

Interactive comment on “Plankton in the open Mediterranean Sea: a review” by I. Siokou-Frangou et al.

Anonymous Referee #1

Missing entirely is any mention of small planktivorous fish which would have added value to the article.

There is a reference on the effectiveness of the Mediterranean food web functioning towards fish in the relevant section, which assumes a link between Mediterranean plankton and the small planktivorous fish. This link will be clarified and enriched with information in the revised manuscript distilling from current literature.

Section 1 Introduction

First paragraph of the Introduction gives no information and is poorly written- should be deleted. Giving credit where it is due- the introduction could give a short history of the major oceanographic studies which provided much of the data described. This would clearly show what was studied and when.

We are quite surprised by such a strong statement by the referee. Certainly we will do our best to improve the clarity of the brief synthesis in the first paragraph of the introduction, which still sketches, in our opinion, some crucial and peculiar features of the Mediterranean Sea. In particular: it is a quasi-enclosed basin which implies that local inputs dominate over the exchange with the ocean (first sentence in the paragraph); the water budget coupled with the Gibraltar constraint forces the basin salinity up to a value 3 psu higher than the global ocean, making salinity the main driver of density fields and thus ruling the thermohaline circulation (point i. in the paragraph); the morphology of the basin and the density fields produce a three layer circulation with only one open cell and two closed cells, thus modulating exchanges among sub-basins (point ii. in the paragraph); as discussed in the text, topography controlled gyres play an important role in the basin, while similar structures are relatively less important in the deep open ocean (point iii. in the paragraph); the thermohaline properties of the basin, the morphology and the spatial scales make mesoscale and submesoscale very important processes in the basin. The latter aspect has been highlighted by numerous experimental, numerical and theoretical studies (point iv in the paragraph). We do not agree that the paragraph must be deleted.

We also disagree on the suggestion to provide a list of the major oceanographic studies conducted in the basin. We have rather chosen to summarize the key points highlighted by previous studies by giving credit, in the following text, to those who contributed to evidence those points. In this respect we are rather confident that we have given credit to all the people who have contributed in understanding the functioning of the Mediterranean Sea. An exception could be made for the oceanographic studies performed at basin scale.

Section 3 Phytoplankton section

pg 11198- Claim of evidence of climate change is made with a poor foundation- The paper cited merely suggests the possibility.

We will change: “effects of climate change are clearly seen” to “effects of climate change have been hypothesized”

pg 11200 - the text reads as if one should believe remote sensing estimates of primary production as 'true' compared to the in situ values which are used to calibrate satellite color data. I believe the values of primary production measured in situ are net, not gross primary production in the sense that DOC production & consumption are not measured. "New primary production" is all together different if the authors refer to production based on nutrients from deep waters. The entire paragraph is murky.

We will rephrase the subsection to remove any possible ambiguities. We agree that satellite estimates are calibrated on in situ values. On the other hand, satellite estimates, which certainly filter the biological variability, provide a highly frequent, spatially dense information which is, to a certain extent, self consistent. In other words, spatial and temporal trends in primary production are more robustly displayed by satellite data, despite possible biases, than by scattered, episodic in situ measurements. The latter reflect instantaneous, i.e., daily, variability in a single point, while the former display patterns, even if values are relative.

Whether primary production derived from C^{14} incubation is net or gross production has been discussed since the introduction of the method. The answer certainly depends on the duration of the incubation, but we did not class the data following this criterion. In situ data were considered to show the possible ranges. On the other hand satellite derived primary production as computed by the cited papers is clearly gross primary production. We will further clarify that export production as measured by nutrient regeneration/fluxes is not necessarily equal to new production, in a boundary driven basin, and is clearly different from gross production derived from satellite.

The sections detailing the distributions and concentrations of various groups of phytoplankton lacks a synthesis- perhaps a table could be added showing major characteristics- E-> W pattern, seasonality, peak values, common taxa, etc. for each group.

Data on phytoplankton composition are very scattered and, for example, do not allow for a description of the seasonality of major groups. Similarly, E-W patterns only make sense in the cases they have been addressed within a same across-basin cruise, which only happened once in the summer season. However, we agree that it is worth trying to add some synthetic tables in the revised version. We will do our best.

Section 4 Microbes Section

Microbe section stands out as providing new data analysis given in the figures.

Section 5 Mesozoplankton section-

This section requires some re-structuring. It seems to go on forever with no real point. Missing are some recent references and discussion concerning mesozoplankton diel vertical migration.

The section will be revised aiming at a clearer focus, by adding information on the diel vertical migration and reviewing the recent literature.

M. Krom

This is an excellent and much needed review of the recent data of Mediterranean Plankton. It sets as background the unusual circulation and chemical oceanography of the system which drives the unusual patterns of productivity found within the system.

