

Review report of Riom et al. “Three decades of marine bioregions evolution through analysis of phytoplankton phenology”

This manuscript addressed an important point in large-scale ocean monitoring: distinguishing interannual variability from persistent, phenology-driven shifts in marine bioregions. The approach used supervised clustering on satellite derived chlorophyll-a products by analysing the phenologies and suggested a poleward extension of less seasonal phytoplankton phenology. The manuscript is mainly clear in conception, methodology, and well-grounded in a previous literature (Baudena et al. 2025). Overall, the study is meaningful and provide new insights on interpretation of marine phytoplankton data sets. I suggest a moderate to major revision for this manuscript. My comments are summarized in the following aspects:

- 1) the need for clearer methodological justification
- 2) a clearer connection between bioregions and underlying environmental drivers
- 3) some organization and wording improvements for readability
- 4) minor corrections in citations and phrasing

Major Comments:

1. There are two satellite CHL products available on Copernicus Marine Service – GlobColour version and OCCCI version both derived from the merged sensor products. The authors have apparently used the GlobColour version but L4 products where CHL is gap free by applying a gap filling method by the GlobColour consortium. This is not indicated in the study.
2. Copernicus CHL product is with 4x4 km resolution but you had clusters with 1x1°, probably it was performed following Baudena et al. 2025 but a clear explanation or a methodology pipeline is necessary.
3. Another major concern is that it is well recognized that CHL products from OCCCI and GlobColour can generate contrast trends (Pauthenet et al. 2024) and OCCCI has better inter-sensor consistency (Zhai et al. 2024). Xi et al. (2025) in OSR9 has tried to tackle this problem for Copernicus ocean color data sets of phytoplankton functional types. I believe this could also bring inconsistencies in the time series clustering in this study however the author did not cover this point and did not justify either why L4 GlobColour product was selected but not L3 or OCCCI.

Pauthenet, E., Martinez, E., Gorgues, T., Roussillon, J., Drumetz, L., Fablet, R., & Roux, M. (2024). Contrasted trends in chlorophyll-a satellite products. *Geophysical Research Letters*, 51, e2024GL108916. <https://doi.org/10.1029/2024GL108916>

Zhai, D., Beaulieu, C., & Kudela, R. M. (2024). Long-term trends in the distribution of ocean chlorophyll. *Geophysical Research Letters*, 51, e2023GL106577. <https://doi.org/10.1029/2023GL106577>

Xi et al: <https://doi.org/10.5194/sp-6-osr9-7-2025>

Clustering methodology needs clearer justification

4. Line 74 ‘To dampen sub-seasonal variations, time series were all smoothed with a 5 weeks centered rolling average method..’ it is not clear why 5-week smoothing window has been chosen. How does this choice influence detection of narrow bloom events (e.g., subpolar spring bloom)? Moreover, there are also mathematically more accurate ways to deseasonalise time series such as LOESS (STL; Cleveland et al., 1990) which is commonly used to decompose trend and seasonal variations.

5. You vary acceptance thresholds (26 thresholds) to capture methodological uncertainty, however, several aspects remain unclear: Why 26 thresholds? A brief methodological rationale (e.g., spacing, range definition, prior sensitivity tests) would strengthen confidence.

How sensitive is the partitioning to threshold choices? You mention $\pm 4\%$ classification uncertainty, but no examples of spatial sensitivity are shown. A supplemental figure could show how cluster assignment changes across extreme thresholds.

L79 how this 111 km has been determined? Is it related to the coarse resolution of the CHL product?

Why seven clusters specifically? Although inherited from Baudena et al. (2025), briefly re-emphasizing ecological or statistical reasoning would improve clarity.

Again, as mentioned in my above comment 2, reading Section 2.2 it is uneasy to get a really clear schematic about the whole methodology, I suggest a flowchart illustrating better the analysis pipeline.

6. Section 2.4 (uncertainties) could be reorganized to first explain the conceptual approach, then list statistical tools – for now it is not very much clear.
7. Treatment of uncertainty in area trends: The use of interquartile range and Spearman correlation is reasonable, but some aspects need elaboration: The area uncertainty of bioregions 6 and 7 is " $\approx 50\%$ "; this raises the question of whether area trends are meaningful at all for these regions.
Clarify whether the trend significance accounts for serial correlation in time series or the uncertainty distribution across thresholds. If not, these limitations should be acknowledged.
8. Interpretation of phenological transitions can be strengthened by adding some environmental context: eg. show coincident trends in stratification indices, SST, or nutrients from independent datasets such as model simulations or literature. This strengthens the claim that phenological changes are climate-driven rather than methodological artifacts.
9. The "stable change" criterion could be better justified. You choose ≥ 6 years per decade to identify a stable phenology shift. This choice is reasonable, but: Why 6 years and not 5 or 7? Could year-to-year autocorrelation bias persistence estimates? A brief sensitivity analysis (even qualitative) would enhance confidence.
10. Bioregion 1 includes oligotrophic/mesotrophic subdivisions. This is a meaningful addition however I suggest to describe the two sub-regions more clearly (Fig. 1b only briefly mentions a bimodality) and state whether the split was used purely for interpretation or also within the clustering procedure.
11. Limitations section is strong but could expand on satellite products bias as mentioned above in comments 1-3.

Minor Comments

Language check throughout the manuscript is necessary, such as:
L8 “these evolution” → “these evolutions”

L9, Rephrase “monitoring indicator approach”.

L13 “bioregions’ extent appear” → “bioregions’ extents appear”.

L36 “limits growth” → “limit growth”.

L49-50 “Which are often used” → “which are often used”.

L108 “This temporal criteria” → “This temporal criterion”.

L61: “(1998–2007 and 2025–2014)” is likely a typo for 2015–2024.

L97-98: rephrase.

Fig 2b 2a and Fig 4a were first mentioned other than Fig.1? This is not commonly seen in articles.

Some citations have small inconsistencies (e.g., Flawosky → Falkowski?).