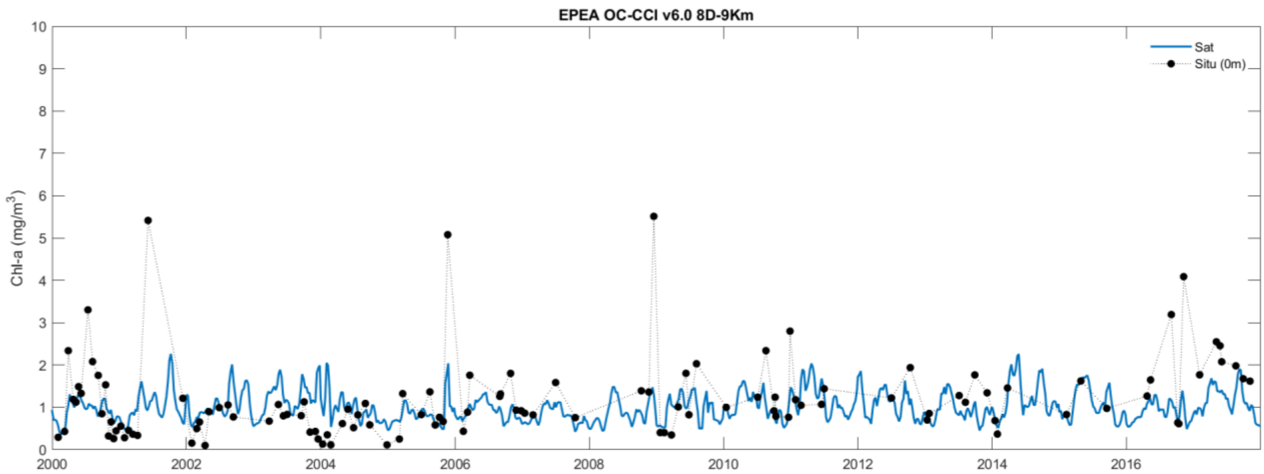
General comments

This paper presents a statistical analysis of phytoplankton bloom phenology along the Argentine Continental Shelf based on a 22-year satellite record of surface chlorophyll a concentration, and its relationships with light radiation and mixed layer depth (MLD) at 7 selected locations. Indeed, I agree with Reviewer 2 that there are no significant flaws regarding the technical part, as similar methodologies have been applied in many previous studies. Having said that, the weaknesses and limitations inherent to satellite-based studies on this topic are also well recognized in ocean color communities. Since Reviewer 1 has already elaborated on these, I will not repeat them here.

For strengthening the paper's contribution, I wonder if there are any in situ chlorophyll or ancillary observations available in the study area. Are there monitoring stations near the 7 selected sites that could provide complementary validation of satellite-derived chlorophyll? Even a simple comparison would enhance confidence in the results.

We appreciate the reviewer's suggestion. In situ chlorophyll observations are available in the study area, for example at one of the selected sites (EPEA). The figure below (unpublished) illustrates a reasonable consistency between the OC-CCI satellite-derived time series and the available in situ chlorophyll measurements at EPEA. However, given the substantial differences in spatial (point measurement vs. 9 km pixel) and temporal (instantaneous vs. 8-day composite) scales, we believe that a quantitative matchup analysis would not provide a reliable indicator of the satellite product's accuracy or its ability to represent seasonal variability.

In the revised manuscript, we will clarify that such in situ data exist at certain sites (e.g., EPEA) and that their availability provides qualitative support for the temporal behavior of the satellite-derived chlorophyll, despite scale differences, which make it difficult to quantitatively assess accuracy in the representation of the seasonal signal. We will also note that the OC-CCI product used here is derived from globally merged and bias-corrected reflectance data that are processed consistently over space and time using the same algorithms and quality-control procedures. This uniform processing ensures that relative variations, such as those describing the seasonal and interannual behavior of chlorophyll-a, are expected to be reliable across regions.



In addition, I found many imprecise uses of terminology, particularly for physical processes, which make the manuscript difficult to follow. Terms such as "mixing," "stratification," and "mixed layer depth" are often used interchangeably, though they represent distinct concepts (e.g., stratification is a water property, not a process). The manuscript also frequently mentions local dynamics such as tidal mixing and upwelling. However, these discussions are mostly descriptive in nature, without quantitative analysis, definition, or explanation. In the current version, they are mixed into the results and distract from the focus on MLD and light analysis. The authors could consider reorganizing these sentences and paragraphs for better clarity. The introduction is also quite loose. Overall, the manuscript would benefit from a substantial writing revision. I have tried to provide some indications in the specific comments, but I did not do it thoroughly.