While it is indeed true that the MS has an N:P > 16:1 and thus the primary productivity in the system is limited by the amount of P present, the actual cause of the oligotrophy in the system is the unusual anti-estuarine circulation which results in the very low total nutrient concentrations despite the fact that it is an almost completely enclosed basin and there is significant both nature and especially anthropogenic input of nutrients from the surrounding coastal regions particularly to the north.

On p1193 the authors describe the Cyprus eddy and Shikmona Gyre as though they are different features whereas they are actually two names for the same feature.

According to POEM (1992) the anticyclonic Shikmona gyre consists of several eddies. During the late 80s the warm core Cyprus eddy was active and was shown to be the northernmost lobe of the Shikmona gyre. In 2002, the co-existence of the Cyprus eddy with a second anticyclonic eddy in the eastern part and a third anticyclonic eddy close to the Egyptian coasts provided evidence for the re-establishment of the previously observed multipole, sub-basin scale Shikmona gyre (Zodiatis et al., 2005). We will correct properly the relevant gyres in figure 2 and we will clarify in the text.

I am intrigued by the data presented in Figure 4 showing the nutrient distribution at 125 m across the MS. It would be helpful if the authors made clear where the data came from and what analytical quality control had been carried out upon it. The text says the depth is 100m while the figure says 125m. There seems to be a particular high value of nitrate in winter in the area of the S.Adriatic that is the source of Levantine Deep Water. Since this is also the time when such deep water is formed and over large parts of the EMS, all the phosphate is consumed during the bloom, leaving a nitrate residual, this implies there would be a significant amount of preformed nitrate but not phosphate in the LDW.

The comment is obviously sound. All of us are aware that the Medar-Medatlas data base on which maps are based is not robust in terms of internal consistency and quality control. The maps were included because, despite the controversial hot spots correctly highlighted by Mike Krom, they show the large scale pattern of nutrient distribution in the basin. The discrepancy between the text and the figure legend about the depth horizon will be eliminated in the revised text.

I am also intrigued by the higher nitrate observed to the east of Crete and wonder what might be the reason for this anomaly. The text notes that the Nile no longer flows as it once did, which is undoubtedly true and that therefore it is much lower source of nutrients which is not necessarily true. The flood which used to jet nutrients including silica into the EMS ceased in 1965. However sewage from Cairo and from the delta now flows into coastal lagoons and these are connected to the offshore. As far as I know there is no data available to know how much of the nutrients from this source reach the coastal shelf and hence the offshore.

This is a relevant issue which concerns basin budget estimates. However we suspect that the nutrient input from Nile to the basin is still small, because satellite images do not display a significant biomass buildup at the Nile mouth. We will do this in the revision.

On p11210 the text suggests that microplankton 'can escape capture by swimming.' I seriously doubt if this is a significant effect.

We agree and will rephrase the sentence.

M. Alcaraz

Periodical review articles as this one about Mediterranean plankton are always welcome.

However, the necessity for including the latest data possible can, in my opinion, introduce some bias in the bibliographic citations, as occurs in this case. Although in Introduction (p. 11191) it is stated that the paper "...aims at providing an updated and integrated picture of the Mediterranean plankton: ...during the last 25 years: ...", the dates for the references included in the revision have been taken too strictly. The first bias is of temporal character. Of a total of about 250 citations, less than 20 refer to papers published before 1985. I have my doubts about the substitution of references to relatively old, but generally seminal papers, by later, modern ones, sometimes of a "clonic" nature, and not always adding significant data. A second bias is probably derived from language problems. The bibliography in French and other languages of the Mediterranean is almost absent, probably due to the scarcity of old, classical citations. The few references in Spanish (and one in Catalan) correspond, of course, to R. Margalef.

With regard to zooplankton, with which I am more familiar, I note the absence of references to the classical works of Nival, Razouls, Furnestin, Mazza, etc., or F. Vives, although most of their publications are in French or Spanish, and sometimes difficult to track.

The choice of 1985 as a limit year for the references considered for the present review is based on the publication in 1985 of the following review papers regarding phytoplankton and zooplankton of the Mediterranean Sea:

Estrada, M., Vives, F., and Alcaraz, M.: Western Mediterranean, in: Margalef, R. (ed): Western Mediterranean, Key Environments, Pergamon Press, Oxford, 1985, 148–197.

Estrada, M.: Deep Phytoplankton and Chlorophyll maxima in the western Mediterranean.

Gaudy, R.: Features and peculiarities of zooplankton communities from the Western Mediterranean, (This review is referring to the papers by Nival, Razouls, Furnestin, Mazza and Vives).

Moraitou-Apostolopoulou, M.: The zooplankton communities of the eastern Mediterranean.

The above three papers appeared in: Mediterranean Marine Ecosystem, edited by: Moraitou-Apostolopoulou, M. and Kiortsis, V., Plenum Press, New York, London, 1985.

