

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

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This review integrates an extense 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

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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).

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. 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 "heterotrophs nanoflagellates" are dispatched in 25 lines.

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 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.

As it stands, section 3.2 is fairly difficult to follow. The phytoplankton structure and

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composition sections could improve, for example, with concise explanations of the typical seasonal cycle in some selected areas. 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.

Other comments

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

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

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.

Page 11196, line 12. "Liguro-Provençal". In many places the units miss spaces; for example, moly-1 instead of; mol y-1; $\mu\text{gchl-a-l-1}$ instead of $\mu\text{g chl-a l-1}$.

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

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.

Page 11197, lines 2 and 4. Define "WMS" and "EMS" on their first appearance.

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).

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

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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.

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

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.

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

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