

We thank Reviewer #1 for the constructive and encouraging comments and for emphasizing the strengths of the manuscript with respect to the dataset, methodology, and interpretation of physical-biological coupling in the Mediterranean Sea. In response to the comments received from both reviewers, the manuscript has been substantially revised. Most notably, PAR has been added as an explicit physical driver throughout the analysis, and a Multivariate EOF (MEOF) approach has been introduced to simultaneously decompose all biological and physical variables and identify coherent coupled modes of co-variability. Following internal reconsideration, PPfo has been removed from the revised manuscript as it was deemed misleading with respect to the primary objectives of the study. Below, we address the specific comments point by point.

General comment

The manuscript presents a well-structured and comprehensive analysis of the spatial and temporal variability of primary production in the Mediterranean Sea over the past 25 years, based on an impressive satellite dataset. The study provides a valuable contribution to our understanding of the coupling between physical forcing and phytoplankton dynamics in this complex basin. The results are coherent and highlight the key role of physical drivers such as mixing, circulation, and nutrient inputs in shaping phytoplankton biomass, diversity, and productivity at both seasonal and interannual scales.

The paper is clearly written, well organized, and offers novel insights through the use of empirical orthogonal function (EOF) decomposition to identify dominant modes of variability. The analysis successfully integrates multiple parameters (SST, MLD, chlorophyll) to characterize the relationships governing primary production. However, some aspects could be further strengthened. In particular, the absence of an explicit analysis of light availability and its variability deserves justification, given its potential influence on phytoplankton dynamics and primary production.

Author's reply: We fully agreed with the reviewer and PAR has been added as an explicit variable throughout the analysis, both in the mean field description and in the univariate and multivariate EOF decomposition. This addition proved pivotal: the first EOF mode of PP is shown to be in phase with PAR, establishing light availability as the primary modulator of the annual PP cycle at the Mediterranean basin scale

Similarly, while the discussion of surface primary production (PPfo) is interesting as a complementary product, it partly duplicates the conclusions derived from surface chlorophyll patterns and could be streamlined to focus on the most relevant findings.

Author's reply: Following internal reconsideration and in line with comments from both reviewers, PPfo has been removed from the revised manuscript as it was deemed misleading with respect to the primary objectives of the study.

Overall, this is an interesting and well-conducted study that significantly contributes to the long-term understanding of Mediterranean primary production. The figures and data presentation are clear and effectively support the conclusions. I recommend publication after minor technical revisions, mainly aimed at clarifying the introduction and improving the description of the applied methodology (e.g., briefly summarizing Volpe et al., 2012, to ensure full reproducibility). Adding a few additional references could also help strengthen the contextual framework.

Technical comments:

Line 28: Both "color" and "colour" are used in the manuscript. Please harmonize the spelling throughout the text for consistency and improved readability.

Author's reply: Done.

Line 63: Although the ^{14}C method for estimating primary production is widely used, some uncertainties remain regarding its accuracy (e.g., Baltar and Herndl, 2019). This point could be briefly acknowledged in the text.

Author's reply: this paragraph have been removed during the rewriting

Line 125: A period appears to be missing after the closing parenthesis.

Author's reply: Method section is completely re-written.

In Figure 1, would it be possible to include the Mixed Layer Depth in the sensitivity analysis of primary production? This could help illustrate its influence more clearly.

Author's reply: We understand the point and it would be very useful if MLD was an input to the model. Unfortunately, this is not the case as the model “only” uses light, chlorophyll and temperature, for which the sensitivity has been checked.

Line 206: For improved readability, consider adding a short definition of “Case-2 waters.”

Author's reply: Done.

Line 230: A brief description of the method developed by Volpe et al. (2012) would be helpful here to ensure reproducibility.

Author's reply: Done

Line 250: Please provide a reference to support the statement on the “well-known longitudinal gradient.”

Author's reply: Done. We have added references to Bosc et al. (2004) and D'Ortenzio and Ribera d'Alcalà (2009), which are already included in the reference list.

Line 255: Could the absence of a longitudinal PPeu gradient be related to an excessively deep integration range?

Author's reply: The absence of a longitudinal gradient in PPeu is consistent with the findings of Volpe et al. (2012) who, while explaining the longitudinal gradient in surface Chl, wrote “Moreover, although the total chlorophyll values differ by a factor of roughly four (ranging from nearly 3, during summer, to more than 5 in spring, Fig. 3a–b), average surface values diverge by an order of magnitude (Santoleri et al., 2008).”. In other words, integration over the entire euphotic layer mitigates the longitudinal gradient between the two sub-basins. Since PP is integrated over the euphotic layer, it is not surprising that it does respond to this dynamics.

