Dear editor,

We first want to thank you very much for your constant help and your guidance all along the review process. A detailed answer has been provided to each of the three anonymous referees. Moreover, we followed your recommendations for the revised manuscript as this is detailed below.

Regional biogeochemical modelling is still a challenge due to the complexity of the respective systems and the complexity of the underlying model systems. As many other papers, this papercannot solve all problems, but it makes a contribution to the scientific discussion about theimportance of DOC in the carbon cycle. Even though the model skill assessment is not perfectlikewise, at least the authors make a comprehensive attempt to assess the performance of theirmodel, which is not always the case. All the reviewer comments have been highly welcome and Ithank all of them for their work, their reasoning, and their patience with this manuscript. Also thepatience and willingness of the authors to improve the manuscript is highly appreciated. I disagreesomewhat on the request by reviewer #3 about the lacking complexity of the particle flux model (nodifferent size classes, no aggregation model). Kriest et al. (2010) [I. Kriest, S. Khatiwala, A.Oschlies, Towards an assessment of simple global marine biogeochemical models of different complexity, Progress in Oceanography 86 (2010) 337–360] have shown, that increased model complexity is not always the remedy against mismatch between simulation and reality. Taken all aspects into account I would like to accept the paper subject to minor revisions. These revisions need to address comments of the reviewers of the already revised paper (referees#3, 4, and 5). In particular, I would like to ask the authors for the following:

1. Please, convert the appendix into “supplementary information”, to be placed separately on the web upon publication. This should address the length problem as raised by referees #3 and #4.

This has been done in the revised version.

2. Add a few sentences of critical appraisal concerning the observational data base (BOUM cruise) and why it may be non-ideal (referees #3 and #5).

Though these datasets were essential for the assessment of the model skill, the DyFaMed and the BOUM cruise datasets may be subject to a critical appraisal as suggested by several referees. The following lines have therefore been added at the end of the supplementary material dealing with the comparison between data and model outputs.

« In conclusion, some critical appraisal concerning the datasets (from DyFaMed station and BOUM cruise) used in this study for the model skill assessment, can also be done. The BOUM cruise took place in summer (June-July 2008) during the stratified period in which nutrient concentrations in the surface layer are very low, and even under the quantification limits in the eastern basin. A rigorous assessment of the nutrient concentrations provided by the model in this layer is therefore impossible. Moreover, the cruise lasted 1.5 months, and the nutrient maps to which model outputs were compared do not correspond to a snapshot but rather to a collection of data gathered at different times. This may partially bias the comparison with model outputs since the latter were averaged over the period of the cruise.
Concerning the DyFaMed site, it is affected by many mesoscale processes that are known to generate high variability in data which are not necessarily representative of the trends at sub-basin scale. This may also generate potential bias for the comparison between model outputs and in situ data. Finally, though the aforementioned bias, these datasets are essential, since, except near the coasts, there is a clear lack of datasets including the large variety of data that are necessary to assess a biogeochemical model such as Eco3M-MED.”

3. Cite the work by Kriest et al. (2010) and/or respective other material to comment on the issue of complexity versus simplicity in ocean biogeochemical models.

The following lines have been added in the Discussion section:

"To conclude on this point, adding complexity in a given model generally leads to the multiplication of the number of state variables and parameters. When these parameters and/or these new processes are not well known, this also adds uncertainty, and in this case complexity does not necessarily lead to a better agreement between model outputs and observations as this is suggested by several studies (e.g. Muller et al., 2009, Kriest et al., 2010, Paudel & Jawitz, 2012). With the addition of a class of large detrital particles, the sinking velocity associated with this detrital compartment as well as the definition of the processes that would have fueled this additional compartment (the latter are not the same in the different aforementioned models that use two detrital compartments) would have added a source of uncertainty in the model. As a consequence, the particulate carbon fluxes wouldn't necessarily have been more realistic than the ones provided by this study. The same conclusion may apply as regards the aggregation models which formulations are empirical and associated with parameters that are hardly measurable. "

Some other minor changes have also been done in the manuscript (they appear in blue in the revised version).

Sincerely yours,

On behalf of all co-autors,

Melika Baklouti

Answer to referee #3

First, we sincerely thank the referee #3 for his comments.