Therefore we considered as more reasonable to review the information appeared after 1985 than to repeat a review effort. However we acknowledge that relevant contributions came out in the literature before and, possibly, after. They will be included in the revised version also because, as remarked by referee no#3, a review becomes a primary source of bibliographic information. It should be noted that references on Mediterranean plankton issued after 1985 and written in French or Spanish language became very rare.

Aside from this general impression, I would like to mention some specific points:

P. 11192: Line 9: “: : :whereas east quadrant winds?”

We will correct the pending phrase

P. 11194: Line 2: Is Fig. 4 the output of a model, or are real data?

Those are real data from Medar-Medatlas data base

Line 15: The first reference to filaments in the Mediterranean is, if I am not wrong, from 1988 (Don Ping Whang et al?).

Honestly we have missed that paper but we will include it in the references

Line 24: the first mention of the importance of atmospheric deposition for nutrient concentration and ratios is, if I am not wrong, by Mignon, probably in 1989.

This is partially true. Bergametti's PhD thesis preceded Mignon et al. paper. However it is clear that the French school pioneered this field. To make the point clear we added a few key references.

P. 1196: Line 10: : : :surface phytoplankton biomass as Chl-a (Fig. 5)?

P. 1197: Line 10: : : :surface chla values?

We interpret the question marks as questioning that satellite data provide surface chlorophyll a concentration because they provide optically weighted chlorophyll. We will clarify this in the text.

P. 11198: Line1: : : : DYFAMED is the only offshore Mediterranean time series: : : I think that in Castellón there was a station sampled for about 30 years, but unfortunately interrupted. J.M. San Feliu or R. Margalef had something about the multiyear pattern of Chla concentration and probably primary production.

Some of the stations sampled in the Castellon area were located up to several miles from the coast, but still well above the continental shelf, their maximum depth being 100-120 m. In some of the relevant papers, Margalef himself referred to the 'coasts of Castellon' in the titles, and defined the plankton as neritic in the text. We chose not to deal with coastal waters in our paper, and discarded other datasets like the ones from Blanes, the Gulf of Naples, the Bay of Villefranche-sur-mer etc.

P. 11212: Lines 24-on and P 11213 lines 1-2: The number of data pairs and the correlations between viruses, bacteria abundance and bacterial production seem a little confuse: Line 25: Virus-Bac.Abund n= 46; Line 27 virus-Bac.Abund n=24; the values of r are also different.

The referee is right, this is a bit confused. We should say in line 27, before viral concentration: “Considering the data set for bacterial abundances and BP, we found that viral abundance was related to both variables (n = 24, r=0.520, p<0.05; n=24, r=0.421, p<0.05, respectively)”.

In P 11212 the V-Bac correlation is qualified of “tight”, and in P.11213 line 1 the same correlation is qualified of “low”.

V-Bac correlation was considered more tighter than V-Chlorophyll a correlation, but this is not in contradiction with the level of the correlation coefficient that is relatively

low ($r = 0.549$), indicating that other organisms than bacteria could be hosts of viruses.

P. 11214: Lines 6 and 7: the gradient of bacterial production is probably not “West-East increasing” (in line 16 it says “: : is several times lower in the eastern..”).

We will correct it

P. 11221: Lines 15-20: The DZM (deep zooplankton maximum) coinciding (during daylight hours) with the DCM is a quasi-permanent structure in the MS (at least in the WMS). Its role for the upward transfer of nutrients by excretion at surface during the night is important (summer zooplankton acting as prudent predators, like spermwhales, feeding at depth layers, excreting at surface). The citations to refer to this DZM (Latasa et al. 1992) are not the best possible, probably because Alcaraz 1985, is hidden in Proceedings of the 19th EMBS, Cambridge University Press, and Alcaraz 1988 in Oceanol. Acta 9: 185-191, and maybe they are not accessible in the standard scientific data bases. In Margalef (ed., 1985), there are also references to the DZM in relation to the DCM, as in Alcaraz et al. (2007, Prog. Oceanogr.) and Alcaraz et al. (2007, Globec International Newsletter, October 2007).

The above mentioned references will be considered for the revision of the sentence on the DZM.

P. 11222: L. 14: I think that the importance of small zooplankters in Mediterranean had been first mentioned in Calbet et al. 2001, JPR.

The above paper refers to a coastal area of the Mediterranean Sea, while this review concerns offshore waters; (very few papers based on data collected in coastal areas are mentioned in the present review, only in cases that published information on offshore waters does not exist). We will acknowledge that the dominance of small zooplankters was reported also for coastal waters in the Mediterranean (Calbet et al., 2001).

L. 19: “specious”?

We will change and present differently the fact that the mentioned copepod genera are represented by numerous species

In L. 25 and following, until P. 11223, L.14, the feeding mechanisms and swimming performance could be probably included in a section like “Zooplankton activity”, including behaviour, feeding, production and probably metabolism and its relation to nutrient regeneration and production.

We will consider the referee’s suggestion and we will try to re-organize the zooplankton chapter, providing also information on metabolism.