Line 267: Might the apparently high productivity in the Gulf of Gabès be influenced by bottom reflectance affecting satellite observations?

Author's reply: Yes. In the revised version we will underline more clearly that the coastal PP values should be regarded with care as the present version of the PP model is optimized for open ocean waters.

Line 274: In Figure 2, if possible, please consider using a color-blind-friendly palette to enhance accessibility.

Author's reply: All figures are reformatted.

Line 370: As a complement to this analysis, relevant results can also be found in Toseland et al. (2013).

Author's reply: The sentence has been removed in the revisited version.

Line 375: Please refer to Marañón et al. (2021) for discussion of the relationship between light and the Deep Chlorophyll Maximum.

Author's reply: The sentence has been removed in the revisited version.

Line 404: It is unclear what “these two” refers to. Please clarify.

Author's reply: The sentence has been rewritten in the revisited version

Line 440: It may be useful to discuss the PSC (phytoplankton size class) distribution in light of how it is derived (i.e., directly from chlorophyll concentration).

Author's reply: The way PSC were derived is mentioned in the Data and Method section as caveat to explain the large correlation with total chlorophyll

Line 468: How confident are you that this pattern results from long-term warming rather than from an increased frequency of marine heatwaves?

Author's reply: We have added a sentence to mention that the thermal trend could be associated with the increased frequency of marine heatwaves.

We thank Reviewer #2 for the detailed and thoughtful assessment of the manuscript. We appreciate the constructive comments aimed at improving the clarity and robustness of the study. In response, the manuscript has been substantially revised. Beyond addressing the specific concerns raised, the revision involved a complete reanalysis of the data, including the addition of photosynthetically available radiation (PAR) as an explicit physical driver, which proved pivotal in resolving the apparent paradox between the seasonal cycles of PP and CHL. A Multivariate EOF (MEOF) analysis was also introduced, simultaneously decomposing all biological and physical variables to directly identify coherent coupled modes of co-variability and to provide a more rigorous basis for the physical interpretation. Following internal reconsideration and in line with several reviewer comments, PPfo has been removed from the manuscript as it was deemed misleading with respect to the primary objectives of the study. The scientific objectives have been restated more clearly, the methodological framework has been restructured and expanded, including a dedicated Appendix describing the PP model in detail, and the Results and Conclusions sections have been fully rewritten to provide a more coherent and focused narrative. Below, we address the specific comments point by point.

General comments

The manuscript presents a basin-scale analysis of Mediterranean phytoplankton biomass, size structure, and primary production using more than 25 years of data from the Copernicus Marine Service products. Seasonal modes of the phytoplanktonic variables with sea surface temperature (SST) and mixed layer depth (MLD) are derived with Empirical Orthogonal Function (EOF). Physical control of phytoplankton is assessed with statistical correlation between the modes.

The dataset is rich and the topic is relevant, however, I find that the scientific objectives and key results are not clearly articulated, I have several concerns about the method which, in general, lacks clear outlining. The text is long with a heavy structure and could largely benefit from a synthesis effort grounded on Volpe et al., (2012) results. Finally, there is no discussion on the used data sets or on the method.

I think the study has potential but needs significant refinement to clearly demonstrate its contributions. As such, I recommend major revision before consideration in the Ocean State Report 10. The main points are addressed below and further detailed through the Specific Comments.

- 1) I found it difficult to identify the key objectives of the study. Is it to demonstrate the capabilities of recent products? The consideration of the primary production integrated over the first optical depth (PPfo) feels like a late addition.

Despite 25+ years of data and while methods are readily available (e.g., filtered EOF in Volpe et al., 2012) the study does not robustly explore long-term trends on the variables.

Author's reply: We thank the reviewer for this observation. Following the revision, we have substantially restructured the manuscript to make the objectives clearer and more focused. The study now explicitly aims at: (i) characterising the dominant seasonal and interannual modes of space–time variability of phytoplankton biomass, size structure, and primary production in the Mediterranean Sea over 1998–2025, and (ii) investigating their relationship with key physical drivers within a coherent multivariate framework. These objectives are now stated explicitly at the end of the Introduction. Regarding PPfo: the reviewer's impression is correct. After careful reconsideration, PPfo has been removed from the revised manuscript, as we agreed it introduced an element that was misleading with respect to the stated objectives of the paper and whose interpretation added complexity without strengthening the core message. Long-term trends are not included here because constitute the topic of another work in preparation

- 2) The method should gain in clarity and reproducibility. In the primary production model several key points need to be addressed or at least discussed (case II against case I water, the validation of reconstructed chlorophyll-a vertical profiles). Second, statistical correlation tests should be better introduced and reinforced with significance tests at least for the one long term trend.

