Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-423-RC2, 2018

Biogeosciences Discussions
Open Access
EGU

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

Anonymous Referee #1

Received and published: 28 November 2018

The authors wrote in their reply to my first comments:

'..he/she doesn't think the model used is appropriate for this study...and the NEMO model is maybe not appropriate to model the Mediterranean sea.'

I'm sorry if the authors misunderstood my previous review and the comment about NEMO. I wrote that '. . .make me wonder if maybe NEMO (at least in the current configuration) is an appropriate choice for making biogeochemical simulations in the Mediterranean Sea'. So what I am unsure is whether the NEMO configuration the authors are using is appropriate to perform biogeochemical simulations, given the already reported problems with vertical stratification strength and, hence, with the position of the nutri-

Printer-friendly version

Discussion paper

cline.

I fully agree in NEMO being a 'nice' hydrodynamic model that has been applied and evaluated in many instances to study Mediterranean Sea characteristics. I am not questioning previous works, I am just saying that the results about biogeochemistry and, particularly, the chlorophyll distribution the authors are showing are not accurate enough to make the analysis they propose.

They also make a couple of claims in their reply which I think are not fair and are not useful to the objective at hand (i.e., evaluate their own work). At some point they say: 'If you look all these other Mediterranean modelling studies, we do evaluate our model performances much more than it is usually done.' It could be discussed whether the model validation the authors perform in the present contribution is more thorough than in other previous works but we should never defend our own work by criticizing other colleague's publications, especially when those other works passed through a similar peer-review process you are undergoing right now.

Then, in another paragraph they say: 'A model will never be perfect in all aspects, and it is so regrettable to be penalized because we provide more evaluation than usually performed'. I am, again, sorry if the authors felt unfairly 'penalized' by my previous review but I try to do an objective evaluation of every work I review, exactly as I want my papers to be reviewed. Being a modeler myself I perfectly know no model is perfect and fully appreciate the effort to compare your model results with different sets of measurements. However (and unfortunately) these comparisons show that your simulations are, simply, too far off the observations to be useful (remember you are underestimating chlorophyll by 60-70% and the position of the DCM in more than 50m!). Not even the total chlorophyll in relative terms (your figure 11) is sufficiently close to the Argo data to be useful for the analysis.

Instead of arguing with me I should rather focus to find another simulation to perform the analysis as I do believe the primary claim by the authors (i.e., the Med bioregions

Printer-friendly version

Discussion paper

will change when DCM is considered) is basically right. You only need to have the right data to show that to the community.

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

Printer-friendly version

Discussion paper