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Interactive comment on "Biogeochemical response of the Mediterranean Sea to the transient SRES–A2 climate change scenario" *by* Camille Richon et al.

Anonymous Referee #2

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The paper by Richon et al presents an implementation of the well established state of the art coupled model NEMO-PISCES to the Mediterranean basin for a transient century long simulation under different climate and nutrient input scenarios in order to assess the relative importance of these in present and future nutrient budget of the Mediterranean Sea. Authors also extend the focus from the nutrient to primary productivity and plankton community composition. The work is very comprehensive and including all the aspects is a significant undertaking and authors need to be recognised for that.

Unfortunately authors missed to fully exploit the model to discuss the observed trend

and pattern, often limiting themselves to the description of those or listing a series of potential causes while the model could theoretically support some of these and discard the others. Moreover, sometime the discussion is confused and it seems that consequences are confused with causes (see below for details).

Furthermore, despite the nutrient budget is one of the core topic of the paper, the information are scattered between figures and tables preventing a clear understanding of the changes between the present day and the end of the century. I would strongly recommend authors to summarise all the information either on a table or figure containing all fluxes and stock in the different considered periods and make that the core table/figure from which all discussion unravel. All other figures and tables are very useful to understand the dynamics, but without a single summary point, it is difficult to mentally connect them all to reconstruct the budget.

Method are generally well described, however information on atmospheric deposition is missing and more detail about the initialisation process are welcomed (more details below)

Title and abstracts are clear, language is also generally clear, however some sentence needs revision for clarity. Some of the figures needs improvements, suggestions are given at the end of the detailed review (in the minor comments section).

Areas where further details/discussion is needed or it needs to be clarified:

lines 160-165: Although I fully sympathize with authors regarding the difficulty to have fully consistent source for riverine water and nutrient discharge, and I am not against the choice the authors made, I would suggest authors to briefly discuss the potential impact associated to the incoherency between these, for instance showing how big this incoherence is.

Section 2: there is no mention of atmospheric deposition of nutrient in all the methods section. Only in the discussion (501 and 502) authors state that deposition was not considered because there are no future estimates of nutrient deposition up to 2100. Is deposition completely neglected or just kept constant at present day value?

Lines 205-210: given the errors at the mouth of the Nile, I suggest to add some more detail on the source of the data for this rivers and the uncertainty associated (e.g. see above).

Lines 210-220: while figure A2 immediately shows area where the model has higher or lower skills, I strongly recommend to add some numerical measures of the ability of the model to capture observed data. Furthermore, authors state that model correctly simulates a DCM, however figure A1 clearly shows how the DCM is much deeper than the data shows (roughly at twice the depth). Authors briefly mention this later in the paper, but I suggest to anticipate this here. It would be also interesting to see a comparison of T, S, nutrient profiles in the same location, to understand the reasons of a deeper DCM

lines 254-256: as authors state, phosphate start increasing in CTRL_RG only at the end of the century, therefore the "strong link" between phosphate and Gibraltar is "proofed" only at the end of the century, for most of the simulation surface phosphate seems to be close to CTRL and CTRL_R despite figure 5 shows quite different P influx around 1990 (positive) and 2040 (negative).

Lines 263-265: the trend highlighted here is apparent only in the A2 forced simulation, raising the doubt that it could be linked to a spin-up issue (see below for other examples on this). Authors explained the initialisation procedure (lines 170-173) although this is not fully clear: all simulations started from the same restart (and in this case initial trend could be due to adjustment to new forcing, particularly in the climate case), or all scenarios have been run for more than 115 year since that initial common restart (and in this case why the trend is only in A2)?

Lines 265-267: the interesting decennial cycle is not evident only in 3e (where is actually weak) and 3f, but also on the surface. Can authors suggest some mechanism

for this? Is this a cycle in intensity of stratification? Is this associated to cycles in the atmospheric patterns?

Line 259 vs 269: authors claims that there is a "slight accumulation of phosphate in the deep Western basin" while "the evolution of nitrate concentration shows marked accumulation in all region". I could be wrong (this is simply a visual calculation), but focussing on A2 trend from 1980 to 2100 figure 3f shows an increase from approx .15 mmol/m3 to 17.5 mmol/m3 (+16%), figures 4e and 4f show a similar relative increase, while 4b and 4c a smaller relative increase. Even if we compare these with the CTRL simulation, the difference between the P accumulation in the deep Western basin and the N accumulation is not that big as the qualification "slight" and "marked" suggest.

Lines 274-277: Can authors explain why riverine discharge seem to have more impact on N than P? How much is simply due to the different evolution of the forcing, and how much is due to internal dynamics?

Lines 283-285: I suggest authors to clarify what they means by "linked with nutrient exhaustion". The link between stringer stratification and lower surface nutrient is well established in literature (due to lower winter mixing), what nutrient exhaustion add to this mechanism and do authors have supporting evidence for this?

Line 301: in the absence of statistical measure of the trends, nutrient fluxes at Gibraltar seems characterised by a high interannual variability until about 2060 rather than a coupled decreasing-increasing trend.

