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Biogeosciences Discussions

# *Interactive comment on* "Ultraphytoplankton distribution and upper ocean dynamics in the eastern Mediterranean during winter" *by* M. Denis et al.

**Anonymous Referee #3**

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The ms "Ultraphytoplankton distribution and upper ocean dynamics in the eastern Mediterranean during winter" by M. Denis and colleagues presents a description of the distribution of Prochlorococcus, Synechococcus, pico and nanoeukaryotes in the eastern basin of the Mediterranean sea in winter, 15 years ago. They also provide seawater temperature, salinity and density data and examine the relationship between these parameters and the distribution of four ultraphytoplanktonic groups.

General comments:

The interest I see in the eventual publication of this ms is that it is actually the first basin-scale description of ultraphytoplankton abundances in eastern Mediterranean



Sea. Furthermore, it may be a base of comparison with the fresher data that will very soon arise from the BOUM oceanographic cruise (from south of France to Cyprus, 2008), although the latter was done in summer. The ms is overall well written but I think it is way too long. It should be condensed. I have two major concerns about this ms, that might hinder its publication in a journal like Biogeosciences. First of all, this study is a bit descriptive, not much innovative. What is the strong message of the ms? I still do not know. So, although this large data set is certainly interesting to share, the whole paper might appear a bit "basic" to some readers. It is well known that Prochlorococcus and Synechococcus have differentiated cell types that occupy different niches (vertically and horizontally). I think it would have been much more interesting to complement the data set with the distribution of the cyanobacterial ecotypes for example. This would have ensured the ms to be published in a good journal. The second concern is that the authors seem to be convinced that hydrodynamism dictate the distribution of phytoplankton, or at least they wrote the ms in this spirit. Obviously, the relationship between hydrodynamism and phytoplankton is not as direct as they claim. I think there is no need to argue about that... This ms suffers a lot from the lack of nutrient data and ideally irradiance data. The authors evoke their existence but it is really a pity they did not include them in the ms. Pigment data are also vaguely mentioned (there is a mention of zeaxanthin somewhere) but it is difficult to see how they were used as they are not mentioned in the discussion. I find the discussion rather poor. The ms does not seem fully accomplished to me, so I cannot really recommend it for publication in BG, unless the authors carry out extensive modifications. Hereafter I list some more or less important problems that, I think, should be fixed.

Specific major comments:

- I think the title should be modified and more attractive in order to high light the interest of the paper. There is nothing about "ocean dynamics" here, as the study takes place in the Mediterranean Sea, not in an ocean. Also, I think "in the eastern Mediterranean during winter" should be replaced by "in the eastern Mediterranean Sea in winter".

"Mediterranean" is in fact an adjective and should accompany a noun in English. This should be checked throughout the ms, also for the other seas.

Introduction

The authors start the introduction by claiming, as an axiom, that hydrodynamism controls phytoplankton distribution, composition and primary production. This is obviously an excessive assertion. The influence of hydrodynamism is not as direct as the authors wrote. Phytoplankton distribution is also dependant, and many people think that it is mostly and primarily dependant on nutrients, light and temperature. The authors mention these parameters later but I believe that this paragraph should be cleverly rephrased in order to not state in a head-on fashion that hydrodynamism is "the" major factor affecting phytoplankton. I find the introduction is overall not balanced. There is a lot of detailed information about the hydrodynamic processes in the Med Sea but very little about the organisms, which are, if I refer to the title, the main subject of the study. Aren't they? And it is not like there was no literature about picophytoplancton in the Med Sea, especially for Synechococcus and Prochlorococcus... The distribution of these organisms in the other seasons should be briefly reported in order to allow comparison in the discussion. There is a lot of knowledge about the way picophytoplanktonic organisms have colonised different niches. I think the introduction should be largely revised in this respect.

Methods

- The samples were filtered on 100$\mu$m mesh. But it seems that the largest organisms the authors see are 10$\mu$m large? I wonder how this can be. If there was an additional filtration, it should be mentioned.

