General reply to editor and reviewers

Dear editor and reviewers,

We thank you for the opportunity to submit a revision and for your comments, which have helped tremendously to improve this manuscript. In addition to our detailed reply to specific reviewer comments, we would like to highlight that we have excluded some data points from our original data set. This is because those points were beyond the 4000 m bathymetry contour line and appeared to have a different seasonal pattern than the rest of the data. Although general patterns have remained the same, this has slightly changed original reported values and regression line parameters.

We look forward to hearing your feedback on the revised version.

Best wishes,

Renee and co-authors

Reply to reviewer 1

Brief Summary of the manuscript:
Schoeman and co-authors present evidence on the importance of monitoring deep-depth integrated chlorophyll-a (chla) and phytoplankton biomass based on over 9,000 glider chla fluorescence profiles collected from 2002 to 2022. They compare surface chla concentrations to chla integrated to the euphotic depth (depth-integrated) and chla concentrations integrated to twice the euphotic depth (deep depth-integrated). The authors derive relationships between surface and the two integrated chla concentrations for three different water column conditions: stratified summer, stratified winter, and mixed conditions. They describe the temporal patterns of deep chlorophyll maximum (DCM) occurrence and, within these DCMs, the patterns of deep biomass maximums (DBMs) versus deep acclimation maximums (DAMs). They also provide details on satellite matchups to surface HPLC chla samples and validation. The authors demonstrate that there are increases within the seasonal cycle of deep-depth integrated chla that are not recorded in the seasonal cycle of standard depth-integrated chla. These local deeper increases are biologically important for krill productivity and subsequent whale foraging times.

Overall impression:
The results presented in this manuscript are compelling and highlight the increasing importance of including the variability of chla with depth in studies. The study shows how the vertical structure of chla changes seasonally and provides further insight into the potential for monitoring depth-integrated chla using satellite remote sensing data. It suggests that parameters for relationships used should be updated for certain regions. If published, this work could stimulate further discussion about improving satellite-based estimates of depth-integrated chla where DCMs are present.

Overall, this manuscript is well-written and well-reasoned. However, there is a need for improved clarity within the method sections, such as the inclusion of a study region map. Additionally, figures could be improved, along with other minor suggestions detailed below.

Dear reviewer 1,

We greatly appreciated your input to provide a clearer methods section and improve figures. We have made modifications throughout the manuscript to correct these unclarities and have added more figures to support our results.

Please find our detailed response to your comments below.

Best wishes,

Renee and co-authors

Introduction:
Line 35: Perhaps provide a range or average of the depth to which satellites observe. A good reference here might be Zaneveld et al. 2005 - https://doi.org/10.1364/OPEX.13.009052.

We have added a range (2-39 m) based on Organelli et al., 2017.

Line 68: This sentence is a bit unclear to what depths was included in these other studies. Did they include deep oceanic samples? the "but" in the sentence makes it unclear and sound unfinished.

Thank you for pointing this out. We have changed “deep oceanic regions” into “open oceanic regions”

Methods:
In general, I'm missing some sort of map in the paper. For readers unfamiliar with the region, it would be helpful to have some sort of map showing the study area, some features mentioned in the text such as the Perth Canyon, and perhaps the locations of glider profiles included in the study (if not too crowded) and locations of HPLC samples that could be satellite matched.

Thank you for this suggestion. We have added a map of the study area containing a magnification of the Perth Canyon and all sample locations used. We have removed the satellite match-up part of our manuscript. Hence, the HPLC samples are no longer used in our manuscript and have therefore not been included in the map.

Section 2.1: Here the authors state that both the HPLC and glider datasets used are restricted to "between 04 July 2002 and 21 June 2022", please clarify the date ranges of each dataset. Perhaps I have misunderstood the data download process, but according to the AODN delayed mode glider data via the link provided, this online dataset only contains glider data from 2008? If I am mistaken please clarify and/or provide more details on how to download the appropriate glider data.

That is correct. The date range has been adjusted accordingly.