Lines 300-304: authors correctly state that the relative increase in the influx is higher than the relative increasing of the outflux, but what's the difference in absolute term? Is there a change in the net flux?

Line 326: similar to comment on 263-265, could the sudden drop be justified by the adjustment to the new forcing (spin-up)?

Lines 335-370: it is not clear if here authors are still discussing sedimentation fluxes, or rather the global nutrient budget. If the latter I recommend to separate this part with a different sub title (and develop this around the new suggested figure-table). Also, if the latter is true, I suggest to clarify the sentence “the sum of nitrogen fluxes in the Mediterranean”: is this the total net flux in the Mediterranean?

Line 354-356: authors seems to suggest that the accumulation of phosphate could be due by the decreased primary production, however this is in contrast with the fact that P is the more limiting nutrient in most of the domain (an accumulation in P should lead to an increase in primary production in a P limited environment). Authors should clarify the mechanisms behind the observed trends and the interaction among those mechanisms.

Section 3.4: from my understanding of the PISCES model, sedimentation of particulate nutrient is mostly driven by primary (sinking phytoplankton) and secondary (faecal pellet) productivity. If this is true, I would suggest authors to use the patterns observed in the biological productivity to discuss and interpret the sedimentation fluxes (an possibly anticipate 3.4 before sedimentation)

Section 3.4: authors discuss at length nutrient limitation presenting co-limitation as a widespread condition in most area of the Mediterranean. To my knowledge there is quite an extensive literature on P limitation in the Mediterranean, particularly in the Eastern basin (e.g. a long list of publications by Krom and co-authors), and authors do not refer to any of this. I strongly suggest to include these in the discussion, compare the results from the model with those findings and suggest potential reasons for the difference. (By the way, I want to emphasize that I have not contributed to any of those papers)

line 409: the use of “thus” suggests that as a consequence of the increased P-limitation in the eastern basin, the surface P concentration will further decrease? Could authors clarify what is the positive feedback between P limitation and P reduction?

Lines 438-439: authors state that “The changes in DCM we observe combined with

external nutrient input changes result in 17 % reduction in integrated chlorophyll production”, however the DCM is a consequence of the chlorophyll production and not a cause. DCM is simply the location of the sub-surface maximum of chlorophyll, therefore, unless authors clarify the meaning of that sentence, is the reduction in the chlorophyll production that leads to the changes in the DCM

line 468-470: as above, could these be a consequence of the A2 model still adjusting to new atmospheric forcing?

Line 512-514: the fact that coastal area (and in particular the Adriatic sea) is largely influenced by coastal nutrient inputs is not a new finding and authors should acknowledge the past literature

Conclusions: the conclusions are too high level and therefore miss to emphasize the importance of the new findings. For instance the fact that the Western Med is more influenced by the Gibraltar influx than the Eastern Med (lines 590-596) is not that surprising. Furthermore, authors cite their work emphasizing that atmospheric deposition can bring up to 80% of phosphate in some region of the Mediterranean sea. As asked earlier, authors do not clarify if atmospheric deposition is completely neglected in this simulation or simply kept constant: is the former, a lot of the results and discussion are heavily biased by the simplifying assumption and authors should careful and rigorously discuss how this simplification affect all the results presented.

Minor comments:

line 46: this sentence “the Mediterranean thermo- haline circulation (MTHC) may significantly change with a consistent weakening in the western basin for greenhouse gases high-emission scenarios...” needs clarification: does authors means that the MTHC weakens in the western basin in climate change scenario characterized by high emission scenario?

Line 76: which daily 3D forcings are needed by the biogeochemical model? Do authors refer to boundary condition at Gibraltar?

Line 112: “that” needs to be removed

line 145: Although I understand that authors refer to original manuscript for details, I would suggest authors to briefly explain (or show within a map) the extent of the buffer zone

line 155: Authors state the four largest rivers of the Mediterranean and Black Sea are the Rhone, the Po, the Ebro and the Danube. Although I agree that these are 4 important rivers, these are surely not the 4 biggest one. Acording to Ludwig 2009 (cited by the authors) the Danube mean flow is 6573 m3/s, Rhone 1721m3/s, Po is 1569m3/s and Ebro is 416 m3/s. Clearly the Nile is the largest, and the Dniper is also bigger than the Ebro

lines 158-160: this sentence does not flow correctly in English, and is not clear, I suggest authors to revise it.

Line 315: nitrate needs to be capitalised after the full stop

line 540: If the authors want to suggest that the loss of nitrate in the CTRL run can be due by an underestimation of riverine fluxes in the CTRL riverine forcing, I recommend authors to rewrite the beginning of this sentence and explicitly state that, instead of simply referring to the discrepancy with A2 (since the latter can’t influence CTRL)

figures 3 and 4: it is very hard to distinguish between grey, blue and green line. Although I recognise that the quality of the picture in the PDF is not at the highest, I strongly suggest the authors to use more contrasting colours.

Figures 8-9-10: I suggest authors to consider to modify the 2080-2099 panels by showing a map of the difference with the 1980-2000 period to better highlight the evolution of N,P and primary production

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