- The 100$\mu$m filtered flow cytometry samples were fixed and preserved in liquid nitrogen. Since this was done 15 years ago, I am wondering when the cytometry analyses were actually carried out. This should be clarified



- 200 fg C per cell is a minimum number for Synechococcus. There are more recent studies that indicate values closer to 250 fg C per cell.

- The use of ratios such as C/Chl a = 50 over large areas and through the whole euphotic zone appears very approximate if not wrong. For example, it is well known that chl cell content varies a lot depending on taxa (a chlorophyte has more chla than a haptophyte...) and physiological status. Notably, it is known that photosynthetic organisms universally show an increase in chlorophyll cell content with decreasing irradiance, and this independently from the C cell content (see e.g. Six et al. 2004 AME 35(1): 17-29; Kana & Glibert 1987 Deep-Sea Research 34: 479-485).

- The authors mention pigment analyses in the methods and there is no pigment data presented in the result section. If these data have been already published as the authors seem to mean at the beginning of the discussion (Vidussi et al 2001?), then the HPLC paragraph in the method section should be discarded.

Results

- p. 6853 line 4: of course the integrated carbon biomass follows the integrated abundance. The method used by the authors cannot give another result. This sentence should be rephrased or discarded.

- p. 6854 line 3 same remark as above

- p. 6856 line 27 and p. 6859 line 13-16: the authors refer to nutrient data. I feel very frustrated to not see them clearly included in this ms. This would have added a lot.

- p. 6858 line 23: the authors wrote that at station 25, at 60-75m, there was "a large abundance decline for all ultraphytoplankton groups.", linking this decrease with density changes. However, this is not what I see on figure 9 for Prochlorococcus, as it actually reaches it maximal abundance at this depth. This should be corrected.

- I am surprised to see that the result section comprises specific paragraphs focused on the southern Adriatic Sea, the Eastern Ionian Sea, the transition from the eastern to the western Mediterranean Sea, but nothing on the areas from the Cretan passage to the Levantine basin. The description of the vertical profiles of this area, which constitutes the main interest of this paper according to me, has seemingly been ignored. I believe the cell abundances and water dynamics of these basins should be described in the results (maybe more precisely than the other areas)...

Discussion

- p. 6862 line 20: the authors wrote that they saw evidence for the presence of the well-known Prochlorococcus ecotypes thanks to the mean fluorescence. This means that they saw "brighter" cells at depth than in surface. I am frustrated that these data are not included in the ms, at least as supplementary material. Moreover, in the result section p. 6855 line 4-5, the authors say in a general way that "As a common feature, the four resolved clusters did not exhibit fluorescence increase with depth (data not shown)." Unless there is something I missed, this should be clarified since it seems that they did see brighter Prochlorococcus.

- p. 6864 line 8, the authors wrote that salinity explains the variations of picophytoplankton distribution between western and eastern Mediterranean Sea. The way this is written sounds like salinity is the only factor explaining the differences. It is hard to believe and in any case, not demonstrated in the ms, as data such as nutrients and light attenuation coefficients, for example, are essential to state this. The authors made some allusions to this type of data that, therefore, seem to exist; but they should include them in the manuscript.

- The authors say that winter phytoplankton data in the Med Sea are scarce. I would have then enjoyed understanding the differences in abundance and water dynamics between the seasons by comparing their data to other studies.

- The authors do not discuss the Levantine basin data (which are not reported in the results). Is there really nothing interesting to say about this area ?



- The authors mention some (of the few) studies that present some data from the areas covered by their study (for example Li et al., I am also thinking of Takana et al. which is not quoted). It is a pity that there is no comparison with these data.

Minor comments :

- The figures could look better and more homogenous in the formatting. They are difficult to read and often too small (but I suspect that the pdf formatting did that) For example:

Fig 1 should be enlarged Fig 5: the 30-32° band could be discarded Fig 6A, I cannot distinguish all the symbols of the legend. A line would be easier to read than numerous dots. Fig 9: the letters and numbers are difficult to read

- p. 6842 Line 28: there's a typo on chlorophyll

- p. 6844 Line 22: should read: "the salinity is reported according to the practical salinity scale and the density as potential density excess"

- p. 6857 line 7-8: should read "when density values"

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