

General comment

In this paper Schoeman and co-authors analyze a set of glider data collected off the west coast of Australia to (1) highlight the occurrence of DCMs and their formation mechanisms, and (2) establish relationships between surface Chl and Chl integrated in the water column.

Ultimately, this should make it possible to use satellite imagery to account for the distribution of the phytoplankton biomass within the whole water column (not just at surface as provided by satellite data) on which krill communities and whale foraging depend to a large extent.

The paper is interesting but, in my opinion, suffers from several weaknesses that should be corrected. First of all, there's a lot of uncertainty and imprecision in the method section that actually prevents a correct assessment of the analysis. Second, I find the focus on satellite validation rather inappropriate, given the data available and the overall purpose of the paper. On the other hand, I find that the analysis of the classification of the DMC profiles (DAM vs DBM), and their spatial and temporal distribution, is too little explored and discussed, even though this is the most interesting subject of the study. There's only one figure for this section and not a single vertical profile in the whole paper. Finally, I find the discussion quite interesting but, as it is presented, I have more the impression of a review of the literature than a real discussion. The results of the study are too little compared with the literature, so that it's hard to understand what the current study adds to what already exists, apart from the statistical relationships between surface Chl vs. integrated Chl, but that would be too little for a paper published in BG! Overall, I find the analysis of the results lacking strength.

Specific comments

Introduction

-l. 38-39 “DCMs predominantly form in equatorial to subtropical regions between 35° N and 35° S, with increased seasonality when moving away from the equator” etc. : The introduction doesn't do justice to the occurrence of DCM in the global ocean as reported in several previously published papers. DCMs comparable to those found in the subtropics are also present in temperate waters, permanently or seasonally (e.g. Mignot et al. 2014; Lavigne et al. 2015; Barbieux et al. 2019; Maranon et al. 2021...) as well as in high-latitude environments albeit resulting from different formation mechanisms (e.g. Holm-Hansen et al 2004, Uitz et al. 2009; Ardyna et al. 2013; Baldry et al. 2020, Boyd et al. 2024).

- l. 76 “that satellite remote sensing accurately reflects in situ conditions”: In my opinion, this objective is out of the scope of the present study. The validation of satellite-based Chl data against HPLC reference measurements in a given ocean region is highly dependent on the satellite algorithm (i.e. product) used for the exercise, and is not very powerful if not

interpreted in the light of contextual bio-optical properties to examine possible sources of error. See also my comment below in relation to the Discussion section.

- l. 79-81 “this study assessed temporal patterns in water column stratification, DCM formation, and DCM characteristics (i.e., type, depth, width); analysed the relationship between surface and depth-integrated chlorophyll values; and validated satellite-derived against in situ chlorophyll measurements”: The present study focuses a lot more on the relationships between surface and depth-integrated Chl than on the first and last topics. In my opinion the last objective (satellite Chl validation) is irrelevant to this study or should be presented in a different manner), and the first one (DCM formation and characteristics) could be enhanced.

Methodology

Section 2.1

- A map of the study region is critically missing with, eg, a satellite Chl as background, the main circulation features, and the location of the glider trajectories and in situ sampling.

-l.90-91 “all samples from waters <100 m deep were discarded to ensure only data from case 1 waters were included (i.e., water in which optical properties are driven by phytoplankton presence; Morel and Prieur, 1977)”: The 100-m bathymetry is threshold seems very empirical. How do you ensure this is ok? Typically, in the absence of bio-optical data, Morel & colleagues would use a criterion of 1000 m to ensure the case-1 water condition is satisfied. This could also be tested using a Chl vs bbp relationship that should show relatively strong covariation.

-Why the title of the subsection is “In situ chlorophyll data retrieval” while you have extracted from the glider database not only Chl data but also bbp data and probably T/S data for MLD computation?

-In my opinion, it is also important that you provide some information on the fluorescence and backscattering sensors implemented on the gliders. The fluorometer type (manufacturer and series) can be important when you discuss the relationship between Chl and fluorescence (see below); the angle at which bbp is measured can make a difference when you apply the method of Cornec et al. (2021) to identify the type of DCM.

Section 2.2

-l. 101-102 “Only profiles with at least one observation within the first 10 m of the water column and at least four samples at different depths were retained (Uitz et al., 2006)”: This criterion was defined for discrete HPLC measurements that can have very low (insufficient) vertical resolution. I fail to understand its relevance for glider fluorescence-based Chl values that are typically finely resolved on the vertical scale.

Section 2.3

- I assume that the MLD calculation is based on glider data but really there is no information on this.

-l. 120-122: Why considering two density criteria for computing the MLD and then which one was used and based on what principle?

Section 2.4

-l. 125-135: I don't understand how the methodology of Cornec, designed for application to continuous Chl profiles from BGC-Argo float measurements, could be applied to low-resolution profiles (cf. previous comment about the minimum of 4 data points per profile).

- The same comment applies to the bbp coefficient profiles that are used to determine the type of DCM. In addition, there is no information on how the Chl and bbp data have been processed and quality controlled.

- I assume that the bbp data were measured from the gliders. This also needs to be specified.

- l. 132 "Where backscattering coefficient data were available": How frequent bbp data are in your dataset. What are the implications of missing bbp data for your analysis?

-Please add "particulate" for bbp: "particulate backscattering coefficient" and maximum particulate backscattering coefficient".

- l 147-149. "While preliminary data analysis revealed a similar change in slope for stratified waters in this study, this change in slope appeared seasonal; thus, we carried out one regression analysis for stratified water conditions from September until April and one for stratified water conditions from May until August": Ok but then how do you deal with that seasonal transition? The seasonal shift from a season to the next may well (is very likely) to change on a yearly-basis. How does that affect the use of your model? What we would need to see is, for example, a representation of the seasonal cycle for each year of your glider dataset. Is each month well represented in general (no seasonal bias) and in each year (no interannual bias)?