We will carefully review and standardize the terminology throughout the manuscript to ensure precise and consistent use of physical terms. Specifically, "vertical mixing" will be used when referring to a process, while "stratification" will be used only to describe the state of the water column. The descriptions of local processes such as tidal mixing and upwelling will be reorganized and moved from the Results to the Discussion section, where they will be discussed in relation to their potential influence on bloom variability and deviations from large-scale MLD-light relationships.

In particular, we will note that strong tidal mixing can deepen or periodically erode the mixed layer, redistributing nutrients and altering the light regime experienced by phytoplankton, whereas upwelling events can inject nutrient-rich waters into the euphotic zone, promoting short-lived productivity pulses that may not coincide with the timing predicted from stratification or light availability alone. Similarly, frontal systems and variable bathymetry can locally enhance vertical shear and mixing, generating heterogeneous conditions that decouple the regional bloom dynamics from the large-scale, seasonally driven MLD and irradiance patterns. These processes will be considered as possible explanations for site-specific deviations identified by the statistical analysis.

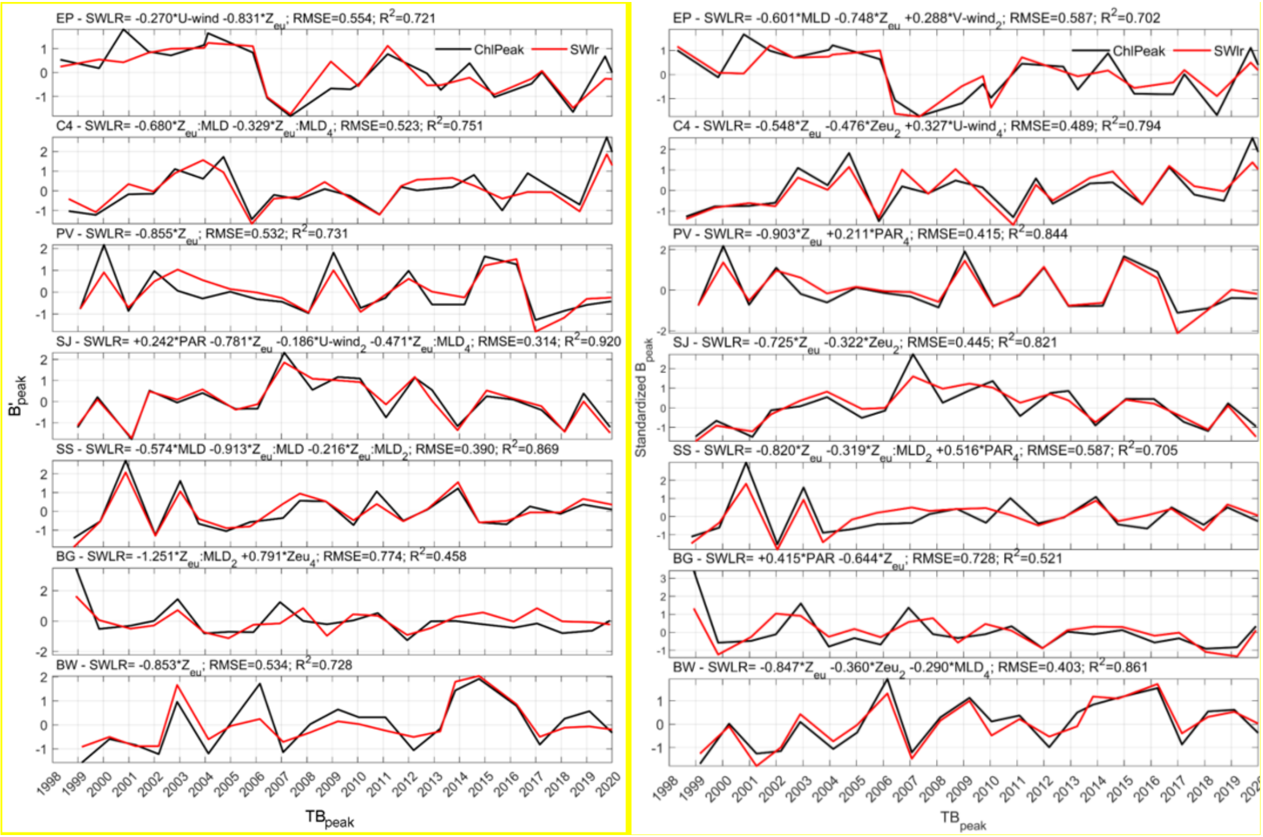
To me, another concern is the removal of seasonality from chlorophyll data. Bloom characteristics are not inherently seasonal. Figure 5 is an example showing large variations in bloom timing and magnitude. As a result, it becomes unclear what component is removed, or whether this step simply reduces variance and artificially increases the variance explained by the multiple regression model. I recommend that the authors also present an analysis without seasonal removal for comparison.