Author's reply: Method section has been re-written. The PP model is explicitly described as primarily optimized for optically simple (Case I) open-ocean waters. For optically complex coastal environments (Case II), such as river-influenced and shallow regions, PP estimates carry higher uncertainty and this caveat is now explicitly stated in the text. The methodology for reconstructing vertical chlorophyll

profiles has been clarified in a specific Appendix dedicated to the PP model, and a dedicated sub-section on statistical correlation analysis has been added to the Methods

- 3) I found that the manuscript form did not help highlighting the key results of the study. I feel that the writing quality is uneven with heavy phrasing, inconsistencies (both in language and statistical metrics) and redundancy (especially through the result section) that add unnecessary complexity. I suggest a full language edit and a restructuring of the results based on a synthesis effort. I believe that clearly stating the result interpretation from Volpe et al., (2012) early in the paper would allow an increased focus on newly considered variables (CHL, PP and PSC). Finally, defining clearly all used acronyms, variables and mechanisms early on (through the introduction and method) would improve fluidity.

Author's reply: The revised manuscript has been substantially restructured and rewritten to address all of these points. The entire manuscript has undergone a thorough language revision. Heavy phrasing has been simplified, redundant passages have been removed or consolidated, and inconsistencies in language and statistical metrics have been corrected throughout. The revised Introduction now explicitly frames Volpe et al. (2012) as the methodological baseline from which the present study departs, clearly identifying the main directions in which the analysis has been extended. All acronyms and physical variables are now defined at their first occurrence in the Introduction or Methods, and a consistent notation is maintained throughout the manuscript.

- 4) The author discusses the oceanographic interpretation of the modes and causality mechanisms inferred from statistical correlation. They do not mention study limitations (e.g. centered on abiotic control) or on the new products, their limitations (e.g. PP model) nor what they recommend in future products developments (e.g. using PSC in PP calculations).

Author's reply: The revised manuscript now more explicitly acknowledges the limitations of the study and of the products used and includes forward-looking recommendations for future product development. We acknowledge that the present analysis is centred on physical (abiotic) drivers of phytoplankton variability, and that biotic processes, such as top-down grazing control, zooplankton dynamics, and inter-specific competition, are not explicitly represented. This is an inherent limitation of the satellite-based, physics-oriented EOF framework adopted here, and is now acknowledged in the revised text. The limitations of the individual CMEMS products used in this study are now more explicitly discussed. In particular, the PP model is acknowledged to be primarily optimised for Case I open-ocean waters, with increased uncertainty in optically complex coastal environments (Case II). Following the reviewer's suggestion, the Conclusions section now includes a forward-looking paragraph on potential improvements to the operational CMEMS processing chain. In particular, we highlight the potential value of incorporating phytoplankton size class information directly into the PP model parameterisation, as larger cells and smaller cells differ substantially in their chlorophyll-specific absorption properties and photosynthetic quantum yields.

Specific comments

Line 9: I suggest to be more explicit on how causality links are assessed. e.g. While EOF is used to find the dominant seasonal modes of variability of the biological variables, their link to physical forcing are assessed through statistical correlation.

Author's reply: The abstract and the Methods section have been rephrased to clarify that EOF analysis identifies the dominant modes of variability, while statistical cross-correlation of the temporal amplitudes assesses their links to physical forcing.

Through the manuscript, there is a lack of continuity in usage of acronyms with SST being defined at line 36, 131, 139 and 211. CHL is first introduced as chlorophyll-a concentration (at Line 34), then Chl is defined as chlorophyll concentration (Line 116) and referred to as 'biomass' from the result section. I suggest homogenizing the references to surface chlorophyll-a concentration.

Author's reply: we reviewed the text, removing repetitions and ensuring that references to CHL are consistent

I feel that two narrative threads coexist in the introduction: a first one from paragraph 1 to 7 and a second one the last three paragraphs. Many ideas are recalled several times (e.g. the need of basin scale products is the core subject of paragraph 6 but also recalled at line 69, 82, 89, 126). I think the introduction could benefit from a synthesis effort.

Many details are stated (or recalled) in the last three sections of the introduction. The comparison of statistical methods in the last paragraph of the introduction might be more fitted to the method section or, considering the word constraint of the OSR, to the supplementary material.

Author's reply: The introduction is entirely revised, condensing it, and we will move the details described for the statistical methods to the methods section

line 31: This paragraph (or the next) is an opportunity to recall that phytoplankton biomass and diversity is categorized as an EOVS by the GOOS.