Section 2.2: Somewhere in this section the reader should be reminded of the total number of glider profiles used after the filtering steps described here. Before this it is only mentioned in the abstract as "~9600" and then again at the beginning of section 3.1 as "We extracted 6438 and 3234 profiles from", unless I missed it.

After careful consideration, we have added the number of samples available after the initial filtering process (i.e., after filtering based on data quality, time of day and water column depth) to the final sentence in section 2.2.

Line 104-105: It would be interesting to see the authors speculate/comment somewhere here or in the discussion (see comment below) on the potential use of daytime satellite data to infer depth-integrated chla based on relationships derived from nighttime chla profiles. Could this be a limitation that should be acknowledged in the final paragraph of the discussion?

The effect of non-photochemical quenching (NPQ) on fluorescence chlorophyll readings is the biggest challenge when using daytime ocean glider data, especially in areas with high solar radiation, such as Western Australia. In the absence of strong tidal effects (max 0.6 m), which could result in short-term diurnal changes in the phytoplankton community, we believe that night-time vertical profiles are representative of day-time conditions.

Section 2.3 Line 115: Perhaps I have missed or misunderstood, please clarify here or elsewhere what is meant by profiles that do not cover the Zeu? Have any steps been taken to ensure that profiles cover to a depth of Zeu x2 for the calculation of Chlzeu2?

Profiles were required to reach the euphotic zone depth so that, at minimum, depth-integrated chlorophyll over the euphotic zone could be calculated (i.e., Chl_{zeu}). Since the calculation of Z_{eu} is described under Section 2.3, the removal of profiles that did not reach Z_{eu} is mentioned here. Chl_{zeu2} was only calculated for those profiles that reached twice the euphotic zone depth, and all profiles that didn't were only excluded from the regression analysis for Chl_{zeu2}. We have clarified in section 2.5 that Chl_{zeu2} was only calculated where possible.

Results:

Section 3.1: It would be easier for new readers to follow these descriptions and get a better idea of the MLD and Zeu characteristics of the study area if there were a plot somewhere here in the main text or in a supplement. Perhaps seasonal box plots or line plots with depth on the y-axis showing both average MLD and Zeu over the seasonal cycle? This might also provide a better background and link to Figure 1. Adding a panel to Figure 1 could also be an option.

Thank you for your suggestion. We have expanded this figure (now Figure 3) with an extra panel to show the median euphotic zone and mixed layer depth with their interquartile ranges.

Figure 1: I find the addition of the density plot without a y-axis at the top of the figure a bit out of place. Perhaps this seasonal distribution should be shown as a secondary axis in Figure 1 or in

another panel. See also other comments below about the addition of a plot showing the annual number of profiles and HPLC samples satellite matched over 2002-2020.

We understand that the density plot was difficult to interpret without any clear axis, and we would like the figures to speak for themselves. We have added a top panel to now Figure 3 with a numeric y-axis to reflect the profiles used for each month and the number of profiles for which the mixed layer depth count be calculated. The latter as some profiles did not reach the bottom of the mixed layer and so, MLD could not be calculated.

Section 3.2: This whole section will be clearer to a new reader if it shows visually the proportion of DBMs vs. DAMs over a seasonal cycle. Perhaps a figure similar to Figure 1. Either as a supplementary figure or possibly as an additional first panel to Figure 2.

Because we removed some offshore profiles, the number of profiles for which backscatter data was available decreased. In addition, after making a slight adjustment to the profile smoothening process, some of the DCMs were removed. Consequently, we identified that there were very little DCMs in winter in general, and even less DCMs for we could identify the "type", making the inclusion of a figure with seasonal proportions of DBMs vs DAMs inappropriate. We have therefore not include a figure as such, but we have included a supplementary Figure on the properties of DBMs and DAMs for those months with sufficient data.

Line 193: Add reference to Figure 1 at end of sentence.

This sentence referred to the seasonal cycle in DCM formation, which we believe is better presented by now Figure 3 and a reference to this figure has been made.

