, 1994): TA = [HCO3-] + 2[CO32-] + [B(OH)4-] + [OH-] - [H+]. Moreover, except for figure 2 for the period 2005-2020, there is no results about ALK in the following sections. Why? Yes, the carbonate chemistry of BFM follows the OCMIP protocols, thus considering the alkalinity as "Total". The manuscript has been modified, including the adjective "total" every time we refer to alkalinity. We included "total" alkalinity in Figure 2 to show that the MFS16-OGSTM-BFM reproduces fairly well the prognostic variables of the carbonate system (DIC and total alkalinity), while, to limit the size of the manuscript we preferred to focus our attention on DIC and pH for the carbon budget, as they have already been discussed in other manuscripts (such as Solidoro et al., 2022), and to leave a deeper discussion of the carbon budget, including total alkalinity, to a future dedicated study.

## The term "tracers" is usually used for conservative elements that can be traced in function of time. It is not the appropriate term for the carbonate system parameters, such as TA, DIC or pH. Please refer to them as biogeochemical parameters/features/properties but not tracers.

We agree with the Reviewer that some of the variables considered such as pH or pCO<sub>2</sub> cannot be considered tracers. However, the dynamics of other variables such as DIC and total alkalinity are described within the BFM by conservative equations and thus they can be considered like tracers. In order to accommodate with Reviewer's comment in the manuscript we refer to about *"biogeochemical tracers and properties"* 

### **Minor comments:**

### Please write the E and W in Eastern and Western in capital letters, and unify this in the text.

The text has been modified accordingly.

While I would suggest to add "sub-basin" to any sub-entity in the Mediterranean (i.e. Alboran Sub-basin, Levantine Sub-basin, etc.), it is OK to use "sea" instead like many other publications (i.e. Adriatic Sea, Aegean Sea). However, I would recommend the authors to unify the terms adopted throughout the manuscript since they use "Adriatic Sea and Levantine basin", why? Also, sometimes you refer to the Gulf of Lion as Gulf of Lions (i.e. L445). Please rectify and unify this in the ms.

We thank the Reviewer for the suggestion. We will extensively check the text and fix the typo related to the Gulf of Lion. Moreover, we would prefer, if not explicitly required by the Reviewer, to maintain the original name of each sub-basin in the text. The reason for that is that, essentially, all these sub-basins (for example the Alboran Sea) are reported in the scientific literature of the Mediterranean Sea with the original name (see Lazzari et al., 2012 or Cossarini et al., 2021), not often followed by the term "sub-basin", although it is well known that they are sub-basins of the Mediterranean Sea. The only exception is the Levantine which is often followed by the term "basin". Because of that we adopt this approach in our manuscript.

### Please make italic the "a" in Chl.a throughout the text.

The text has been modified accordingly.

Please write "time-series" instead of "timeseries" throughout the entire manuscript (as you have already done it in L502).

The text has been modified accordingly.

### Abstract:

P1, L16-18: Please write it as follows "The analysis shows significant changes in the dissolved nutrient content of the euphotic and intermediate layers of the basin, of the net primary production, phytoplankton respiration and carbon stock (including phytoplankton, zooplankton, bacterial biomass and particulate organic matter)."

Agreed. The text has been modified as follows (lines 16-18):

"The analysis shows in both scenarios changes in the dissolved nutrient content of the euphotic and intermediate layers of the basin, net primary production, phytoplankton respiration and carbon stock (including phytoplankton, zooplankton, bacterial biomass and particulate organic matter)"

### P1, L20: Please avoid using personal pronouns. The sentence can be written as follows "Moreover, an acidification trend (signal) was observed in the upper water column...".

Agreed. The text in the manuscript has been extensively modified following the Reviewer's suggestion.

P1, L22-23: Please write it as follows "The projected changes are stronger in the Eastern Mediterranean due to the limited influence of the exchanges in the Strait of Gibraltar in that part of the basin."

Agreed. The text has been modified as follows (lines 22-23):

"The projected changes are stronger in the RCP8.5 (worst-case) scenario and, in particular, in the Eastern Mediterranean due to the limited influence of the exchanges in the Strait of Gibraltar in that part of the basin."

### Introduction:

P1, L31: These are some key references (Lascaratos, 1993; Nittis and Lascaratos, 1998) but they are old. I would suggest to also add newer ones, i.e.

- Fedele, G., Mauri, E., Notarstefano, G., and Poulain, P. M.: Characterization of the Atlantic Water and Levantine Intermediate Water in the Mediterranean Sea using Argo Float Data, Ocean Sci. Discuss. [preprint], https://doi.org/10.5194/os-2021-68, in review, 2021
- Fach, B. A., Orek, H., Yilmaz, E., Tezcan, D., Salihoglu, I., Salihoglu, B., & Latif, M. A. (2021). Water mass variability and Levantine Intermediate Water formation in the Eastern Mediterranean between 2015 and 2017. Journal of Geophysical Research: Oceans, 126, e2020JC016472. https://doi.org/10.1029/2020JC016472
- Velaoras, D., Papadopoulos, V.P., Kontoyiannis, H., Cardin, V. and Civitarese, G., 2019. Water masses and hydrography during April and June 2016 in the cretan sea and cretan passage (Eastern Mediterranean Sea). Deep Sea Research Part II: Topical Studies in Oceanography, 164, pp.25-40.

Agreed. The references have been included.

P1-2, L41-42: These are some key references but they are old. I would suggest to also add newer ones, i.e.

- For Marine heatwaves: Ibrahim, Omneya, Bayoumy Mohamed, and Hazem Nagy. 2021. "Spatial Variability and Trends of Marine Heat Waves in the Eastern Mediterranean Sea over 39 Years" Journal of Marine Science and Engineering 9, no. 6: 643. https://doi.org/10.3390/jmse9060643
- For Med. droughts: Mathbout, Shifa, Joan A. Lopez-Bustins, Dominic Royé, and Javier Martin-Vide. 2021. "Mediterranean-Scale Drought: Regional Datasets for Exceptional Meteorological Drought Events during 1975–2019" Atmosphere 12, no. 8: 941. https://doi.org/10.3390/atmos12080941

Agreed. The references have been included.

### P3, L75: Please remove "thus far".

The text has been modified accordingly.

P3, L97: I would suggest to replace "provide" by "sustain".

The text has been modified accordingly.

P3, L74-97: There are some missing articles in this section. For example, Howes et al. (2015) also derived the same conclusions using the RCPs 4.5 and 8.5. There is also Macias et al. (2018) who used two different global circulation models (GCMs; equivalent to RCP4.5 and RCP8.5), and other studies.

- Herrmann, M., Estournel, C., Adloff, F., and Diaz, F. (2014), Impact of climate change on the northwestern Mediterranean Sea pelagic planktonic ecosystem and associated carbon cycle, J. Geophys. Res. Oceans, 119, 5815–5836, doi:10.1002/2014JC010016.
- Howes EL, Stemmann L, Assailly C, Irisson JO, Dima M, Bijma J, Gattuso JP (2015) Pteropod time series from the North Western Mediterranean (1967-2003): impacts of pH and climate variability. Mar Ecol Prog Ser 531:193-206. https://doi.org/10.3354/meps11322
- Macias, D., Garcia-Gorriz, E. and Stips, A. (2018), Major fertilization sources and mechanisms for Mediterranean Sea coastal ecosystems. Limnol. Oceanogr., 63: 897-914. <u>https://doi.org/10.1002/lno.10677</u>

We thank the Reviewer for the suggestion. We included and discussed Hermann et al. (2014) because we would prefer to address in the introduction section only the scientific literature directly dealing with future projections under different emission scenarios, while Howes et al. (2015) focused on the period 1967-2003 and Macias et al. (2018) on the period 1959-2013. We modified the text as follows (lines 86-91):

"Hermann et al., (2014) assessed the response of the pelagic planktonic ecosystem of the North Western Mediterranean to different emission scenarios and showed that, at end of the 21st century, the biogeochemical processes and marine ecosystem components should be very similar to those observed at the end of the 20th century, although quantitative differences might be observed such as an increase in the bacteria growth, gross primary production and biomass of small-size phytoplankton group."

### P3, L99: Please write "All the above-mentioned works..." instead of "All of these previous works...".

The text has been modified accordingly.

### P4, L112: Please write "non-living" instead of "nonliving".

The text has been modified accordingly.

### Data and methods:

### P6, L184: Please write "non-living" instead of "nonliving". And edit it through the entire text.

The text has been modified accordingly.

### P6, L187: Please write "physico-chemical" instead of "physical-chemical".

The text has been modified accordingly.

### P6, L191-192: Please pay attention to the subscripts throughout the manuscript, i.e. CaCO<sub>3</sub>

The text has been modified accordingly.

### P7-8, section 2.4: The subscripts are sometimes too small to read. Please rectify it.

The text has been modified accordingly.

### **Results:**

### P8, L284: It would be helpful to add the ranges, SDs, maybe in a table. What are the precisions of T and S derived from this model?

We revised the sentence by clearly stating that the comparison of the physical quantities simulated by MFS16 against different ocean reanalyses was thoroughly addressed in previous works (Lovato et al., 2013; Galli et al., 2017). In particular, Lovato et al. (2013) reported the comparison of the MFS16 simulations against a reanalysis product (Adani et al., 2010) focusing on the temporal evolution of temperature, salinity, and water fluxes, while Galli et al. (2017) provided an extensive comparison against CMEMS reanalyses (Simoncelli et al., 2019) of marine heatwave events and temperature data at different depth levels (see their supplementary material 3).

We revised the sentence as follows (lines 332-334):

"MFS16 modelling system performances under present climate conditions were previously analyzed (Lovato et al., 2013; Galli et al., 2017), showing that the main spatial-temporal characteristics of the Mediterranean Sea physical properties reliably compared against ocean reanalysis datasets." However, we considered this request also in the light of what suggested by the Editor and we provided in the supplementary material a new table that summarizes the mean values of the key physical and biogeochemical variables under the two scenarios (including the three time-windows considered in our analysis) for the Western and Eastern Mediterranean sub-basins at the two considered layers. Please refer to the replies to the Editor's comments.

Adani, M., Dobricic, S., Pinardi, N., 2010: Quality assessment of a 1985–2007 Mediterranean Sea reanalysis. Journal of Atmospheric and Oceanic Technology 28, 569-589.

Simoncelli, S., Fratianni, C., Pinardi, N., Grandi, A., Drudi, M., Oddo, P., & Dobricic, S. (2019). Mediterranean Sea Physical Reanalysis (CMEMS MED-Physics) (Version 1) [Data set]. Copernicus Monitoring Environment Marine Service (CMEMS). https://doi.org/10.25423/MEDSEA\_REANALYSIS\_PHYS\_006\_004

### P10-11, L317-322, Fig. 2:

• I would recommend to add the Chl-a unit on the next to the bar dedicated for a & b.

The figure has been modified accordingly.

• Also add the unit of the depth, on the profiles or the caption.