P. 11225: Regarding Mediterranean Cladocerans, there are previous data from Della Croce, Casanova and Alcaraz, as regarding Ostracods (Alcaraz 1977).

These references will be considered in the revised text unless they have been considered in the review by Gaudy (1985)

P. 11230: L. 12: Units of ingestion? m^3/mg : : of what?

For respecting the original, the units are reported exactly as in the paper by Gaudy and Youssara (2003). However, according to the information provided in their text, we can specify: μm^3 particles mg^{-1} zooplankton dry weight h^{-1} ,

P. 11231: L. 2: : : :summer where?...

It is mentioned in the beginning of the sentence that the results concern the Catalan Sea.

L. 28: “: : :well below the saturation level of copepod clearance: : :rates?” In any case, the sentence seems a bit confusing.

It will be clarified

P. 11233: L. 9 – 20: Could not the whole argumentation be simply explained by the low quotient Production/Biomass (Margalef) that one would expect in oligotrophic, mature ecosystems? The higher trophic efficiency in oligotrophic systems is well known.

We understand the remark and we agree that we are basically describing the same scenario giving emphasis to different aspects. We will discuss this in the revised text with links to generalizations made by Margalef, Frontier et al., Colinviaux, and others.

P. 11234: L. 24: : : :Constrained?

P. 11235: L. 18: Suppress “in”.

The language will be taken care in the revision

L. 21-23: Again the old relation between diversity and the quotient P/B.

Please see above.

P. 11236: I would not say that the Mediterranean, in general, is “strongly oligotrophic” (70-100 $\text{gC}/\text{m}^2/\text{year}$ is probably not a “strong oligotrophy”).

We will change the word. Maybe pronounced is more appropriate.

Of course, all the above comments, mainly those concerning the apparent bias in the references chosen, are my personal feelings. The lack of significant data previous to the Internet era makes me think about the fact that future generations would be in danger of re-inventing the wheel by ignoring the science made, say, 40 years ago. We are too dependent on scientific databases that can be consulted very easily and that contain only part of the science made, and where papers older than 25 years are very seldom included.

M. Estrada (Referee)

This review integrates an extend amount of information and represents a good contribution to the knowledge of the diversity and ecological role of the main organisms of the Mediterranean plankton. Particularly interesting is the attention devoted to the physical and chemical environment, including water circulation. However, some aspects of the manuscript could be improved or clarified, as explained below.

In many places, the choice of references could be refined. This is particularly important in the case of review articles, because many readers never extend their literature search beyond them. For example, although Marie et al. (2006) "and

references therein" (page 11190, line 28) may be an excellent source on molecular approaches to picoplankton distribution, surely there are earlier and more appropriate references concerning in situ data on the western-eastern chl-a gradient in the Mediterranean. In some parts of the text, like section 2, there are numerous cases where only a very recent reference is quoted when mentioning a phenomenon that has been known sometimes for decades. The addition of "and references therein" does not help much to improve scholarship and should not be an excuse to forget the actual scientists who first made a strong contribution to an idea or concept. As a collective effort, I would recommend to quote also the volume edited by Minas and Nival (1988).

We agree and will pay attention to cite more appropriate references.

A consistent definition of "microbes" and "microbial" should be adopted. In some places, "phytoplankton" seem to be microbes (I believe that this would be the most general criterion, nowadays); in others, like the abstract (page 11188, line 20) the phytoplankton" and "microbial (both autotrophic and heterotrophic)" components are listed separately.

We will pay special attention to this remark in the revised text.

It is a challenge to organise a text ranging from viruses to mesozooplankton. However, the importance (read length of text) devoted to some of the groups seems somewhat unbalanced. For example, the main species of copepods and their ecology are described in detail, while the "heterotrophic nanoflagellates" are dispatched in 25 lines.

We agree that not all groups received the same space, but this obviously reflects the different amount of information available for these groups. Little is known about heterotrophic nanoflagellates diversity and functional diversity on an international level. Their abundance as a group is only addressed in studies based on epifluorescence. All other studies based on light microscopy lump together heterotrophic and autotrophic dinoflagellates and flagellates which belong to this size class. Molecular studies are just starting to provide more detailed information on this group. We can now add in the HNF part a very interesting work that appeared after the completion of the first version of the paper (Rodriguez-Martinez et al. 2009) deals with the distribution of MAST-4 in open Med waters.

Some general affirmations need a rethinking or a better justification. For example, the more or less oligotrophic character of the Mediterranean and the probable limiting role of phosphorus are repeatedly mentioned in the text. However, the abstract (page 11190, line 7) concludes that "the system is top-down controlled". Perhaps the problem is that classifications such as "bottom-up" and "top-down" are not adequate to describe the complex reality of the marine ecosystem.

The issue raised by the referee is very relevant and, in fact, we do not provide a conclusive answer for it. On one hand there is evidence that phosphorus availability controls the carrying capacity of the system on the other hand there are several evidences that within the given carrying capacity most of the microheterotrophs are top-down controlled. We will think about and try to clarify this point in the revised text.