I have read the revised version of the manuscript submitted by Guyennon et al. as well as the answers provided by the authors to the reviewers comments. I am not convinced that this revised version can now be published in Biogeosciences. Indeed, the authors decided to put in the appendix a substantial part of the manuscript in order to answer to the comments of the reviewers that the manuscript is too long (which was true). This results in a very long appendix (more than a half of the main text length). I understand that the authors have to show that their model has the ability to understand the key questions at the core of this manuscript but it would have been more appropriate to have a full paper dedicated to model validation issues and then to have another one focused on the estimation of the export. This is my advice because it would allow describing extensively the validation exercise which is a crucial problem in modelling that cannot be put in the appendix but rather deserves an entire paper.

We agree with the editor that the best solution consists in moving the comparison with data into supplementary material.

Moreover, the validation of the results is in some places not convincing as also noticed by the other reviewers. In particular, I am not satisfied with the answer provided by the authors as concerns the validation of the physical model and in particular, on the ability of the physical model to simulate vertical processes such as convective ones. A large part of the conclusions on the vertical export is determined by the capacity of the physical model to generate deep convection (e.g. in intense convective regions like the Gulf of Lions).

The systematic analysis of the physical run was beyond the scope of the present paper. However, the physical processes that were determinant for the analysis and discussion of our biogeochemical results have been mentioned and assessed when necessary. Moreover, it is quite difficult to assess the carbon export fluxes at 100 m with deep water formation rates. The latter would have been useful to assess carbon export fluxes in deep water and carbon sequestration but are not direct indicators of water (and carbon) fluxes at 100 m. For these reasons, we rather used the in situ estimations of carbon export fluxes instead to assess the quality of the carbon fluxes calculated by the model at 100 m. More details on deep convection are however delivered in what follows. In the Gulf of Lions, the MED12 model allows a good representation of the deep convection in key areas, as mentioned by Beuvier et al. (2012b, Mercator Newsletter, see their Figure 2) running the *MED12-long* simulation, which is the one used in this study to force the biogeochemical model. More details on deep convection are given in what follows for the Gulf of Lions and the Aegean Sea.

(1) For the Gulf of Lions :

- It was first shown by Beuvier et al. (2012a) running a similar but shorter (10-year) *MED12* simulation (Figure 1), that the simulated convection reached the sea bottom in the Gulf of Lions (> 2400 m depth) from 1999 to 2006. For example, the formation rate of Western Mediterranean Deep Water (WMDW) for waters denser than 29.10 kg.m-3 calculated in 2005-2006 was equal to 1.68 sverdrups, a huge value though lower than the 4.8 sverdrups estimated from the observations by Schroeder et al. (2008).
- The *MED12-long* simulation relies on the same *MED12* configuration as the MED12 simulation, but correspond to a longer run. This simulation also provides a good representation of the deep convection in the Gulf of Lions. The depth reached by the convection is in agreement with the shorter simulation *MED12* during their common 1999-2008 period, and with other estimates from observations (Figure 2), as described in the PhD of Beuvier (2011). The formation rate for the 2005-2006 period for WMDW in *MED12-*

long is nevertheless lower than the *MED12* estimate. It is equal to 1.4 sverdrups, a value similar to those obtained during strong winter convection episodes in the Gulf of Lions.

(2) For the Aegean Sea :

- As documented by Beuvier et al. (2010) running the long-term *NM8-atl-riv* simulation mainly differing by the ocean horizontal resolution compared to *MED12-long*, the convection in the Aegean Sea can reach depths deeper than 1200 m depth, allowing the formation of dense volume of deep waters in the Aegean Sea (Figure 3) from 1972 to 2001. The formation rate of Cretan Deep Water (CDW) denser than 29.20 kg.m-3 can reach 1.25 Sv during the Eastern Mediterranean Transient event of 1993 described by Roether et al. (2007). These formation rates allowed a good representation of the Aegean export toward the Levantine basin through the straits of the Cretan Arc compared to observations.
- The *MED12-long* simulation used in this study to force the biogeochemical model also provides a good representation of the formation of CDW water denser than 29.20 kg/m3 (Figure 4), with a formation rate of 1.22 Sv in 1993. This value is consistent with the *NM8-atl-riv* simulation (see the PhD of Beuvier (2011) for more details), though slightly lower. In particular, the Aegean export of dense water toward the Levantine basin is higher in the *MED12-long* run than in the *NM8-atl-riv* due to the formation of denser deep water in *MED12-long*, and thus in better agreement with the observations at the strait sills.

