

## ***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.**

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Please find below our reply to the questions and comments to our submitted manuscript from reviewer #1. They are presented in a point-by-point manner. The numbering of lines or pages refers to the submitted version of the manuscript.

(Reviewer) 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 de-

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rived 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, 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.

(Authors' response) We thank Referee #1 for his/her quick review and comments. Our intention in this manuscript was to address the basin-wide seasonal average of chlorophyll concentration in the water column (not only at the surface), which is, to our knowledge, not well documented in the Gulf of Mexico. We agree that this data set confirms some of the main aspects of what is known about the seasonal variability of surface chlorophyll. Our main -and we think- new contributions are a) to get time-series of full-water column chlorophyll vertical distribution measurements and b) to make the analysis of the seasonal variability at depth and its relation with the surface chlorophyll content. The reviewer suggests there are some serious problems in our paper. We address all the reviewer comments and criticisms in detail below, hoping they will answer the reviewer concerns and provide a better interpretation of our results.

(Reviewer) 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

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multiplied times 1.5 the estimated euphotic depth.

(Authors' response) Indeed, the first step of the procedure is to correct profiles for non-photochemical quenching (NPQ). We applied the method of Xing et al. (2012) which is actually implemented in the international BGC-Argo program and consists of extrapolating the highest fluorescence value encountered within the mixed layer up to the surface. Once the fluorescence profile is corrected from the NPQ, we determine instrumental gain and offset using ocean color satellite-derived estimates of chlorophyll concentration. For the comparison against satellite-derived estimate of chlorophyll concentration, the whole 1.5 euphotic layer is used instead of only surface records to minimize the error that would be induced by a wrong NPQ parameterization. Note that the whole procedure is described in detail in Lavigne et al., (2012) paper. So, our near-surface chlorophyll concentration estimates are based on currently accepted international standard procedures (see answer below too).

(Reviewer) 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 deep chlorophyll maximum (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 chlorophyll fluorescence quenching artifact, as the authors observed.

(Authors' response) We are aware that the calibration/interpretation of fluorescence measurements is critical, and this is why we paid a great deal of attention to the calibration of the data in the manuscript (section 2.2). The Xing et al. (2012) procedure has been validated and applied in various regions (e.g. BATS, HOT, DYFAMED) where

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a DCM is present. The relevance of the NPQ correction in these conditions/regions was specifically assessed in Lavigne et al (2012) which shows it has a positive and significant impact on the estimates of chlorophyll. That is the reason why we chose the Xing et al (2012) method for our study.

We would like to point out that we do not say that the DCM is not real or does not exist in the Gulf of Mexico. Quite the opposite, the manuscript shows how this DCM varies in concentration and depth both for the entire time series (Fig. 3) as well as seasonally in the climatological averages shown in Table 2. In fact, the NPQ correction only concerns the mixed layer and as a consequence, the impact of the correction on the fluorescence profile is generally limited to the surface and does not impact the observed DCM (except on the occasions when the mixed layer is deep enough to reach the DCM. Note that during XIXIMI 2 and XIXIMI 3 cruises no HPLC observations were made.

(Reviewer) It is not clear to me whether the constant  $CHL_{tot}$  seasonal cycle that they find is an artifact of the way they computed the vertical profile with the quenching correction.

(Authors' response) The vertically integrated chlorophyll ( $[CHL]_{tot}$ ) depends largely on the chlorophyll content at the DCM. Hence, the contribution of the NPQ correction (which is limited to the mixed layer) is small. This can be verified in Fig. 1 below, which shows times-series from one float (float number 02 in the submitted manuscript) with and without the NPQ correction. One can see the constant  $[CHL]_{tot}$  seasonal cycle is not an artifact of the NPQ correction.

(Reviewer) 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."

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(Authors' response) The reviewer is absolutely right. The sentence in the abstract (page.1 line 19) "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" is wrong. We thank the reviewer for noticing that, and the term primary production will be changed to biomass, which is what we actually meant and missed to correct before submitting. Note that this was correctly stated in our conclusion number 3 (page 12, lines 4-6).

(Reviewer) 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.

(Authors' response) We fully agree with the reviewer that biomass is not the same as productivity and regret the confusion that our mistake in the abstract caused. Indeed, chlorophyll measurements were described and interpreted in terms of biomass (see section 3.1 and 3.2) whereas primary production (and more precisely new primary production), was evoked when nutrients fluxes are estimated (section 3.3), with the aim to discuss it as a hypothesis and/or a possible mechanism (not as a direct result of chlorophyll measurements). In order to make this clearer, some changes will be made in the manuscript.