We agree that the rationale for removing seasonality needs to be clarified. In the revised manuscript, we will expand the description of this step in the Methods section, specifying that the seasonal cycle was removed by subtracting the 8-day climatology from each variable used in the statistical analysis, including chlorophyll-a, PAR, Zeu, and MLD. This procedure will be explicitly stated as follows:

"To avoid spurious results caused by the seasonal variability observed in the time series of physical variables and chlorophyll-a, we removed the seasonal cycle by subtracting the weekly climatology from the data. In addition, due to the large dissimilarities in the anomaly ranges, we standardized the time series prior to the stepwise linear regression (SWLR) analysis."

We found that most of the time series involved in the SWLR analysis exhibit strong seasonality with some interannual variability. Initial tests showed that correlations between chlorophyll-a and most environmental variables were very high ($r > 0.9$), primarily reflecting the common seasonal signal rather than direct mechanistic relationships. Removing the seasonal component therefore allowed the model to identify which variables best explain the anomalies in chlorophyll-a peaks beyond the regular seasonal cycle.

Following the reviewer's suggestion, we also performed an alternative analysis without removing the seasonal signal (see figure below, right panels). The resulting regression models required more predictors (compare with left panels) and, in general, explained similar or slightly higher variance than the detrended models. However, this higher variance is likely attributable to shared seasonal co-variation among variables rather than to improved predictive performance. We will summarize *these findings briefly in the revised manuscript and emphasize that the seasonally detrended analysis provides a more conservative and physically meaningful assessment of the relationships between chlorophyll-a anomalies and environmental drivers.*



I do not have an issue with the use of a threshold-based method for bloom detection. However, different phenological indices and methods have been studied and proposed in recent years. I think the authors need to conduct a more thorough literature review that includes recent studies on bloom phenology (e.g., Marchese et al., 2017; Zhao et al., 2022). At present, the introduction cites many older references while overlooking newer methods such as Gaussian fitting. The examples I list here are just two I am aware of.

We agree and will expand the literature review to incorporate recent studies employing Gaussian fitting and other advanced phenological approaches (e.g., Marchese et al., 2017; Zhao et al., 2022). We will also clarify the rationale for retaining a threshold-based method to maintain consistency with previous work in this region and with widely used global approaches (e.g., Racault et al., 2014; Ferreira et al., 2021).

Specific comments

Line 21: The meaning of "geographic trends" is unclear. Do the authors mean spatial distribution patterns?

We will replace this phrase with "distinct mean spatial distribution patterns in bloom dynamics."

Line 25: The attribution "due to deeper MLDs and colder Subantarctic waters" is not clearly supported. Please clarify.

We will rephrase as "...probably due to deeper mixed layers associated with colder Subantarctic waters."

Line 34: The term “mixing” is used frequently. Consider using “vertical mixing” for clarity. Also, please be cautious: mixed layer depth and mixing represent different concepts, and stratification is a property of water, not a process.

We will revise the text to use “vertical mixing” consistently and ensure that “mixed layer depth” and “stratification” are used in their correct physical sense.

Line 37–39: This paragraph may need revision, particularly what n represents.

We interpret the comment as referring to a possible ambiguity. We will rewrite the passage to read: “Understanding the effects of these variables is essential for predicting changes in marine ecosystems. One of the key drivers is the alteration of wind patterns, which modulate the mixed-layer depth through their influence on vertical mixing.”

