

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

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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 de-

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rived 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.

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

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

P. 11194: Line 2: Is Fig. 4 the output of a model, or are real data? Line 15: The first reference to filaments in the Mediterranean is, if I am not wrong, from 1988 (Don Ping Whang et al?). 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.

P. 11196: Line 10: “. . . surface phytoplankton biomass as Chl-a (Fig. 5)?

P. 11197: Line 10: “. . . surface chl-a values?

P. 11198: Line 1: “. . . 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 Chl-a concentration and probably primary production.

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

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.”).

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P. 11221: Lines 15-20: The DZM (deep zooplankton maximum) coinciding (during day-light 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).

P. 11222: L. 14: I think that the importance of small zooplankters in Mediterranean had been first mentioned in Calbet et al. 2001, JPR. L. 19: "specious"? 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.

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

P. 11230: L. 12: Units of ingestion? $\mu\text{m}^3/\text{mg}$. . .of what?

P. 11231: L. 2: . . .summer where?... L. 28: "...well below the saturation level of copepod clearance. . .rates?" In any case, the sentence seems a bit confusing.

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.

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

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P. 11235: L. 18: Suppress "in". L. 21-23: Again the old relation between diversity and the quotient P/B.

P. 11236: I would not say that the Mediterranean, in general, is "strongly oligotrophic" (70-100 gC/m²/year is probably not a "strong oligotrophy").

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.

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