For sake of consistency, we added the unit of depth in the caption

• You need to add in the caption that the vertical profiles are shown for the Mediterranean scale, Western Mediterranean, and Eastern Mediterranean. This was not mentioned in the corresponding text as well

Agreed. The caption has been modified as follows:

"Fig.2 Average Chl-a in the first 10m in CTRL (a) for the period 2005-2020 and CMEMS-SAT (b) together with CTRL average vertical profiles (blue lines) for the period 2005-2020 of Chl-a (c,mg m<sup>-3</sup>), PO<sub>4</sub> (d, mmol m<sup>-3</sup>), NO<sub>3</sub> (e, mmol m<sup>-3</sup>), DIC (g, µmol kg<sup>-1</sup>), pH(h) and total alkalinity (i, µmol kg<sup>-1</sup>). The averaged profiles are computed for the entire (MED), Western (WMED) and Eastern (EMED) Mediterranean Sea. The light blue areas represent the spatial standard deviation of the monthly model data. The model data are compared with CMEMS reanalysis (Chl-a and pH; Colella et al., 2016: Teruzzi et al., 2021) and observations provided by EMODnet (PO<sub>4</sub>, NO<sub>3</sub>, Dissolved oxygen, DIC, total alkalinity; Buga et al., 2018): annual mean (black squares) and related standard deviations (black bars). Depth is measured in meters."

### Please write the unit appropriately for "µmol kg1" in the caption as well as throughout the ms.

The text has been modified accordingly.

### P11, L338: Please add "such as..." instead of "as...".

The text has been modified accordingly.

P12, L354-355: Please make it clearer, i.e. General freshening of the upper layers and saltening of the intermediate layers are observed over the entire basin during the MID-FUTURE period (Fig. S3 in the supplementary materials).

Agreed. The sentence has been reformulated as (lines 424-425):

"A general freshening of the upper layers and saltening of the intermediate layers over most of the Mediterranean basin is observed during the MID-FUTURE period (Fig. S3)"

P13, L382: Please edit: "Only for the Aegean Sea, the changes in the winter mixed layer maximum depth are less marked, ...".

The text has been modified accordingly.

P14, Fig. 4: Please correct the presentation of scenarios in the plots: RCP 4.5 and 8.5 instead of RCP 45 and 85.

The plots have been modified accordingly.

P14, L404: Please delete both "the" in "the nutrients at the river mouths."

The text has been modified accordingly.

P16-17, Fig. 5-6: Please add the unit on both color bars. Please correct the presentation of scenarios: RCP 4.5 and 8.5 instead of RCP 45 and 85.

The figure has been modified accordingly.

P18, L445-449: Please try to make this sentence clearer, i.e. Between 2055 and 2075, the peak in both nutrients' concentration, for RCP4.5, timely corresponds to a peak in the inflow of nutrients at the Gibraltar strait (Fig. S7). Additionally, in both scenarios the intermediate layer of the Western basin, after 2035, experiences a negative tendency in the nutrient concentration which is greater than 0.01 mmol m<sup>-3</sup> for PO<sub>4</sub> and 0.1 mmol m<sup>-3</sup> NO<sub>3</sub>, this is related to a reduced westward transport of nutrients associated with LIW (Fig.S5).

Agreed. We reformulated the sentence as follows (lines 534-536):

"Between 2055 and 2075, the peak in both nutrients' concentration, for RCP4.5, timely corresponds to a peak in the inflow of nutrients into the Alboran Sea (Fig. S7). Additionally, in both the scenarios, the intermediate layer of the Western basin, after 2035, experiences a negative tendency in the nutrient concentration (greater than 0.01 mmol m<sup>-3</sup> for PO<sub>4</sub> and 0.1 mmol m<sup>-3</sup> NO<sub>3</sub>) related to a reduced westward transport of nutrients associated with LIW (Fig.S5)".

### P20, L479-80: Please add references here.

Agreed. The following references were included in the manuscript:

Keeling, R. F., A. Kortzinger and N. Gruber Ocean Deoxygenation in a Warming World Annual Review of Marine Science 2: 199-229, (2010)

Shepherd, J. G., Brewer, P. G., Oschlies, A., & Watson, A. J.: Ocean ventilation and deoxygenation in a warming world: introduction and overview. Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 375(2102), 20170240, 2017

### P20, L484: Please correct "in vertical processes' intensity".

The text has been modified accordingly.

### P21, L489: Do you mean "in both basins"? Please correct.

Agreed. We now explicitly state that we are talking about:

"in both Western and Eastern Mediterranean"

### P21, Fig. 9: Please correct the presentation of scenarios: RCP 4.5 and 8.5 instead of RCP 45 and 85.

The figure has been modified accordingly.

### P21, L498: Please remove "the" from "both the scenarios".

The text has been modified accordingly.

### P21, L499: Please add a "," after scenario.

The text has been modified accordingly.

### P22, L508: Please add a "," after scenarios.

The text has been modified accordingly.

### P22, L514: Please add a "," after scenario.

The text has been modified accordingly.

### P22, L519: Please add a "," after scenarios.

The text has been modified accordingly.

### P23, L526, Fig.11: Please add the unit on both color bars. Please correct the presentation of scenarios: RCP 4.5 and 8.5 instead of RCP 45 and 85. Please unify the term "Mid-FUTURE" in the captions and the plots.

We thank the Reviewer for spotting the errors in the figures and captions that were modified accordingly.

### P23, L535: Please write it "parts of the Levantine basin".

The text has been modified accordingly.

### P23, L538: Please add a "," after FUTURE.

The text has been modified accordingly.

### P24, L562: Please keep either "of approximately" or "of about".

The text has been corrected with "of approximately"

### P26, L580: Please add a "," after FUTURE.

The text has been modified accordingly.

### P27, Fig. 14: Why did you choose the abbreviation BACTC to bacterial biomass? It is not conventional. I would suggest to make it "BACT".

Agreed. The abbreviation has been changed to BACT

### P27, L592: Please remove the "," after also, and add it before "the decline".

The text has been modified accordingly.

P28, L616-617: The influence of the air-sea CO2 exchanges on DIC concentrations in the Mediterranean were already highlighted in multiple studies, i.e.

### from models

• D'Ortenzio, F., Antoine, D. and Marullo, S., 2008. Satellite-driven modeling of the upper ocean mixed layer and air-sea CO2 flux in the Mediterranean Sea. Deep Sea Research Part I: Oceanographic Research Papers, 55(4), pp.405-434.

### for observations

- Wimart-Rousseau, C., Lajaunie-Salla, K., Marrec, P., Wagener, T., Raimbault, P., Lagadec, V., Lafont, M., Garcia, N., Diaz, F., Pinazo, C. and Yohia, C., 2020. Temporal variability of the carbonate system and air-sea CO2 exchanges in a Mediterranean human-impacted coastal site. Estuarine, Coastal and Shelf Science, 236, p.106641.
- Hassoun, A.E.R., Fakhri, M., Abboud-Abi Saab, M., Gemayel, E. and De Carlo, E.H., 2019. The carbonate system of the Eastern-most Mediterranean Sea, Levantine Sub-basin: Variations and drivers. Deep Sea Research Part II: Topical Studies in Oceanography, 164, pp.54-73.

## • De Carlo, E.H., Mousseau, L., Passafiume, O., Drupp, P.S. and Gattuso, J.P., 2013. Carbonate chemistry and air–sea CO 2 flux in a NW Mediterranean bay over a four-year period: 2007–2011. Aquatic geochemistry, 19(5), pp.399-442.

We thank the Reviewer for the suggested references that have been included in the text.

### P28, L620: Please replace "fairly" by "equal".

The text has been modified accordingly.

### P28, L621: Please replace "consistently".

The text has been modified accordingly.

### P28, L625: Please remove "than in".

The text has been modified accordingly.

### P30, L649: Please remove the "s" from "produces".

The text has been modified accordingly.

### P30, L651: Please correct the subscript in "pCO<sub>2</sub>".

The text has been modified accordingly.

### P30, L654-655: Please re-write, i.e. "consistent with the estimates of Solidoro et al. (2021)."

The text has been modified accordingly.

### P30, L657-658: Do you mean "by the end of the 21st century for RCP8.5?". Please rectify.

We thank the Reviewer for spotting the error in the sentence. We reformulated the sentence as (lines 769-772):

"However, the fate of the absorbed carbon is quite different: the Western basin during the  $21^{st}$  century (RCP8.5 scenario) accumulates only 0.85 PgC, while 1.7 PgC are retained in the water column of the Eastern basin."

### P31, Fig.17: Please write the unit appropriately for "μmol kg<sup>1</sup>" in the caption. Please correct the presentation of scenarios: RCP 4.5 and 8.5 instead of RCP 45 and 85.

The caption and figures have been modified accordingly.

P31, L673-674: I would recommend to also check Goyet et al. (2016), as the pattern of your results are somehow in harmony.

Agreed. We reformulated the paragraph as follows (lines 785-790):

"Consequently, to the CO<sub>2</sub> invasion and DIC increase, the change in the carbonic acid equilibrium causes a generalized decrease in pH, as also shown in Solidoro et al. (2022) in the case of the A2 scenario. The change in pH, which is statistically significant everywhere and very well correlated in time and space with the DIC change (Fig.S14) and almost similar in both Western and Eastern Mediterranean (as already projected by Goyet et al., 2016), is approximately by the end of the century equal to 0.1 in the RCP4.5 and 0.25 pH units in the RCP8.5 scenario (Fig. 18)."

### P31, L675: I would suggest to mention "by the end of the 21<sup>st</sup> century for RCP8.5" or "by the end of the century for RCP8.5".

We reformulated the sentence as:

"by the end of the century equal to 0.1 in the RCP.45 and 0.25 units in the RCP8.5 scenario"

### P3, L682-684: Please unify the term "Mid-FUTURE" in the captions and the plots.

The text and plots have been modified accordingly.

#### **Discussions and conclusions:**

### P33, L689: Please add a "," after "In this study".

The text has been modified accordingly.

### P33, L693: Please add a "," after "To the best of the authors' knowledge".

The text has been modified accordingly.

#### P33, L693-696: Please re-write this sentence taking into consideration the major comments above.

We thank the Reviewer for the comment that we have already addressed above. Following the suggestion we reformulated the sentence as follows (lines 810-814):

"To the best of the authors' knowledge, this work is the first one that analyzes long-term eddy-resolving projections of the biogeochemical dynamics of the Mediterranean Sea under two different emission scenarios. In fact, the horizontal and vertical resolution (1/16° and 70 vertical levels) of the long-term projections here analyzed is higher than that of previous works available in the scientific literature that focuses on the area (e.g Lazzari et al., 2014; Macias et al., 2015; Richon et al., 2019; Pagès et al., 2020; Solidoro et al., 2022)."

### P33, L718: Please remove the "s" at the end of "shows".

The text has been modified accordingly.

P35, L768-769: This sentence is important for coastal ecosystems, and needs thus better elaboration, and references too.

