Dear editor,

Thank you very much for your recommendations that allowed us to further improve this manuscript. Detailed answers are given in what follows. English language has been improved too through an additional correction by a different English language expert and we sincerely hope that it has now reached an acceptable level. As for the previous revision, the joined file encompasses the revised article as well as a second version in which all the changes that have been undertaken are apparent.

A) The quota based approach needs a better description. What is the observational basis for it? How is it implemented in the model? What is the benefit of it? What can this approach do that other methods cannot? Please, put yourself into the position of a reader who is not familiar with the topic, and take your description from there.

To answer all these questions, the following paragraph has been added in the Material & Methods section:

*Each P.F.T. of the model is represented through several state variables, namely C, N, P (and Chl for producers) concentrations and a cell number (i.e. an abundance), except for mesozooplankton which is only represented through its C concentration and its abundance (in individuals per unit volume). Intracellular ratios (i.e. the ratio between two elemental concentrations) as well as intracellular quotas (i.e. the quantity of a given element per cell) can therefore be calculated dynamically by the model.*
*Intracellular ratios are indicators of plankton stoichiometry, i.e. of its C:N:P elemental composition. Early biogeochemical models (NPZD models) have considered a constant C:N:P ratio in plankton given by the canonical Refield ratio of 106:16:1 (Redfield, 1958). Based on Droop's work (e.g. Droop, 1968, 1975), an increasing number of biogeochemical models (e.g. Baretta et al., 1995; Geider et al., 1998) have in recent decades assumed flexible plankton stoichiometry. Though Droop's original quota function relating growth rate to the intracellular quota of the limiting element was based on cell quotas, these biogeochemical models have used intracellular ratios instead of quotas to regulate the rate of biomass synthesis (and other process rates) with quota functions similar to that of Droop. These flexible stoichiometry models have been widely used in the framework of theoretical batch or chemostat studies (e.g. Geider et al., 1998; Baklouti et al., 2006b) or for large-scale studies with ERSEM (Baretta et al., 1995), BFM (Vichi et al., 2007) or others (e.g. Moore et al., 2002) models. In such models, substrate uptake and biomass synthesis are decoupled, but cell division is not explicitly represented.*
*Intracellular quotas (or cell quotas) as they are defined in the present paper are indicators of the C, N and P cellular content of plankton. They are an original feature of the Eco3M-MED model in the category of 3D coupled physical-biogeochemical models. This model is based on the assumption that there are a minimum ($Q_{min}^X$ ) and a maximum ($Q_{max}^X$ ) intracellular content for each element X among (C, N, P). $Q_{min}^X$ can be interpreted as the amount of element X used in cellular structure and machinery, and the accumulated surplus as storage for future growth (Klausmeier et al., 2008). The variability in cell quotas has indeed been widely evidenced through several experimental and in situ studies (e.g. Brown and Harris, 1978; Fukuda et al., 1998; Lovdal et al., 2008; Heldal et al., 2003; Bertilsson et al., 2003; Wilhelm et al., 2013).*
*The use of cell numbers as state variables and of the associated intracellular quotas offers severaladvantages: firstly, it makes it possible to distinguish between cell division (which is described by aspecific equation, see Eq. 1), biomass synthesis, and uptake. Second, intracellular quotas are indicative of the actual internal status of cells, i.e. they indicate whether cells are rich or depleted in a given element, while intracellular ratios only provide relative values. In other words, a given value of intracellular ratio $Q^{XY}$ can correspond to several different cell statuses (for example, a given C:N ratio can be obtained with an infinity of pairs of C and N intracellular concentration values). Thus, intracellular ratios can only provide information on the internal relative quantity of X as compared to*

*that of Y,, while intracellular quotas inform on intracellular absolute quantities. The latter information is very useful for the analysis of plankton dynamics since it is informative about the nutritional statusof each P.F.T. of the trophic web (see the Discussion section). It is also a good proxy of the quality of the prey available for zooplankton (i.e. whether prey are rich or depleted in a given element). Thirdly, the parameters determined at cell level can be used without using conversion factors. For example, uptake rate measured at cell level (Talarmin et al., 2011), or grazing parameters expressed in number of prey per predator per unit time, such as the ones provided in Christaki et al. (2009) for HNF and ciliates can be used directly.*
*Intracellular quotas have already been used in previous modeling studies to study phytoplankton growth (Klausmeier et al., 2004) or the dynamics of the planktonic food web (Thingstad et al., 2005). In the latter study, however, cell quotas of carbon were assumed to be 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 lagrangianexperiments).*

B) The manuscript needs a comprehensive English language overhaul. Though you addressed this issue in response to referee#1, I think there are many awkward and difficult to understand phrases in this manuscript (some examples below). Please, let it be corrected by a different English language expert than before.

The manuscript has been again corrected by a different English language expert (see the changes through the blue/red colors in the second version where changes are apparent).

1. Abstract:
“We here propose a Mediterranean basin-scale view of the export of organic carbon, under its dissolved and particulate forms.”
This sentence does not really make sense to me. What information do you want to convey to the reader here?

We agree with the editor that this sentence was not clear and it has been removed. Furthermore, the abstract has been substantially improved in this new version.

2. P 2 , L 48-50.
“The pathway of organic carbon not only allows to estimate the total amount of fixed carbon, but it is also crucial to determining biological pump efficiency. Modeling was chosen to adress this question, taking into account the high heterogeneity of situations encountered in the Mediterranean Sea.”
It is not clear what question you address here.

Again, we agree with the editor that this sentence is not clear and it has been removed. Moreover, the end of the Introduction section has also been substantially reworked.

3. P 5, L 113:
“…several biomasses…” sounds awkward. Perhaps the term *different biomass types* or *biomass categories* would be better?

We agree that it is somehow awkward to mention several biomasses. This term has been replaced by concentrations or intracellular concentrations.

4. P 5, L 115 and following:
The sentence starting with “If we denote…” is incomprehensible; please rephrase.

This sentence has been removed. Instead, a new paragraph including much more details on the quota based approach has been inserted (see section A of this answer).

5. P 7, L 185-190:

The 2 m $d^{-1}$ sinking velocity for POC needs to be better justified as requested also by referee#2. It should be discussed in terms of the degradation rates for particulate matter chosen. In principle, unrealistic sinking velocities can be compensated by unrealistic degradation rate constants in biogeochemical models.

In the Discussion section (subsection "Discussion on results robustness") of the previous revision, we indeed tried to argue that the underestimated sinking rate (at least as concerns largest particles) was likely compensated by the low degradation rate used in the model. This part of the manuscript has been substantially rewritten and better formulated in the new revised version (see section 4.4, lines 639-649).

6. In the discussion chapter 4, the subsection headings need numbering (4.1, 4.2, and so on).
This has been done for the new revised version.

The subsection heading "Discussion on results robustness" sounds strange. Better would be *Robustness of results*.
The subsection heading has been replaced by « Robustness of results ».

We look forward to receiving a further revised version. Please, make sure that there will be a step change relative to the present version concerning the tangibility of the text and standard of the English language.

We sincerely hope that this new revised version will meet the editor expectations and we look forward to hearing from you.

Best regards.

Melika Baklouti (the new corresponding author since the last revised version) on behalf of all coauthors.