The last paragraph of page 11200 should be clarified. In situ estimates of primary production are generally accepted to be close to net production. This is not the same as new production, which is the part of the production based on new rather than recycled nutrients.

We acknowledge that our wording resulted in ambiguous statements (see also comments of ref #1). We will rephrase the section to remove them.

As it stands, section 3.2 is fairly difficult to follow. The phytoplankton structure and composition sections could improve, for example, with concise explanations of the typical seasonal cycle in some selected areas.

Unfortunately there is no area where a complete seasonal cycle was reliably described for phytoplankton structure and species composition. We do not think it is appropriate to infer a 'typical' seasonal cycle for offshore areas from scattered cruises. This problem is dealt with when we report the high interannual variability e.g. in chl-a concentrations in a few areas where observations were made in different year. We interpret this high interannual variability as the result of very loose temporal scale of observations which were made based at the best on 4/6 cruises over the year.

A table with some examples of data on the proportion of biomass or production accounted for by pico, nano or microplankton would be also helpful to ideas.

We will try to produce such table. It is a good idea just to put together the available cases, although these may refer to different seasons of the year and be based on different methods.

Other comments

Page 11190, line 8. "significant" (eliminate space).

Page 11190, line 23. "oligotrophy seems to be mainly" (add "be").

OK

Page 11194, lines 6-8. It is likely that nutrient concentrations in upwelling areas are lower than those found in other systems, not only because of the short temporal scale of the upwelling events, but also because the source waters have lower nutrient concentrations, as stated above. The relationship of the antiestuarine circulation with the Atlantic with this lower nutrient content could be discussed.

We will discuss more clearly both issues in the revised version

Page 11196, line 12. "Liguro-Provençal". In many places the units miss spaces; for example, moly-1 instead of; mol y-1; _gchl-al-1 instead of _g chl-a l-1.

We will correct

Some figures (e.g. 2,5, 6) are difficult to see. Perhaps their resolution could be improved.

We will improve them, but beyond a certain threshold it is absolute size of the figure that matters.

Page 11196, lines 15-20. I don't understand the reasoning there. Where is the OC/ON ratio used? All the given values seem to be organic C (OC), no organic N (ON?) is mentioned.

We believe that it might depend on our unclear wording. We would appreciate if the referee could clarify her comment in order to solve the problem.

Page 11197, lines 2 and 4. Define "WMS" and "EMS" on their first appearance.
OK in the revised text

Page 11197, last paragraph. The distinction between the province no. 5 and the subtropical ones is not clear from the description (all have a winter bloom or maximum).

Province number 5 displays a peak in spring. We will clarify this in the revised text.

Page 11198, lines 10-11. The "Atlantic Current" proper does not reach the Catalan Front.

The sentence will be corrected.

Page 11202, lines 1-5. It is hardly surprising that phytoplankton differences across basins are not simply quantitative, although it depends on what is defined as "quantitative". Dolan et al. (1999) deals mainly with ciliates and is not an adequate reference here.

We simply meant that, in addition to the W-E gradient in biomass, phytoplankton composition and population may also differ among the two Mediterranean basins. To support this concept, we have cited the only papers (to our knowledge) that include information on phytoplankton collected in the course of one single cruise. This choice was made based on the need to have quasi-synoptical information and, mainly, to avoid comparing different methodologies rather than different basins. We agree that the paper by Dolan et al (1999) mainly focuses on ciliates, but it also gives some figures about phytoplankton and is one of the few papers matching the requisites explained above (synopticity and methodological uniformity).

Page 11215, lines 26-29. perhaps you could quote (sorry for the autocite) Morán et al. (2002, *Microbial Ecology*, 44: 217-223).

We will add this reference

Page 11220, lines 25-29. I don't agree that there is virtually no quantitative data on heterotrophic dinoflagellates in the open MS. Many genus of dinoflagellates are known to be heterotrophic and are included in general phytoplankton lists.

(This is in page 11219). The referee is right. Studies dealing with community composition presented list of genus as well as species, however in most of them they do not show the average abundance of them. In addition due to the methods used, only some groups, genus or species can be recognize as really heterotrophic. There are a bunch of small dinoflagellates that under light microscope and fixed with lugol is hard to tell if they are heterotrophic, mixotrophic or phototrophic. As explained in the phytoplankton section, if we consider that heterotrophic dinoflagellates include

both thecate and naked species, and that most of the latter are naked forms, we must admit that a quantification of their total abundance is not possible based on the available data. Even considering that large thecate forms are in the lists, their number are generally too low to be reliable when obtained at the same time as phytoplankton. Indeed, they should be counted along with ciliates on larger volumes. We will change the relevant sentence to: “However, heterotrophic dinoflagellates are included in lists of phytoplankton community composition, and few data on abundance is available for the open MS” .

