

Summary

The authors of this work introduce a new ocean monitoring indicator to track the temporal evolution of seven marine bioregions—characterized by similar phytoplankton biomass seasonality—using satellite chlorophyll-a data from 1998 to 2024. They observe a long-term poleward shift of less seasonal phytoplankton patterns, linked to climate change. This approach offers insights into how marine ecosystems are adapting over time.

General comments

I acknowledge the substantial work and the effort made to synthesise complex information. The analysis conducted by the authors is robust, and the results obtained are meaningful. I believe that the proposed indicator could provide added value to the set of indicators currently available in the Copernicus Marine Service Catalogue. I also appreciate the effort to estimate a form of associated uncertainty, as well as the discussion of the indicator's limitations and its potential for further integration with other products.

I recommend minor revisions before the publication in Ocean State Report 10.

In particular, since the present paper is an extension of a companion study (i.e., Baudena et al., 2025), I suggest briefly recalling some key concepts from that work and making more explicit the differences between the two studies. In addition, I recommend improving the presentation of the results, both in the main text and in some figures and their captions.

These issues, along with other minor points, are addressed in the *Specific Comments* section below.

Finally, although I am not a native English speaker, I believe the manuscript could benefit from a language revision to improve clarity and fluency.

Specific Comments

Line 58: Does the term “update” refer to the climatology derived from Baudena et al. 2025? In that paper the climatology used is 1998-2022 (Figure 1 of that paper), whereas in your study you are considering 1998-2024. If so, I suggest making the different time periods explicit.

Line 66: “therefore compensating for the lack of observation inherent to satellite ocean color (Stock et al., 2020).” I understand the advantage of using L4 for this kind of study, but Baudena et al. 2025 use L3 satellite, along with an interpolation method. The authors declare in the Introduction that this study is an extension of Baudena et al 2025, since it is finalized to track variations in time in the extension of the bioregions, therefore I think that the choice of using L4 instead of L3 data should be (at least shortly) discussed in connection with that paper. I suppose that any possible differences are small, but I suggest clearly addressing this aspect in the text. Finally, I suggest specifying the spatial resolution of the product used.

Line 79: “within 111 km” I suggest an explicit reference to the 1° grid box mentioned in line 72.

Line 97-98: Here, as in other lines of the 2.3 paragraph, the authors cite some figures they show only in Section 3. I understand that, in some cases, it can help to illustrate some specific choices adopted in the methods (as done at lines 110-116, for Figure 1b), but I suggest doing it only if it is

strictly necessary. In particular, I think that the citation of Fig. 2b here can be deleted, as well as the ones at line 102 for Fig. 2a and Fig. 4a. This would also allow to cite Fig. 1b as the first one in the paper, i.e., it would avoid citations of Figures that do not follow the order Fig. 1, Fig. 2, and so on. Finally, I suggest formally rephrasing the sentence at 97-98 as: “The frequency of phenology change refers to the number of years within the last 26 years during which a pixel changed its assigned bioregion”.

Line 117: In the short paragraph 2.4 authors discuss not only the uncertainties related to the clustering method, but also the approach to identify long-term changes. I suggest modifying the title of the paragraph accordingly to better reflect its content.

Line 129 (general comment on paragraph 3.1): Even if the seven bioregions are illustrated in detail in the companion paper, I think that it could be useful just to recall the biogeochemical interpretation of the bioregions when they are mentioned in the paragraph, as done in the Conclusion section of Baudena et al., 2025 (e.g., bioregion 3 can be interpreted as “subtropical winter nutrient-limited”, or bioregion 4 as “bloom”). I think it could help to even more appreciate the importance of the detected cascading changes (e.g., from bioregion 4 to 3) highlighted in the Discussion and conclusion Section.

Lines 135-136: The citation of Fig. 2a is not clear to me here. I suggest deleting “(Fig. 2a) ,”.

Lines 137-138: I suggest replacing “bioregion 1” with “cluster 1” and rephrasing “to those with pronounced single or double peak events typical of blooms in temperate and high-latitude systems (bioregion 4-7)” as “to those with pronounced single or double peak events (clusters 4-7), which are typical of blooms in temperate and high-latitude system, as we found in Fig. 2a”.

