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Interactive comment on "Deep maxima of phytoplankton biomass, primary production and bacterial production in the Mediterranean Sea during late spring" *by* Emilio Marañón et al.

Anonymous Referee #3

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This MS addresses the ubiquitous subsurface feature, the deep chlorophyll maximum, phytoplankton biomass and production, and heterotrophic prokaryotic production in the Mediterranean sea's stratified water column during the later spring season (May 10, 2017- June 11, 2017). This subsurface feature in the world ocean is known for long, more prominent in waters of lower latitude, are often found at nutracline depth well below the remote sensing reach, thus supports the importance of seaboard measurements to capture this feature. Chlorophyll a, an indicator of the phytoplankton biomass, is regulated by light, nutrient, etc. Here, the authors mainly aim to quantify photoacclimation's relative role and enhanced growth as an essential DCM mechanism. Secondly, the trophic coupling between phytoplankton and heterotrophic prokaryotic pro-

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Discussion paper

duction is also addressed. Based on shipboard measurements in the Mediterranean sea, authors conclude that the DCM located at subsurface depth coincides with both biomass and primary production but not in growth rate and explains that the photoacclimation process leads to the increased chlorophyll a at the DCM. This study contributes vital insight into likely future ocean changes under the ocean warming scenario, thus merits publication of this work. However, I do not recommend a journal publishing this work in the present form. A few concerns about the methodology and the data interpretation need to be taken care of before considering this work for publication (see below).

Flow cytometry tool followed to obtain estimates of the carbon biomass in different size categories does not seem to have taken account of the autotrophic cells >150 microns in size neither their contribution is quantified, if minimal. The authors could have easily viewed these samples (>150micron) under the microscope to support the finding. If this were a significant observed, increased carbon biomass from the surface to the euphotic layer base would have been different and could lead to a different conclusion. The authors need to take care of this part in the section result and subsequently draw a conclusion at the end of the discussion section. Also, it is unclear whether definite size beads were run on flow cytometry to conclude the mean cell diameter used for carbon calculation. It is essential to show the reader the error introduced by assuming the mean cell diameter (2um or 4 um or 6um). On the other hand, while calculating Fucoxanthin to total chlorophyll a ratio calculation, I find authors have ignored that besides diatoms, Phaeocystis spp. are also potential sources of Fucoxanthin (see Latasa and Bidigare, 1998) instead accounted to diatom community. Furthermore, the presence of divinyl chl a, a marker for prochlorophyte, seems to have ignored and accounted for diatoms. Suggest authors revisit HPLC based pigment (depth-wise) analyses to rule out Prochlorococcus community is not missed out. In my opinion, low light-adapted Prochlorococcus at the DCM may be sizably contributing to the DCM community.

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In my opinion, this manuscript (MS) needs revision in context to the points discussed above in the second paragraph before considering this paper for publication.

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