The conclusion number 4 (page 12, lines 7-11) will be rewritten:

"In addition, our observations show that the winter mixed layer is generally not deep

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enough to reach the nitracline. This suggest that, on average, only small amount of nutrients are potentially injected to the surface layer through vertical mixing, although some short time-scales events of important nutrient inputs associated to very deep mixed layers during winter storms cannot be discarded. This also suggests that nutrients supply by winter mixing is not necessarily the main cause of the seasonal surface chlorophyll variability, although it is difficult to say with certainty with our dataset". instead of: "In addition, our observations suggest that the winter mixed layer generally does not reach sufficiently deep to provide large quantities of nutrients to the surface (although some episodic events of [CHL]<sub>tot</sub> increase associated to very deep mixed layers produced by winter storms cannot be discarded). This result stands in contradiction with the current paradigm of an enhanced primary production in winter, triggered by nutrient input through vertical mixing."

Sentences in the section 3.3.2 (page 10 lines 21-23) "This is in full agreement with results obtained from [CHL]. Thus, the idea that winter production in the GOM is enhanced in winter by new nutrients availability may be a misconception." will be removed.

For the same reasons and given it implies of lot of suppositions and that it does not add significant information we will remove the sentence in the section 3.3.3 (page 11, lines 21-23): "Nevertheless, we can note that the estimated NPP in CG is higher than in AG by a factor  $1.13 \pm 0.02$ , on average, which is surprisingly close to the mean [CHL]<sub>tot</sub> ratio between CG and AG ( $1.15 \pm 0.08$ )."

(Reviewer) 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.

(Authors' response) The term "belief" used in the manuscript did not mean to disregard nor minimize previous studies at all. We agree term is not appropriate and we have changed "belief" to "interpretation" in the two sentences where it was used (page 1, line 16 in the abstract; page 2, line 8 in the introduction). We were trying to emphasize the

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fact that, in the Gulf of Mexico, the seasonal cycle of chlorophyll, at a basin scale, has been almost exclusively addressed using satellite measurements (see the review of Biggs and Ressler, 2001), which only provide surface information. To our knowledge, and prior to the deployment of BOEM floats, available chlorophyll vertical profiles in the Gulf of Mexico did not have the required spatio-temporal resolution to resolve the seasonal cycle of the DCM or the chlorophyll content within the water column at the basin scale.

(Reviewer) 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.

(Authors' response) As we answered above, we agree that we can only address biomass with chlorophyll data, and modifications have been done to make it clearer in the manuscript (see comment above). We indeed observed that the integrated content of chlorophyll does not show a clear seasonal variability, which we interpret as total biomass remaining constant throughout the year at a monthly timescales (which is consistent with the analysis of the bbp data shown in the supplementary material). In addition, the temporal resolution of our floats measurements (14 days) prevented us to infer hour-to-hour and day-to-day changes in biomass to estimate in an appropriate manner primary production. We are currently addressing this question using a coupled biogeochemical/physical model (NEMO-PISCES) with the objective to check to what extent the hypothesis of the present work is validated.

(Reviewer) 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 nu-

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trient 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, measurable 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.

(Authors' response) We agree with the reviewer's comment "Nutrients will not be measurable as they are taken up by the phytoplankton", and it is stated page10, line18. We thus will make the following modifications in section 3.3.2 in the hope this helps to clarify our arguments:

Page 10, line 13 "does not necessarily reflect" instead of "does not reflect".

Page 10 lines 14-22 (from "Fig. 7" to "(NPP)") is rewritten as follow "Fig. 7, which represents the monthly mean and standard deviation of the nitracline depth and the nitracline steepness, shows that ZN is always found at depth and does not show a clear seasonal pattern (regardless of the group). When MLD (Fig. 5) and ZN are compared, one can note, that the winter mixed layer is generally shallower than the ZN. Hence if we assume that large inputs of nutrients can only be expected when the MLD reach below the average ZN, it is likely that nutrients injections by vertical mixing, are low, even in winter. In our dataset, a MLD much deeper than the inferred ZN was observed only once (in AG), January 23th, 2014. During this event, the MLD reached 171 m (Fig. 4, the maximum value measured by the floats), and the [CHL]tot reached more than 60 mg m<sup>-3</sup>, i.e. twice the mean winter [CHL]tot value (i.e. 0.22 mg m<sup>-3</sup>). Apart from this event, it is likely that surface water are always nutrients depleted. Nutrient may not be measured in surface as they are taken up by phytoplankton. However, the fact that we

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do not observe NN accumulation in surface means that nutrient refueling is relatively small or at least slower than its uptake by biota.”

Section 3.3.2 (page 10 lines 21-23), see above.

Conclusion number 4 (page 12, lines 7-11), see above.

(Reviewer) 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.