Agreed. We reformulated the sentence as follows (lines 903-906):

"On the other hand, the greatest threat for the oxygen water content might be linked to the combination of surface warming and faster respiration processes in the coastal areas of the basin, which could result in hypoxia conditions and alteration of the local marine ecosystem functioning and structures (Bindoff et al., 2019)".

We included the following reference corresponding to the IPCC report chapter dedicated to the ocean and cryosphere which provides an extensive review of the local effects of the deoxygenation on the marine ecosystems:

Bindoff, N. L., Cheung, W. W., Kairo, J. G., Arístegui, J., Guinder, V. A., Hallberg, R., ... & Williamson, P. : Changing ocean, marine ecosystems, and dependent communities. IPCC special report on the ocean and cryosphere in a changing climate, 477-587, 2019.

P35, L771-772: The exchanges via the Strait of Gibraltar are surely crucial, but there are other factors that should be taken into consideration such as the difference in the ventilation period between both basins, among other factors (i.e. Pujo-Pay et al., 2011; Álvarez et al., 2014; Stöven and Tanhua, 2014; Cardin et al., 2015; Hassoun et al., 2015; Goyet et al., 2016; etc.).

- Álvarez, M., Sanleón-Bartolomé, H., Tanhua, T., Mintrop, L., Luchetta, A., Cantoni, C., Schroeder, K., and Civitarese, G.: The CO2 system in the Mediterranean Sea: a basin wide perspective, Ocean Sci., 10, 69–92, https://doi.org/10.5194/os-10-69-2014, 2014.
- Cardin, V., Civitarese, G., Hainbucher, D., Bensi, M., and Rubino, A.: Thermohaline properties in the Eastern Mediterranean in the last three decades: is the basin returning to the pre-EMT situation?, Ocean Sci., 11, 53–66, https://doi.org/10.5194/os-11-53-2015, 2015.
- Goyet, C., Hassoun, A., Gemayel, E., Touratier, F., Abboud-Abi Saab, M. and Guglielmi, V., 2016. Thermodynamic forecasts of the mediterranean sea acidification. Mediterranean Marine Science, 17(2), pp.508-518.
- Hassoun, A.E.R., Gemayel, E., Krasakopoulou, E., Goyet, C., Abboud-Abi Saab, M., Guglielmi, V., Touratier, F. and Falco, C., 2015. Acidification of the Mediterranean Sea from anthropogenic carbon penetration. Deep Sea Research Part I: Oceanographic Research Papers, 102, pp.1-15.
- Pujo-Pay, M., Conan, P., Oriol, L., Cornet-Barthaux, V., Falco, C., Ghiglione, J.F., Goyet, C., Moutin, T. and Prieur, L., 2011. Integrated survey of elemental stoichiometry (C, N, P) from the western to eastern Mediterranean Sea. Biogeosciences, 8(4), pp.883-899.
- Stöven, T. and Tanhua, T.: Ventilation of the Mediterranean Sea constrained by multiple transient tracer measurements, Ocean Sci., 10, 439–457, https://doi.org/10.5194/os-10-439-2014, 2014.

We thank the Reviewer for the suggestion. The sentence refers to the particular case of DIC. It was worth noting how the two basins react differently to the same driver (i.e., the spatially uniform increase of atmospheric pCO<sub>2</sub>). Indeed, as suggested by the Reviewer, differences in resident times of water masses, which are in turn influenced by the different ventilation, circulation and influence of the exchanges with the Atlantic waters, can be indicated as the major cause of the different rate of carbon absorption and accumulation in the two basins. The sentence has been reformulated as follows (lines 917-920):

"This difference in the response to climate change between the Western and Eastern basins has been also observed for the dissolved inorganic carbon accumulation and reflects indeed different factors such as the different ventilation and residence time of water masses in the two basins as well as the exchanges in the Gibraltar Strait (e.g. Alvarez et al., 2014; Stöven and Tanhua, 2014; Cardin et al., 2015; Hassoun et al., 2019)."

### P35, L778: Please also compare it with Mediterranean projections (i.e. Goyet et al., 2016).

We thank the Reviewer for the suggestion. The sentence has been modified as follows (lines 909-916):

"The overall accumulation of  $CO_2$  in the basin resulted in an acidification of the Mediterranean water with a decrease in pH of approximately 0.23 units in the worst-case scenario, which is slightly lower than the 0.3 projected on a global scale (Kwiatkowski et al., 2020) and lower than the value provided in Goyet et al. (2016), who projected, using thermodynamic equations of the  $CO_2$ /carbonate system chemical equilibrium in seawater, a variation of 0.45 pH units in the basin under the worst SRES case scenario (and 0.25 pH units in the most optimistic SRES scenario). However, this last estimate probably tends to overestimate the future acidification of the basin, as it does not consider the decrease in the exchanges and the penetration of  $CO_2$  across the ocean-atmosphere interface due to the warming of the water column (MedECC, 2020)"

### P35, L790-792: Is it possible to estimate these uncertainties? It would be great to mention the level of overestimation derived from the model compared to the present conditions.

We thank the Reviewer for the suggestion. The overestimation between model results and observations are now better addressed in section 3.1. However, we want to stress here that we do not expect these biases to significantly impact the conclusions of our work. This is because of two reasons: (i) our protocol for climate signal (section 2.4) removes the CTRL simulation from scenario calculation (and thus also the biases observed in the CTRL and scenario simulations are removed), (ii) in the "Discussions and Conclusions", we focused our attention mainly on the future trends in the biogeochemical variables rather than on the absolute values of the projected changes. In the case of a few variables (for example oxygen at 200-600 m) the absolute values of the projected changes are similar or lower than the biases discussed in sect 3.1. In any case, having removed the model drifts, the signs of the projected changes are robust in the sense that they are a consequence of simulated mechanisms (*e.g. temperature and respiration increase, weakening of the thermohaline circulation, increase in the stratification*) that are extensively discussed in the manuscript, whereas the absolute values could be affected by uncertainty associated to these biases. Therefore, we focused the discussion of our work on the sign of the projected changes, similarly to previous scientific works such as Richon et al. (2019). In order to accommodate the Reviewer's comment we added the following paragraph in the "Discussion and Conclusions" section (lines 941-949):

"Additional sources of uncertainties in the modelling framework can be traced back to the BFM biogeochemical model. For instance, the model tends to overestimate the chlorophyll-a at the surface and, even more, the oxygen concentration below 200 m (section 3.1). These biases are propagated by the integration into the future projections. However, the conclusions of the present work should not be significantly affected by that because, at the same time, the CTRL simulation is also removed from both the scenario simulations. Moreover, the signs of the projected changes (not their absolute values) result from different physical and biogeochemical processes (e.g., temperature and respiration increase, weakening of the thermohaline circulation, increase in the stratification) which are linked to the climate forcing and are independent from model uncertainties that generated the biases discussed in section 3.1."

P35, L794-801: A recent study by Gazeau et al. (2021) is a good fit in this section as well, as it highlights the potential impact of aerosol deposition (dust in this case) both in present and future climate conditions in the Mediterranean.

Gazeau, F., Ridame, C., Van Wambeke, F., Alliouane, S., Stolpe, C., Irisson, J.-O., Marro, S., Grisoni, J.-M., De Liège, G., Nunige, S., Djaoudi, K., Pulido-Villena, E., Dinasquet, J., Obernosterer, I., Catala, P., and Guieu, C.: Impact of dust addition on Mediterranean plankton communities under present and future conditions of pH and temperature: an experimental overview, Biogeosciences, 18, 5011–5034, https://doi.org/10.5194/bg-18-5011-2021, 2021.

Agreed. We reformulated the paragraph as follows (lines 953-961):

"Atmospheric deposition is an important source of nutrients for the basin and it has been shown that the biogeochemical dynamics of the Mediterranean Sea is influenced by aerosol deposition (e.g. Richon et al., 2018, 2019), especially during periods of stratification. The projected lower nutrient supply from sub-surface waters caused by climate-driven stronger stratification, could likely increase the importance of the atmospheric deposition as a source of nutrients for the euphotic layer (Gazeau et al., 2021). Thus, possible future changes in the deposition of aerosols could influence the biogeochemistry of the basin and the nutrients concentration at the surface as projected for the 21st century and depicted in Section 3.3. However, in both RCP4.5 and RCP8.5 simulations, a present-day phosphate and nitrogen deposition is used. Potential improvements will be achieved indeed by the inclusion of more accurate deposition information derived from CMIP6 global estimates for the 21st century (O'Neill et al., 2016)."

### P36, L826: Please write it "such as".

The text has been modified accordingly.

#### Supplementary document:

Fig. S2: Please add the temperature unit on both color bars (°C). Please correct the presentation of scenarios: RCP 4.5 and 8.5 instead of RCP 45 and 85. Please correct "blue" instead of "blu".

The figure has been modified accordingly.

### Fig. S3: Please correct the presentation of scenarios: RCP 4.5 and 8.5 instead of RCP 45 and 85.

The figure has been modified accordingly.

Fig. S4: Please correct the presentation of scenarios: RCP 4.5 and 8.5 instead of RCP 45 and 85. Please correct "blue line" instead of "blu line".

The figure has been modified accordingly.

Fig. S8: Please add the unit on both color bars. Please correct the presentation of scenarios: RCP 4.5 and 8.5 instead of RCP 45 and 85.

The figure has been modified accordingly.

Fig. S9-14: Please add the unit on both color bars. Please correct the presentation of scenarios: RCP 4.5 and 8.5 instead of RCP 45 and 85.

The figure has been modified accordingly.

Fig. S14: Why are you mentioning the DIC unit in ug kg<sup>1</sup> here while it is in  $\mu$ mol kg<sup>1</sup> in the text? Please adopt the second one.

The figure has been modified accordingly.

### Author responses to Reviewer#2 comments for the manuscript: "Acidification, deoxygenation, nutrient and biomasses decline in a warming Mediterranean Sea" April, 2022

This paper describes new high-resolution coupled (physic and biogeochemistry) simulations made under the RCP 4.5 and 8.5 scenarios for the Mediterranean Sea (MED). This work is in line with the studies that have been carried out over the last ten years to assess the effects of climate change on the circulation and biogeochemistry of the MED. The main improvement here is the use of a higher spatial resolution than the previous studies. I acknowledge the huge amount of work and computational time that is required to run such simulations and I do think that this study is a step forward to a better understanding of the effect of climate change on the MED. However, despite the overall good quality of this work, this paper could be improved.

We thank the Reviewer #2 for their positive feedback and for providing detailed comments and suggestions that helped us to improve the manuscript. Reviewer's comments are in bold, authors' responses are in normal font, italicized where they quote the changes to the manuscript.