Line 139: I suggest replacing “As demonstrated by Baudena et al. (2025)” with “In agreement with Baudena et al., 2025”.

Line 144 “and the highest in the high-latitude bloom regions”: please add the reference to the corresponding bioregions. Moreover, since the authors (rightly) distinguish between the oligotrophic and mesotrophic group of cluster 1, I suggest adding here (i.e., in the part of general illustration of Figure 2a) also a sentence in the text about their spatial distributions. This is a (small) difference with respect to the corresponding Figure 1 in Baudena et al., 2025 and I think that it should be at least mentioned.

Line 154: I suggest rephrasing the first sentence as: “The boundaries of bioregions in Fig. 2a are not static” and replacing “changes of bioregions” as “changes in the bioregion of affiliation” or something similar.

Line 159: Since black dots in Figure 3 represent estimates of the bioregions’ areas along the years and with varying acceptance threshold, I suggest specifying here “(time evolution of black dots)” instead of simply “(black dots)”.

Line 161-162: Please better clarify the comment about bioregions 6 and 7, e.g., quantifying the temporal variabilities.

Figure 3 (plot): I suggest using “Area of Global Ocean (%)” in the label of the y axes. Moreover, to better highlight interannual variabilities, I also suggest zooming the y axis of the single panels around the dots and lines represented, instead of making it always start from zero: I think that it could help the readability of the figure (which also has many panels).

Line 164: I suggest using “bioregions’ surface areas” or “bioregions’ areas” instead of “bioregions’ surfaces” and “bioregion area” instead of “bioregion surface”.

Line 168: I think that “Note the varying coordinate scale” could be ambiguous. I suggest using “Note the varying y-axis scale across panels”.

Lines 170-172: Since here the authors are commenting Figure 4a, which is the starting point for the following considerations, I suggest moving within this (Results) section Figure 4, that is currently in the Discussion and conclusion Section. Moreover, I suggest moving here (i.e., within the Results Section) the comment to Fig. 4b which is currently in the Discussion and conclusion Section (lines 187-189). Finally, I suggest rephrasing “Stable changes... stable conditions”, as the use of the term “stable” in case of both phenological changes and “reference” conditions may cause ambiguity.

Lines 180-182: Are these estimates reported as medians, along with interquartile ranges? The value for the South Atlantic Ocean is given as 0.20% within a 0.21%-0.21% range and this seems inconsistent or possibly a typo. What does the bold text indicate and why are the percentages associated with “3 to 2” in the Atlantic Ocean and in the South Pacific Ocean not in bold (they are similar to other percentages that are in bold)? I also suggest using “area” and “areas” instead of “surface” and “surfaces”, respectively, and “grouped by” instead of “divided per”.

Line 195: I would add here (or elsewhere in this Section) a comment on the uncertainties associated with the proposed indicator.

Line 201: I think it should be written “27-year time series” (since the time period is 1998-2024).

Line 211: Since also Global ocean hindcast is a 4D (modeling) product, I suggest “and other four dimensional products”.

Line 217 (Figure 4): I suggest to better balance the dimensions of panel (a) and panel (b).

Technical corrections

Lines 12-15 “While the bioregions’ extent... subtropical bands”: I suggest adding commas after “variability” and after “phenology” and replacing “are generally” with “is generally”.

Line 55: I suggest using “dynamics” instead of “dynamic”.

Line 61: The second time period indicated should be “2015-2024”. I also suggest that the comma be moved from before to after the parenthesis.

Line 86: I suggest using “twenty-six” in place of “twenty six”.

Line 100: I suggest replacing “partitioning” with “partitions”.

Line 113: I suggest specifying “(below the threshold)”.

Line 182: I suggest adding a comma after “less than 0.1% of the global ocean”.

Lines 187-189: This sentence should be split in two.

Line 208: I suggest replacing “is behaving” with “are unfolding” or something similar.

Lines 218-219: I suggest replacing “identified contoured in black” with “identified by black contours” and rephrasing: “(b) Latitudinal distribution of bioregions between the 2015-2024 decadal period, filled, compared with the 1998-2007 decadal period, indicated by the red line.”