(Authors' response) We have not found literature which address the seasonal variability of the vertical distribution of chlorophyll in the Gulf of Mexico. We would appreciate if the reviewer could point us to those historical articles, given that in the 90s Muller-Karger et al., (1991) stated:

“(.. .) in situ oceanographic data set for the Gulf of Mexico is still insufficient to address questions and processes affecting the distribution of biological and chemical properties.”

and recently (Muller-Karger et al., 2015):

“The waters of the interior of the Gulf of Mexico seaward of the continental margin continue to be seriously undersampled. (. . .). We were not able to derive a good chlorophyll concentration dataset from historical field observations archived at the NOAA NODC to compare with either CZCS, SeaWiFS, or MODIS chlorophyll estimates. Most samples in the Gulf of Mexico available at the NODC are from the northern and eastern shelf regions, with relatively few samples available from offshore waters. Thus, information derived from remote sensing is essential for characterization of the deep water areas of the Gulf.”

In fact, we are not aware of studies showing mixed layer depth climatologies based on direct measurements for the Gulf of Mexico either, since the ones available are indirect

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estimates based on models or parametrizations (Mandoza et al., 2005; Muller-Karger et al., 2015; Zavala-Hidalgo et al., 2014). In this paper we specifically focused on the measured MLD and considered the ecosystem from a bottom-up perspective. We agree that biomass is regulated by a wider range of processes (e.g. biotic processes) which are not directly addressed in our manuscript since to answer these questions would require other measurements that were not at our disposal. This is a limitation to our work which we were careful to mention in the manuscript (section 3.3.2 page 11 lines 19-23, section 4 page 13 lines 19-34). Hence, we consider that the new measurements of water column chlorophyll and bpp seasonal variability invite to new hypotheses that are worth exploring.

(Reviewer) 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.

(Authors' response) Heileman and Rabalais, (2009) is only used in the introduction as a general characterization of net primary production in the Gulf of Mexico. We would appreciate if you could provide us a more appropriate reference.

(Reviewer) 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.

(Authors' response) The main objective of our work was to study the chlorophyll variability in the water column at a basin scale and on a seasonal basis. Given the lack of time series of vertical profiles of chlorophyll in the Gulf of Mexico, except for in-

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direct measurements obtained from satellite or models, we would be very grateful if the reviewer could tell us which relevant bibliography we missed that addressed the seasonal cycle of the chlorophyll content at depth and at the basin scale using in situ measurements.

(Reviewer) 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.

(Authors' response) We agree that the observed changes may not only be due to photoacclimation, and we do not state that this is the only relevant processes involved. In the discussion section 3.1 (page 7, line 27-34 and page 8, line1-9) and conclusion number 7 (page 12, line 19-23), we also mention other possible mechanisms. However, this new dataset suggests that photoacclimation may be relevant and worth exploring. The above references of Behrenfeld et al., (2005), Mignot et al., (2014) are included in the discussion to support this.

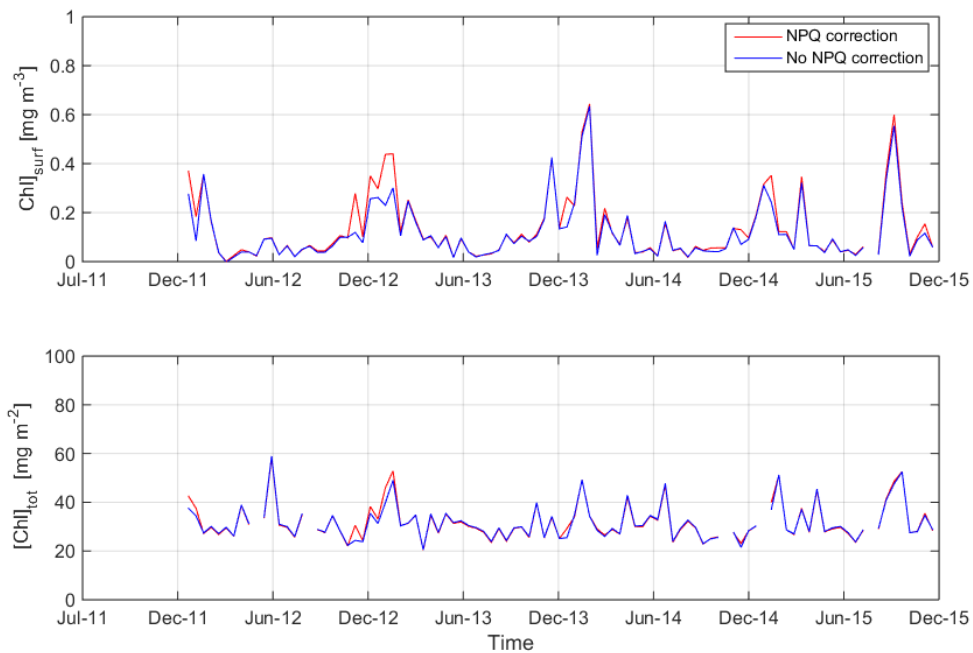
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**Fig. 1.**

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