The use of higher resolution simulations could significantly improve our understanding of the MED in the context of climate change, but the authors don't really highlight the interest of such high resolution in the manuscript. The authors propose an analysis based on two averaged integrated depths 0-100 m and 200-600 m for 8 Mediterranean sub-basins losing in my opinion the opportunity to fully take advantage of their model resolution. Furthermore, the discussion as proposed here lacks rigorous comparison and discussion with the existing literature which could emphasize more the importance of high resolution. I also have concerns about the methodology, for example (but not only) the choice of the reference periods (2005-2020) that have been taken into account including the beginning of the scenarios, and the choice of allowing only the dissolved inorganic carbon to change at Strait of Gibraltar.

We thank the Reviewer for raising different interesting points. We are confident that the changes to the manuscript will respond to their concerns.

In particular, in the new version of the manuscript, we discussed the importance of using a high resolution for detecting spatial gradients involving signs and the statistical significance of the projected changes. Moreover, we improved the comparison with the existing literature and the description of the followed methodology, including the motivation of choosing the reference period. Finally, we justified and discussed the choice of the boundary conditions at the Strait of Gibraltar.

### **Specific comments:**

#### **Biogeochemical spin-up:**

Some important information about the spin-up is missing for biogeochemistry. Please specify how you did the spinup and how long it runs before being stable. It is briefly mentioned in section 3.1 but this should be presented before. We thank the Reviewer for raising this point. We realized that this important information was missing in the manuscript. All the simulations discussed in the manuscript use as initial conditions the resulting final fields from a spin-up simulation. The latter starts on January, 1<sup>st</sup> 2005, following a spin-up of 100 years which reproduces an average present condition corresponding to the 2005–2014 period looped for both physical forcing and river discharge. Results of the spin-up simulation (not shown in the paper) show that after the first half of the spin-up simulation, the evolution of most biogeochemical variables is almost stable in the first 1000 m and below 2000 m, while it takes a little longer to adjust between 1000-2000 m (i.e., specifically for nutrients and oxygen).

In order to better address the Reviewer's comment, we included the following sentence (lines 261-263):

"Finally, all the simulations discussed in the next sections, use as initial conditions the resulting final fields from a run that started in January,  $1^{st}$  2005 following a spin-up of 100 years made with a loop over the 2005–2014 period for the physical forcing, the river nutrient discharge and atmospheric forcing (nutrient deposition and  $CO_2$  air value)."

#### **Boundary conditions:**

The boundary conditions are kept constant except for the DIC. Why all the variables of the carbonate cycle aren't treated the same way? Furthermore, did you use boundary conditions from a global RCP 8.5 for the MED RCP 8.5 simulation and a global RCP 4.5 for the MED RCP 4.5 simulation? Overall, this needs to be discussed as it could change the DIC / ALK ratio at Strait of Gibraltar that might be important for pH variations in the MED (in conclusion). Please, specify how the DIC concentration evolves at Strait of Gibraltar. A table representing nutrients and carbonate system variables for Strait of Gibraltar and rivers will be appreciated.

We thank the Reviewer for raising this point that was not thoroughly discussed in the manuscript. Setting the biogeochemical BCs at the Strait of Gibraltar is not straightforward because no-verifiable and anomalous simulated tendencies outside the Strait can result in spurious trends within the Mediterranean basin.

We considered the temporal evolution of the carbonate system and dissolved nutrients concentration in the Atlantic area outside the Strait of Gibraltar during the 21st century in the CMCC-CESM global simulation (Vichi et al., 2011). However, after a preliminary analysis, the projected changes simulated at the Strait of Gibraltar for nutrients have not been used as boundary conditions due to the anomalous nitrate/phosphate ratio observed. Because of that, we preferred to keep the vertical profiles of nutrients corresponding to the present conditions (data from WOA2018). Moreover, the analysis of the temporal evolution during the 21st century of DIC and total alkalinity in the Atlantic area outside the Strait of Gibraltar, showed a continuous increase in the DIC concentration (more than 4% with respect to the present) and a stable value of total alkalinity (the variation is around zero in the 21st century). We considered these signals reasonable and, because of that, we used the increasing over time of DIC at the Strait of Gibraltar simulated by CMCC-CESM, while the total alkalinity remained constant throughout the simulation. We acknowledge that we should have used two different BCs for the BGC scenarios. However, besides the lack of availability of a consistent RCP4.5 global scenario, we decided to test the impact of the different scenario atmospheric forcings (i.e., atmospheric forcing but, possibly, could have underestimated a potential difference between the two scenarios. The manuscript was modified taking into account the Reviewer's concern. In particular we wrote in section 2.3 (lines 224-235):

"Boundary conditions are adopted to represent the external supply of biogeochemical tracers and properties from the Strait of Gibraltar and the rivers into the Mediterranean basin. The exchanges of nutrients and other biogeochemical tracers in the Strait of Gibraltar are achieved by relaxing the 3D fields in the Atlantic zone (Fig. 1) to average vertical profiles which, for dissolved oxygen, phosphate, nitrate and silicate, refer to Salon et al. (2019), while total alkalinity is based on what was described in Cossarini et al. (2015). These profiles do not consider a seasonal cycle or a future temporal evolution, with DIC as the only exception, which is prescribed from a global ocean-climate simulation under RCP8.5 emission scenario performed within the framework of the CMIP5 project (Coupled Model Intercomparison Project Phase 5; Taylor et al., 2012) and based on the CMCC-CESM modeling system (CMCC-Coupled Earth System Model; Vichi et al., 2011). The reasons for these choices rely on: (i) anomalous values observed in N:P ratio under the RCP8.5 emission scenario, (ii) negligible variation, under emission scenario RCP8.5, of the total alkalinity along the 21<sup>st</sup> century, (iii) lack of a consistent RCP4.5 scenario, (iv) the possibility, using the same conditions at the Atlantic boundary, to test the impacts of the atmospheric and ocean forcings for the two scenarios."

Moreover, in the Discussion and Conclusions section we acknowledged that (lines 978-981):

"Moreover, the use of the same Atlantic boundary conditions for the two scenarios (section 2.3) could have led to an underestimation of a potential difference between the two scenarios in the areas most influenced by the Atlantic boundary (e.g., Alboran and South Western Mediterranean)"

Finally, we acknowledge that the Strait of Gibraltar is an interesting area to be investigated. However, the paper provides evidence of basin-wide signals without focusing on specific areas due to the paper length limit and some plots of evolution on some specific areas, including Alboran Sea, are already in Fig. S4-7. If not further required by the Reviewer, we would prefer not to include other figures and tables due to the already high amount of materials discussed in the manuscript and just to direct the potential Readers to the provided references.

### Periods selection PRESENT MID and FAR:

A PRESENT period of 15 years was chosen and compared with MID and FAR periods of 20 years. How this could impact your results? Why don't you choose 20 years as well for the PRESENT period? During PRESENT period (2005-2020), the simulations are already in scenarios mode, as the RCP scenarios start in 2005. Therefore, the PRESENT period encompasses change linked to climate change which could lead to bias for evaluating the effect induced by climate change. Indeed, major differences between the 2 simulations considering the 2 RCP scenarios may appear during the PRESENT period, as observed for zonal stream function on your figure 4. Usually, the reference period is chosen among the hindcast part of the simulation to avoid those issues (Richon et al. 2019; Pages et al. 2020b, and others). Did you run a hindcast part before the scenarios? Please, at least, discuss your choice of the reference period and how it may affect your results.

We thank the Reviewer for highlighting this point, since we realize that this very important information was missing in the text. We are aware that, in the CMIP5 simulations protocol, there is an historical simulation which covers the period 1950-2005 and then a scenario simulation which spans over the period 2006-2100. The choice to consider the period 2005-2020 as present climate for the validation relies on the idea to use more advanced datasets such as the CMEMS reanalysis (Teruzzi et al., 2019; Cossarini et al., 2021) and the satellite Chl-*a* dataset (Colella et al., 2016) to evaluate the

performance of the modeling system. Being that both the reference datasets cover a period spanning from 1999 to 2020, in order to avoid the overlapping between historical and scenario we limited the evaluation to the period 2005-2020. We agree with the Reviewer that the choice of the period could introduce a potential error in our analysis due to the fact that we are already in "scenario mode". However, we tested the statistical significance of the differences between two scenarios in the period 2005-2020 for temperature, salinity and current speed fields and we found that the differences are not statistically significant over most of the basin (with only exception of small areas in the Southern Ionian, Adriatic Sea and Levantine basin) and anyhow lower than the climate change signal. We do not expect that the choice of the period of different length as PRESENT and MID-FUTURE / FAR-FUTURE significantly impacts our findings. However, in order to address the Reviewer's comment, we added the following sentence (lines 294-299):

"The period 2005-2020 has been chosen as reference (also in the forthcoming validation) due to: (i) the availability, after year 2000, of more advanced satellite and assimilated datasets to evaluate the biogeochemistry of the basin, (ii) to avoid the overlapping between historical and scenario part of the simulations (with the latter starting in 2005). It is important to stress here that the choice of the period should not significantly affect the results of the study as the observed differences during this period between the two scenarios for temperature, salinity and current speed fields have been found to be not statistically significant over most of the basin (not shown)."

#### Statistical significance:

There is no indication in the text about the significance of the differences obtained for comparison between the MID and FAR periods. The word 'significant' is used but without statistics. In order to evaluate whether the numeric differences are substantial, it is necessary to calculate some parameters such as the t-student (See line 517 as an example) and to indicate if the variations are significant in the text and in the figures.

We thank the Reviewer for raising this point. We redrew all the figures following their suggestion. In particular, we assessed the statistical significance of the differences in each point of the domain between PRESENT and MID-FUTURE and between PRESENT and FAR-FUTURE using a Mann-Whitney test with p<0.05. Just as an example here we report the "New" Figure 5 with the relative caption.



Fig. 5 - Phosphate concentration (in mmol  $m^3$ ) in the layers 0-100 m and 200-600 m in the PRESENT (2005-2020, a,b,c and d), and relative climate change signal (with respect to the PRESENT) in the MID-FUTURE (2040-2059, e,f,g and h) and FAR-FUTURE (2080-2099, i,j,k and l) in the RCP4.5 (left column) and RCP8.5 (right column) emission scenario. The Mediterranean average relative climate change signal in each period (with respect to the PRESENT) is displayed by the top-left colored value (blue or dark orange when negative or positive). Values in the green boxes are the average relative climate change in each period and in each sub-basin shown in Figure 1. Domain grid points where the relative climate change signals are not statistically significant according to a Mann-Whitney test with p<0.05 are marked by a dot.

Some temporal correlations are indicated over the manuscript, for example between oxygen concentration and decrease of MLD, but any values are given (r and p-value) to assess the results. Please add some statistics.

We thank the Reviewer for the suggestion. In order to accommodate with the Reviewer's comment we reformulated the sentence as follows (lines 579-583):

"Under RCP8.5 (Fig.9 a,b), the progressive decline of oxygen concentration is timely corresponding to the progressive decrease in the maximum mixed layer depth (Fig. S4) and the weakening of the zonal stream function (Fig.4) discussed in Section 3.2. For example, in the North Western Mediterranean the correlation coefficient between the average dissolved oxygen concentration in the first 100m and the maximum mixed layer depth has been found equal to 0.64 (statistically significant with p < 0.05)."