Author's reply: Thank you for the suggestion. We took it into consideration and the reference to phytoplankton biomass, size structure and productivity as Essential Ocean Variables (EOVs) recognised by GOOS has been added to the revised manuscript.

line 106: The study of Volpe is firstly cited here. This paper seems to be the methodological inspiration of the presented manuscript. I recommend making more explicit the methodological similarities and differences (SST and MLD are used here while SST and sea surface anomalies in Volpe et al., 2012). I also suggest mentioning, here or in the method, what the environmental variables are proxying for. This would fluidify the reading of subsequent sections.

Author's reply: The revised Introduction now explicitly frames Volpe et al. (2012) as the methodological baseline, clearly stating similarities and differences: notably, MLD replaces sea level anomaly as a proxy for upper-ocean mixing and nutrient entrainment, and PAR is added as an explicit driver of primary production variability. The Methods section also includes a brief description of what each physical variable proxies for, improving the fluidity of the subsequent sections.

From line 109: the authors state the objective of the study. I think that this paragraph could gain in clarity in relation to the above comments. Are long term trends considered or only seasonal patterns? To what purpose are PPeu and PPfo considered?

Author's reply: The objectives paragraph has been fully revised for clarity. Long-term trends are not considered in this study, as they constitute the focus of a companion paper in preparation; this is now explicitly stated. Regarding PPfo, following the reviewer's comment and internal reconsideration, it has been removed from the revised manuscript as it was deemed misleading with respect to the stated objectives.

I suggest rephrasing line 114 to 115 especially the goal of the work, and the way it was applied. Line 114: I do not understand the author's intention with this method's header. It is long and redundant with what should only be stated in dedicated sub-sections. The sentence starting at line 119 ends at line 127. I suggest simplifying this header by stating only the first sentence and then sorting each idea in the respective subsequent sub section (phytoplankton sizes classes should be described in section 2.1.2, PP in section 2.1.1, line 130 in section 2.1)

Author's reply: Method section is completely re-written taken into account these and other suggestions.

Line 128: I suggest using the exact value from Morel and Berthon, 1989 instead of one-fifth as it is already an approximation.

Author's reply: not taking into account the PPfo, this comment it is not considered.

line 130: Volpe et al., 2012, consider sea level anomaly as a proxy for currents, the study keeps only SST and MLD as proxies of oceanic forcing, what is the reason behind that?

Author's reply: MLD dynamics is among others the best physical parameter or proxy that shapes phytoplankton dynamics at seasonal or annual scale. In 2012 a model with a good representation of the MLD dynamics was not available yet, and MADT was used instead.

Line 144: What are the spatio-temporal resampling techniques? Is it a monthly average or a snapshot?

Author's reply: We added that the nearest-neighbour approach was used for spatial regridding before computing the monthly averages.

Line 146: I suggest rephrasing which constitutes the topic of the work.

Author's reply: We have substituted the sentence with: "The monthly temporal resolution minimizes the influence of high-frequency variability, thereby emphasizing seasonal and interannual signals"

line 146-147: I do not understand what links the spatial and temporal resolution here.

Author's reply: The phenomena under investigation point out the required spatiotemporal resolution of the data, as spatial and temporal scales are inherently coupled. Small-scale phenomena (on the order of 1–2 km) evolve over days, whereas large-scale phenomena (spanning many kilometres) evolve over months. Therefore, to focus on annual and inter-annual signals using monthly data, a sufficiently coarse spatial resolution must also be chosen. This effectively filters out smaller-scale, high-frequency noise and retains the relevant large-scale patterns. We have substitute the sentences with: “The choice of a 4 km spatial resolution is consistent with the adopted monthly time scale and allows mesoscale features to be retained while filtering out finer-scale variability that is incompatible with monthly sampling.”.

Line 152: I fail to see why the author reconstructed a chlorophyll-a profile up to the optical depth instead of directly using the satellite chlorophyll-a concentration which characterizes the FOD. Furthermore, It remains unclear to what purpose the FOD is considered.

Author's reply: not taking into account the PPfo, this comment it is not considered.

line 161: the presented equation of $P=P(z,\lambda,t)$ is not yet column integrated. Depth and wavelength integration are required to get column integrated PP(t).

Could the author furnish more information on how the photosynthetic quantum yield evolves in respect of chl(z) and T(z)?

Author's reply: We have corrected the formula with

$$P=12 \int_{z=0}^{\text{Depth}} \int_{t=0}^{24} \int_{\lambda=400}^{700} \text{Chl}(z) a_{\text{Chl}}^* \text{PAR}(z,t,\lambda) \varphi_{\mu}(\text{Chl},T)(z) d\lambda dz dt$$

And we have added a sentence at the end of the paragraph: “The daily, column-integrated primary production (PP) was computed as the triple integral of the production rate with respect to wavelength (over band, 400–700 nm), time (over 24 hours), and depth.”

