

**Revised version of the paper untitled « New insights into the organic carbon export in the Mediterranean Sea from 3-D modeling » by A. Guyennon et al.**

The authors thank the referees for their extensive and useful reviews. This allowed to go further in the exploitation and the analyzis of the model results. The manuscript has been deeply reorganized and substantially rewritten so as to (i) better focus on POC and DOC stocks and export fluxes at the scale of the Mediterranean Basin, (ii) better highlight the new insights, and (iii) better explain the processes that drive the main results. Except some few points for which an argumented answer has been given (see later in the text), most of the changes asked for by the referees have been included in the revised version. In substance, the main changes can be summerized as follows :

- (i) the manuscript has been significantly shortened by condensing the description of the model skill assessment and by moving all this part in appendix,
- (ii) in the model skill assessment, new metrics have been used for some comparisons with data,
- (iii) the results on DOC and POC stocks and fluxes have been reorganized and partly rewritten and this section is now better structured,
- (iv) information on carbon pathways have been added, thus allowing a better understanding of the biological pump,
- (v) the discussion section has been strengthen by a better analyze of the processes involved in POC and DOC export, in the light of carbon pathways and intracellular quotas in phytoplankton and bacteria. The robustness of the results is also discussed in the revised version.

**Answers to the Anonymous Referee #1**

**General comment**

This manuscript is actually touching upon an important issue of the biogeochemical dynamics of the Mediterranean Sea. In terms of nutrient concentrations and ratios, the Mediterranean is classified oligotrophic, with large regions of apparently low surface biomass with a widespread but small seasonal bloom, in contrast with areas where intense production of organic matter occurs throughout the year and especially during wintertime. Hence, the role of particulate and dissolved organic matter are crucial for a proper understanding of the metabolic functioning of this basin.

The authors are indeed proposing one of the only possible methods to investigate this issue, which is the usage of coupled physical-biogeochemical models. However, the presented manuscript suffers from major flaws, which in my own judgement cannot be addressed by just a major revision of the current structure and do require a substantial rewriting. I do hope that the following comments will be taken into consideration for a future resubmission.

After a very careful examination of the present review, we could understand that the manuscript should be shortened, that comparison with data should be strengthened by the inclusion of new metrics, that the results should provide more information on the underlying processes of organic carbon (OC) export, and that the discussion should include some new aspects as for example the the robustness of the main results. We agree with all these suggestions and have taken them into account as described in what follows.

*1. This work appears to be more a demonstration of the model capabilities rather than a study of*

*the role of organic carbon in the Mediterranean. The modelling approach is actually not new as most of the previous biogeochemical models used in the Mediterranean basin (and cited by the authors) incorporate the same functionalities that they claim to be innovative (see detailed comment below).*

*The authors have barely looked at the existing literature on the modelling of organic matter dynamics in the Mediterranean (most of the work was done in in the Adriatic Sea (the search “DOC biogeochemical model Adriatic” would return most of the relevant literature, all published over the last 10 years). item The authors’ claims are not substantiated by the available observations or by findings that are robustly demonstrated with the aid of their own model. What are the new insights that they say their model is able to reveal? Their model returns a larger export of DOC with respect to POC at the basin scale, a feature that has been indeed observed by some authors (Santinelli et al. 2013, Lefevre et al. 1996 , see references in the manuscript). How much is this a unique feature of the Mediterranean Sea (for instance in contrast to other similar basins or ocean regions) and how much is it dependent on the model parameterizations? This is one of the first questions that the authors should have asked themselves or at least considered in the discussion.*

This is the first paper that investigates DOC and POC export in the Mediterranean sea at basin scale and this work can't be reduced to a demonstration of the model capabilities since after a thorough assessment of the model skill (on several variables of the biogeochemical model, at several space and time scales), quantitative fluxes of DOC and POC export have been presented and analyzed. The spatial distribution and the saisonnality of theses fluxes have been investigated and discussed, as well as the processes that were driving this export and all these features are better adressed in the revised version .

Our model indeed presents some unique functionalities that are not present in previous models, but we acknowledge that this was not sufficiently clearly explained in the manuscript. The most original one is that it includes a representation of each living organisms (i.e. functional type) in terms of an abundance (in cells/m<sup>3</sup> or ind/m<sup>3</sup> for mesozooplankton) in addition to that in terms of biomasses. This allows to calculate intracellular quotas (in mol X per cell where X = C, N, P) in addition to intracellular ratios (in mol X per mol Y where X and Y are chosen among C, N, P). Intracellular quotas provide a very important additional information since intracellular ratios are only indicative of the relative quantity of a given biomass as compared to another one. But for a given intracellular ratio, cells can either be depleted or repleted. By contrast, intracellular quotas give an additional information relative to cell status, that is if cells are rich or depleted in a given element. It also gives an indication of prey quality for predators. There are many other consequences in the model, as for example, the fact that the specific growth (i.e. the cell division rate for unicellulars or the recruitment for mesozoplankton) is distinguished (though coupled with) from the synthesis of new biomass. Therefore, this fonctionnality is very powerful and far from being anecdotal. Other characteristics of the model can be underlined: the mechanistic basis of most of the formulations of biogeochemical processes (some of them being the result of our own research, see Baklouti et al., 2006a), as well as the original way of model parametrization (a coherent set of parameters is used, based on relationships between some parameters (for example, maximum quotas are deducted from minimum ones as done in Thingstad (2005),...). All this has been better explained in the revised manuscript (see section 2.2 in the revised manuscript).