Page 11224, line 24. Explain briefly (probably in section 2) what is the "Eastern Mediterranean Transient".

We will explain this event in section 2.

Anonymous Referee #3

Overall, this is a valuable review putting together all the scattered works of the last decades regarding biogeochemistry and planktonic marine foodwebs in the MS. It reveals the patchiness of the existing studies and outlines the need for more homogeneous and coordinated efforts to be undertaken in the future, including the extended use of modern tools (from satellite sensors to autonomous systems and to “-omics”). Compiling this information, particularly under “key-players” sections is of great importance.

From the few transmediterranean cruises and comparative works the general trends and patterns have been outlined while local works have studied the mechanisms and feeding relationships within planktonic marine foodwebs. Using this information, the review presented here nicely reveals a glimpse of the thousand-piece puzzle of the Med ecosystems’ functioning. An interesting and well made synthesis of existing ideas is made in the conclusion section.

On a more technical level, this work reads differently in the different sections, apparently reflecting different writers’ styles. However, I do not consider this as a problem in the same way that different writing styles are totally acceptable in scientific books, where different writers write each chapter. More importantly, a serious reworking of the English needs to be done throughout the text to avoid compromising comprehension.

Improving the English overall will also smooth differences among sections.

Publication recommended after minor revisions.

General editorial comments:

Special care has to be taken to uniformly present short names and abbreviations, i.e.

-the use of word “sea” in i.e. Med Sea, Aegean sea

-Wmed, WM, EMed, EMS, NW Med, Basin, basin, appearing both in text and in figures/tables’ legends.

-always separate the values from units

-avoid very general statements, usually in the introduction of a paragraph, that do not add substantially to the text

-add commas after introducing words, in head of sentences: e.g. Interestingly, surprisingly, however, moreover ect.

OK, they will be checked and considered in the revised text.

Detailed comments:

Abstract:

- p. 11188, line 18: Overall, the basin .. (add the comma)
- p. 11189, line 4: ...enhance export towards 'high' : : : should be 'higher' trophic levels
- p. 11189, line 8: "signi ficant" , replace with "significant"

Section 1 Introduction

- p. 11189, line 15: The Mediterranean Sea (MS) is the largest quasi-enclosed sea on the Earth, its "extension" being : : . you mean "surface"?
- p. 11189, line 19: The phrase "The MS' size, location, morphology, : : . and submesoscale activity" is a copy paste from the Abstract. Consider rephrasing or deleting it in one of the two sections.
- p. 11189, line 23: Oligotrophy seems to "mainly due" to the very : : : -Replace with "be mainly due" or to "mainly be due"
- p. 11189, line 25: add Thingstad et al., 2005, in the parenthesis
- p. 11190, line 5: "whose impact on the marine environment "have" still to be clearly.. -Replace with "has" still to be...
- p. 11190, line 18: ... were hence devoted to "constrain" carbon and nutrient fluxes and to provide insight "on" the key players : : : -Replace with "study" and "into"?
- p. 11190, line 27: Clearly these studies have provided new insights "in" the MS plankton in terms of its components, "besides a more extended geographic coverage".-Rephrase
- p. 11191, line 9: A review could be helpful, among "others", : : : "other things"?
- p. 11191, line 10: "In addition, as detailed in the following sections, the main forcings on the basin and their scales display peculiar features. As a consequence, non-trivial responses might be triggered in plankton communities, which could be of significant interest for a wider than Mediterranean community". -Please rephrase

All the above will be considered in the revision of the text.

Section 2 Physical and chemical framework

- p. 11191, line 23: : : : and its connection with one of the proportionally largest catchment areas. -Please explain

We will clarify this point.

- p. 11192, line 3: ...with saltier and denser water exiting the basin at Gibraltar and a compensating entrance of the fresher Atlantic water. - Why the Black Sea Water inputs in the EMed are not mentioned? Is it quantitatively not comparable?

The Black Sea water inputs are mentioned in p. 11193 line 28-29.

- p. 11192, line 4-5: As the unbalance between evaporation and precipitation plus "runoff" (the E-P-R term) "in-creases" towards east... -Replace with "run-off", "increases", add towards "the" east
- p. 11192, line 5: ..the eastern basin is anti-estuarine respect to the western basin. -Add "in" respect to the western basin.
- p. 11192, line 13: ..which are mainly anticyclonic in the southern regions and cyclonic in the northern ones (Pinardi and Masetti, 2000). -Add (Fig.2) after this phrase
- p. 11192, line 15: The MAW adds a haline component to the thermal contribution to stratification in large areas of the SW MS decreasing the winter mixed layer depth.