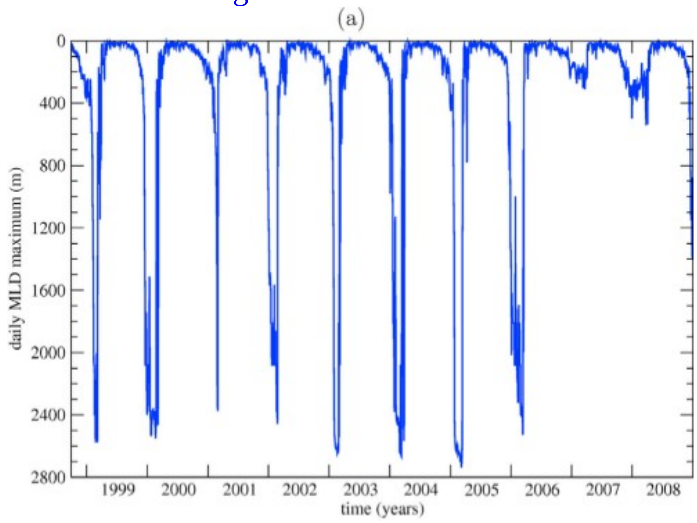

Figure 1 : Daily values of the maximum of the turbocline depth (m) in the northwestern Mediterranean [0°E-9°E;39°N-45°N], from 1 October 1998 to 1 December 2008, according to the 10-year MED12 simulation of Beuvier et al. (2012a, Figure 7a, page 10).

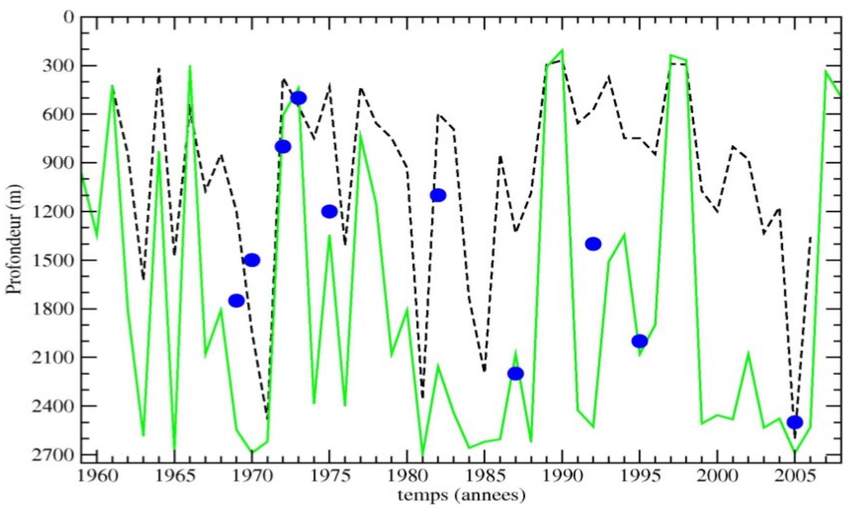

Figure2 : Monthly values of the maximum of the turbocline depth (m) in the northwestern Mediterranean [0°E-9°E;39°N-45°N], from 1960 to 2008, according to (green) the MED12-long simulation of Beuvier et al. (2012b), and (blue dots) according to observation estimates, as reported in Beuvier (2011, PhD, Figure 5.26, page 187).

