

Authors sufficiently addressed all of my comments, and the quality of the manuscript is significantly improved. The contribution of this modeling study in the effort to understand the impacts of climate change on the Mediterranean Sea biogeochemistry is clearer than before. However, some points in the present version of the manuscript have to be restructured. This mainly concerned the uncertainties and limitations associated to this study, and one part of the results (see my major comments). In addition, several minor corrections are necessary (see my minor comments). Therefore, I believe that this paper will likely be a significant contribution and reach the quality standards for publication in the Biogeosciences journal after minor revisions of the manuscript.

Major comments:

1) Sections “4.2 Climate Change Scenario” and “4.3 Uncertainties from the PISCES model”: these sub-sections need to be regrouped, completed, and rearranged at the end (or at the beginning) of the discussion section. In order to have a better insight into all uncertainties and limitations of the study (most of them already mentioned by the authors), to consider their influences on the results and processes discussed in the other sub-sections of the discussion (i.e., 4.1 and 4.4), and maybe to discuss some perspectives. For this, this sub-section should have a more general title, for example, "Limitations of the Study", and divided into three parts (at least):

- a. Climate change scenario: mostly section 4.2
- b. PISCES model: mostly section 4.3
- c. External sources of nutrients used (riverine and Atlantic inputs) and missing (atmospheric deposition): scattered in the manuscript

For example, I was expecting to have information in the discussion section about the influence of the following uncertainties and/or limitations on the biogeochemical response of the Mediterranean Sea,

- How the Atlantic condition and Atlantic nutrient concentrations used in the model could influence the biogeochemical results obtained, mainly for the Western Basin? For example, the authors wrote,

Line 646 - “...the choice of atmospheric and Atlantic conditions has a strong influence on the MTHC.”

with no information or discussion about possible consequences on the biogeochemical results obtained in their study.

- How the atmospheric deposition (not represented in the model, line 157 - “We did not include atmospheric deposition...”) could influence the fact that the Mediterranean Sea is becoming more P-limited at the end of the century in your study?

2) In the conclusion – between lines 755 and 763 – authors highlighted the differences between the western and eastern basins in their biogeochemical responses to the climate change. This part of the results, which, I believe, is one of the major contribution of the manuscript, was absent in the discussion. There were even some contradictory statements, between the results, discussion and conclusion sections (see below), which make difficult to determine the author's opinion on some key aspects:

Line 449 – “But the strong difference between CTRL_R and HIS/A2 at the end of the century indicates that vertical stratification leads to a decrease in surface layer nitrate concentrations, probably linked both with lower winter mixing and nutrient consumption by phytoplankton.”

Line 739 – “Stratification may lead to increased productivity in the surface because of the nutrient concentration increase (see also Macias et al., 2015)...”

Line 751 – “increased stratification, and changes in Atlantic and river inputs, can lead to a significant accumulation of nitrate and a decrease in biological productivity in the surface...”

Line 760 – “the eastern basin is more sensitive to vertical mixing and river inputs than the western basin [...] and the stratification observed in the future leads to a reduction in surface productivity...”

Please, clarify your interpretation of the results in the discussion, in order to support all your statements in the conclusion.

Minor comments:

1) Line 68 – “Macias et al. (2015) simulated [...]. They found that [...] primary productivity over the eastern Mediterranean basin may increase as a result of **density changes (increased stratification isolating the upper layer from the rest of the water column).**”

Need to be corrected, because in the abstract of Macias et al. (2015),

“In the eastern basin, on the contrary, all model runs simulate an increase in surface production linked to a **density increase (less stratification) because of the increasing evaporation rate.**”

2) Line 81 – “This study aims at understanding the biogeochemical response of the Mediterranean to a “business-as-usual” climate change scenario throughout the 21st century.”

The objective of the study should be more detailed. Please, highlight, for example, that the study mainly focus on the influence of the external sources of nutrients (rivers, Atlantic).

3) Lines 127 - 139 – “Temperature and salinity increase strongly, leading to a decrease in surface density and an overall increase in vertical stratification. Average sea surface temperature of the Mediterranean rises by up to 3°C by the end of the century. However, the temperature rise is not homogeneous in the basin, regions such as the Balearic, Aegean, Levantine and North Ionian undergo a more intense warming (over 3.4 °C) probably due to the addition of the atmosphere-originated quasi-homogeneous warming with the local effect of surface current changes. The salinity increases by 0.5 (practical salinity units) on average across the basin. In the A2 simulation, the entire Mediterranean basin is projected to become more stratified by 2100 and deep water formation is generally reduced. These variations in hydrological characteristics of the water masses generate important changes in the circulation and in particular in the vertical mixing intensity. The strong reduction in vertical mixing observed in all deep water formation areas of the basin is linked with the changes in salinity and temperature of the water masses.”