Line 217: Perhaps remind reader here that surface chla concentrations referred to here is the Chlzpd defined in the methods.

Good idea. Reference to Chl_{zpd} has been provided.

Line 221: Remind reader that it is Chlzeu being describe here and similarly in Line 222 that Chlzeu2 is being described.

Absolutely. Reference has been made to Chl_{zeu} and Chl_{zeu2}.

Figure 3: Increase size of panels numbers (here and all other multi-panel plots) and reduce the amount of blank space at the top of panel (a). Also revise the width of the multi-panel plots with months on the x-axis, here the month text labels are too close together in my opinion and look better in Figure 2.

Thank you for giving pointers to improve the cosmetics of Figure 3 (now Figure 5). We have changed the month labels to be presented at a 45 degree angle. The y-axis range of panel a has been drecreased.

Figure 4: I suggest making it clear within the figure panels which is summer and which is mid-winter, perhaps with annotations Stratified: summer; Stratified: mid-winter? I would also find it easier to follow the text and compare relationship visually if Figures 4 and 5 were combined into one figure. Perhaps into a figure with 3 columns and 2 rows; a-c showing the Chlzeu relationship and 2nd row d-f showing the Chlzeu2 relationship, with an annotation "Mixed" added to panels showing the relationship in mixed conditions. Also include the definition of stratified vs. mixed in the caption to remind the reader, e.g. Zeu<Zmld.

Thank you for your suggestions. The figures have been combined into Figure 6 with headings to indicate the different conditions. The definition of mixed and stratified has been reiterated in the figure caption.

In figures with regression lines, I suggest to extend lines to edges of the plots.

We did play around with the length of the regression lines before, but because of the differences in slope, some regression lines ended up running off the sides and others at the bottom. This made the figures look messy, so we decided against extending regression lines to the edges. However, we have extended the regression lines as we could see that some only just covered the plotted data range.

Figure 6: Add units to each axis.

This figure is no longer included in the manuscript.

Discussion:
Line 330: Add the value of the R2 in the parenthesis.

This sentence has been removed.

Line 350: Related to an earlier comment, this part of the discussion could be a good place to perhaps discuss or acknowledge the impacts/limitations of using night time relationships of the surface with depth-integrated values if satellite surface values possibly used in the future are during day time.

Please refer to our earlier feedback on the use of night-time profiles.

Reply to reviewer 2

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.

Dear reviewer 2,

We greatly appreciated 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. They have been invaluable to the improvement of this manuscript.

We agree that the classification of DCM profiles and their spatial and temporal distribution deserved more attention and have added some clarification on this to the methods. We have also added supplementary figures to 1) highlight data availability on a temporal scale and 2) provide a spatial presentation of data for each month of the year.

We have added figures to present our results regarding the DCM width, peak chlorophyll concentration, and depth relative to the euphotic zone and mixed layer depth and of course, figures with vertical chlorophyll profiles for each season used in the regression analysis and each month. Vertical profiles of chlorophyll and backscatter data have been added to the supplementary material. The discussion has been re-written to put more emphasis on our results and what they mean/contribute.

Moreover, after all changes made, we agreed more and more with your comment on the inappropriate inclusion of in-situ vs satellite data matching. These sections have therefore been removed.

Please find below our detailed reply to all your comments.

Best wishes,

Renee and co-authors

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 diminished the global occurrence of DCM’s. We have modified this paragraph to read:

DCMs are observed throughout the global oceans, with a year-round and consistent presence in tropical and most sub-tropical regions (Mignot et al., 2014; Bock et al., 2022; Quartly et al., 2023). Seasonal patterns of occurrence become more evident from temperate to high-latitude regions (Cornec et al., 2021), during which DCMs are present in summer but tend to break down or occur less frequently in winter (e.g., Mignot et al., 2014; Baldry et al., 2020; Bock et al., 2022). In well-studied tropical and temperate regions, DCM formation has been linked to permanent or seasonally stable stratified water conditions (Cornec et al., 2021), with light and nutrient availability driving the formation of true phytoplankton biomass maxima (i.e., deep biomass maxima, DBM) and deep photo-acclimation maxima (DAM; Mignot et al., 2014; Cullen, 2015).