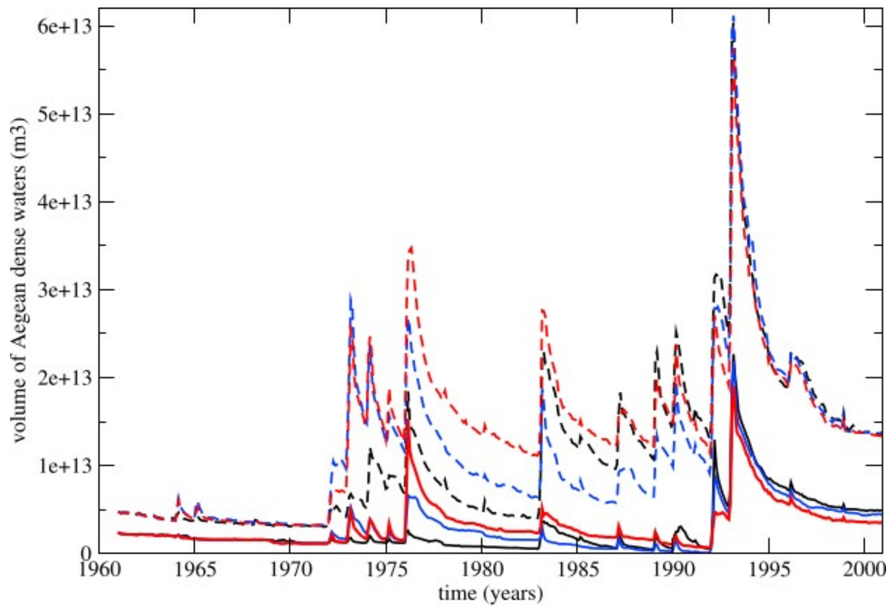

Figure 3 : Monthly volume (in m3) of Aegean waters denser than 29.2 kg.m-3 (dashed line) and 29.3 kg.m-3 (solid line), for three NM8 companion simulations, in particular, in red, the NM8-atl-riv simulation, reported in Beuvier et al. (2010, Figure 12, page 16).

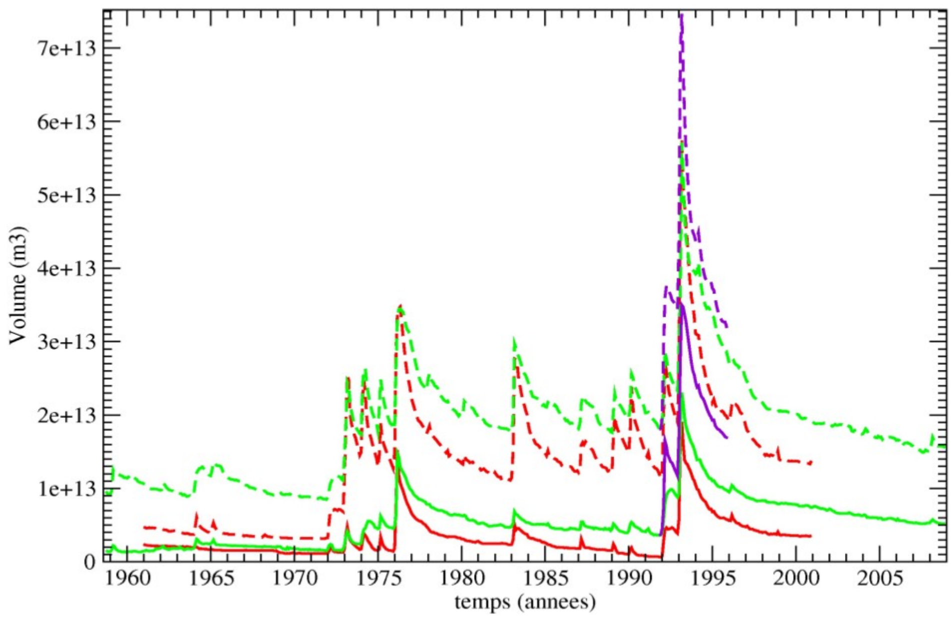

Figure 4 : Monthly volume (in m3) of Aegean waters denser than 29,2 kg.m-3 (dashed line) and 29,3 kg.m-3 (solid line), in particular again for NM8-atl-riv in red (Beuvier et al., 2010) and for MED12-long in green from Beuvier (2011, PhD, Figure 4.22, page 115).

Another process that is essential for the vertical export is the sedimentation process of POC. Here, the authors used one class of POM with one sinking speed. There are other models that used several size classes or aggregation models in order to refine the representation of the export. This refinement would be needed considering the questions that the model has to address.

