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.

We greatly appreciate your expression of concern with regard to the clarity of the methods section, the balance of attention to the various data analyses, and the disconnected discussion. We agree that the classification of DCM profiles and their spatial and temporal distribution deserve more attention and have taken your comments into consideration to ensure that the revised version does not appear to lean mostly on the relationship between surface and water-column integrated chlorophyll. The discussion will be revised to put more emphasis on our results and what they mean/contribute to the existing field.

While the presentation of relationships between surface and water column values is not new, the Western Australian marine environment is unique with the poleward flowing Leeuwin Current and a wide continental shelf. In addition, while previous studies have confirmed the presence DCMs along the Western Australian shelf, little is known on the formation mechanisms (DBM vs DAM). We will put more emphasis on this aspect in the results and discussion.

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).

We can see how the first sentence of this paragraph diminishes the global occurrence of DCM's. We will modify this paragraph to acknowledge the widespread presence of DCMs and the different formation mechanisms.

- 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.

Thank you for expressing your concern. We have taken the removal of the satellite validation section into consideration. However, we do believe that this is an important section considering the overall goal of our manuscript, which is to encourage discussions on using water-column integrated chlorophyll estimates from satellite data in models that aim to understand/predict marine animal hotspots. We feel that pure focus on DCM formation and regression analysis leaves a gap, because what is the value if readily available satellite data isn't relevant in the first place? The OCI algorithm is commonly accepted for Australian waters, but a validation purely focussed on our study area has been missing. Hence, we consider it to be an important addition to our manuscript. Of course, the applicability of fluorescence based relationships on satellite data should be discussed.

- 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.

We appreciate your feedback regarding the apparent focus on surface and water column integrated relationships. We agree that the presentation and discussion of results related to the seasonal DCM formation patterns and DCM characteristics can be strengthened. Hence, we endeavour to revise the manuscript to provide a more balanced insight into the listed topics by providing additional figures on the DCM presence and type (i.e., DBM vs DAM). The discussion will be revised to provide a clearer outline of results and how these results contribute to our understanding of potential driving forces behind whale foraging activities in the area.

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.

Thank you for this suggestion. A map of the study area, including the Perth Canyon, ocean glider transects, and HPLC samples will be added to the manuscript.

-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.

The 100 m bathymetry threshold is based on depth limitations set in previous remote sensing chlorophyll studies in Australia (Antoine et al., 2020; doi: 10.26198/5e16a91249e7c), suggesting that waters >100 m deep at least approximate case 1 conditions. In addition, previous comparisons between in situ chlorophyll values and SeaWIFS satellite data in the study area found that SeaWIFS data fell within a ~36% error band for waters >40 m (Fearns et al., 2007; Koslow et al., 2008), decreasing to 13% for waters >300 m deep (Koslow et al., 2008). We, therefore, believe that the 100 m threshold is sufficient enough to limit the inclusion of case 2 waters. However, we agree that caution should be provided and writing “to ensure only data from case 1 waters were included” can be considered an overstatement. We will rephrase our sentence to a more conservative statement and include a section in the discussion on potential scatter introduced by this empirical approach.

-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?

Thank you for drawing our attention to this. We will change the subsection title to “In situ data retrieval” and amend the text under this subheading to prevent focus on chlorophyll data alone.

-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.

Absolutely. Information on glider models, fluorescence and backscattering sensors, and temperature/pressure/salinity CTD sensors will be added

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.

You are 100% correct. The data analysis presented here started as a larger project, including data from various platforms, such as moored stations with a coarser resolution to which the “minimum of four samples” rule applied. While this line of code was active when analysing ocean glider data, it did

not remove any profiles. After filtering data based on IMOS quality flags, the minimum number of samples per profile was 20, and the maximum resolution was 4.3 m. Note, this was before the removal of profiles that did not reach the euphotic zone depth. Hence, lines 101-102 are redundant. We apologise for this error and will remove this sentence from the revised version.

Section 2.3

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

Thank you for drawing our attention to the lack of clarity within the methods section. Indeed, MLD calculations were based on ocean glider data. To highlight this, we will include a subsection "Ocean glider data analysis" under which current subsections 2.3, 2.4, and 2.5 will be placed as subsubsections.

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

Good question. For extraction of the MLD, we first calculated the MLD based on the commonly used temperature difference criterion (in our case $\Delta T = 0.2$ °C) and the density difference criterion ($+\rho$=0.03 kg m^{-3}). Then we took the shallowest of these two calculations as the final MLD, following Boettger et al. (2018). While water density in the study area appears mostly temperature-driven, we did not consider it suitable to exclude the possibility of barrier layer formations for which the density criterion is more suitable. Especially along the continental shelf edge, where warm saline water from the continental shelf may interact with colder offshore water at times when the Leeuwin Current does not intrude the continental shelf.

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).

Please see our previous comment about this.

- 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 date have been processed and quality controlled.

Raw data processing and quality control were done externally, which provided data quality flags on which our filtering process was predominantly based. We will include a summary of this process in the methods section with reference to the more detailed documentation.

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

Thank you for letting us know that this needs clarifying. Our suggested adjustment of sub- and subsubsection titles will provide a clear distinction on what data originates from the ocean glider data sets.

