

Review of paper BG-2010-214  
« Deep silicon maxima in the stratified oligotrophic Mediterranean Sea »  
Submitted to BGD  
by Crombet et al.

This paper presents a large set of new data on biogenic silica (BSi) in the Mediterranean Sea from two different cruises covering the whole basin. Undoubtedly the data are of excellent quality, original and represent an important quantity of work. The discussion on the role of diatoms in Med. Sea and on the occurrence of Deep BSi Maximum (DSM) in Med. Sea is interesting, novel and timely since these features are often overlooked. The interpretations of the data are also generally well supported by the results. Overall I am convinced that the quality of this work justifies a publication in BG. Before that, however, I believe the manuscript would greatly benefit from a significant revision.

My main concern is that I believe the article to be overly lengthy given. As such, it prevents the reader from having a clear picture of the main outputs of this work. The most interesting parts (e.g. role of DSM on export; link with diazotrophy; Si cycle in Med. Sea) are flooded by too detailed descriptions of results. I recommend the authors to prepare a version with more focused objectives and a more straightforward discussion. In its present form, I have the feeling the manuscript oscillates between (1) a review on DSM in the ocean and (2) a review of Si cycle in the Med. Sea (while the title restricts the work to DSM in Med. Sea). I am not sure it could really be either and most certainly not (1) as only Med. Sea data are presented.

The introduction is well structured and nicely written and could stay as it is. Sections that need a substantial reduction are:

- Results. There is no need to describe by words every feature (e.g. min-max, average for each depth layer, sub-basin and campaign for each nutrient, LSi and BSi...) that can be directly seen on figures. Please restrict the writing of the results' section to the most relevant features that will be of interest for further discussion.
- Discussion. Several (long) parts of the discussion are again only a description of (more) results from other studies without clear link made with the current work: e.g. the last part of section 4.2 and the first 2/3 of section 4.3. Large parts of Sections 4.4 & 4.5 are also limited to results' description from previous Med. Sea cruises + new diatoms counting (not even mentioned before) from the cruises studied in this work. In its present state, this presents little interest since there is no concise discussion/summary on the link between the new biogeochemical data presented here and other results lengthily described in § 4.4 & 4.5. The last part of section 4.5 is more interesting and should appear more at the forefront. I don't really see how the authors relate the presence of DSM to higher export? Please explain.
- Figures. There are 15 figures (which display a total of 27 ODV sections), this is really a lot, but here I cannot suggest which one(s) to be removed. Are all of these being published for the first time? If not, I suggest the authors to

consider either not to present all nutrient data and/or using a single transect instead of 2 for PROSOPE. (see also my comment below)

- References. There are about 120 references cited in this paper, which I believe is too much. Many references are not related to Med. Sea and could be removed. More importantly, there is no reference from BOUM and PROSOPE cruises (the only three cited are missing in the list: Leblanc et al., 2010; Moutin et al., 2010; Bonnet et al., 2010). I wonder also if this manuscript is the only one presenting the nutrient/POC/PON.... data of these two cruises. If these data have already been (or are being) published elsewhere, provide appropriate references. Actually this could be a way to reduce the description of results and/or number of figures. By refocusing the discussion on Si cycle in Med. Sea, the paper should also take advantage of comparing the interpretation directly with other works from the same special Med. Sea issue (e.g., Pujo-Pay et al., Tanaka et al., Lopez-Sandoval et al., Mauriac et al.....) rather than papers from other areas of the ocean. Currently none of Med. Sea special issue papers is discussed.

Minor / technical comments:

- p.6795: What is the accuracy / reproducibility of the method for  $H_4SiO_4$ ? (only DL is provided)
- I would prefer the use of the term “silicicline” rather than “silicacline” since it refers to silicic acid ( $H_4SiO_4$ ) gradient and not silica ( $SiO_2$ ) gradient
- Figure 7 is cited before 6. Same for 9 before 8 and 14 cited before 13. A figure 16 is cited p. 6804 and probably refers to figure 15.
- Why diatoms species are provided in the “discussion” and not in “results” nor in “method”?

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