Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-201-RC2, 2017

Biogeosciences Discussions

Interactive comment on "Temporal variability of chlorophyll distribution in the Gulf of Mexico: bio-optical data from profiling floats" *by* Orens Pasqueron de Fommervault et al.

Anonymous Referee #2

Received and published: 11 July 2017

The manuscript Temporal variability of chlorophyll distribution in the Gulf of Mexico: bio-optical data from profiling floats by Pasqueron De Fommervault et al. attempts to evaluate the temporal and spatial variability of chlorophyll concentration in the Gulf of Mexico. The study utilises data from eight bio-optical profiling floats. The paper addresses the winter increase in sea surface chlorophyll concentration and the impact of mesoscale eddies on phytoplankton biomass in the Gulf of Mexico. This is done by applying already published methods to a rather new dataset. The problem is that, most of the time, the methods cannot be applied or are not applied correctly. Consequently, most of the results presented in the manuscript are flawed. Additionally, the manuscript is poorly written, the arguments are hard to follow, and often not justified by the appro-

priate references. The amount of work needed for publication in Biogeosciences is considerably more than for a major revision.

Comment 1: One of the conclusion of the manuscript is that the winter increase in sea surface chlorophyll concentration is due a photoacclimation process or a re-entrainment of phytoplankton cells at depths. I felt like the authors have chosen to cop-out on testing one hypothesis over another. The authors have all the data necessary to investigate what causes the winter increase in sea surface chlorophyll concentration in the Gulf of Mexico. They need to do more than just commenting on the float observations.

Comment 2: The authors use the depth of the 6C isotherm to classify the eddies in the Gulf of Mexico. They argued that this isotherm has a mean depth of 795 m and that Bung et al (2002) found that this isotherm separates the deep stable water from the eddy-influenced surface water. Finally, it is said that Hamilton et al. 2017 found a strong correlation between the isotherm and upper layer eddies. First, Bung et al. (2002) did not identify eddies with this isotherm. It was used to delineate the depth of the Loop Current in the Yucatan channel. Consequently, the use of the 6C isotherm to identify mesoscale eddies cannot be justify by this reference. Second, the vertical extent of eddies core is comprised, in average, between 300 and 400 m. What is the rationale of using such a deep isotherm to detect eddies that impact the first 400m of the water column? Third, the authors claimed that Hamilton et al. (2017) found a correlation between mesoscale eddies and the 6C isotherm. However, this reference seems to be an oral presentation and again it cannot be used to justify the utilisation of the 6C isotherm.

Comment 3: The section on the impact of mesoscale eddies on phytoplankton biomass is largely inspired by the study of Dufois et al. 2014. The authors compared the float observations when the floats were profiling in a cyclonic structure with the observations when the floats were profiling in an anticyclonic structure. By averaging observations that were collected at different times of the year and locations in the Gulf of Mexico,

you are looking at signals that are both influenced by the seasonal and large scale variability and the mesoscale activity; this is not correct. To properly assess the impact of eddy signal on a given variable, one needs to look at the departure from the seasonal mean (see Cushman-Rosin, 1994). All sections about the role of mesoscale structures need to be changed.

Comment 4: The depth of an isopycnal surface cannot be used to determine a nitracline depth. In the open ocean, nutrient concentrations are controlled by both physical processes such as vertical/horizontal advection and diffusion, convection, and biological processes such as phytoplankton growth, remineralization, etc... At a pinch, Eq. (2) can be used to give a crude estimation of the nitracline depth for a quasi-1D steady-state system with a surface layer depleted in nutrients, with no change in solar radiation and mixed layer depth. In your case, these assumptions do not hold. Sections 3.3.3 and 3.3.3 need to be removed and the conclusion need to be changed accordingly.

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