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Interactive comment on "New insights into the organic carbon export in the Mediterranean Sea from 3-D modeling" *by* A. Guyennon et al.

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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.

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.
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 Pag 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.

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.

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.

Detailed comments

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

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

P6150L14 isotopics following?

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

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, than 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.

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

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.

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

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?

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.

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.

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.

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).

P6165L9-20 This paragraph belongs more to the discussion rather than the presentation of results.

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.

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.

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.

P6169L2 At the very end of a long paragraph one learn 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.

P6169L25 I thing 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.

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.

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.

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.

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).

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

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.

P6177L3-5 Please add a reference to this statement on the phytoplankton community structure.

P6177L9-10 Do the authors question their initial conditions?

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

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.

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.

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.

P6179L10 Slightly? I would say twice!

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

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

P6180L7 How representative is DyFaMed of the whole Mediterranean?

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

References

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