In section 3.2 we now state (lines 786-790):

"The change in pH, which is statistically significant everywhere and very well anti-correlated in time and space with the DIC change (on the basin scale the correlation coefficient is lower than -0.90 with p<0.05; Fig.S14) and almost similar in both Western and Eastern Mediterranean (as already projected by Goyet et al, 2016), is approximately by the end of the century equal to 0.1 in the RCP4.5 and 0.25 pH units in the RCP8.5 scenario (Fig. 18)."

### Model validations:

### Please add more quantitative validations with for example a Taylor diagram.

Following also the request from the Reviewer#1, we enriched the validation part by including a more quantitative description of the biases. The use of the Taylor diagram to provide a synthetic summary of validation was discussed during the preparation of the manuscript, but it was discarded because the standard deviation of the modelled and observed data was rather different below 200 m, which could result in a misleading interpretation of the main messages of the validation. In fact, we preferred to focus on a more qualitative but explicative comparison where showing the biases is the main objective. Indeed, as also suggested by Reviewer#1, we revised the section adding more explanations about model performance in reproducing the mean present state.

The model is validated based on 2005-2020 averages made with the CTRL simulation forced with RCP scenario. As mentioned before, it is not well suited to use that for model validations because the scenarios already have an impact. Could you discuss the potential implications?

Thanks for stressing this point. We agree with the Reviewer that the choice of the period could introduce a potential error in our analysis due to the fact that we are already in "scenario mode". However, we tested the statistical significance of the differences between two scenarios in the period 2005-2020 for temperature, salinity and current speed fields and we found that the differences are not significant over most of the basin (with only exception of small areas in the Southern Ionian, Adriatic Sea and Levantine basin) and lower than the climate change signal. To address the Reviewer's concern the following statement has been included in the manuscript (lines 297-299):

"It is important to stress here that the choice of the period should not significantly affect the results of the study as the observed differences during this period between the two scenarios for temperature, salinity and current speed fields have been found to be not statistically significant over most of the basin (not shown)."

### **Discussion limitations:**

The discussion is interesting overall but there is a lack of links between the paragraphs and some parts could be more detailed.

We thank the Reviewer for this observation. The discussion and conclusions were revised in order to address these concerns as detailed below.

Line 718, it is indicated that the signals are in agreement with the previous literature. This is not exact at least for the NPP as Macias et al. (2015) pointed out a decrease of the primary production rates in the Western Basin and an increase in the Eastern Basin, and Pages et al., 2020 a general decrease of the NPP. Please, address the variables one by one, the differences between the models results are a strength if they are discussed.

We significantly revised the discussion and conclusions sections (lines 844-927), providing a more detailed discussion on the comparison between our projected changes and those discussed in the scientific literature. First, we clearly stated the importance of considering the scenario dependency of the projections when compared with other studies:

"Our projections for the biogeochemical tracers and properties at the end of the 21st century shows several signals (see Table SP1 for a synthetic overview) that are mostly in agreement with previous studies, at least with those based on the use of the worst-case emission scenarios. The magnitude of the projected changes has been shown to be, in general, scenario-dependent with the largest deviations from the present climate state observed in the RCP8.5 emission scenario (Table SP1). On the other hand, the analysis of the projections under RCP4.5 found in most of the biogeochemical variables (for example dissolved nutrients and biomasses) by the end of the 21st century a tendency to recover the values observed in the present climate (Table SP1).

As shown in the previous sections, our simulations, by covering also the RCP4.5 scenario, highlight how an intermediate greenhouse emission scenario produces results that are not simply an average between the present condition and the RCP8.5, but (at least for some variables) something quantitatively different. For example, the temporal evolution of pH (Fig.15) is similar in two scenarios in the first 30 years of the 21st century. Conversely, after 2050, pH undergoes a substantial decrease under RCP8.5 while it remains almost stable under RCP4.5 with a final projected variation lower than the half with respect to the worst-case scenario.

This supports the idea - possibly based on the existence in a system like the Mediterranean Sea of a certain buffer capacity and renewal rate- that the implementation of policies of reducing CO<sub>2</sub> emission could be, indeed, effective and could contribute to the foundation of ocean sustainability science and policies"

Then we discussed separately the scenarios outcomes related to nutrients, net integrated primary production and respiration, deoxygenation and dissolved inorganic carbon and pH.

"The decline in the dissolved nutrients at the surface under RCP8.5 scenario is comparable with that observed in Richon et al. (2019). However, they project an overall increase in the concentration of both nutrients at the surface after 2050, which is ascribed by the authors to river and Gibraltar inputs that are not constant over time (as in our case) but are based on a global climate scenario simulation. As highlighted by Richon et al. (2019), the sensitivity of the biogeochemical fluxes at the river loads and Gibraltar exchanges is of paramount importance, and surely worthy of further investigation. Nevertheless, the increase in the concentration of nutrients in the intermediate layers of both the Western Mediterranean and Levantine basin can be also traced back to the reduced vertical mixing resulting from the increase in the vertical stratification (Somot et al., 2006; Adloff et al., 2015; Richon et al., 2019).

Different levels of increase in the net primary production and respiration are projected in both scenarios although many recent studies in the Mediterranean region have shown a different response of integrated net primary production to climate change in both Western and Eastern basins (e.g. Macias et al., 2015; Moullec et al., 2019; Pagès et al., 2020). In fact, this response may vary according to the sensitivity of the assumptions (model equations) for primary production and recycling processes to changes in temperature (Moullec et al., 2019). In the BFM model temperature regulates most of the metabolic rates with a Q10 formulation (Vichi et al., 2015). The increase in net primary production is a consequence of such dependence. In other studies (Eco3M-Med model; Pages et al., 2020) organisms are always optimally adapted and no temperature dependence is accounted for in the physiology. This different parameterization could be connected to the different results in terms of trends; in fact, the scenarios based on Eco3M-Med model results in a reduction of net primary production. In that case surface nutrient reduction, rather than temperature, affects the net primary production trend producing a decrease. The relative impact of different drivers (nutrient supply versus organism's adaptation to average water temperature) could be explored with dedicated sensitivity experiments. Our projections of net primary production and biomass dynamics show how different levels of warming of the water column and consequent stratification have a direct impact on the ecosystem functioning by increasing the metabolic rates. Similar to the results obtained in other simulations of the BFM model (Lazzari et al., 2014; Solidoro et al., 2022), the increase in metabolic rates augments both primary production and respiration, but with the net effect of reducing living and non-living particulate organic matter, as suggested from theoretical considerations in O'Connor et al. (2011). The decoupled formulation of carbon uptake and net growth in the BFM model induces a further mechanism related to how carbon is channeled in the food web. In fact, the decrease in biomass is partially compensated by an increase in dissolved organic matter production in the basin by the end of the century (Solidoro et al., 2022; results not shown here).

Basin-wide deoxygenation tendencies are found in both scenarios and are comparable to trends observed on the Mediterranean scale by Powley et al. (2018) and, under RCP8.5, on the global scale by CMIP6 simulations (Coupled Model Intercomparison Project Phase 6; Kwiatkowski et al., 2020). The former, using a box model, found a decrease in the oxygen content of the intermediate layer in the range of 2-9% as a consequence of different projected changes in the

solubility (due to the temperature increase) and in the thermohaline circulation of the basin. Furthermore, our projections show that, in both our scenarios, deoxygenation is higher in the Eastern than the Western basin, where the Atlantic boundary condition might have dumped the deoxygenation trend, and in several coastal areas such as the Northern Adriatic (until -25 mmol m<sup>3</sup>). As also observed by Powley et al. (2018), the main driver of deoxygenation is the change in solubility, whereas changes in the circulation (i.e weakening of the thermohaline circulation) should not substantially affect deep ventilation, and it is unlikely, even in the worst-case scenario, to reach hypoxia conditions in the deep layer of the basin by the end of the century. On the other hand, our projection shows higher and significant changes in coastal areas (Fig. S8). Indeed, the greatest threat considering the oxygen water content might be linked to the combination of surface warming and faster respiration processes in the coastal areas of the basin which could result in lower oxygen conditions and, thus, alteration of the local marine ecosystem functioning and structures (Bindoff et al., 2019).

An increase in the dissolved inorganic carbon content and acidity of the water column (Solidoro et al., 2022) is also found in both scenarios. The overall accumulation of  $CO_2$  in the basin resulted in an acidification of the Mediterranean water with a decrease in pH of approximately 0.23 units, which is slightly lower than the 0.3 projected on a global scale (Kwiatkowski et al., 2020) and lower than the value provided in Goyet et al. (2016), who projected, using thermodynamic equations of the  $CO_2$ /carbonate system chemical equilibrium in seawater, a variation of 0.45 pH units in the basin under the worst SRES case scenario (and 0.25 pH units in the most optimistic SRES scenario). However, this last estimate probably tends to overestimate the future acidification of the basin, as it does not consider the decrease in the exchanges and the penetration of  $CO_2$  across the ocean-atmosphere interface due to the warming of the water column (MedECC, 2020).

This difference in the response to climate change between the Western and Eastern basins has been also observed for the dissolved inorganic carbon accumulation and reflects indeed different factors such as the different ventilation and residence time of water masses in the two basins as well as the exchanges in the Strait of Gibraltar (e.g. Alvarez et al., 2014; Stöven and Tanhua, 2014; Cardin et al., 2015; Hassoun et al., 2019). Results show that, in both scenarios, the Western basin while adsorbing greater quantities, accumulates only a half of the atmospheric carbon stored by the Eastern basin because in the former the carbon is partly exported to the northern Atlantic Ocean. On the other hand, the Eastern basin is also affected by a more intense reduction of the thermohaline circulation and therefore in the vertical transport processes, and the carbon is retained together with the atmospheric CO<sub>2</sub> sink. Additionally, in our case, the use of a high resolution for the biogeochemical projections has allowed to show that in many coastal areas the observed acidification is lower by approximately 8% with respect to the open ocean due to damping effects of alkalinity input from the rivers (not shown here).

Finally, we discussed the physical mechanisms driving the projected changes and the related source of uncertainties (lines 928-933):

The decline in many biogeochemical tracers and properties in the euphotic layer begins in the 2030-2035 period, in correspondence to the weakening of the thermohaline circulation in the basin (Fig. 4), and it is particularly marked in the Eastern basin. This shows that the modification of the circulation resulting from future climate scenarios has substantial effects on the biogeochemical properties of the basin. Changes in the thermohaline circulation of the basin

also explain the increase in the nutrient concentration in the intermediate layer of the Levantine basin, which is a result of the weakening of the westward transport of nutrients through the Strait of Sicily (Fig.S5)."

The use of 1/16 resolution grid is one of the strengths of this study but I think this needs to be more discussed as said in the General comments section. This point has been highlighted in the introduction and is supposed to be the main improvement of this study. However, in the current state of the discussion, it seems that the same conclusions may have been reached with a lower resolution.

