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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 #1

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This manuscript examines estimates of the temporal and spatial distribution of chlorophyll concentration estimates in the Gulf of Mexico derived from eight profiling floats. These floats provided chlorophyll estimates by in vivo fluorescence. The authors have therefore collected a novel dataset.

The primary objective result of the paper is that the spatial and temporal patterns of surface chlorophyll concentrations derived from the profiling buoys confirms the temporal and spatial chlorophyll estimates observed by ocean color satellites since the late 1970's, and published on extensively. They confirm the seasonal deepening of the surface mixed layer depth in northern winter, in the interior of the Gulf of Mexico,

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and shoaling in summer. This has also been published on extensively. The causes for the seasonal surface increase and decrease in chlorophyll, and the spatial patterns defined by circulation have been also explained extensively in the literature over the past 20+ years.

So, in general, the paper finds similar temporal and spatial patterns (i.e., associated with circulation features) in the Gulf of Mexico as have many other people in the past.

The paper is well written in the sense that it flows well, has good prose, and is written in good English.

Yet there are several problems with the paper, which may be serious enough to warrant a very deep revision and withholding publication.

For example, I don't understand what the authors did to compute near-surface chlorophyll concentration from the float data. They say that they took the fluorescence profile, found the highest FLUO value found above 0.9 times the mixed layer depth (MLD) and extrapolated this to the surface (as per Xing et al., 2012). They calibrate this against an ocean color satellite-derived estimate of chlorophyll concentration multiplied times 1.5 the estimated euphotic depth. One problem with this approach is that they make the same assumption of Xing et al (who did a study in the Southern Ocean) that the vertical profile of chlorophyll observed is largely due to quenching of fluorescence, and that the DCM is therefore not 'real'.

The authors probably know that there are data collected and published since the 1960's-1970's to show that the DCM in the Gulf of Mexico is real and seasonal. I wonder if the XIXIMI-2 (July 2011) and XIXIMI-3 (February-March 2013) cruises used by the authors to obtain more than 900 water samples from 74 profiles also had some chlorophyll data?

There are DNA profiles, bacterial profiles, and actual spectrophotometric and HPLC observations that show that the DCA is real and not simply an in vivo chl fluorescence

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quenching artifact, as the authors observed. It is not clear to me whether the constant CHLtot seasonal cycle that they find is an artifact of the way they computed the vertical profile with the quenching correction.

It seems a major flaw in this paper is the conclusion that: "the present dataset reveals a vertically integrated content of chlorophyll which remains constant throughout the year, suggesting that the surface increase results from a vertical redistribution of subsurface chlorophyll or photoacclimation processes, rather than a net increase of primary productivity."

The problem is that the integrated water column productivity of a water column with a DCM is not the same as that same water column under a "spring bloom" condition, when phytoplankton biomass is high throughout the mixed layer. The literature is replete with actual measurements of primary productivity that show this. In my opinion, the ecological and biogeochemical interpretation that biomass is the same as productivity is a fatal flaws for this paper. The authors need to go back and fully investigate what mixing can do to phytoplankton blooming in the ocean. They need to review what chlorophyll represents (a crude index of biomass), what productivity is (a rate), and what other factors may play a role in changing these over time and space. What is amazing is that the authors consider past biological oceanographic studies and conclusions of observations in the Gulf of Mexico to be 'beliefs', and proceed to completely misinterpret the chlorophyll signal they observe. They interpret their observations to mean that there are no water-column integrated changes in chlorophyll AND in primary productivity in the Gulf of Mexico. This is clearly a gross misinterpretation of the crude biomass index data they collected. The authors did not exploit the data to make inferences on primary productivity (e.g. perhaps by looking at hour-to-hour and day-to-day changes in biomass). The authors should note that estimates of primary productivity and of chlorophyll concentration are also out of phase in time in the Gulf of Mexico. This has also been reviewed in the literature.

Another problem is the interpretation of nutrient data. The authors have a rich nutrient dataset with the density data computed from the buoy profiles and the nutrient data from the XIXIMI-2 (July 2011) and XIXIMI-3 (February-March 2013) cruises. The analysis of the density vs. nutrient data is very nice. The problem starts when the authors interpret the nutrient profiles in a biogeochemical and ecological manner. They assume that simply because we see a winter-time increase in chlorophyll concentration in the mixed layer, there also needs to be a clear, measureable signal in nutrient concentrations. Since they don't see this, they conclude that "there are no significant inputs of nutrients by vertical mixing to sustain significant winter new primary production (NPP)". This is incorrect. Nutrients will not be measurable as they are taken up by the phytoplankton. This has been published over and over in the course of the past half century or longer.

The authors seem to somehow dismiss biological oceanography theory in general, including historical knowledge of patterns of vertical distribution of chlorophyll concentration, how these vary in time, and how all this and oceanographic conditions (both biotic and abiotic) affect primary productivity.

Note: the reference: Heileman, S., and Rabalais, 2009, cited to provide a reference on the productivity of the Gulf of Mexico is not a reference for the characterization of productivity in the Gulf. It does not provide summary data. The authors should cite where the actual productivity data comes from that they use to characterize productivity in the Gulf of Mexico. The authors do this often– they cite relatively recent references (in the decade of the 2000's). When they cite earlier literature, they do this in passing and in a dismissive manner, not fully acknowledging that many of the points treated in this paper has already been discussed and explained previously. The problem is that, in doing this, they miss important background knowledge about the oceanography of the Gulf of Mexico. Also, the authors cite studies by Behrenfeld et al (2005), Mignot et al (2014), etc. as suggesting that all temporal changes in chlorophyll observed by satellite are due to changes in pigment concentration in phytoplankton cells. This may be part of what happens, but it is not an accurate characterization of the changes that occur in the Gulf that they measured.

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