

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. Tselepidis, 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