Following the original model, the quantum yield is scaled relative to its maximum value $\varphi_{\mu,\max}$ through a

dimensionless light-response function: $\varphi_{\mu} = \varphi_{\mu,\max} f(x)$ with $x = \frac{\text{PUR}}{K_{\text{PUR}}}$ where PUR is the

photosynthetically usable radiation and K_{PUR} is a scaling irradiance controlling the transition between light-limited and light-saturated photosynthesis. The function $f(x)$ follows the formulation of Platt et al. (1980), accounting for saturation and photoinhibition. Temperature influences photosynthesis through its effect on the scaling irradiance K_{PUR} , which is parameterized using one of the Eppley-type formulations: $K_{\text{PUR}}(T) = K_{\text{PUR}}(20^{\circ}) 1.065^{(T-20)}$. As a consequence, temperature modulates the realized quantum yield indirectly by shifting the photosynthesis–irradiance response, without altering either the functional form of $f(x)$ or the definition of $\varphi_{\mu,\max}$. In the original formulation, the maximum quantum yield $\varphi_{\mu,\max}$ is assumed to be constant, under the hypothesis that its variability compensates for opposing changes in the maximum chlorophyll-specific absorption coefficient $a_{\max}^*$. In the present study, following Bricaud et al. (1998), the chlorophyll-specific absorption spectra are allowed to vary in both magnitude and shape as a function of chlorophyll concentration. Consequently, $a_{\max}^*$ is no longer constant but varies with chlorophyll concentration (thus with depth), implying that $\varphi_{\mu,\max}$ cannot be treated as constant either. Following Morel et al. (1996), the maximum quantum yield has been expressed as a function of chlorophyll concentration as: $\varphi_{\mu,\max} = 0.05 \left[\frac{\text{Chl}^{0.66}}{(0.44 + \text{chl}^{0.66})} \right]$. This formulation accounts for observed photo-physiological adaptations across trophic regimes while preserving the original representation of light and temperature effects on photosynthesis. By introducing the explicit chlorophyll dependencies for both $\varphi_{\mu,\max}$ and $a_{\max}^*$, the present formulation makes explicit the physiological variability that was implicitly embedded in the constant product $a_{\max}^* \varphi_{\mu,\max}$ in the original model.

We have accurately detailed, in the Appendix, the PP model and we have added the effect of chlorophyll and temperature on the photosynthetic quantum yield.

Line 171-172: the authors describe how light penetration is modelled through the water column for PP calculation. The used methods from Bricaud et al., 1998 and Morel et al., 2002 are valid for Case I water. I think this point questions the validity of the subsequent PP estimation in case II water (straits, western Adriatic, and the Gulf of Gabes). (Line 251, 266, line 381, line 466)

Author's reply: The reviewer is right, PP model offers better performance in Case 1 waters, given that its various parameterizations were developed for open ocean waters. We have added statements here

and in other sections of the paper to clarify that in coastal waters the model estimates have a higher associated uncertainty and that results require careful interpretation. In any case, the Mediterranean Sea is overall a “Blue water” (i.e. Case 1) basin, and the optically complex waters, where the ad-hoc algorithm is invoked, account for ~1% of the basin’s pixels over the entire time series.

From line 172 to line 182: the authors describe the statistical reconstruction of chlorophyll-a vertical profiles from surface observation using a k-means clustering on the MedBiop dataset. This methodological point is pivotal for the study and seems to be redundant with the method in the QUID of product ref. no. 1 (Colella et al. 2025). I could not find a statistical validation of the reconstructed vertical profiles of chlorophyll-a from surface observations, could the authors forward the corresponding citation?

Author’s reply: Presently, there is no other study that describes this method for reconstructing chlorophyll profiles. Therefore, in the Appendix, we have included and detailed it, comparing the reconstructed profiles for the Mediterranean Sea with the historical reconstructions by Morel and Berthon (1989) and Uitz et al. (2006).

Line 180 and later on: the reference could be to product ref. no. 3 so to be more concise.

Author’s reply: We have updated the reference, thanks.

The sensitivity analysis confirms how PP behaves in respect of phytoplankton biomass, illumination and temperature. I have trouble identifying the need for this sensitivity analysis as to me, it is expected and highlights even further that the reconstructed vertical profiles of chlorophyll-a (discussed above) must be proven to be correct.