Line 87: The manuscript often mentions local processes. What is the purpose of highlighting them, given that they occur at different scales and are not the focus of analysis? Could they potentially conflict with the large-scale variables analyzed (e.g., MLD, PAR)? This requires clarification.

We will clarify in the revised manuscript that, although the large-scale analysis identified PAR and MLD as the dominant factors controlling bloom variability, the proportion of variance explained by these variables differed substantially among sites, i.e., from about 46% at BG to 92% at SJ. This range indicates that additional, unmodeled local processes likely play an important role in regulating bloom magnitude and timing.

Specifically, we will explain that while the large-scale analysis captures the spatial patterns of bloom phenology indices across the shelf and their climatological relationships with environmental drivers at each site, the site-specific SWLR analysis isolates only the most consistent large-scale controls. The residual unexplained variability may therefore reflect local processes not represented in the analysis, such as tidal mixing, upwelling, frontal activity, or cross-shelf exchanges, which can modify nutrient supply, stratification, and light exposure in ways that differ from the general MLD-light regime. These local dynamics are expected to vary spatially along the shelf and may account for the observed site-to-site differences in model performance.

Line 285: Does “mean timing and intensity” refer to climatological averages over 22 years, or another metric? Please specify.

Yes “Mean timing and intensity” refers to the phenological metrics of the 22-year climatological annual cycle. We will modify the sentence to read: “Figure 2 illustrates the 22-year climatological annual cycle of key phenological phases of the phytoplankton bloom, i.e., time of bloom initiation (TBinit) and peak (TBpeak), and mean maximum chlorophyll-a concentration (Bpeak).”

Line 296: The statement “deeper mixed layers require more time for warming and stratification” is not convincing as written. Either rephrase or provide more explanation.

We will rephrase the sentence for clarity and expand it slightly to explain the physical mechanism involved. The revised text will read: “These cold waters require more time to warm and for the mixed layer to shoal, which delays bloom initiation.”

We will further elaborate that in colder Subantarctic waters deeper winter mixed layers retain more heat capacity and are slower to stabilize under increasing solar radiation in spring. As a result, the onset of stratification, and hence the period of sufficient light exposure for phytoplankton growth, is delayed compared to regions with shallower winter mixing. This process explains the later bloom *initiation observed on the Patagonian Shelf, as opposed to the northern and mid-shelf regions, where warmer waters and shallower mixed layers allow stratification to develop earlier in spring.*

Line 313–318: This paragraph reads more like a discussion. It would benefit from additional explanation, e.g., "frontal systems promote vertical mixing" or "a deeper mixed layer moves phytoplankton below the euphotic zone.

We will retain the paragraph in the Results section and expand it to include a clearer explanation of the underlying mechanisms. The revised text will indicate that frontal systems promote vertical mixing and nutrient injection into the euphotic zone, enhancing phytoplankton growth on the stratified side of the front, while deep mixed layers can transport cells below the sunlit zone, limiting light exposure and growth. The paragraph will be modified as follows:

"Frequent upwelling events bring nutrients to the surface, and frontal systems promote vertical mixing that disrupts the stable stratification, supporting intense phytoplankton blooms on the stratified side of the front and underscoring the high productivity of the Patagonian Shelf as a biological hotspot. In contrast, offshore regions farther from nutrient sources exhibit lower Bpeak values. In these deeper waters, nutrients are scarce, and strong winds drive deeper mixed layers, causing phytoplankton cells to be continuously mixed below the euphotic zone. As a result, they do not remain in the sunlit layer long enough for optimal growth. This spatial variation in Bpeak across the study area highlights the critical role of water-column structure in modulating nutrient availability and light exposure, thereby determining bloom intensity."

Line 325–326: This sentence is not clear to me. What does 'the maximum frequency' refer to?

It refers to the frequency of Bpeak occurrence at a given season. We will clarify by rewriting the sentence as: "At the two northern sites (EP and C4), the bloom peak (Bpeak) occurred most frequently in autumn and winter (Figure 3)."

Figure 4: Four variables are plotted with two y-axes. Were Chl-a and PAR standardized with the other variables? Please explain the method clearly.

We will specify in the Methods section that variables in Figure 4 are shown in their original units (not standardized) and that standardization was applied only to the time series used in the stepwise regression analysis.