We thank the Reviewer for raising this issue that was missing in the text. The result section and the discussion were integrated with a wider explanation on the relevance of using a high-resolution model to enable the detection of spatial gradients in the same sub-basins (for example Adriatic Sea) or between open and coastal areas (for example North Western Mediterranean) that involve the projected changes and their statistical significance.

In particular, in the "Discussion and Conclusion" we added the following text (lines 819-826):

"The use of eddy-resolving resolution and of a higher vertical resolution allows a more detailed representation of the vertical mixing and ocean convection processes, which play a fundamental role in the ventilation of the water column and in the nutrient supply into the euphotic layer of the basin (Kwiatkowski et al., 2020). Moreover, the use of a 1/16° horizontal resolution for the projections has allowed to resolve, identify and characterize, for the first time, spatial gradients existing in some sub-basins (such as in the Adriatic Sea) or between coastal and open ocean areas (such as in the North Western Mediterranean). A more detailed representation of the spatial distribution of the projected changes and of their statistical significance for different biogeochemical tracers represents a clear advantage for the future assessment of climate change impacts on specific organisms, habitats or target areas, also at sub-basin scale."

### Could you accentuate the discussion around the difference observed between the RCP 8.5 and the RCP 4.5?

We thank the Reviewer#2 and the Editor for raising this point. Initially, the Discussion and Conclusion was generally focused on RCP8.5 due to the fact that available results in the scientific literature of the Mediterranean Sea mainly concern worst-case emission scenarios (e.g. Moullec et al., 2019; Richon et al., 2019; Solidoro et al., 2022). Only recently, we became aware that RCP8.5 has been proposed as an "implausible" scenario by some part of the scientific community (e.g. Hausfather and Peters, 2020). However, in order to better address the request from Reviewer and Editor we extensively modified the manuscript to accentuate the importance of the intermediate scenario and to discuss the differences between the two scenarios.

For example, in the abstract we now state (lines 16-28):

"The analysis shows in both scenarios changes in the dissolved nutrient content of the euphotic and intermediate layers of the basin, net primary production, phytoplankton respiration and carbon stock (including phytoplankton, zooplankton, bacterial biomass and particulate organic matter) [...] The projected changes are stronger in the RCP8.5 (worst-case) scenario and, in particular, in the Eastern Mediterranean due to the limited influence of the exchanges in the Strait of Gibraltar in that part of the basin. On the other hand, the analysis of the projections under RCP4.5 emission scenario shows a tendency to recover the values observed at the beginning of the 21st century for several biogeochemical variables in the second half of the period. This result supports the idea - possibly based on the existence, in a system like the Mediterranean Sea, of a certain buffer capacity and renewal rate - that the implementation of policies of reducing CO<sub>2</sub> emission could be, indeed, effective and could contribute to the foundation of ocean sustainability science and policies."

In the results section we further accentuated the discussion of the differences between the two scenarios. For example in section 3.3 we write (lines 556-559):

"During the 21st century, a continuous decrease in the oxygen concentration is projected in both scenarios in the Mediterranean Sea (Fig. 8). The simulated reduction of the oxygen values is slower in the RCP4.5 with respect to RCP8.5. For example, under the RCP8.5 emission scenario, the concentration of the dissolved oxygen in the upper layer decreases by approximately 15 mmol m<sup>-3</sup>, which is three times the value observed in the RCP4.5 scenario (Fig. 8)."

or in section 3.4 (lines 656-660):

"The evolution of the carbon organic matter standing stock is similar to that observed in the dissolved nutrients, with a substantially stable signal in the first 30 years of the 21st century and a decrease after 2030. Afterwards, while RCP4.5 shows a recovery at the end of the 21st century, the projected decline in RCP8.5 is approximately equal to 5 mgC m<sup>-3</sup>. The same dynamics is observed in the intermediate layer, where the decline after the period 2030-2035 is approximately equal to 0.3 mgC m<sup>-3</sup> for the carbon stock"

Finally, in Discussion and conclusion we state for example (lines 844-860):

"Our projections for the biogeochemical tracers and properties at the end of the 21st century shows several signals (see Table SP1 for a synthetic overview) that are mostly in agreement with previous studies, at least with those based on the use of the worst-case emission scenario.

The magnitude of the projected changes has been shown to be, in general, scenario-dependent with the largest deviations from the present climate state observed in the RCP8.5 scenario (Table SP1). On the other hand, the analysis of the projections under RCP4.5 emission scenario found in most of the biogeochemical variables (for example dissolved nutrients and biomasses) a tendency to recover the values observed in the present climate by the end of the 21st century (Table SP1). As shown in the previous sections our simulations, by covering also the RCP4.5 scenario, highlights how an intermediate greenhouse emission scenario produces results that are not simply an average between the present condition and the RCP8.5, but (at least for some variables) something quantitatively different. For example, the temporal evolution of pH (Fig.15) is similar in two scenarios in the first 30 years of the 21st century. Conversely, after 2050, the pH undergoes a substantial decrease under RCP8.5 while it remains almost stable under RCP4.5 with a final projected variation lower than the half with respect to the worst-case scenario.

This supports the idea - possibly based on the existence of a certain buffer capacity and renewal rate in a system like the Mediterranean Sea - that the implementation of policies of reducing CO<sub>2</sub> emission could be, indeed, effective and could contribute to the foundation of ocean sustainability science and policies."

We are confident that all the changes made to the manuscript will address both Reviewer and Editor's concerns. **Mixed layer depth:** 

### How did you define the mixed layer depth? Please add this information to the manuscript.

Agreed. The mixed layer depth is already provided as output by the physical model and is diagnosed using the criterion based on a density difference with the surface lower than  $0.01 \text{ kg.m}^{-3}$ . In order to accommodate the Reviewer's comment we added this information to the caption of Fig. S4 as follows:

"Fig. S4 Time series of the yearly maximum value of mixed layer depth (in m) observed in the Gulf of Lion, Southern Adriatic, Aegean Sea and Levantine basin in the period 2005-2099 under emission scenario RCP4.5 (blue line) and RCP8.5 (dark orange line). The mixed layer depth corresponds to the depth where water has a difference in density lower than 0.01 kg m<sup>-3</sup> with respect to the surface density."

Vertical stratification affects the sinking velocity of particles: It is suggested that vertical stratification affects the sinking velocity of particles (lines 544-545). To my knowledge, even if stratification might impact the downward flux of particles, most of the models didn't take that into account. Could you please explain how it is taken into account in BFM?

We thank the Reviewer for raising this point because we realize that the formulation of the sentence was incorrect. We modified the sentence as follows (lines 653-654):

"The overall increase in the respiration community has as a consequence the decrease in the organic stock matter in the water column."

### NPP:

The effects of the model equations on the NPP trends could be more discussed. This is an important difference between the models which needs to be addressed. Your model equations assume that the planktonic community will remain the same over the next century without adaptation to warmer temperatures, which is unlikely. Furthermore, O'Connor et al., (2011) is cited to explain particulate organic matter decrease despite NPP increase. This paper focuses on terrestrial plants and herbivores which are very different from phytoplankton. Please, add other more appropriate references.

We thank the Reviewer for raising this point. The present configuration of the BFM does not consider any physiological adaptation to average temperature over a certain time window. Given the lack of clear indications on the entity and velocity of adaptation to be added to future scenario simulations, performing a sensitivity study, with numerical simulations based on different degrees of adaptation to the environmental change, could be an interesting theme to be explored in future works.

We report that the paper cited *O'Connor et al., (2011)* has a theoretical nature so it is of broad scope. Additionally, its results are applied to plankton data (see their Fig. 3 for example) so we think it is appropriate to cite it in the present manuscript.

We added the following sentence (lines 875-891):

"In the BFM model, temperature regulates most of the metabolic rates with a Q10 formulation (Vichi et al., 2015). The increase in the net primary production is a consequence of such dependence. In other studies (Eco3M-Med model; Pages et al., 2020) organisms are always optimally adapted and no temperature dependence is accounted for in the physiology. This different parameterization could be connected to the different results in terms of trends; in fact, the scenarios based on the Eco3M-Med model results in a reduction of net primary production. In this case surface nutrient reduction, rather than temperature, affects the net primary production trend producing a decrease. The relative impact of different drivers (nutrient supply versus organism's adaptation to average water temperature) could be explored with dedicated sensitivity experiments.

Our projections of net primary production and biomass dynamics show how different levels of warming of the water column and consequent stratification have a direct impact on the ecosystem functioning by increasing the metabolic rates. Similar to the results obtained in Lazzari et al. (2014) and Solidoro et al. (2022), the increase in metabolic rates augments both primary production and respiration, but with the net effect of reducing living and non-living particulate organic matter, as suggested from theoretical considerations in O'Connor et al. (2011). The decoupled formulation of carbon uptake and net growth in the BFM model induces a further mechanism related to how carbon is channeled in the food web. In fact, the decrease in biomass is partially compensated by an increase in dissolved organic matter production in the basin by the end of the century (Solidoro et al., 2022; results not shown here)."

### Nutrients:

## A nutrients peak is obtained with the RCP 4.5 simulation between 2055 and 2075 (figure S7) and described in lines 455-456. How do you explain this peak? At Gibraltar's Strait the nutrient concentrations are supposed to be fixed, so did the surface water flux change? Could you explain why?

In our computational domain, the Western boundary is located at ~8.8°W (see Figure 1), while the fluxes in Figure S7 are computed at 5°W (inner part of the domain with respect to the Strait of Gibraltar). Thus, the shown fluxes account for the variability of water masses inflowing through the Strait of Gibraltar combined with the changes in the concentration of nutrients determined by the underlying biogeochemical dynamics. These peaks are primarily driven by the variability of water masses entering the system through the lateral boundaries conditions, taken from the CMCC-CM global simulations (further detailed in the reply to the next comment). The associated signal then extends to a larger part of the Western Mediterranean (see Fig. 7), whereas the changes in nutrients concentration is rather small ( $\pm 0.01$  mmol m<sup>-3</sup> for PO<sub>4</sub> and  $\pm 0.1$  mmol m<sup>-3</sup> for NO<sub>3</sub>) when compared to their mean values (see Fig. 5 and Fig. 6).

We recognized that the text at lines 455-456 might be misleading, since we refer there to the inflow of nutrients at the Strait of Gibraltar while Figure S7 illustrates the fluxes into the Alboran region (as reported in the related caption) for a longitudinal section located at 5°W that is far from the model open boundary.

The manuscript has been modified to comply with the content of Fig. S7 as follows (lines 529):

"[...] to an increase in the nutrient inflow into the Alboran Sea (Fig. S7)."

and, accordingly, the caption of Fig.S7 reported the exact location of the section

"Fig.S7 Timeseries of the yearly total transport (in Mmol year<sup>-1</sup>) of Phosphate (left panels) and Nitrate (right panels) in the Alboran Sea in the period 2005-2099 (longitudinal section at  $5^{\circ}W$ ), [...]".