- l. 156-157 "Replicated and depth profile samples were averaged to one measurement per station in time (i.e., Chl_{HPLC})": I don't understand why you averaged the Chl values measured at depth. I don't understand either why you considered the samples collected >10 m in this section (2.6) that focuses on a comparison between satellite and in situ measurements. I'm not sure either if this applies to HPLC or glider Chl data. Please clarify your method and objective here.

- l. 165-174: The temporal window used for the matchups could be specified/justified (although I don't think you need this section).

Results

Section 3.1

This section is a bit dry. It would be interesting/informative to readers who are not familiar with the study region to read a bit about the processes underlying the temporal and spatial changes in the MLD, distribution of the Chl, the krill productivity and whale foraging activity, and justification of glider transects (along with a map).

Section 3.2

- I find extremely problematic not to see a single Chl vertical profile in a study treating the question of DCM and the importance of accounting for heterogeneity of the Chl vertical distribution within the water column. Why not showing how profiles (or example profiles) from the different types look like? Average seasonal time series of Chl and bbp profiles would be great to see.

- Fig. 2 showing the seasonal distribution of the depth of the DCM with density is interesting but it could be complemented with a seasonal frequency distribution (% occurrence of DCM per month). The monthly % are given in the text but it would be more striking to visualize them in support of the results (eg "DCMs were common in stratified water conditions (~60 % of profiles; 3892/6438), where the formation followed a seasonal trend", l. 192-193))).

- l. 195-197 "Backscattering data were available for 1985 stratified profiles with a DCM, revealing that DBMs were more common over September–March (58–75 % of DCMs) than over May–August (23–38 % of DCMs)": In the Method section the authors mention that bbp measurements were available for 1995 of the profiles, hence ~50% of the 3892 profiles with a DCM. We have no idea how the available bbp data are distributed seasonally, we don't know if the profiles with both Chl and bbp measurements are representative of a full seasonal cycle in the study region and, thus, whether the temporal trend in the distribution of the DCM and the mechanisms responsible for the formation of the DCM (here DAM vs DBM) are robust. I insist on the importance of showing vertical profiles of Chl (and bbp when present) and indicating the time periods for which both Chl and bbp are available.

Section 3.3

- Please clarify how the Chl_{zeu} and Chl_{zeu2} values are calculated? Is it based on glider fluorescence data or HPLC measurements?

- I find the formulation “depth-integrated” and “deep depth-integrated” very awkward and suggest to reformulate it (especially true for the subsection title). You could simply say “water column-integrated” in the subsection title and, in the text, specify “integrated within the euphotic layer (0-Zeu) or twice the euphotic layer (0-Zeu2)”. That would be much clearer for the readers.

- Fig. 4: It would be informative to color the data points depending upon the depth of DCM or on the type of DMC in order to identify clearly what process introduces scatter in your regression. You could even merge Figs. 4a&c and 4b&d with a color code. The other indices (half-peak width, maximum Chl at DCM could also be analyzed).

Discussion

The bibliography presentation is interesting but relatively disconnected from the results of the present paper. It is thus difficult to determine how previous studies come in support of the present one and what the present study brings in terms of novel information. I have a feeling that the separated Results / Discussion sections do no help in this matter. I encourage you to cite your results more clearly with the corresponding figure number where appropriate.

- l. 313-314 “However, we may have introduced additional errors with our definition of the euphotic zone depth as $Z_{1\%}^{PAR}$ and simple extension to “twice the euphotic zone depth””: Error or scatter in the relationship?

- l. 323-324 “Finally, Roessler et al. (2017) recently found that factory-calibrated chlorophyll concentrations, as estimated by optical sensors, overestimate measured chlorophyll on average 325 by a factor of 2.”: The factor of 2-with strong regional variability- does not apply to any optical sensors but to the SeaBird (previously WETLabs) ECO series fluorometer. Is it relevant to your glider data? Please check and correct the sentence.

- l. 325-332: Regardless of the average factor of 2 specific to ECO sensors, the Chl-fluorescence relationship shows great regional variability due to physiological changes in phytoplankton cells (in relation to light, macro- and micro-nutrient availability) and phytoplankton composition (see for instance Proctor & Roesler 2010; Petit et al. 2022; Schallenberg et al. 2022). These factors of variability apply here regardless of the type of sensor installed on the gliders. This could add variability compared to previous studies based on either HPLC or spectrophotometric data that reflect the “true” Chl concentrations, i.e. not being affected by the physiology and/or composition of the phytoplankton cells. I would assume that the relationships between surface and depth-integrated values are affected by the variability in the response of the fluorometer. This should be discussed.

- l. 350-360: In my opinion, this validation exercise is of no particular interest in itself and would be better suited as the first methodological section of a large-scale spatial and temporal application of the surface vs. integrated Chl relationships developed here. This could be

presented in another separated paper. In addition, and maybe more importantly, the problem I can see here is that you have developed relationships using fluorescence-derived Chl data and you expect to apply them to satellite Chl data. The validation of satellite data against HPLC reference values won't ensure this application is robust.

Minor comments/Edits

- l. 29 "visual assessment of ocean color": is this the appropriate term (visual)? I think that "radiometric" for instance would be more appropriate.
- l. 67 "Z1%PAR": Subscript and exponent missing in this notation.
- l. 110 "Zmld": I find the notation "Zmld" very odd as it comprises the notion of depth twice, with "Z" and "d". I suggest the authors choose either "MLD" or "Zml".
- l. 232 "Roessler et al. (2017)": Only one 's' for Roesler.