

Interactive comment on “The Mediterranean subsurface phytoplankton dynamics and their impact on Mediterranean bioregions” by Julien Palmiéri et al.

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Received and published: 22 January 2019

The authors would like first to thank the Referee #2 for his/her effort in reviewing this study. The paper will very probably be rejected, but I would like to reply anyway to some of your comments. Working back on BGC-ARGO, to prepare the corrected version of the paper, I realised that the chlorophyll field needed corrections that were not included in the data-set I first used. The correction decreases the BGC-ARGO chlorophyll fields by a factor 2 as explained in Barbioux et al. (2018). This reduces the difference between observed and modelled chlorophyll concentration. The model Chl bias is then less important than said in the paper, but this has obviously no impact on

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the phenologies, on the too deep modelled DCM, or on the model-satellite differences. About the satellite data, we know there are newer data-set. We tried to do the analysis with Volpe et al. (2007). Mediterranean satellite product, but for an obscure reason we got weird artefacts on the clusters derived from it. As I could not get rid of these artefacts, I finally worked with the Bosc et al. (2004) Mediterranean data-set. Although using a Mediterranean satellite data-set enable to improve the Chl concentration with a better estimate of the CDOM, it “seems” to not have a big impact on the phenology (for instance D’Ortenzio and Ribera d’Alcala (2009) did their Mediterranean bioregion analysis using SeaWiFS – But the CDOM impact on phenology has not been shown as far as I know). About the model, we have to hear the critics, and accept the fact that it is not considered realistic enough for this study. We then have no other alternative than to find a better biogeochemical model and do the analysis again. But finding a more realistic model might not be obvious.

– References:

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-423>, 2018.