We acknowledge that, despite the attention paid to bibliography, we may have missed some of the dedicated literature. However, in the introduction, we aimed at only mentioning the previous modeling studies that either addressed the coupled-physical modeling at the scale of the whole

Mediterranean Basin or the carbon export in the Mediterranean Sea (even at regional scale), since these topics are the heart of the present paper.

Concerning the new insights provided by this work, they indeed exist (but we acknowledge that they were likely not sufficiently emphasized) and they can be summarized as follows:

- For the first time, a global vision of time and space DOC and POC stocks distribution has been presented at the scale of the Mediterranean Basin. The spatial and temporal patterns of these stocks have been analyzed through hydrodynamical and biological considerations.
- Moreover, in the revised version, the underlying processes associated with OC production and DOC accumulation have been better described in the light of intracellular quotas in phytoplankton and bacteria. The concept of « malfunctioning microbial loop » developed by Thingstad et al. (1997) for the eastern basin has therefore been generalized for the whole basin, with the exception of specific regions that have been identified.
- Again for the first time, the DOC and POC export fluxes at 100m and 200m at the scale of the Mediterranean Basin have been presented and a first quantitative estimation of the DOC/POC export ratio in western, eastern and in the whole basin has been provided
- The differences in POC and DOC stocks and export fluxes have been analyzed in the light of the differences in the processes and the environmental conditions involved in DOC and POC production and export
- In the revised version, new insights on the carbon pathways have been included, and the DOC inputs by rivers all around the Mediterranean Basin has been compared to the total amount of exported DOC.
- Apart from the quantitative aspects, this study also aimed at providing new directions for reflexion and we hope that this is more obvious in the revised version. Moreover, a few lines have also been included in the conclusion of the revised manuscript (see the end of the conclusion of the revised manuscript) about DOC export in similar oceanic regions.
- At last, the question on how the large DOC:POC export ratio is parameter-dependent is answered elsewhere in this document (see the specific comments), and a discussion on these aspects has now been included in the discussion section of the revised manuscript.

*2. The authors are discussing the role of dissolved and particulate carbon (DOC and POC) but these variables are only mentioned in the model description. The first time the DOC model variable is mentioned is at Page 6156 and related to the DOC input. The carbon pathways among the various PFT are not explained and, more importantly, they give little consideration on the quality (in terms of nutrient content) of the organic matter (also in the results and discussion). Indeed, labile and semi-labile organic matter is defined in term of the presence of nutrient- mediated chemical bounds.*

The model equations have already been extensively presented in another paper (Alekseenko et al., 2014) and we didn't want to lengthen the text with these already published features. However, we agree with the reviewer that we should have at least reminded the processes that drive DOC and POC production/consumption. This has been done in the revised manuscript as follows (see section 2.2 of the revised manuscript) :

*«The processes used in the model are extensively described in the aforementioned reference. However, for the needs of the present paper, we remind that POC is fueled by the natural mortality of largest organisms (mesozooplankton, diatoms and ciliates) and by the egestion of fecal pellets*

*and sloppy feeding by mesozooplankton, and consumed by POC hydrolysis to DOC. The DOC pool has many inputs (phytoplankton exudation, zooplankton excretion, mortality of small organisms, POC hydrolysis) and a single output (uptake by bacteria). »*

The second point mentioned by the reviewer concerns DOM stoichiometry. We indeed not present and discuss it since we didn't want to go beyond the scientific question addressed by the present paper (i.e. carbon export). There are indeed so many information that can be provided by such a model that a strong effort has been made to remain in the heart of the matter. This is why we did not treat this question at this stage.

*3. The manuscript is too long, with sections that go very much into details and some others that simply do not address the questions being raised (see the detailed comments below). I have the neat impression that this manuscript is an extract from a PhD thesis, which would actually explain its length and use of subjective sentences describing the quality of the simulation (e.g. overall agreement, well-represented, agrees well are generic terms that should be substantiated by objective indicators of quality). It needs to be thoroughly streamlined and restructured giving more emphasis to the problem being addressed. English also needs improvement because some sentences are rather difficult to be understood.*

We acknowledge that the manuscript is likely too long, and it has been significantly shortened, especially in the section concerning model skill assessment. Our objective was to give an extensive and sincere description of the model results since, in our mind, before addressing the question of DOC and POC export, it was absolutely necessary to assess model's ability in reproducing at least the order of magnitude and the main patterns and dynamics of all the compartments of the model (i.e. those for which data were available).

Concerning the comparison of the model outputs with data, at some places in the paper, we did indeed use classical sentences to qualify the quite good agreement that was emerging from quantitative comparisons. This is difficult to avoid when proceeding to the assessment of a model skill and this is generally done in other modelling papers, but some of these sentences have been removed. We however agree that we could have introduced some additional metrics for the comparisons with data and this has been done in the revised version (see the new RMSD columns in tables 1 and 2, the new figures 20, 23, 24 and 25, and the Taylor diagram (Fig. 26)).

Concerning the quality of the English used in the manuscript, we want to mention that an english professor (a native english people), familiar with this science field, has extensively corrected the manuscript (except sections 1 and 2, namely the introduction and the Material and Methods sections) before its first submission.