Author’s reply: The sensitivity analysis has been moved to Appendix in the revised manuscript, where it accompanies the detailed description of the PP model. We agree with the reviewer that its outcome is physically expected; however, we retain it as it provides a quantitative basis for interpreting the relative role of CHL, temperature and light in driving PP variability, which is directly relevant to the EOF analysis discussed in the Results.

To improve readability I suggest moving the PP section (2.3.1) after sections 2.1.2 and 2.1.3 which introduce Chl and SST.

Author’s reply: Done. The PP section has been repositioned accordingly in the revised manuscript.

From line 200: I suggest rephrasing the paragraph on chlorophyll-a to improve reading fluidity.

Author’s reply: All the section has been rewritten

Line 217: I suggest rephrasing the sentence to in modulating space-time variability of phytoplankton biomass, production and diversity.

Author’s reply: All the section has been rewritten

I suggest rephrasing section 2.2 and to mention earlier that the method is a copy of Volpe et al., 2012 which derived EOF for all variables (PP, CHL, SST, MLD) by centering all time series and recalling that only mean fields and seasonal anomalies will be investigated (no filtered EOF).

Author’s reply: The EOF methodology section has been rewritten. It now explicitly states from the outset that the approach follows Volpe et al. (2012), that all time series are centred by removing their long-term mean prior to decomposition, and that the analysis focuses on mean fields and dominant seasonal to interannual modes of variability without temporal filtering.

Section 2.2 did not introduce how mean and anomalous fields of biomass and productivity fields are correlated to environmental variables. In the results section, metrics related to linear correlation (sometimes with temporal lag) are used. The type of correlation subsequently used (spatial, temporal or lagged between EOF modes) should be briefly explained here.

Author’s reply: A dedicated sub-section on statistical correlation analysis has been added to the Methods. It introduces the Pearson linear correlation and clarifies its applications in the manuscript.

Line 231 and the header of section 3 (line 237) both announce how the results will be addressed. I think that only one of the two is necessary.

Author’s reply: Agreed. The redundant announcement has been removed, retaining only the section

header.

In the caption of figure 2 the averaged fields are shown up to 2023 (which is fine) while at line 250 the average Chl concentration is stated to be over the entire period. This should be checked for consistency.

Author's reply: The error is in the caption for Figure 2. The reference period for the datasets is 1998–2025. We have checked and correct for any inconsistencies

Line 285: In accordance with a previous comment, the method behind the metric given as percent is not presented. I understand it is the amount of variance explained by the linear model between the two variables, but is it a linear correlation on the mean field (shown in figure 2) or a correlation on each individual time series that is averaged later on? At line 290, r^2 coefficients are considered and raise identical questions. This should be addressed in the method with a subsection dedicated to statistical correlation.

Author's reply: These metrics refer to spatial correlations computed between the long-term mean fields shown in Figure 1. This is now clarified in the dedicated statistical correlation sub-section added to the Methods, and a summary table of Pearson spatial correlation coefficients between all variable mean fields has been added (Table 2 in the revised manuscript).

Figure 3: For clarity, I suggest to change the caption to :

Spatial dominance of PSC size classes (Micro, Nano and Pico in panels a, b and c, respectively) in months. Panel d shows the overall dominance of each size class in percent over the entire time series.

Author's reply: We have modified the caption

I also suggest defining what are the percentages in the legend of panel d (temporal or spatial dominance?) And to define how to compute a pixel dominance in the method.

Author's reply: The revised version now includes a clear definition of pixel dominance and how it is computed.

Thorough section 3.2.1: I would suggest presenting first, the mode and then, discussing its spatial variability. I found that the reversed order (spatial variability before the EOF mode) was confusing.

Author's reply: For the single-variable EOF analysis, we find that presenting the spatial pattern first and then the temporal amplitude provides a more natural reading flow, as the spatial structure gives the physical context needed to interpret the temporal evolution. We have therefore retained this order in the revised manuscript.

From line 318: and up to the end of the manuscript, the term phytoplankton biomass is used instead of Chl (or CHL). This change brought confusion when discussing seasonal evolution of periods (line 355) as seasonal DCM formation could also be invoked to discuss diminution of phytoplanktonic biomass in the surface layer.

Author's reply: The term "phytoplankton biomass" has been replaced, when needed, throughout the manuscript with "CHL" to avoid ambiguity. We also added a brief clarification in the Methods acknowledging that CHL is used as a proxy for surface phytoplankton biomass, while noting that photoacclimation effects may partially decouple CHL from actual biomass at seasonal scales.

Line 330: I suggest rephrasing this sentence as the other temporal evolution is vague (other modes ? of MLD or of the other variables?)