To the best of my knowledge, the Atlantic inflow is hypothesized to increase a bit over the next century due to stronger evaporation in the eastern basin that will increase the SSH gradient. Might this strong peak (and the even higher peak in 2095) be related to model instability? Could you also explain why we didn't observe the peak in the RCP 8.5 simulation where the effect of climate change should be even stronger? This is concerning as this nutrient input affects the response of the WMED (see for example your figure 12 where a peak is visible for the organic matter).

The MFS16 simulation under RCP8.5 scenario projects an increase of the water flow into the Mediterranean basin of about 0.015 Sv (computed as the difference between 2080-2099 and 2005-2020), which is close to results obtained by Adloff et al. (2015) under similar A2 scenario conditions (see Table 2 therein). The simulation under RCP4.5 conditions is instead characterized by a smaller increase of the water flow between 2080-2099 and 2005-2020, equal to 0.005 Sv.

The variability of water inflows through the Strait of Gibraltar in the RCP4.5 scenario is determined by the entrance of water masses with lower salinity through the lateral boundary conditions extracted from CMCC-CM global simulations. These events originate from episodic 'leakages' of Arctic waters toward the eastern north Atlantic and are a consequence of the CMCC-CM internal climate variability. In particular, the RCP8.5 scenario is characterized by a far stronger increase of temperature which does not determine the occurrence of similar Arctic waters 'leakages'.

The competing changes in temperature and salinity fields of water masses entering the Strait of Gibraltar is the main driver of the resulting changes in water transport, as previously reported also by Somot et al. (2006) and Adloff et al. (2015).

Such variability of water fluxes in the RCP4.5 simulation results in specific events of larger inflow of nutrients which extend over the Western Mediterranean and stimulate the microbial loop, with increased bacterial and organic matter concentrations (See Fig. 14), and to a lesser extent also the growth of phytoplankton and zooplankton (see Fig. 13). As noted in the previous reply, the changes in biogeochemical variables related to these episodic events is rather smaller when compared to their mean values.

### DIC and pH:

Line 626: "Disentangle the temperature and DIC contributions ..." how did you do this?

We thank the Reviewer for raising this point that is missing in the manuscript. In order to disentangle the temperature and DIC contributions to pCO<sub>2</sub> we used the carbonate system solver of the BFM in an offline mode with the 2005-2099 simulated variables of each scenario. The offline calculation was made twice, keeping constant, alternatively, temperature and DIC concentration at their 2005-2020 values. By comparing the offline calculations with the actual scenario results, we computed the relative impact of the two factors. The sum of the impacts of two factors (computed with the offline method) differs from the actual scenario result of less than 0.2% (i.e., the offline calculations do not account for the synergic impact of the changes of both variable) which is reasonably low to consider our estimations of the relative impacts as robust. We modified the sentence as follows (lines 735-739):

"In order to assess the temperature and DIC contributions to the  $pCO_2$  temporal evolution, the carbonate system equations of the BFM model have been solved in offline mode with the simulated variables of each scenario. The offline calculation was made twice keeping constant, alternatively, temperature and DIC concentration at their 2005-2020 values. The increase in the temperature has been shown to account only for the 25% of the total increase in the  $pCO_2$ . The remaining part of  $pCO_2$  increase can be ascribed to the DIC concentration increase."

#### **Figures:**

There are 18 figures in the manuscript and 14 in SM which is a lot. The figures like figure 5 (14 are concerned) presented in the supplementary material could be sum-up in a table. The manuscript will become easy to read (mostly the result section) with fewer references to the supplementary material. In general, there are issues with the figures. The captions are generally not detailed enough and need to be re-written. The units need to be on the figure and not only in the captions. There are cosmetic issues on the figures.

We thank the Reviewer for the suggestions. We followed this point and to extensively redraw all the figures. We agree with the Reviewer about the high numbers of figures included in the manuscript. However, we think that all of them provide interesting information to a potential Reader about spatial patterns, temporal dynamics and statistical significance of projected changes and thus, if not specifically required by Reviewer, we would prefer to maintain all of them in the manuscript. As an example, here we show the new version of Figure 5 and relative caption, where we are confident that the cosmetic issues have been solved:



৻৴৵৻

46°N

PRESENT (b) 0-100m RCP8.5

<u>ৰ্</u>চস্ব

PRESENT (a) 0-100m RCP4.5

46°N

Fig. 5 - Phosphate concentration (in mmol  $m^3$ ) in the layers 0-100 m and 200-600 m in the PRESENT (2005-2020, a,b,c and d), and relative climate change signal (with respect to the PRESENT) in the MID-FUTURE (2040-2059, e,f,g and h) and FAR-FUTURE (2080-2099, i,j,k and l) in the RCP4.5 (left column) and RCP8.5 (right column) emission scenario. The Mediterranean average relative climate change signal in each period (with respect to the PRESENT) is displayed by the top-left colored value (blue or dark orange when negative or positive). Values in the green boxes are the average relative climate change in each period and in each sub-basin shown in Figure 1. Domain grid points where the relative climate change signals are not statistically significant according to a Mann-Whitney test with p<0.05 are marked by a dot.

### Here are a few random examples (more are given in the minors comments section):

### - Figure 3 the labels are too small, units for temperature?

Done. The figure has been modified following the Reviewer's suggestions.

### - Figure 4 units? and please, add a map showing the location of the transect

Done. The figure has been modified following the Reviewer's suggestions. Concerning the Reviewer's request, in the present manuscript, the zonal stream function (ZOF) has been computed following the approach described in Adloff et al. (2015) where it is the result of the meridional integration from south to the north of the zonal velocity which is then integrated from the bottom to the top of the water column. This means the domain of integration is the entire Mediterranean Sea shown in Figure 1. The Atlantic Ocean area, outside the Strait of Gibraltar, has been excluded from the computations.

In order to accommodate with the Reviewer's comment, we will state in the manuscript (lines 444-448):

"Figure 4 shows the temporal evolution of the Mediterranean thermohaline circulation during the 21st century using the zonal overturning stream function (or ZOF; Myers and Haines, 2002; Somot et al., 2006). The ZOF has been computed by the meridional integration from south to north and from the bottom to the top of the water column of the zonal velocity (see Adloff et al., 2015). The domain of the integration is the same as shown in Figure 1 with the exclusion of the Atlantic area outside the Strait of Gibraltar."

### - Figure 16, there is no label for the year, and apparently, you give twice the WMED, d,e,f should be EMED

The figure has been corrected

- Figure S7, the y-axis label is too stretched, use scientific notation you will have more space.

The figure has been corrected

#### -Figure S8 (S9) the size of both color bars seems to be differents

The difference size of two colorbars is a consequence of the different numerical interval considered. Because of that the size of the two colorbar can be reduced but will not be the same.

**Technical corrections:** 

### Figure 1: Please highlight extend of the Atlantic buffer zone in the figure. In general, I would like to see the unit below the scale/color bar of a figure, this comment applies to all the figures of the manuscript.

The figure has been redrawn following the Reviewer's comment. The buffer zone is essentially the area outside the Strait of Gibraltar and is already shown in Figure 1 (and not shown any more in other figures). In order to better address the Reviewer's request, the caption of Figure 1 has been modified as follows:

"Mediterranean Sea bathymetry (in m) and relative sub-basins considered in the analysis: Alboran Sea (ALB), North Western Mediterranean (NWM), South Western Mediterranean (SWM), Tyrrhenian (TYR), Adriatic Sea (ADR), Ionian Sea (ION), Aegean Sea (AEG), Levantine basin (LEV). The dark orange line marks the 200m isobath in the model domain. The domain boundary is set at longitude 8.8°W, westward of the Strait of Gibraltar."

### Line 75: "all focusing on high emission..." replace all by "mostly" as Macias et al., 2015 use the RCP 4.5 that is not a high emission scenario.

Agreed. As reply to Reviewer#1, we propose to include and discuss the suggested references in the manuscript. In the introduction we will state (lines 85-86):

"An assessment of the effects of climate change on the biogeochemistry and marine ecosystem dynamics of the Mediterranean Sea has been considered in a number of previous studies based on different emission scenarios".

### Line 76: A1B climate change: please explain

Agreed. We meant "emission scenario". The text has been changed accordingly.

### Line 83: A2 emission scenario: please, relate to the RCP scenarios used here

Agreed. The sentence has been reformulated as follows (lines 101-103):

"Richon et al. (2019) observed, under the A2 emission scenario (which is similar to the RCP8.5 emission scenario in terms of the magnitude of projected changes in the global mean temperature), [...]"

### Lines 94-95: "Moreover, the work also projected ..." change by "Moreover, Solidoro et al 2020 also ... "

Agreed. The text has been modified accordingly

#### Line 109, line 115: same paragraph as before

The paragraph has been reformulated as follows (lines 132-139):

"These considerations emphasize the importance of providing eddy-resolving future projections of the Mediterranean Sea biogeochemistry under different emission scenarios. In fact, although observational and modeling studies have been already carried out in the recent period to assess the importance of the mesoscale dynamics on the physical and biogeochemical state of limited areas of the Mediterranean Sea (e.g. Hermann et al., 2008; Moutin and Prieur, 2012; Guyennon et al., 2015; Ramirez-Romero et al., 2020), long-term eddy-resolving biogeochemical projections under different emission scenarios, to the best of the authors' knowledge, have not been analyzed so far in the region. Such projections might be used in future studies specifically focused on the analysis of climate change impact on specific organisms, habitats and/or local areas."

### Line 116: You say 70 vertical levels and later (line 150) you say 72 please correct that.

We apologize with the Reviewer because the different number of vertical levels in the two models could lead to some confusion. The reason for referring to 72 levels in the MFS16 original configuration is that the domain of the physical model covers a wider area outside Gibraltar (i.e., the western boundary is at 20° W, see Figure 1 of Lovato et al., 2013) and, above all, deeper with respect to our computational domain shown in Fig.1 (see Lovato et al., 2013 for details). The biogeochemical domain, which starts at 8.8°W, has therefore 70 out of the 72 levels active (i.e., the last two levels are all land cells in the biogeochemical domain).

In order to avoid this confusion in the manuscript we reformulated the sentence including "original" (lines 174-175):

"The original MFS 16 domain covers the whole Mediterranean Sea and part of the neighboring Atlantic Ocean region with a horizontal grid resolution of  $1/16^{\circ}$  (~6.5 km) and 72 unevenly spaced vertical levels (ranging from 3 m at the surface down to 600 m in the deeper layers, see Lovato et al., 2013)".

Reference: Lovato, T., Vichi, M., Oddo, P.: High-resolution simulations of Mediterranean Sea physical oceanography under current and scenario climate conditions: model description, assessment and scenario analysis. CMCC Research Paper, RP0207.2013, 2013

### Line 118: You need to define the acronym you use MFS16 and OGSTM-BFM and add citations for both.

The text has been modified accordingly

#### Line 138: Same thing as before for OPA this time.

The text has been modified accordingly

### Line 145: Same for NEMO.

The text has been modified accordingly

### Line 153: Same for CMCC-CM. I will not continue to list those errors, please be sure that every acronym is defined as it is first used.