*4. I did appreciate the extensive assessment of the model quality against the available observations done in Sec. 3.1. However, it is too long and not well explained (for instance, the BOUM cruise is not really a descriptor of the basin scale spatial variability; it is more a snapshot of the summer spatial distribution; why the satellite data that could provided a sufficient comparison of shorter term variability are climatologically averaged to the seasons?). The authors should assess the quality of the simulation related to the aim of the paper, that is the DOC and POC dynamics. It is therefore more relevant that the model shows a resemblance to reality in the region where the DOC*

*and POC data are available. a table with a set of root mean square errors and some objective diagnostics as the ones proposed by Friedrichs et al. (2009); Vichi and Masina (2009); Doney et al. (2009) would have served much better than the long comparison of means and difficult-to-see colouring indices on the plots. I also wonder why the comparison with chlorophyll was only done using climatological fields (seasonal means) and not assessing the interannual variability. This should be done particularly for the regions where DOC and POC data are available.*

We did all these comparisons because we wanted to assess the model outputs in most locations and environmental conditions. Indeed, we do believe that results in terms of chlorophyll, nutrients or primary production have to be assessed before considering POC and DOC results, to insure that the biological production of organic carbon is correctly captured by the model. In other words, the objective for us was not just to obtain a good agreement with data for DOC and POC, since this could be obtained artificially or due to « bad reasons ». However, in the revised version, the model skill assessment has been shortened, completed with new primary production, DOC, and nutrients data, and moved in Appendix. Concerning the data we used for comparison, we agree that we should have better exploited the short term variability of satellite data and this has been done in the revised version in figure 20. BOUM cruise data provides additional variables (nutrients, ...) and mostly a vision of the vertical structure across the whole Mediterranean Sea. We believe that it is also an indicator of the spatial structure of the Mediterranean Sea (including the East-West gradient), especially for the deep concentrations. Finally, DyFaMed station is a provider of relatively high time frequency data and of long time series, and therefore represent another important source of data.

Comparisons with DOC data measured during the BOUM cruise have been added in the revised manuscript (see fig. 25 in Appendix A4). Concerning the detrital POC compartment, there are no data for comparison since the measured POC is always the total POC, i.e. including the living and the detrital POC, from which the detrital part only represents a very small proportion. Moreover, some metrics (RMSD, Taylor diagrams) as the ones used in the papers mentioned by the referee have been added for comparison, including the regions where DOC data were also available.

At last, as already said, we tried to focus on the heart of the matter (here DOC and POC export), and the aim was not to study in details the interannual variability of chlorophyll. This could be done in future work but for the present paper, the manuscript is already too long to explore this topic.

## **Detailed comments**

*P6149L10 Why the word basin is written with the uppercase first letter throughout the whole manuscript?*

In the revised manuscript, the uppercase has been used only for the Mediterranean Basin.

*P6150L4 The end of the carbon pathway? There is no end in a biogeochemical cycle!*

We acknowledge that this formulation is awkward and it has been removed.

*P6150L14 isotopics following?*

It has been replaced by isotopic measurements.

P6151L2 How can you design a model that is potentially efficient in every region? What does efficiency mean here?

« potentially efficient » has been replaced by « relevant » which is indeed much more appropriated.

P6151L13 This is false and rather disturbing to be found in a recent manuscript. It implies that the authors have a somehow limited knowledge of the state of their field. Stoichiometric models have been introduced since the '80s and a simple research on Google Scholar (for instance “variable stoichiometry plankton model”) would return the most significant literature. If the authors meant to refer to coupled physical-biogeochemical models, then the ERSEM model (Baretta et al., 1995) is more than 20 years old. If they meant to refer to applications to the Mediterranean Sea, then the majority of models applied after the first works by Crispi et al. and Crise et al (both 1998, see reference in the manuscript) have used variable stoichiometry because they all derived from ERSEM.

We totally agree with the reviewer that flexible stoichiometry models (such as ERSEM, BFM,...) are not new and have allowed to significantly improve biogeochemical modelling. The reference to these previous models has been explicitly added in the revised manuscript. However, as already said before, Eco3M-MED is the first biogeochemical model that handles cellular abundances thus allowing to calculate intracellular quotas (in mol X per cell where X is chosen among C, N, P, ...) in addition to intracellular ratios (in mol X per mol Y where X and Y are chosen among C, N, P, ...). The latter were indeed already provided since a long time by previous existing biogeochemical models with flexible stoichiometry. The importance of abundances and intracellular quotas has already been described previously in this document (see the answer to the general comment number 1). The sentence in the revised manuscript has therefore been modified as follows (section 2.2 of the revised manuscript).

« Every P.F.T. is represented in terms of several biomasses (C, N, P, and Chlorophyll for producers) and an abundance (cells per unit volume), except for meso-zooplankton which is only represented through its C biomass and its abundance (individuals per unit volume). If we denote X and Y two molecules among C, N, P and Chl, this allows to dynamically calculate for each P.F.T., not only intracellular ratios  $Q_{XY}$  which are the ratio between X and Y biomasses (as this is done in previous variable stoichiometry models such as ERSEM (Baretta et al., 1995) and BFM (Vichi et al., 2007), but intracellular quotas  $Q_X$  which are the X content per cell (expressed in mol X cell<sup>-1</sup>). »

Moreover, a reference to Thingstad (2005) model which uses intracellular quotas has also been added through the following lines :

« Intracellular quotas have already been used in a previous modelling study (Thingstad et al., 2005) but cell quotas of carbon were assumed fixed in the protozoa, while fixed C:N-ratios were assumed for bacteria and phytoplankton. Moreover, this model was used without being coupled with a physical model (i.e. for the simulation of microcosm and lagrangian experiments). »

P6154L18- Is the prey-switching formulation used for all zooplankton? Is this considered to be relevant for the Mediterranean food-web dynamics?