Adding complexity in a given model necessary leads to a multiplication of the number of state variables and parameters. When these parameters and/or these new processes are not well known, this also adds uncertainty, and in this case complexity does not necessarily imply better results as suggested by various studies (e.g. , Muller et al., 2009, Kriest et al., 2010, Paudel & Jawitz, 2012). With the addition of a class of large detrital particles, the sinking velocity associated with this detrital compartment as well as the definition of the processes that would have fueled this additional compartment (the latter are not the same in the different models that use two detrital compartments) would have added a source of uncertainty in the model and would have led to particulate carbon fluxes that wouldn't necessarily have been more realistic than the ones provided by this study. The same may apply to the aggregation models. Finally, POC to DOC degradation rate is at least as much important to consider for obtaining realistic POC export, and it is likely that the main issue lies in a right balance between POC sinking fluxes and degradation rates. Overall, the comparison of the POC fluxes at 100m provided by this study and the available in situ estimations shows that these values are in the same order of magnitude.

I am not convinced by the answer provided by the authors for justifying the period 1973-1977 for performing the adjustment to initial conditions and then to run the simulations in the 1990s since the conditions drastically change in the 1990s. This choice makes that the nutrient content of deep waters show discrepancies with results from the BOUM cruise as pointed by the authors. The authors justify this choice by saying that they would like to be outside the period of EMT but if as they mentioned EMT drastically change the nutrient structure, then there is nosense to use 70s conditions for simulating the 90s.

The SeaDataNet climatology used as initial conditions combine pre and post-EMT values. That means that the changes in nutrient contents in the deep layers due to the EMT event should at least partially be included in the initial data set. Moreover, another simulation has been run (Palmieri, 2014) with the same hydrodynamical model though with a different biogeochemical model (i.e. PISCES). This simulation has started in 1965 with the same initial conditions as ours, and it reveals that the variations in deep NO3 and PO4 concentrations after the EMT event are very weak (+0.1 µM for NO3 and +0.01 µM for PO4) and lower than the differences between BOUM data and our initial conditions. In other words, even if our simulation would have started before the EMT event and would have been run until 2012, the deep concentrations of NO3 and PO4 would have likely be still lower than the ones provided by BOUM data, especially for nitrate.

Finally (minor), I am not convinced by the answer they provide to justify the innovative aspect of their model. I agree with the reviewer that since years, models (e.g. ERSEM, BFM) simulate the nutrient content of PFTs and not only the ratios as mentioned by the authors. The new part here is the explicit representation of the cells number.

Classical variable stoichiometry models indeed allow to calculate PFT concentrations in terms of mol C/m^3, mol N/m^3, etc. but not intracellular contents in terms of mol C/cell, molN/cell, etc.

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Answer to Referee #4 :

From the editor's recommendation and from the authors' answer to it I see that this manuscript is now in the 3rd review stage. I am myself a physical oceanographer and from this fact and the fact that the manuscript was already reviewed several times, I consider necessary now to prove if all objections of the reviewers and the editor were incorporated. This has indeed been done. Also English writing was improved so that the manuscript is clearly readable. But one objection still remains, which was claimed by both reviewers and which I also think, is critical: the manuscript is much too long. In my opinion this is not only a technical objection but it makes the manuscript only interesting for a small group of researchers as it is going into detail too much concerning biogeochemical modeling. A solution can be, to split the paper into two: one describing the technics and reliability of the model and one describing the results and consequences for the Mediterranean Sea. I recommend the paper to be worthwhile for publication but with the restriction that the paper is too long. It should be left to the editor, if he thinks that it is relevant in its present form for publication in the journal.

First, we sincerely thank the referee for his comments. The manuscript has already been shortened in the previous stages of revision and it is quite difficult to shorten it further. We therefore agree with the editor that, since the comparison with data can be considered as an independent part, and put in supplementary material.

Answer to referee #5

First, we sincerely thank the referee #5 for his comments.