This paragraph needs to be re-write because there are some repetitions, for example, about the stratification and vertical mixing.

4) Line 166 – “Nutrients input from rivers are derived from Ludwig et al. (2010) before 2000, Dissolved inorganic carbon (DIC) and Si are derived from Ludwig et al. (2009).”

What are included in “Nutrients”? Replace “Dissolved inorganic carbon” by “Dissolved Inorganic Carbon”. “Si” means silicates? DIC and Si inputs are derived from Ludwig et al. 2009?

5) Line 190 – “This may result in higher nutrient concentrations at the river mouth. [...] However, nutrients from river discharge are consumed rapidly at proximity of the river mouth and we believe these potential higher concentrations don’t have a large impact of the results.”

If all nutrients from river discharge are consumed near the river mouths, why did you study the influence of the river inputs at the scale of the Mediterranean basin?

6) Line 205 – “This period was chosen in order to avoid including in the CTRL years with too important warming such as the 1980s and 1990s.” Need to be corrected.

7) Line 212 – “present-day conditions [...] present-day conditions”. Need to remove these expressions, or to define them.

8) Line 235 – “from satellite estimations from MyOcean Dataset (<http://marine.copernicus.eu>)”. Which product of MyOcean, quote the full link to access this dataset.

9) Line 236 – “All chlorophyll values in the article and the data are chlorophyll-a.” I do not understand. Do you mean that the word “chlorophyll” stands for “chlorophyll-a” throughout the manuscript? If so, it needs to be define when you use the word chlorophyll for the first time.

10) Line 240 – “with values that”, are you talking about the difference in magnitude?

11) Line 243 – “Moreover, several studies (see e.g. Claustre et al., 2002; Morel and Gentili, 2009) show that satellite estimates have a systematic positive bias in the coastal regions because of the high concentrations of colored dissolved organic matter and the presence of dust particles in seawater back scattering light.”

Need to be corrected. The bias in the coastal regions is due to the presence of sediment: turbid water (case-2 water, higher concentration of inorganic particles). The “general bias” over the Mediterranean basin is due to the presence of colored dissolved organic matter in seawater and other components (not well known yet) that modify the optical properties of the seawater.

12) Line 247 – “the average chlorophyll surface concentration”. That has been normalized?

13) Line 252 – “2 independent datasets” Replace “2” by “two”. These datasets are not independent, the original data are from the same satellite sensors.

14) Line 267 – “200 m is $233 \pm 146 \cdot 10^{-9} \text{ g L}^{-1}$ (average over the 1991–2005 period), while the model value for the HIS/A2 simulation over the same period is $159 \pm 87 \cdot 10^{-9} \text{ g L}^{-1}$.” The unit should be modified into: $\mu\text{g L}^{-1}$ or mg m^{-3} , and this sentence should be in the previous paragraph.

15) Maybe the paragraphs in the method (Section 2.2, lines 122 – 139) should be located into the Section 3.2.

16) Lines 290 – 295, already mentioned lines 210 – 219.

17) Line 301 – “Phosphate content in the entire Mediterranean has increased in our simulation by 6 % over the 21st century...”

Line 310 – “However, climate change effects lead to a global enhancement of 10 % in phosphate content in 2080–2099 in comparison to 1980–1999.”

Is it 6 % or 10 %?

18) Line 384 – “P and N”, you wrote phosphate and nitrate before, and now P and N. Please, stay consistent throughout the manuscript.

19) Line 417 – “Nutrient concentrations in the eastern part...” Replace nutrient by phosphate.

20) Line 422 – “a large annual variability” Replace by “a large interannual variability”.

21) Line 427 – In the previous paragraph, about the western basin, some suggestions/interpretations were included. However, in this paragraph about the eastern basin, there were no suggestions to explain your results, why?

22) Line 459 – “In the Mediterranean Sea, primary productivity is mainly limited by these 2 nutrients and their evolution in the future may impact the productivity of the basin.” Not necessary, the sentence can be removed.

23) Line 461 – “showing an accumulation of nitrate in large zones of the basin,” add “by the end of the century”.

24) Line 463 – “...and a small area in the southeastern Levantine” Also in the Gulf of Lion and south of Crete.

25) Line 465 – “...except near the mouth of the Nile and in the Alboran Sea.” Also in the Ionian Sea, in the Tyrrhenian Sea, Algerian basin and between Crete and Cyprus.

26) Line 466 – “the N:P ratio (i.e. increase in P discharge and decrease in N discharge) in this river in our scenario.” Replace by “the N:P ratio in this river in our scenario (i.e. increase in P discharge and decrease in N discharge).”.