-Add comma: SW MS. Decreasing

p. 11192, line 19: : : : in both the Alboran Sea and “in” the Algerian basin... -Delete “in”

p. 11192, line 23: which separates two “subregions”... -Replace with “sub-regions”

All the above suggestions will be considered in the revision of the text.

p. 11193, line 11: : : : Shikmona (south of Cyprus) and Cyprus Eddy (south-west of Cyprus).. -Are one and the same

We replied to the same comment above (comments by M.Krom)

p. 11193, line 11-12: ...Local deep convection events occur periodically in the deep troughs (>1000m) of the North Aegean Sea and in the deep basin of the South Aegean sea. -Add references

p. 11193, line 23: Main features are: : : . – Add “the” main features: : :

p. 11194, line 8-10: Rephrase! Also add comma (Therefore,: : :)

p. 11194, line 23: : : : with a “dramatically” decrease – Replace with “dramatic”

The above suggestions will be considered in the revision of the text.

p. 11195, line 11: This adds to riverine inputs. Complete the sentence

OK

p. 11195, line 15: To complete the picture on respirable carbon not produced inside the basin we have also to account for the net DOC input through Gibraltar: : : Again DOC inputs from the BSW inputs are not considered, yet they have been considered to be important (Sempéré et al., 2002)

We will add the information from this reference

Section 3 Phytoplankton

p. 11197, line 8: These gradients clearly reflect the physiography of the basin and the related circulation patterns. – Very general. Not informative.

Ok, we will rephrase the sentence

p. 11198, line 6: Similarly high peak values: : : . Add comma; Similarly, : : :

p. 11198, line 13: between the two MS sub-basins : : : Add full stop: : : sub-basins.

p. 11198, line 23:p. showed a “two-threefold” variability... –Replace with: showed a “two to threefold” variability

OK for the above comments

p. 11199, line 21: The DCM progressively deepens from west to east (Fig. 7) from 30m in the westernmost area (..), to 70m in the South Adriatic Sea (..), down to 120m (..). – Where is this 120m?

In the Levantine Sea, we will clarify in the text.

p. 11199, line 22: ..probably related with lower productivity.. Replace with: related to

p. 11200, line 15: : : : primary production rates were 240–716mgCm⁻² (in 14 h).

Define parenthesis content

p. 11202, line 15: In the following,: : : Add: In the following section,..

p. 11202, line 19: : : :the different groups depicted below are “included” in completely distinct trophic pathways: : : Replace with are involved. . .

p:11202; line27 : As an average on the whole basin; . . . :primary production: □

□ Add reference

p:11203; line3 : . . . and size fraction considered: Replace with : size fractions. . . :

p:11203; line5 : . . . in the Straits of Sicily during\the summer: Delete\the

p:11204; line7 □ 8 : ::have also found to be abundant. . . Add : have also\been found.

. . .

p:11205; line4 : . . . in addition to a limited number of small solitary diatom species:

Add : \and coccolithophores:

p:11205; line12 :. . . probably because it is kept in check by.... Replace with : controlled

p:11206; line3 : In a June 1999 transmediterranean study;. . .Rephrase

p:11207;line29Apparently diatoms in highly \dynamics areas are only associated with the highest biomass values: Rephrase sentence and replace\dynamics with\dynamic

p:11208; line7 :. . . the main contributors also to high\chl patches: Replace with chl a

p:11208; line10 :. . . have been defined the\oasis of the Mediterranean desert: Add :

\as the oasis

p:11208;line15:....as in the exceptional case of a monospecific \blooms of:: Replace with\bloom

p:11209; line22 :. . . pulses of diatom growth in deep waters might contribute explaining::Add : \in explaining

[All these corrections will be done in the revised text](#)

Section 4 Heterotrophic microbes and viruses

p. 11212, line 23: Existing data (Table 2) also suggests that: : : Replace with “suggest”

p. 11215, line 26: Surprisingly little information exists : : : Add comma: Surprisingly, : : :

[We will follow these suggestions for the revision of the text](#)

p. 11216, line 8: In the MS an accumulation of DOC in the surface waters has been hypothesized as resulting from nutrient limitation of bacterial activity, specifically BGE. Please rephrase!

[The referee is right, the sentence should be written: ‘In the MS an accumulation of DOC in the surface waters has been hypothesized as resulting from nutrient limitation of bacterial activity \(Thingstad and Rassoulzadegan, 1995; Gasol et al., 1998\).](#)

p. 11218, line 4: ..where ciliate abundance “were” always lower: : : Replace with “was”

OK

p. 11218, line 9: It could be that the relationship between ciliate abundance and chl-a concentration is stronger in the WMS than in the EMS indicating a better coupling with phytoplankton stock in the WMS. – Have you any clue about this?

[We do not really have ‘clues’, but a possible explanation taken from our own experience working in WMS waters is that higher chla concentration in these waters](#)

are due to higher abundances of autotrophic nanoflagellates which are a preferential food for ciliates. We will think about including this in the revised version.

p. 11218, line 28: Dolan et al. (1999) have found that large mixotrophic ciliates were more abundant, both in absolute and relative terms, in the EMS than in the WMS. – Any clues why?