- 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?

Good point, thank you for attending us to the lack of clarity. We will create supplementary material on the temporal and spatial coverage of chlorophyll and particulate backscatter data. Potential implications of missing data will be included in the discussion.

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

We will change this in table 1 and the text.

- 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 ship 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)?

Absolutely, it is impossible to define a set transition date from one season to the next. We know, for example, that north-westerly storms are downwelling favourable and induce vertical mixing, while post-storm restratification occurs when the wind slackens and turns southerly. While storms predominantly occur in June-August, the number of storms and the onset of the “stormy” season varies interannually. The effect of storm events on the chlorophyll distribution along the continental shelf of our study area has been investigated by Chen et al. (2020: 10.3389/fmars.2020.00287), but has not been studied as extensively for offshore waters. The effect of storm events and other environmental drivers on the relationship between surface and depth-integrated chlorophyll is something that needs to be explored further with a dedicated dataset targeting seasonal and interannual patterns. We will add a section to the discussion on this.

- 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.

Section 2.4 purely describes the process of validating satellite data against HPLC data. We will amend the subsection title to make this clear. Regarding data processing: Only HPLC samples collected within the top 10 m of the water column were used, and samples collected from the same station at the same time of day were averaged to one chlorophyll value (i.e., one average value for the top 10 m of the water column). We will re-write the first and/or second sentence of section 2.4 to clarify this.

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

We will re-rewrite the first two sentences of the second paragraph in section 2.4 to clarify that a temporal window of 24 h was used and a justification.

Results

Secton 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).

This would be interesting but is better suited for the introduction and discussion. We will therefore address these topics more in the introduction (based on what is known) and discussion (based on the new knowledge obtained here).

Secton 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.

Great idea. Average/example profiles for the three different water conditions (i.e., stratified summer, stratified mid-winter, mixed) or as a monthly pattern will be added to the manuscript.

- 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))).

Thank you for your suggestion to provide a better visual presentation of the data. We will add a panel to figure 2 to present the seasonal occurrence of DCM and the proportion of DBMs vs DAMs.

- 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.

Agree. Please refer to earlier statements on including vertical profiles in the manuscript text and supplementary material highlighting the seasonal and spatial data coverage.

Sec$on 3.3

- Please clarify how the Chlzeu and Chlzeu2 values are calculated? Is it based on glider fluorescence data or HPLC measurements?

Similar to our suggestion for the methods section, we will introduce a subsection titled "Ocean glider data analysis", under which the current subsections 3.1, 3.2, and 3.3. will be placed as subsubsections. This will provide clarity on what data analysis originated from which data set. The current title for subsection 3.4 will also be changed to include a reference to the use of HPLC data.

- 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.

Thank you for the suggestion. We acknowledge that there is room for personal preference around using the terms "depth-integrated" and "deep depth-integrated" chlorophyll. The manuscript was initially written with simple references to "depth-integrated chlorophyll over the euphotic zone" and "depth-integrated chlorophyll over twice the euphotic zone". However, after reading through our original draft, we found that it made sentences overly wordy, and it was unpleasant to read over consistent repetitions of these references. Hence, we opted to use two simpler yet still clear terms. We prefer to keep using the terms "depth-integrated" and "deep depth-integrated".

In terms of the subsection title, we think that water column integrated indeed suits better, so we will replace "(deep) depth-integrated chlorophyll" with "water column integrated chlorophyll" in the subsection titles 2.5 and 3.3.

- 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).

Thank you for the suggestion to colour-code the scatter plots. This is a great idea. We would like to suggest colour coding by DCM absence, DCM presence unknown (since Bbp data was not available for all Chl profiles), DBM presence, and DAM presence. We can certainly include results on the temporal patterns of the DCM half-peak width and/or maximum Chl at the DCM too.

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.

Thank you for providing your insight on the content of our discussion. We will revise the discussion for clarity and emphasise what our study contributes to the existing literature.

- 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?

Scatter! Appreciate you picking up on this malapropism.

- 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.

The ocean gliders used to collect the data for this manuscript were indeed equipped with ECO Puck WET Lab sensors. We will clarify the use of WET Lab sensors in our methods section and reiterate in the discussion that the findings by Roesler et al. (2017) pertained to the WET Labs ECO series.

- 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.

Absolutely. We will include a discussion section on the uncertainty introduced by the assumption of a constant chlorophyll fluorescence to “true” chlorophyll ratio.

- 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 separate 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.

Please refer to our earlier feedback under the introduction section.

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.

We believe that visual assessment is more appropriate as we are referring to the historical use of the Forel-Ule colour scale.

- l. 67 “Z1%PAR”: Subscript and exponent missing in this notation.

Thank you, this will be corrected.

- l. 110 “Zmld”: I find the notation “Zmld” very odd as it comprises the notation of depth twice, with “Z” and “d”. I suggest the authors choose either “MLD” or “Zml”.

Good point. We will change “Z_{mld}” notations in text and figures throughout the manuscript to the more commonly used notation “MLD”.

- l. 232 “Roessler et al. (2017)": Only one ‘s’ for Roesler.

Thank you for spotting this spelling error, this will be corrected.