General comments:

This work is aimed at providing a basin-scale description of organic carbon stocks and export fluxes in the Mediterranean Sea through a modelling approach based on a coupled model combining a mechanistic biogeochemical model (Eco3M-MED) and a high-resolution (eddy-resolving) hydrodynamic simulation (NEMO-MED12). Overall, the model seems to mimic the main spatial and seasonal biogeochemical characteristics of the Mediterranean Sea, although some regional patterns are not entirely reproduced, which can be attributed to the presence of mesoescale phenomena that are not resolved by the model. The authors conclude that the main contributor to organic carbon export throughout the whole of the Mediterranean Sea is DOC accumulation and production, with phosphate limitation of both bacteria and phytoplankton growth being responsible for this finding. Although explanations regarding POC export in relation to the natural mortality of large organisms and production of fecal pellets and sloppy feeding by mesozooplankton seem easy to follow and plausible, DOC patterns are, in contrast, difficult to understand in the light of the data. I find some doubts with respect to the biogeochemical processes behind DOC distribution, particularly those regarding phosphate limitation. In fact, the model does not seem to reproduce the nutrient concentrations and particularly, the levels of phosphate are clearly underestimated, which are used as the main line or argumentation to explain the DOC export in the basin. It is even recognized by the authors that an excessive P limitation in the model may result in a high DOC production. In addition, the BOUM cruise was conducted in summer when nutrients are logically depleted in the euphotic layer. Plus, I do not fully understand why nutrients contents are shown in the first 30 meters of the water column in Table 1 whereas the model outputs always refer over the first 100 meters. The uncoupling between nitrate and phosphate in DOC production rather than the levels of phosphate itself could have been explored. Therefore, my major concern regarding the main conclusion of the paper lies on the role of phosphate. Nevertheless, the overview presented in the paper is still interesting and deserves publication in Biogeosciences but assuming the limitations of the model to reproduced some of the observed patterns in the basin.

The role of phosphate limitation on DOC distribution is already well documented since the pioneering work of Thingstad et al. (1997) and though model outputs seem to indicate a slight underestimation of PO4 concentrations, this does not call into question the role of PO4 in DOC distribution. Moreover, if the PO4 concentrations in the surface layer are likely underestimated (this can't be formally verified in the eastern basin since concentrations are below the detection limit), this could indeed explain the overestimation of DOC accumulation calculated just below the free surface as this is mentioned in the manuscript line 1266. Elsewhere in the 0-100 m layer, DOC concentrations are not overestimated and the modeled 0-100m DOC stocks are very close to the measured ones. This is the most important since the 0-100m stock of DOC will be essential for the determination of the DOC flux exported under 100 m. Finally, the deficit in phosphate in deep waters is very weak in the eastern basin (0.15 µmol/l for the model against 0.16 µmol/l according to BOUM data) and it does not exceed 0.04 µmol/l in the western basin (see table 1). The discrepancy between deep nutrient concentrations and those of SeaDataNet data therefore mainly concern nitrate concentrations and this has been clarified in the revised manuscript.

Concerning the data provided in Table 1, they were shown in the first 30 meters of the water column because the nutricline is located between 30 m and 250 m depth and we considered that it was more relevant for the comparison to divide the water column into three layers in which concentrations are relatively homogeneous thereby giving more sense to the mean values. Nutrient concentrations integrated over the 0-100 m layer could have been delivered as well but this seemed less relevant for the aforementioned reasons and for the fact that this is somehow disconnected from the export fluxes at 100 m.

Some minor modifications could be also made in the manuscript in order to clarify some of the assumptions taken by the authors.

Specific comments:

Line 226. Given the inaccuracies in phosphate measurements, we decided to compute phosphate profiles from that of nitrate by imposing a Redfield ratio of 16 in order to be more consistent with observed NO3:PO4 ratios in this region (Gómez, 2003). I do not agree with this assumption. In the so-called by the authors buffer zone, there have been many studies over the last decade that address the nutrient exchange through the Strait of Gibraltar. These works provide very accurate measurements of phosphate in the area and even the values of the Redfield ratio in the water column (which are far from the canonical ratio of 16) and the nutrient transport rates through the Atlantic and Mediterranean (Dafner et al., 2003 GRL; Macías et al., 2007PiO; Huertas et al., 2012GBC). Any of these values could have been used to fuel the model rather than the data given by Gomez (2003). I wonder how this assumption would affect the model outputs, maybe it is not a crucial issue, as it is taken as an average ratio, but considering a fixed Redfield ratio of 16 in this buffer zone is definitively not correct, especially if we take into account that phosphate is normally in excess in the Atlantic jet that feeds the Alboran Sea. Therefore, calculations of phosphate through nitrate concentrations could have been avoided, as literature provides recent and good nutrient data in the area. If the authors are confident with this procedure, at least nutrients transport rates through Gibraltar should be mentioned and explained why they have not been chosen.