Probably the abundance of large mixotrophs in the EMS compared to the WMS could be related to the more oligotrophic character of the EMS. In general, larger cells are more 'starvation resistant' than smaller cells and one supposes that mixotrophs are better off as well in times or places of low food. We would expect then to find more mixotrophs and large species in lower patchy food environments. We could think about including this in the revised version.

Section 5 Mesozooplankton

p. 11220, line 3: An overview of the distribution of mesozooplankton standing stock in epipelagic Mediterranean waters highlights a general paucity in most regions, which reflects the oligotrophic character of the basin (Fig. 16, Table 5) – This is not apparent to non-specialists

We can clarify it by including in the text some comparative data with the adjacent Atlantic.

p. 11221, line 21: In the annual cycle, : : : in comparison to coastal waters. – Add reference

OK

p. 11223, line 3: : : :as reported for Paracalanus (..), that has similar swimming behavior. – Delete comma

OK

p. 11223, line 20: Although their populations largely overlap, : : :., similarly to what observed in coastal waters. Rephrase, not clear

We will modify in “similarly to the peak succession observed in coastal waters (Mazzocchi and Ribera d’Alcalà, 1995).

p. 11224, line 7: West-to-east differences in the community : : :. and paths in the systems. – Very general statement, not adding anything to the text

This sentence introduces the spatial differences in community composition that are presented in the following paragraph. We will rephrase better the concept.

p. 11225, line 8: Among crustaceans, cladocerans, which are a very abundant component of zooplankton in coastal waters, expand their occurrence beyond the continental slope only in narrow neritic areas at their maximum abundance observed in summer. – Not clear, rephrase

We will improve this statement to make it clearer.

p. 11225, line 11: In open waters of the Straits of Sicily and the EMS during autumn, cladocerans accounted for only 0.3 p. 11226, line 15: Among the highest

contributions should be that recorded in the Ligurian Sea in December: : : - Please rephrase

[It will be rephrased](#)

p. 11229, line 3: : : and “the” contrast might be attributed to factors other than nutrition such as zooplankton mortality due to predation. – Replace with: : : and this contrast might be attributed to factors other than nutrition, such as zooplankton mortality due to predation.

p. 11229, line 6: In situ measurements evidenced that mesozooplankton grazing impact on phytoplankton could be significant. – Add reference

[We agree with the above two suggestions](#)

p. 11230, line 6: The observed uncoupling between mesozooplankton and microheterotroph standing stocks in the North Aegean Sea could be due to the same factor.

- Isn't this contradicting to the previous section? (p.11229, l22 – p.11230, l.6)

[The referee is right. The above sentence will be deleted.](#)

Section 6 Planktonic food webs in the Mediterranean epipelagos

p. 11232, line 24: The heterotrophic biomass would then “have been” quickly channeled: : : - Replace with : The heterotrophic biomass would then “be” quickly channeled: : :

p. 11232, line 25: The relaxation of P limitation produced a “luxurious” accumulation of P in both bacteria and picophytoplankton (presumably less in the latter) with a P enriched diet for grazers: : : Replace with : The relaxation of P limitation produced a “luxurious” accumulation of P in both bacteria and picophytoplankton (presumably less in the latter) forming a P enriched diet for grazers: : :

p. 11233, line 9: The leading role of heterotrophs, in the MS, as it emerges from a plankton standing stock prevalently heterotrophic and dominated by microbes, is the dominant situation in the basin. – Rephrase

p. 11234, line 23:.. but its overall weight on the production of the basin is poorly “constrained”. –Replace with “studied”.

p. 11235, line 17: All this suggests that in the MS is characterized by a “multivorous food web” (: : :), including a continuum of trophic pathways spanning from the herbivorous food web to the microbial loop and dynamically expanding or contracting along with seasons, areas and transient processes. Very general, better rephrase

[The above suggestions will be considered in the revision](#)

Section 7 Perspectives

p. 11236, line 27: The diversity and distribution patterns of autotrophic and heterotrophic prokaryotes, viruses, and eukaryotes which are the major component of the MS epipelagos are still largely “underestimated”. – Replace with: “understudied”.

p. 11237, line 13: The relatively close proximity with land intensifies the effect of climatic changes and anthropic-driven impacts such as increased nutrient fluxes and/or overfishing might affect the biological structure of the basin at a more rapid scale as compared to the large oceans, and strongly support the role of Mediterranean as a sensitive sentinel for future changes. – Rephrase, not clear

p. 11237, line 29: Among these, a great opportunity is represented by a clever merge of modern oceanographic tools such as Autonomous Systems and the sophisticated methods of the “omics”, whose results may feed tentative integrated conceptual

models of the system dynamics to approach a broad range of marine environmental issues such as fisheries, climate change impact, harmful blooms, emerging diseases and pollution. – Please rephrase, not clear

p.11272, Table 4: exchange column 1 (Period) with column 2 (Location)

We will do the above suggested changes