27) Line 471 – “All the most productive zones of the beginning of the century are reduced in size and intensity by the end of the century.” This statement does not convince me... Too broad and unclear...

28) Line 481 – You already mentioned this area around Cyprus in the paragraph before.

29) Line 499 – “South Adriatic”, North Adriatic?

30) Line 501 – “One specificity of Mediterranean biology is that most planktonic productivity occurs below the surface at a depth called the deep chlorophyll maximum (DCM). Hence, most of the chlorophyll concentration is not visible by satellites (Moutin et al., 2012).” I do not think that these sentences are necessary, and I do not think that this satellite limitation is discussed in Moutin et al., 2012.

31) Line 508 – “as the South Ionian and the Tyrrhenian basin.” Not visible in these areas, mainly visible in the south of Crete.

32) Line 511-524 – statements in this paragraph are not supported by the results. For example,

- “but surface concentration is enhanced by about $25 \cdot 10^{-9} \text{ g L}^{-1}$ ”, that means $0.025 \mu\text{g L}^{-1}$, which represents almost nothing.
- “This shows that local variability in the Mediterranean circulation and biogeochemistry is important.” A general statement not supported by results.
- “the average chlorophyll concentration is reduced by almost 50 %”, looking at figure 14a, there is not a 50 % decrease in the chlorophyll concentration.

33) Lines 527, 532, 533 – all percentage quoted in the text are different in Table 4. Please, double-check the values.

34) Line 546 and 556 – lack of quantitative estimates.

35) Section 4.1 Biogeochemical forcings – Not a good title. This section mainly discussed the influences of external sources of nutrients

36) Line 617 – “these regions” replace by “this region”.

37) Line 624 – “2” replace by “two”

38) Line 701 – close parenthesis.

39) Figure A3a – It is impossible to read the colorbar.

This revised manuscript is improved from the previous version and the authors have taken on board the comments by the reviewers. There is now quantitative arguments throughout the manuscript and I accept what the authors say regarding the limitations of the model and that this should not stop the manuscript from being published as it is the best model and model inputs that they have available. They have clarified what is in the model and now acknowledge that organic P and N may also affect the budget. In addition they have created a nice figure summarising the inputs and outputs of phosphate and nitrate to the basin. However, I have still found this manuscript relatively difficult to read in places with the keys points lost within the text. There are now places with extremely long paragraphs (i.e lines 166-195, 331-362, 589-620, 696-731) and in these paragraphs it becomes unclear which key point the author wanted the reader to get from the it. I think both the results and discussion sections can still be improved and there should be increased emphasis on the impact of the results in the discussion. I feel that the results the authors present are important and should be published but at present their impact does not come across strongly enough in the discussion.

Lines 166-195: There is now a very long paragraph in the method section discussing the riverine input. The authors have addressed my concerns in this paragraph but have additionally added further sentences which in my opinion become too detailed in regards to differences between the different inputs (Ludwig vs Adloff etc) in different models which I find confusing. In addition, there is now a lot of repetition between this section and the first paragraph of the discussion. Although the authors obviously do need to acknowledge the potential errors in their results I think these sections can be reduced.

Section 3.3: I still find this section hard to follow. The authors have now added quantitative metrics in this section but they are generally put in brackets rather than integrated into the text which is making it awkward to read. In addition although I appreciate that the authors have tried to change this section, the authors still explain trends such as decrease in phosphorus content by decrease in riverine inputs before presenting the river inputs and therefore are having to repeat things. I suggest putting the results on P and N content after discussing the other terms in the budget.

Discussion: The discussion feels dominated by statements about the limitations/uncertainties of the model with weak statements on the impact and interpretation of results. I think it would improve the manuscript if there was a better integration of the literature with the authors own arguments and conclusions from this study within the discussion. In this revised manuscript the authors have added additional comparisons with other literature which is important and I think was needed, but it currently reads as a list (**Lines 696-731**). The authors are trying to justify why their results are different than what is in the literature rather than using the literature to put their results into context and strengthen their arguments. For example, in lines **727-731** rather than explaining why you can not observe the effects of temperature on nutrient recycling within this model, you can maybe say how an increase in nutrient cycling due to warmer temperatures may strengthen or weaken your conclusions.