Before imposing a redfield ratio in the Atlantic waters, a first attempt has been done using the WOA data in NO3 and PO4. In these data, the NO3:PO4 was always higher than 16 in surface waters. This led us to make the choice of the Redfield ratio for Atlantic Waters (AW). We acknowledge that, though better than the first one, this solution could be further improved, and could be reconsidered for future work. However, this condition is applied at the eastern boundary of the buffer zone in which the NO3:PO4 ratio varies since it is dynamically calculated by the biogeochemical model. As a result, the AW entering the Gibraltar Strait are characterized by NO3:PO4 ratios comprised in the range [12;24], the highest values being calculated in winter. When we compare these ratios with the ones found in the articles cited by the referee, they are higher than the ones provides by Huertas et al. (2012). In this paper, the following NO3:PO4 ratios can be inferred from the NO3 and PO4 concentrations in the Atlantic waters, i.e. 10.7 and 12 respectively westward and eastward the Gibraltar Strait. However, higher ratio values can be found in Dafner et al (2001) with a NO3:PO4 ratio which is indeed lower than the classical Redfield ratio at the western entrance of the Strait (NO3:PO4 = 13.8 (±0.5)), but close to the Redfield ratio in the middle of the Strait (15.6 (±0.6)), and increases dramatically to 23.6 (±3.4) at the eastern entrance of the Strait. The Redfield ratio in the Gibraltar straight is therefore highly variable and seems as much conditioned by the ratio in AW than by the physical and biogeochemical dynamics in the middle and in the immediate vicinity of the strait. Further in situ and modelling work in this field is indeed necessary.

Page 10, first paragraph. The considerable amount of work performed by the authors is greatly appreciated and the modeling effort is really valuable. However, I do not see the necessity to use the DyfaMed database (or patterns), as DOC distribution in this site is affected by many mesoscale processes that do not reflect the trends at a basin scale. As the authors recognize, they have to perform an artifact for the model to reproduce the spatial patterns in the region, as otherwise in situ data and model outputs would never match. The BOUM data are indeed appropriate and useful for validation and therefore, it would be enough at a large scale. Also, considering the length and dimension of the paper, the DyFaMed exercise could be well omitted without compromising the validity of the results. In fact, nutrients patterns in the site for instance are not reproduced by the model and significant discrepancies between observed concentrations and modeled values can be found.

We agree with the referee that the DyFaMed site is affected by many mesoscale processes that are known to generate high variability in data which are not necessarily representative of the trends at a basin scale. However, these mesoscale features are partly represented by the model. Moreover, the comparison between model outputs at the DyFaMed station and those averaged over a region around the DyFaMed site did not evidence significant differences. Finally, the BOUM data alone wouldn't have allowed us to assess the capacity of our model to represent the main seasonal features and the DyFaMed dataset has a valuable role to play in this regard. Hence, although the aforementioned potential bias, we consider that the DyFaMed data could not be ignored for the present study.

Page 10, line 276. Why not to use the recently developed PHYSAT algorithm for the Med Sea to validate the P.F.Ts model output? Please see Navarro et al., (2014) RSE, 152: 557–575. The method provides accurately the distribution of the main PFTs in the basin, which could be well introduced in the model.

In the paper of Navarro et al. (2014), six phytoplankton groups are identified but in our model only two groups are represented, corresponding to small (including pico- + small nanophytoplankton) and large (including. large nano- and microphytoplankton). The comparison would have been possible if we could have the raw data in order to sum the contributions of several groups, but it is actually not possible with the data provided in the paper. It would however be interesting to obtain from the authors the set of raw data for future work.

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