Author's reply: Agreed. The sentence has been rephrased to explicitly state that the comparison refers to the temporal amplitudes of the first EOF mode of the other variables (SST, CHL, PAR and PP), removing any ambiguity.

From Line 335: the author discusses the spatial connection with SST 1st mode which is related to seasonal cooling and warming. I recommend describing this mode as such first.

Author's reply: We retain the original order, presenting MLD first followed by SST.

I suggest rephrasing line 345 and line 353-354.

Author's reply: Thank you for the suggestion; we have rewritten these sentences to make them clearer.

From line 365: the authors discuss the temporal mismatch of PPeu and CHL and link it to the subsequent warming and stratification.

Over the course of a year, regimes transition from high nutrient low light (winter) - high nutrient high light (spring) - low nutrient high light (summer) - low nutrient low light (fall) (Bellacicco et al., 2016). Therefore seasonal modulation of illumination should also be invoked when discussing seasonality of PP. I expect illumination to cause the spring onset in PP production which stops when nutrients are consumed.

Author's reply: We fully agree with the reviewer. PAR has been added as an explicit variable in the revised manuscript, both in the mean field analysis and in the univariate and multivariate EOF decomposition. This addition proved pivotal: the first EOF mode of PP is now shown to be in phase with PAR, establishing light availability as the primary modulator of the annual PP cycle at the Mediterranean basin scale.

From line 366: and following the PP model, the use of SST to show temperature control on phytoplankton metabolism could be strengthened with the use of modelled temperature vertical profiles.

Author's reply: That is indeed correct: the PP model uses the vertical temperature profiles modelled and distributed by CMEMS, as described in lines 179-184. We have revised the text to clarify this point.

line 371: Is this the intention with the consideration of the FOD to show that PP calculation should be integrated over the euphotic depth?

Author's reply: not taking into account the PPfo, this comment it is not considered.

Looking at figure 5, the lag between minimum PP and minimum SST seems to be longer than maximum PP and maximum SST.

What are the differences between the correlation of PPeu and SST at line 371 and at lines 390 and 391 ?

Author's reply: We thank the reviewer for this careful observation. In the revised manuscript, this part of the analysis has been substantially reorganised following the addition of PAR as an explicit variable. The correlation analysis between PP and SST temporal amplitudes has been replaced by a more physically coherent interpretation based on the tight coupling between PP and PAR, which directly resolves the apparent asymmetry noted by the reviewer. The previously reported correlations between PPeu and SST at different lags, which caused confusion, have been removed or consolidated into a clearer and more consistent presentation

Line 375 defines the First Optical Depth, and is redundant since it is already defined in the method.

Author's reply: not taking into account the PPfo, this comment it is not considered.

From line 380: the authors discuss PPfo referencing figure 6, I find it particular to have this figure at the end of the manuscript while being described here. I suggest finding a way to add the panels and EOF modes in respectively Figure 4 and Figure 5.

Author's reply: not taking into account the PPfo, this comment it is not considered. Anyway the figure have been reformatted to a better readability.

Line 385: I suggest explaining what being more resilient means and slower variability. Averaging PP for the FOD is known to offer an incomplete view of column integrated PP as rightly stated by the authors beforehand.

Author's reply: not taking into account the PPfo, this comment it is not considered.

line 387: I suggest rephrasing to Surface phytoplankton biomass present inverse correlation with SST (phase inversion or 6 month lag) and strong correlation with MLD.

Author's reply: The sentence has now been corrected to explicitly state that surface CHL presents an inverse correlation with SST and a positive correlation with MLD, consistent with the winter deepening of the mixed layer driving nutrient supply to the euphotic zone.

Line 390: surface biomass accumulation or biomass accumulation ?

Author's reply: Biomass accumulation in the euphotic layer.

Line 391: a reference would be expected for the top down control. Furthermore, are the authors now discussing surface biomass or integrated biomass?

Author's reply: This sentence has been removed during the extensive rewriting of the Results section

Line 393: a citation would be expected for how size differential cell sinking contributes to phytoplankton dynamics.

Author's reply: This sentence has been removed during the extensive rewriting of the Results section

from line 393: the summary of findings on the control mechanisms of primary production miss to mention the seasonal DCM and illumination.

line 393: If sea water temperature is a proxy for nutrient entrainment, is it sea water temperature that dictates production or winter replenishment of nutrients stocks? As such, MLD isn't a better indicator of nutrient stocks? I think the causal link relating to the statistical correlation could be here better highlighted

Author's reply: Following the extensive revision and the inclusion of PAR as an explicit variable, this section has been fully reshaped. The role of illumination is now central to the interpretation of the annual PP cycle, with PAR identified as the dominant physical modulator.