The prey-switching is indeed used for all zooplankton in Eco3M-MED. Considering that it has been evidenced in several studies that zooplankton, including micro and nano-zooplankton is multivorous (e.g. Gasol (1994), Legendre & Rassoulzadegan, 1999, Christaki et al., 2001), it seemed to us more



relevant to use a prey-switching formulation instead of arbitrary fixed preferences.

*P6154L24 What is the meaning of this sentence? Parameters are derived from other parameters? If this is a justification for not discussing the parameter choices, it is certainly vague.*

There are indeed many consistent relationships between parameters. For example, maximum intracellular quotas are 3.5 times minimum ones as done in Thingstad (2005). Another example is given by maximum uptake rates  $V_{max}$  that are set equal to the maximum specific growth rate times the maximum cellular quotas  $Q_{max}$ , etc...

These parameters have already presented in detail in Alekseenko et al. (2014) and that's why they are not discussed again here accounting for the fact that the paper is already quite long. The following sentence has been written to increase the clarity of this sentence :

*«Since the model relies on mechanistic basis, parameters are mainly physiological (and measurable) and they were either taken from literature or derived from other parameters on the basis of greater consistency between parameters. For example, maximum intracellular quotas are inferred from minimum ones as done in Thingstad et al. (2005). Another example lies in the relationship between the maximum uptake rate of a given element which is the product of the maximum specific growth rate and the maximum intracellular quota in that element. Other examples as well as the whole set of parameters are given in Alekseenko et al. (2014). »*

*P6155L24- Define “imprecisions”. How can the phosphate measurements be imprecise and at the same time provide a usable N:P ratio?*

In the Atlantic waters west of Gibraltar strait, the deep  $NO_3 : PO_4$  ratio is around 16 or even lower. Moreover, the  $NO_3$  and  $PO_4$  data provided by the WOA database in this region give  $NO_3:PO_4$  ratios that are abnormally high near the surface. To overcome this problem, we decided to use the  $NO_3$  data from the WOA database since they seemed us more reliable ( $PO_4$  values near the surface are very close to the detection limit and therefore less reliable), and to infer  $PO_4$  concentrations from the latter using a ratio of 16.

*P6156L12-17 The role of land-derived DOC is mentioned here and never discussed. Is it an important source of organic carbon to the basin. How is it compared to the export? Why is it all considered DOC and not DOM?*

In the revised version, the annual input of land-derived DOC has been compared to the annual export at 100 m and this revealed that river inputs don't exceed 5 % of the mDOC annual export (see section 3.1.3 of the revised manuscript).

Concerning DOM, as already said, we wanted to remain focused on our topic, namely the investigation of the biological pump, and more specifically DOC and POC export. The study of DOM composition is a full topic in itself and would have required significant lengthening of the text which was already very long. Moreover, since the model only includes the labile fractions of DON and DOP (contrarily to DOC for which the semi-labile fraction is also a state variable of the model), we have considered that they were negligible in land inputs.

*P6156L26- The spin-up strategy is not completely clear to me. Why are the authors adjusting to the atmospheric forcing of the '70s and then shifting to the '90s with an additional spin-up? It cannot*

*be because of the deep water spin-up as they are adjusting to a pre Eastern Mediterranean Transient period when the waters were in a completely different state and then simulating a post-transient period. I don't get it.*

The initial conditions for biogeochemical state variables were taken from a climatology of the Medatlas database (including recent and less recent data). The main priority was therefore to obtain an adjustment of the biogeochemical variables to the hydrodynamical and hydrological characteristics of water. For this, we choose a period (1973-1977) of relative stability of the hydrodynamical and hydrological properties, that means, sufficiently far from the initial conditions of the physical run (which started in 1958), but also far from the beginning of the EMT period (1991). Due to high computational costs, we could not let this simulation run until the year 2012. We therefore choose to start a second simulation from the year 1996, using the consistent conditions delivered by the first simulation as initial conditions for the second one. More importantly, it has been verified that beyond the year 1998, the stability was ensured (no drift on state variables). In the revised manuscript, all this has been better explained (see section 2.5).

*P6158L15 Do you mean using the same dates of the cruise data? This is not much clear because in most of the analyses the authors used a climatology. You should also make clear that this assessment allows to appreciate the quality of the simulation during the stratified summer period.*

For comparison with BOUM cruise data, the model outputs provided at the same dates as the cruise period and averaged over this period have been used. This has been clarified in the revised version with the following sentence (section 2.6.2) :

*«Measurements of nutrients and DOC concentrations were used to perform a Basin-scale comparison during the summer stratified period with the model outputs obtained at the same dates as the cruise, and averaged over this period. »*

*P6159Sec3.1 The authors never discuss how representative the BOUM data are and how likely is that the model capture the proper physical conditions. There is a generic comment at the end of the paragraph that is not very clear.*

In the beginning of old sec 3.1 (which is now Appendix A in the revised manuscript), the following sentence has been added : *« Data collected during the BOUM cruise allow to appreciate the quality of the simulation during the stratified summer period. »*. Moreover, the generic comment has been clarified as follows : *« Finally, some discrepancies between model and observations are attributable to the mislocation of the anti-cyclonic eddies, but this failure of the hydrodynamical model has only a local impact on modeled nutrients .»* . Concerning how the model capture the physical conditions during BOUM cruise, looking at salinity and temperature vertical profiles, we didn't find any convincing argument that could explain the differences that were observed in biogeochemical data, and this is the reason why we didn't mention this.