Along these lines the discussion on N and P limitation (**Lines 713-726**) could be a paragraph/section to itself. In this section the authors state the their results are "*in contrast with previous literature on the matter*" (**Lines 715-716**). However there is evidence within the literature for P and N co-limitation in the Mediterranean. Whilst generally the spring phytoplankton bloom is P limited, N and P co-limitation has been observed, especially during the stratification period (Thingstad et al., 2005; Tanaka et al., 2011) and there is some evidence of the spring phytoplankton bloom being N and P co-limited in the Western

Mediterranean (Pasqueron de Fommervault et al., 2015) or even N limited (Marty et al. 2002). In addition N limitation has been predicted in the Alboran Gyre aswell (Ramirez et al., 2005; Lazzari et al., 2016). What time period do you calculate the N and P limitation for (i.e annual mean, spring bloom etc)? This may also affect what you are predicting compared to the literature

Finally, at other reviewers suggestion the authors have now included a scenario on atmospheric deposition but only present this within the discussion (**Lines 621-638**). I feel it should be fully integrated into the text (i.e in the methods and results section) rather than tagged onto the discussion. It does provide some important insite on the effect of climate change despite only considering a climatology of atmospheric inputs rather than potential future ones. The authors could further hypothesise what potential future changes in atmospheric deposition may have on the results in the discussion based upon regional projections of atmospheric inputs into the future (i.e Lambarque et al., 2013). Whilst I appreciate they can't actually run a scenario they may be able to comment on whether it is likely to enhance/dampen the trend they see.

Figure 18: I suggest reversing the input and output arrows through the Strait of Gibraltar so that they are the same as the actual water flow. Currently it is suggesting an estuarine flow rather than anti-estuarine.

References

- Lamarque, J. F., F. Dentener, J. McConnell, C. U. Ro, M. Shaw, R. Vet, D. Bergmann, P. Cameron-Smith, S. Dalsoren, R. Doherty, G. Faluvegi, S. J. Ghan, B. Josse, Y. H. Lee, I. A. MacKenzie, D. Plummer, D. T. Shindell, R. B. Skeie, D. S. Stevenson, S. Strode, G. Zeng, M. Curran, D. Dahl-Jensen, S. Das, D. Fritzsche, and M. Nolan (2013), Multi-model mean nitrogen and sulfur deposition from the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP): evaluation of historical and projected future changes, *Atmos Chem Phys*, 13(16), 7997-8018, doi: 10.5194/acp-13-7997-2013
- Lazzari, P., C. Solidoro, S. Salon, and G. Bolzon (2016), Spatial variability of phosphate and nitrate in the Mediterranean Sea: A modeling approach, *Deep Sea Res Part I Oceanogr Res Pap*, 108, 39-52, doi: 10.1016/j.dsr.2015.12.006
- Marty, J. C., J. Chiaverini, M. D. Pizay, and B. Avril (2002), Seasonal and interannual dynamics of nutrients and phytoplankton pigments in the western Mediterranean Sea at the DYFAMED time-series station (1991-1999), *Deep-Sea Res Pt II*, 49(11), 1965-1985, doi: 10.1016/s0967-0645(02)00022-x
- Pasqueron de Fommervault, O., C. Migon, F. D'Ortenzio, M. Ribera d'Alcalà, and L. Coppola (2015), Temporal variability of nutrient concentrations in the northwestern Mediterranean sea (DYFAMED time-series station), *Deep Sea Res Part I Oceanogr Res Pap*, 100, 1-12, doi: <http://dx.doi.org/10.1016/j.dsr.2015.02.006>
- Ramirez, T., D. Cortes, J. M. Mercado, M. Vargas-Yanez, M. Sebastian, and E. Liger (2005), Seasonal dynamics of inorganic nutrients and phytoplankton biomass in the NW Alboran Sea, *Estuarine Coastal Shelf Sci*, 65(4), 654-670, doi: 10.1016/j.ecss.2005.07.012

Tanaka, T., T. F. Thingstad, U. Christaki, J. Colombet, V. Cornet-Barthaux, C. Courties, J. D. Grattepanche, A. Lagaria, J. Nedoma, L. Oriol, S. Psarra, M. Pujo-Pay, and F. Van Wambeke (2011), Lack of P-limitation of phytoplankton and heterotrophic prokaryotes in surface waters of three anticyclonic eddies in the stratified Mediterranean Sea, *Biogeosciences*, 8(2), 525-538, doi: 10.5194/bg-8-525-2011

Thingstad, T. F., M. D. Krom, R. F. C. Mantoura, G. A. F. Flaten, S. Groom, B. Herut, N. Kress, C. S. Law, A. Pasternak, P. Pitta, S. Psarra, F. Rassoulzadegan, T. Tanaka, A. Tselepides, P. Wassmann, E. M. S. Woodward, C. W. Riser, G. Zodiatis, and T. Zohary (2005b), Nature of phosphorus limitation in the ultraoligotrophic eastern Mediterranean, *Science*, 309(5737), 1068-1071, doi: 10.1126/science.1112632