We thank the Reviewer for the suggestion. The manuscript has been extensively checked for that.

### Line 188: chemical reactions

The text has been modified accordingly

### Line 191-192: CaCO3

The text has been modified accordingly

Lines 294-276: I think this is an interesting approach but this paragraph is difficult to read. Could you try to make it a bit more clear? I think that bringing the S1 figure here could be helpful for the reader.

We thank the Reviewer. The methodology part has been extensively written modifying the terminology as follows (lines 278-334):

"Under each specific emission scenario and in the CTRL, our simulation protocol computes the time series of the mean annual 3D fields of the following variables: dissolved nutrients and oxygen, chlorophyll-a, net primary production, phytoplankton respiration, organic matter, plankton and bacterial biomass, POC, DIC and pH.

First, the annual 3D fields are vertically averaged over two separate key vertical levels: the surface zone and the intermediate zone. The first one spans the upper 100 m of the water column, which represents the location of MAW and the euphotic layer of the basin where most biological activities are concentrated. The second one covers the 200-600 m level, which includes the location of LIW. Only for the net primary production and phytoplankton respiration, a vertical integral over the 0-200 m layer is considered (Lazzari et al., 2012).

Second, the temporal evolution of the unbiased scenario starting from the present state,  $U(k)_{SCEN}$  (with k = 2005, ..., 2099), is defined as:

#### $U(k)_{SCEN} = X'_{SCEN} + X(k)_{SCEN} - X(k)_{CTRL}$ (1)

where  $X'_{SCEN}$  is the average of  $X(k)_{SCEN}$  over the 2005-2020 period (hereafter the PRESENT, Fig.S1), and  $X(k)_{SCEN}$  and  $X(k)_{CTRL}$  are the yearly average in the scenario and CTRL simulations, respectively. We introduce the concept of "unbiased scenario" because equation (1) removes the effect of potential model drifts due to unbalanced boundary conditions and model errors. The time series of CTRL are filtered with a linear regression to keep the long-term drift and remove spurious variability.

The period 2005-2020 has been chosen as reference (also in the forthcoming validation) due to: (i) the availability, after 2000, of more advanced satellite and assimilated datasets to evaluate the biogeochemistry of the basin, (ii) to avoid the overlapping between historical and scenario part of the simulations (with the latter starting in 2005). It is important to stress here that the choice of the period should not significantly affect the results of the study as the observed differences during this period between the two scenarios for temperature, salinity and current speed fields have been found to be not statistically significant over most of the basin (not shown).

Finally, the temporal evolution of the climate change signal (CCS) with respect to the present is given by:

 $CCS(k)_{SCEN} = U(k)_{SCEN} - U_{SCEN-PRESENT} (2)$ 

where USCEN-PRESENT is the average of U(k)SCEN in the PRESENT. Hereafter, if not differently specified, all the shown timeseries will be represented by CCSSCEN.

Horizontal spatial averages are computed considering the sub-basin defined in Fig. 1, the whole Mediterranean basin scale, and two macro areas: the Western Mediterranean (WMED which includes ALB, SWM, NWM, TYR) and the Eastern Mediterranean (EMED which includes ION and LEV). The Adriatic and Aegean Sea are usually not considered part of the Eastern Mediterranean due to the importance of local forcing, such as riverine loads, in shaping the variability of the biogeochemical dynamics in those two basins. Because of that, following the approach already adopted in previous works (Lazzari et al., 2012; 2016; Di Biagio et al., 2019; Reale et al., 2020 a,b) they are not considered in the spatial averages related to WMED and EMED.

Temporal averages of the climate change signals are computed over two 20-year periods: 2040-2059, hereafter referred to as "MID-FUTURE" and 2080-2099, hereafter referred to as "FAR-FUTURE" (Fig.S1). The relative climate change signals (in %, except for pH which will be measured in units of pH) in the MID-FUTURE or FAR-FUTURE periods with respect to the PRESENT are computed as:

U%mid-future=100\*(Uscen-mid-future-Ucen-present)/Uscen-present(3) U%far-future=100\*(Uscen-far-future-Uscen-present)/Uscen-present(4)

where  $U_{SCEN-MID-FUTURE}$ ,  $U_{SCEN-FAR-FUTURE}$  and  $U_{SCEN-PRESENT}$  are the averages of  $U(k)_{SCEN}$  for the MID-FUTURE, FAR-FUTURE and PRESENT periods, respectively. Hereafter, if not differently specified, all the percentages shown in the maps are represented by  $U_{MID-FUTURE}$  and  $U_{FAR-FUTURE}$ . The statistical significance of the relative climate change signals in each point of the basin is assessed by means of Mann-Whitney test with p < 0.05."

We agree that moving Figure S1 in the manuscript would improve the readability of this section. However, we are concerned about the already high numbers of figures. Thus, if not insisted by the Reviewer, we would like to maintain S1 in the supplementary materials to avoid the inflation of materials in the manuscript.

### Line 297: Specify the periods of the satellite climatology.

Done.

#### Line 310 - 315: The spin-up information needs to be completed and added before in the manuscript.

Please see our replies to Your major comment concerning the spin-up.

### Figure 2: Please add the units on the figure for the profiles, and the map. This remark applies to all the figures of the manuscript.

We thank the Reviewer for the suggestion. The figure has been redrawn following the Reviewer's suggestion.

### Figure 2 Add the source of the dataset cited in the caption.

The text has been modified accordingly

Figure 3: The font size of "Year" is too small, this applies to other figures that show time series. If I understand correctly, the variables that you show here are corrected from the CTRL bias (X\_anom(k)scen), please mention it in the caption. Those comments apply to the other times series of the manuscript. Furthermore, why did you apply a 10-years running mean here and not in figure 7? This 10-years running mean is not mentioned in the manuscript (unless I miss it) so it is confusing.

We thank the Reviewer for pointing out this error in the figures. All the figures have been redrawn following the Reviewer's suggestions. All the time series shown are corrected for the CTRL, except for the temperature and salinity where the CTRL run is not available. This is mentioned in the manuscript at the lines 304-305 where it is stated that:

### "Hereafter, if not differently specified, all the shown time series will be represented by CCS<sub>SCEN.</sub>"

Please note that the methodology part has been rewritten (see below). Because of that this information is not included in the captions which are already long enough. The caption also contains the information about the 10-year running mean which has been applied to reduce the noise in the figure. Because of that this filter is discussed only in the caption and not in the main manuscript. We thank the Reviewer because we forgot to include this information in the caption of Figure 7 that has been modified accordingly.

### Line 338: Give more citations here.

Done. Adloff et al. (2015) has been included

Line 348: remove "Fig. S2 in the supplementary material" by (Fig. S2 and S3). The "in the supplementary material" is not necessary and you are not using it all the time, so, remove it everywhere in the manuscript.

Done. The text has been modified accordingly.

### Line 360: replace "increased freshwater deficit" by "decreasing freshwater discharge"

Agreed. The text has been modified accordingly.

### Figure 4: I would like the unit near the color bar. You need to show the location of this zonal stream function over a map.

Figure 4 has been redrawn following the Reviewer's suggestion. Concerning the Reviewer's request, in the present manuscript, the zonal stream function (ZOF) has been computed following the approach described in Adloff et al., (2015) where it is computed through the meridional integration from south to the north of the zonal velocity which is then integrated from the bottom to the top of the water column. This means the domain of integration is the entire Mediterranean Sea shown in Fig.1. The Atlantic Ocean area, outside the Strait of Gibraltar, has been excluded from the computations.

In order to accommodate with the Reviewer's comment, we will state in the manuscript (lines 444-448):

"Figure 4 shows the temporal evolution of the Mediterranean thermohaline circulation during the 21st century using the zonal overturning stream function (or ZOF; Myers and Haines, 2002; Somot et al., 2006). The ZOF has been computed by the meridional integration from south to north and from the bottom to the top of the water column of the zonal velocity (see Adloff et al., 2015). The domain of the integration is the same as shown in Figure 1 with the exclusion of the Atlantic area outside the Strait of Gibraltar."

Figures 5: Again, place the units near the color bars. For the caption, there is not enough information to understand the figure. What are the values in green squares? What is the MID-FUTURE/FAR-FUTURE? The unit given is just for the 4 top figures. Please change the caption to take this into account. This comment is valid for all the other figures in the manuscript and in the supplementary material that looks like figure 5.

We thank the Reviewer for the important comment about the figures. The captions of all the figures have been rewritten including some important information that were previously missed. Just as an example we state:

"Phosphate concentration (in mmol m<sup>-3</sup>) in the layers 0-100 m and 200-600 m in the PRESENT (2005-2020, a,b,c and d), and relative climate change signal (with respect to the PRESENT) in the MID-FUTURE (2040-2059, e,f,g and h) and FAR-FUTURE (2080-2099, i,j,k and l) in the RCP4.5 (left column) and RCP8.5 (right column) emission scenario. The Mediterranean average relative climate change signal in each period (with respect to the PRESENT) is displayed by the top-left colored value (blue or dark orange when negative or positive). Values in the green boxes are the average relative climate change in each period and in each sub-basin shown in Figure 1. Domain grid points where the relative climate change signals are not statistically significant according to a Mann-Whitney test with p<0.05 are marked by a dot."

### Line 384: Define EMT.

Agreed. The acronym has been defined as follows (lines 466):

"EMT (Eastern Mediterranean Transient)"

#### Line 418: Fig.S6 add space.

Done. The text has been modified accordingly

### Line 507: (Fig. 12) should be Fig. 11

Agreed. The text has been modified accordingly

### Line 555: (both phyto- and zoo-) remove the -

Done. The text has been modified accordingly

### Line 658: Should be Figure 17

We thank the Reviewer for spotting this error in the numbering of the figures. It has been corrected as Figure 16.

### Line 724: change Pages et al 2020 for Pages et al 2020b. This applies to all the manuscript.

In the manuscript we have the following manuscript having as first author R. Pagès

"Pagès R., Baklouti, M., Barrier, N., Ayache, M., Sevault, F., Somot, S., and Moutin, T..: Projected Effects of Climate-Induced Changes in Hydrodynamics on the Biogeochemistry of the Mediterranean Sea Under the RCP 8.5 Regional Climate Scenario. Frontiers in Marine Science, 7, 957, 2020"

We do not have any other reference related to R. Pagès

### Line 856: in the bibliography, certain references format are not coherent. For example at line 856 all the authors aren't listed there is an et al. This is the same for lines 890, 902,916, 933, 1006, 1080, 1103.

We thank the Reviewer for pointing out the errors in the references. The bibliography has been extensively checked and corrected.

### Line 902: Punctuation is wrong

Agreed. The reference has been corrected

### Line 982: Format issue

Agreed. The citation has been changed with that one downloaded directly from the website https://www.medecc.org/first-mediterranean-assessment-report-mar1/