*P6162L3 Why using the RMSD here? This is a typical measure for goodness-of-fit and not to consider patchiness and spatial variability. The RMSD should have been done with the observations and a spatial standard deviation would have been sufficient to assess the impact of binning and averaging (e.g. Smith and Rose, 1995).*



We agree with this comment and have replaced the RMSD by spatial standard deviation (see table 2 in the revised manuscript).

P6165L9-20 This paragraph belongs more to the discussion rather than the presentation of results. We agree with the reviewer. However, in the revised manuscript, the model skill is now discussed in the model skill assessment section (which has been moved in Appendix), and now the comparisons between the model outputs and data are discussed throughout this section.

P6165L22 How can we appreciate that the “timing” is correct with seasonal means? A time-series extracted from the region of interest would have helped.

We acknowledge that this was not relevant and figures 7 and 8 of the manuscript have been replaced in the revised manuscript by two figures:

- the first figure (figure 19 in the revised manuscript) aims at assessing chl spatial variability through the comparison of mean annual chl with the one provided by satellite data,
- the second figure (figure 20 in the revised manuscript) aims at investigating and assessing chl variability and includes the maps from the old figure 8 as well as time-series extracted from several regions of the Mediterranean Sea

P6166L1 I am not very convinced of this indicator. What happens if the maximum is an extreme value? Did you use the maximum of the monthly means for both model and data? The coefficient of variation is usually the best measure for variability or the normalized difference between maxima and minima.

This indicator seems relevant and robust to us since it is applied to a climatology of chlorophyll outputs, that means that extreme values have already been smoothed. Moreover, accounting for the fact that the chlorophyll time distribution does not follow a normal law, the mean and the standard deviation are not sufficiently representative of the distribution. That's why we choose the median for our calculations. This choice has been better explained in the revised manuscript through the following sentence :

*« Since chlorophyll time distribution does not follow a normal law, this indicator is likely more relevant than the mean and the standard deviation. Moreover, since applied to a climatology of chlorophyll outputs, extreme values have already been smoothed. »*

P6168L16 oIPP is used in place of IPP. I understood that the suffixes indicated observations when they were compared with the corresponding model variable, not when discussing primary production between observations.

We agree with the reviewer and this has been corrected.

P6169L2 At the very end of a long paragraph one learns that everything is summarized in Table 3. There is no need to comment all numbers that are given in a Table, but only to highlight the features that are needed for the aim of the paper. This makes the manuscript more difficult to read and cumbersome.

As suggested by the reviewer, the text has been shortened so as to only retaining the main

interesting features. Among the changes operated, a new line has been added to table 3 in order to include the different estimations of IPP at basin scale.

P6169L25 I think that saying “very similar” is definitely overstating. The comparison is not correct because maxima and minima may not come from the same year. This was the typical way of comparing model and data about 10-15 years ago. The authors should consider to compare the empirical probability density functions from the two datasets. Since their simulation is starting from 1998, I would also suggest to start from that period either, so at least 2 years overlap.

New IPP data from the DyFaMed station have been recently obtained. Now, oIPP and mIPP come from the same years and the comparison is shown in figure 23 of the revised manuscript.

Sec3.2 This should be the central focus of the manuscript. However this description is kind of dull and unfocused. It is not clear if it is a comparison or a description of the model features. The whole section should be restructured following a clearer stream of thoughts. The model is first integrated down to 100 m (why?) and then the comparison is carried out at DyFaMed site (grid point?) only with the profiles.

Spring is described before winter and it is not clear at all what are the characteristics of the data (they are referred as climatologies but I do see the individual profiles in the figure). Fig. 13 is the best figure of the paper and it should have been given a central role. It does show some interesting discrepancies in the vertical distribution of data and model but they are not discussed at all in the light of the model functionalities.

Section 2.3 has been thoroughly re-organized in order to improve the clarity and the coherence of the manuscript. First, the comparison with available DOC data have been included in the model skill assesment section (Appendix A4 in the revised manuscript). The results section is indeed now only focused on model outputs at basin scale, including DOC and POC stocks as well as DOC and POC fluxes. Concerning the old figure 13 (figure 24 in the revised manuscript) showing the comparison between DOC stocks provided by the model and measured at DyFaMed, data have been gathered so as to provide mean profiles instead of several profiles. Mean absolute bias between mDOC and oDOC have been plotted. 0-100 m vertically integrated DOC stocks have also been calculated and plotted for both data and model outputs (see Figure 24). Moreover, the discrepancies in the vertical distributions between mDOC and oDOC are now better discussed in the lines of the model functionalities (see section A4 in Appendix). Finally, a comparison with oDOC data from the BOUM cruise has also been plotted (figure 25) and discussed.

Sec3.2.3 This section should come after the assessment of the model quality for DOC and POC distribution. Only a numerical model can provide the basin-scale fluxes and they are composed of both the physical and biological components, therefore the quality of both should be considered. It is not simple to disentangle the role of production and transport processes, because advection and diffusion are driven by vertical gradients. This section is a rather long description of what the model looks like without giving insights on why the model does it and what kind of processes drive it. It would be fine if the discussion was directly linked to this section, but there are no direct cross-references.

Now that DOC and POC assesment have been included in the model skill assesment section in Appendix, the conclusions relative to this assesment are considered to be known before the

presentation on DOC and POC export. Moreover, the description of DOC and POC fluxes has been shortened to only focus on the main interesting results (see section 3.1.3 of the revised manuscript). Finally, the discussion now includes much more details on the processes that drive the export (see the paragraph entitled «*POC and DOC export are characterized by different processes and timing* » in the discussion section of the revised manuscript. Furthermore, the cross-references between both sections are now much more visible.