- 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. After re-evaluation of the direction of this manuscript and adding extra figures and content, we agreed more and more with this comment. Consequently, we have indeed decided to remove these sections from the revised version.

- 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 could be strengthened. Hence, we have put more emphasis on seasonal patterns of DCM presence and characteristics in both the results and discussion.

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 a magnification of the Perth Canyon, and used ocean glider vertical profiles locations has been added as Figure 1.

-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 was 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, an addition reason for including data from the 100 m bathymetry contour line is to ensure the inclusion of data points around the Perth Canyon head, which is an important foraging spot for pygmy blue whales. We have rephrased our sentence and highlighted that our dataset choice was based on known migration corridors of pygmy blue whales.

-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 have changed this heading into “In situ ocean glider data retrieval” since we no longer make use of the HPLC data.

-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. Details on ocean gliders, flourometers and backscatter sensors have been added:

All ocean gliders were equipped with a Sea-Bird Conductivity-Temperate-Depth sensor (CTD: models CTD41CP, GPCTD, or SBE_CT) and a WET Labs ECO Puck optical sensor pack (models BBFL2S, BBFL2VMT, FLBBCDSLK, or FLBBCDSLC), including a fluorometer and backscattering sensor (650-700 nm, 117° centroid angle).

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 have removed 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. However, since we no longer make use of data collected via other platforms, we trust that this is clear in the current version.

-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 (+ ρ=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 (by IMOS), which provided data quality flags on which our filtering process was predominantly based. We have clarified in Section 2.1 of the methods what that quality control entailed:

All downloaded data were pre-processed and quality-controlled by IMOS, which included the conversion of raw sensor counts into chlorophyll and particle backscatter coefficient (bbp) parameters with instrument-specific calibration coefficients and dark count values (Mantovanelli and Thomson, 2016; Woo and Gourcuff, 2023). Chlorophyll dark count values were corrected for any mission with >1% of negative values (Woo and Gourcuff, 2023). Quality control processes included automatic sensor drift corrections; automatic flagging of impossible location, date, and range values; manual flagging of measurements affected by biofouling or sensor malfunction; and manual flagging of near-surface measurements (<0.5 m).

- 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 needed clarifying. As for the above comment on the MLD calculation from ocean glider data, we no longer make use of any other datasets and trust that this is clear in the current manuscript.

- 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 have created supplementary material on the temporal and spatial coverage of chlorophyll and particulate backscatter data. The lack of consistent temporal and spatial coverage is attended to in the discussion.

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

We have changed 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.

- 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 has been removed and this comment is no longer applicable.

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

Section 2.4 has been removed and this comment is no longer applicable.

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).

We have added some extra background information to the introduction regarding the effect of the Leeuwin current, winter storms, and eddy activity on mixed layer depth as well as when the pygmy blue whales forage in the Perth Canyon. This information is reiterated in the discussion to help interpret our findings and their meaning for further research.

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.

We apologise that this was lacking from our previous version. Of course this should be included. We have included seasonal and monthly chlorophyll vertical profiles as well as chlorophyll and backscatter profiles in the supplementary material for those months in which there was sufficient backscatter data.

- 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))).

The seasonal presence of DCMs is now reflected in Figure 3.

- 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 previous comments. This unclarity should no longer be an issue.

- 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. We have incorporated this comment as shown in Figure 6. We have also added additional figures and result sentences regarding the DCM width, maximum chlorophyll, and distance of the DCM relative to the euphotic zone and mixed layer depth.

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. The discussion has been completely rewritten to provide a more meaningful interpretation and discussion of our results. For clarity, we have included several subheadings in the discussion too.

- 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 have clarified this in the methods.

- 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 have included a separate heading in the discussion (4.5. Future habitat models and potential pitfalls) to highlight uncertainties around our results and further research required.

- 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.

This section has been removed.

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 has been 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 have changed "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. Corrected.