Review of the paper **New insights into the organic carbon export in the Mediterranean Sea from 3D modeling** by Guyennon et al., submitted to Biogeosciences for possible publication.

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.

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.

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.

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.