P6174L14 Why DOC fluxes are larger in winter while primary production is larger in spring? I think the authors should focus more on the relationship between bacteria carbon demand and nutrient availability as for instance done in Polimene et al. (2007). The sentence at lines 20-26 is rather obscure and should be better explained.

As this is better explained in the revised manuscript (see the paragraph «POC and DOC exports are characterized by different processes and timing » of the discussion section in the revised version), DOC export is not correlated to primary production (except in some very specific situations), but is rather the result of the mixing of the water column (that occurs in winter) after the summer and autumn stratification periods during which DOC has accumulated in the surface layer. Moreover, the sentence at line 20-26 has been removed. Instead, more details on intracellular quotas and on DOC exudation by phytoplankton have been included (see the new sections 3.2 and 3.3 in the Results section of the revised manuscript) in order to better focus on the processes driving DOC accumulation.

P6175L1-7 This is partly related to the comment above. The authors seem to imply that bacteria are carbon-limited in the Mediterranean. However, I wonder if there is any evidence that this is likely given the extreme P-limitation of the basin and the higher P:C and N:C ratios of bacteria (Goldman et al., 1987).

We agree with the fact that bacteria are mainly P-limited in the upper layers of the Mediterranean Sea. However, in this section, we were not talking about the surface layer but about the 100 m depth region where bacteria may be C-limited at some periods of the year. However, figure 17 has been removed from the revised version and more relevant figures on intracellular quotas have been included in the place of this figure (see figures 9 to 12 in the revised manuscript). Moreover, intracellular quotas are now better used for the purpose of the discussion.

P6176L21-26 The quality of the physical simulation was not discussed anywhere so the authors cannot draw any inference.

It was not the aim of this paper to describe the quality of the physical simulation. We rather choose to mention some of the main failures of this physical simulation throughout the text when this was relevant for a given analysis of the model results.

P6177L1-3 I believe that in the clear Mediterranean waters the satellite optical depth is deeper than 10 m so I do not understand this argument.

We agree with the reviewer about the optical depth. However, since the Deep Chlorophyll Maximum (DCM) is shallower in the model than in data, the 0-10m integration includes part of the high DCM Chl concentrations while this is likely not the case in satellite data. This has been better explained in the revised manuscript through the following sentence :

« *Furthermore, surface chlorophyll in the model is estimated as the mean over the first 10 m of the water column, and therefore includes part of the chlorophyll gradient towards the Deep*

*Chlorophyll Maximum (DCM) which is shallower than the observed one in the Eastern Basin during the stratification period (results not shown though the same bias is observed at the DyFaMed site, see Appendix (A2.3)). »*

P6177L3-5 Please add a reference to this statement on the phytoplankton community structure. The reference Siokou-Frangou et al. (2010) has been added.

P6177L9-10 Do the authors question their initial conditions?

We do question here the initial conditions since in deep waters, there are significant differences between the nutrient concentrations provided by the Medatlas climatology and by the BOUM measurements. As a consequence, and due to the stability of nutrient concentrations in deep water during the simulation, the same disparities can be observed between the model outputs and the BOUM cruise data. The sentence has been clarified as follows :

*« There are indeed significant differences between the nutrient concentrations in deep waters provided by the Medatlas climatology and by the BOUM measurements. As a consequence, and due to the stability of nutrient concentrations in deep water during the simulation, the same disparities can be observed between the model outputs and the BOUM cruise data.*

P6177L15-29 Is this paragraph implying that POC flux is not to be trusted?

In the paragraph the referee refers to, we are not talking about POC export fluxes at 100m but rather about the attenuation between 100 and 200m. It is explained that this attenuation is likely too great and that this can be attributed to an overestimated POC-DOC hydrolysis rate (or to a too weak sinking rate which is equivalent since this would produce the same effects). All this has been better explained in the new paragraph entitled «Discussion on results robustness» in the discussion section of the revised manuscript.

P6178L1-5 This statement is not backed-up by sufficient supporting evidences. I do not see the unique insight because any previous model application could have produced fluxes of DOC and POC but they did not. The scientific issue is how reliable they are.

The term «first» should have been used instead of «unique». Moreover, this term was relative to the Mediterranean scale since previous work has already been done at regional scales. However, this sentence has been removed from the discussion section which has been almost entirely rewritten. Moreover, the reliability of our results is now discussed in the revised manuscript (§ «Discussion on results robustness»).

P6178L13-14 Is this predominance of DOC fluxes a consequence of the parameterization or a specific feature of the Mediterranean? The authors should demonstrate that this is robust to model choices and uncertainties.

The model shows a predominance of the DOC fraction in the annual OC export in most of the mediterranean regions. Due to high computational costs it is unfortunately impossible to test several values of the sinking rate and/or the POC to DOC hydrolysis rates. However, the discussion section now includes some considerations on these parameters and their impact on the model results. Moreover, it has been shown that values of the mean 0-100~m mDOC are very close to the

measured one, and that the method used for in situ estimations of DOC export systematically underestimate the actual DOC export (this has been verified by applying the in situ method to the model outputs). Moreover, the mPOC export fluxes are in the range or, at least in the same order of magnitude in the eastern basin, as the oPOC ones. All this led us to the conclusion that :

*« Finally, it is very unlikely that the aforementioned uncertainties could put in question the predominance of DOC in the OC export in the Eastern Basin. This conclusion also applies in the Western Basin (though with less certainty), all the more that in situ measurements allow to draw the same conclusion (Copin-Montégut et al. (1993), Avril (2002), Miquel et al. (2011)) ».*

P6179L1-5 The model simulation is interannual and covers the periods of the data. Why are the authors not comparing with the corresponding model data? Use the model to fill the data gaps and infer specific processes.

Accounting for the uncertainties associated not only with model outputs but in situ measurements, comparisons at the same date, it seems to us that comparisons at the same dates are not really necessary. Instead, we aim at comparing the order of magnitude and the range of values provided by measured and modeled values. However, more fluxes comparisons have been added in the revised manuscript (see the first § in the Discussion section).

P6179L10 Slightly? I would say twice!

We acknowledge that the term slightly is not optimal in this case and it has been replaced by « in the same order of magnitude ».

P6179L14-16 Which measurements? Give references. Also below, when mentioning “in situ estimations”. There are too many generic sentences that sound very anecdotal.

The following sentences « When compared with the few available measurements of DOC export from the DyFaMed station, the Adriatic Sea, and the Tyrrhenian Sea, the model always provides higher DOC export values. These differences in DOC export may be partly attributable to model failures, but, as already mentioned, high uncertainties are also associated with in situ estimations. »

have been replaced by :

*« Furthermore, when compared to in situ estimations of DOC export from the DyFaMed station (Avril, 2002), and the Adriatic and the Tyrrhenian seas (Santinelli et al., 2013), the model always provides higher DOC export values. »*

P6179L24-25 I am confused here. The authors have discussed the inconsistencies and now they say it is consistent.

We did not discuss the inconsistency between modeled export fluxes and in-situ estimations. Our discussion rather concluded that, when compared to the few available in-situ estimations, and accounting for the large uncertainties associated with the latter, we could conclude to a quite good coherence between model outputs and data. However, due to the fact that the discussion section has

been almost entirely rewritten, this sentence has been removed from the revised manuscript.

P6180L7 How representative is DyFaMed of the whole Mediterranean?

DyFaMed is the only station at which several long time series are available and we didn't have any other estimations of this kind of DOC export (namely including the export all over the year and not only during winter).

P6180L12-14 This discussion comes out of the blue and it was not presented in the result section.

The referee refers to the following sentence: « Finally, a strong correlation between annual primary production and POC export can be evidenced (spearman's rank correlation coefficient is 0.84), while this is not the case for DOC export (correlation below 0.01). » This is not properly a result but rather a calculation that has been done for the purpose of discussion. However, in the revised version, this sentence is better integrated in the text :

*« Thus, higher concentrations of large organisms in the Western Basin, primarily due to the spring bloom in the liguro-provencal region associated with high primary production rates is the main reason for the higher POC production and export in this basin. Hence, POC export is maximum in spring (i.e. from March to May in figure 7) since it is the period including the maximum and the end of the bloom during which detrital concentrations of large organisms are the highest. Moreover, according to the model, mortality is the main process that fuels the POC pool, far ahead of the egestion and sloppy feeding process. More generally, a strong correlation between annual primary production and POC export has been evidenced at basin scale (spearman's rank correlation coefficient is 0.84), while this is not the case for DOC export (correlation below 0.01). »*

## **References :**

Gasol, J.M. (1994) A framework for the assessment of top-down vs bottom-up control of heterotrophic nanoflagellate abundance. Mar. Ecol. Prog. Ser. 113, 291–300.

Legendre, L. and F. Rassoulzadegan (1999). Stable versus unstable planktonic food webs in oceans. Proceedings of the 8th International Symposium on Microbial Ecology. pp.1-7

Christaki, U., Giannakourou, A., Van Wambeke, F. and Grégori, G. (2001). Nanoflagellate predation on auto- and heterotrophic picoplankton in the oligotrophic Mediterranean Sea. J. of Plank. Res. 23(11). 1297-1310



## Answers to the Anonymous Referee #2

### General comments

The paper tries to describe and understand the dynamics of DOC and POC in the Mediterranean Sea. It fails to do so since it does not analyze the carbon pathways in the food web and does not explain how this carbon export is produced by microbial and/or higher trophic components of the model.

We agree with the reviewer that these aspects were lacking for a full comprehension of the biological pump, and several information on carbon pathways have been added in the revised manuscript. Moreover, the processes associated with OC production are also better described (see the new Discussion section in the revised manuscript).

It is too long of a paper, with many details and comparisons that do not shed light in the processes under investigation. Especially the comparison with chlorophyll shows much larger productivity than in the satellite data and this is not commented properly. In addition this larger primary productivity will enhance the DOC pool and there is no cautionary statement about this, especially for the Eastern Mediterranean where the malfunctioning bacterial pump will allow the DOC to accumulate if the primary production is large.

We agree with the referee that the section relative to the model skill assessment, especially the one dedicated for chlorophyll, is too long, and it has been significantly shortened and moved to the Appendix section. Concerning the overestimation of the chlorophyll pool by the model, this does not directly impact nor the DOC pool, neither DOC export fluxes. More importantly, it can be seen (and this has been better shown in the revised version), that primary production rates are in the range of the measured data, and this is particularly important since, in the model, DOC production by autotrophs is controlled by primary production rates and by the intracellular quotas in algae. In substance, feedback regulation by algal nutritional state affects the primary production rate PP through the different intracellular quotas. However, this regulation does not directly affect light utilization by light-harvesting pigments, and the pool of DOC can be saturated with newly synthesized organic compounds when photosynthesis takes place more rapidly than is required to supply the needs of growth. When this situation occurs (for example when nutrients are depleted), DOC excess is released in the medium. This has been clarified (see the paragraphs entitled « *POC and DOC exports are characterized by different processes and timing* » and « *DOC accumulation in the light of intracellular quotas* » in the Discussion section of the revised manuscript).

The authors start to talk about the POC and DOC pools at page 21, and actually the discussion of the ratio starts at page 30. This is too far from the main aim of the paper, as stated in the introduction.

We agree with the reviewer and in the revised version, the model skill assessment has been moved in appendix and results on DOC and POC at the scale of the Mediterranean sea are now the first that are presented.

I believe the paper should be totally re-written and shortened with the addition of the study of the carbon flux pathways. If the POC/DOC small ratio means that the Mediterranean is dominated by the microbial food web processes, this should be spelt out clearly from the beginning. I do not believe this is a ground breaking result for the Mediterranean Sea but it could be worth to show given the full Mediterranean scale of the study.

The paper has been shortened and substantially rewritten in order to better analyze and discuss the results provided by this study. The role of the microbial loop is better described, as well as the major role of DOC exudation (as compared to the other sources of DOC production). The small POC/DOC ratio can't be reduced to the fact that the Mediterranean is dominated by the microbial food web processes since this is also observed in the bloom period in the Liguro-Provençal region during which large organisms are abundant. Instead, it is the strong P-limitation (which is also observed soon after the beginning of the bloom) that infers large DOC exudation fluxes by phytoplankton (since organic carbon production through photosynthesis is not controlled by P availability) beyond the carbon needs of bacteria which are also strongly P-limited.

### **Few detailed comments**

Line 152. Chosen value of 2 m d<sup>-1</sup> is not credible and this will favor the production of large pools of DOC. Larger values up to 200 m d<sup>-1</sup> were found from sediment traps in the Ionian Sea (Patara et al. Biogeosciences, 6, 333-348, 2009).

Since the model only includes a single detrital pool, we couldn't use extreme sinking values such as 200 m/day (this would have resulted in a large overestimation of POC export since the detrital carbon wouldn't have been hydrolyzed at all in the water column). However, we acknowledge that the value of 2m/d is likely too weak, though this is difficult to verify. In the revised version, a whole paragraph is dedicated for a discussion on results robustness (see section 4, last paragraph).

Line 191. The choice of initializing from an initial condition corresponding to 20 years before the 1996 is not discussed nor justified.

The same answer as the one for the referee 1 can be given here :

The initial conditions for biogeochemical state variables were taken from a climatology of the Medatlas database (including recent and less recent data). The main priority was therefore to obtain an adjustment of the biogeochemical variables to the hydrodynamical and hydrological characteristics of water. For this, we choose a period (1973-1977) of relative stability of the hydrodynamical and hydrological properties, that means, sufficiently far from the initial conditions of the physical run (which started in 1958), but also far from the beginning of the EMT period (1991). Due to high computational costs, we could not let this simulation run until the year 2012. We therefore choose to start a second simulation from the year 1996, using the consistent conditions delivered by the first simulation as initial conditions for the second one. More importantly, it has been verified that beyond the year 1998, the stability was ensured (no drift on state variables). In the revised manuscript, all this has been better explained (see section 2.5 of the revised manuscript).

Line 280-281. Mislocation of anticyclonic eddies is given as the reason for the difference in nutrient pools between model and data, this should be demonstrated by the analysis of model outputs.

Some other explanations have been put forward to explain the difference in nutrient pools between

model and data, namely the rough representation of the nitrification process in the biogeochemical model, the underestimation of the initial deep concentrations, and the depth of the mixed layer. The mislocation of anticyclonic eddies has only a local impact on the model results (this has been precised in the revised manuscript, see the end of section A1.1 in the revised manuscript) and a detailed analysis of this failure of the hydrodynamic model didn't seem us relevant for the present paper.

Line 385. Authors mention that there is agreement between spatial patterns of chlorophyll concentration between model and satellite obs. This is really difficult to accept! Values are 5 to 10 times larger in the model than observations.

In this sentence, we were talking about spatial patterns (location of the maximum and minimum, gradients, etc...) and not about absolute values. The type of comparison was not inded the best way to asses the actual discrepencies, but in any case it can't be said that Chl modelled values are always 5 to 10 times larger. This is better shown in the revised manuscript through figures 19 and 20 which now explicitey show the spatial and temporal discrepancies between the modeled and measured Chl.

Line 707. The main result of the paper is not explained. Authors say that:” One of the main results of this study is that DOC export exceeds POC export in the whole Mediterranean Basin” But they never show the food web structure that creates such POC/DOC flux ratio.

In the revised version, some details on the role of different compartments of the food web on the high DOC/POC flux ratios, as well as the different fluxes that fuel DOC and POC pools have been presented and analyzed (see the Discussion section).

Line 804 Again the authors say that POC and DOC export is attributable to the differences between the processes involved but they do not analyze them.

Some elements of analysis were given in the previous version of the manuscript where it was explained that POC export was mainly correlated to primary production (lines 12-15, p. 6180 of the submitted manuscript) while DOC export was mainly driven by DOC accumulation when bacteria and phytoplankton are strongly P-limited, followed by winter vertical mixing (end of p 6180 and beginning of 6182 of the submitted manuscript). However, in the revised version, this has been further detailed and discussed (see the sub-sections entitled «*POC and DOC exports are characterized by different processes and timing*» and «*DOC accumulation in the light of intracellular quotas*» in the discussion section of the revised manuscript).