Line 418 and Figure 5c: The second mode of surface chlorophyll-a seems to correlate well with the first mode of MLD (both temporally and spatially) and less with its second mode. In general I fail to see why the second mode of a variable should be necessarily linked to the second mode of another variable. In Volpe et al., 2012, the seasonal EOF modes are independently derived from one another. (see header of Volpe et al., 2012 4. Results and discussion)

Author's reply: The reviewer raises a valid methodological point. We agree that in the univariate EOF framework, modes of different variables are independently derived and there is no mathematical reason why the n-th mode of one variable should necessarily correspond to the n-th mode of another. However, one can intuitively expect the first modes of surface ocean variables to describe a similar scale of variability, which constitutes the rationale for exploring inter-variable correlations in this way. To address this limitation more rigorously, the revised manuscript introduces a Multivariate EOF (MEOF) analysis, which simultaneously decomposes all variables and directly identifies coherent coupled modes of co-variability, providing a more robust basis for the physical interpretation.

In figure 5b, the second mode of SST explains 1.2% of the variance, is this mode significant ?

Author's reply: We agree that 1.2% of explained variance is low. However, the explained variance alone does not determine the statistical significance of an EOF mode. In the revised manuscript we have added a clarification noting that the very low explained variance of the SST and PAR second modes reflects the fact that these variables are almost entirely dominated by their annual cycle, captured by the first mode, with only weak residual higher-order variability remaining.

Line 432: the author compares the 2nd modes of P_{eu} and P_{fod} , finding that these two modes are of different shape: P_{eu} mode stretches longer over summer. As interpretation, the author seems to suggest a higher production in the first optical depth due to warmer surface water compared to the production of the full euphotic depth (i.e. $P_{fod} > P_{eu}$). However, with the column integrated equation of line 161 and assuming the euphotic depth to be deeper than the first optical depth then P_{eu} necessarily sums P_{fod} in its calculation. Furthermore, the EOF modes are normalized, hence amplitude differences on the origin variables can not be inferred from amplitude difference in these modes. I suggest clarifying this discussion.

Author's reply: not taking into account the PP_{fo} , this comment it is not considered.

Line 456 and 460 and the last paragraph of the summary and conclusion section are redundant.

Author's reply: "Summary and Conclusions" are completely rewritten

Line 465: In similarity between PP_{fo} and Chl fields reinforces the dominant role of phytoplankton biomass as the main driver of production.

I am unsure about what the authors are highlighting here. Recalling that biomass is here proxied by surface chlorophyll-a concentration, I think that what is highlighted is the need to account for the full euphotic depth when doing primary production calculation.

Author's reply: not taking into account the PPfo, this comment it is not considered

line 466-467: The higher biomass and production hotspot of the bloom regions have been quantitatively estimated as climatological fields. I would have liked to see how these biomass hotspots evolve with climate change, are they resilient as suggested hereafter ?

Author's reply: As mentioned in General comment 1, long-term trends are not included here because constitute the topic of another work in preparation.

Line 468: The trend is only observed on surface chlorophyll which does not permit to infer much about oligotrophication. For that purpose the full euphotic zone biomass should be considered.

Author's reply: Since CHL and SST are representative of the surface layer, as stated throughout the manuscript, the observed decrease in CHL and increase in SST are implicitly referred to the surface layer.

From line 472: the authors summarize the findings on the first EOF mode, which is linked to seasonality. However, at line 475 deep convection is mentioned. Deep convection is, from Line 487, linked to the second EOF mode. As a result I have trouble making a clear distinction between the two modes. They both have an annual period and thus are seasonal modes but relate to different mechanisms (stratification and deep convection, respectively). In general, I fail to see how each mode is connected to each other.

Author's reply: The reviewer raises a valid point. The Conclusions section has been restructured to more clearly separate the two modes. The first EOF mode is now explicitly described as the basin-wide annual cycle driven by the light-temperature cascade, while the second EOF mode is described as capturing the sub-basin scale spring bloom dynamics driven by episodic deep convection in the northwestern Mediterranean. The distinction between the two mechanisms is now clearly stated, and the MEOF analysis is presented as the framework that most rigorously confirms their physical coherence and separability.

From line 484: The coherence between phytoplankton size classes and total biomass further indicates that the seasonal cycle is expressed consistently across community structure, with microphytoplankton showing the largest amplitude of variability and picophytoplankton the most stable.

Again, isn't the biomass considered only at the surface? I would expect this finding - high correlation between PSC and CHL - to be discussed in relation to the PSC derivation method.

Author's reply: The revised Methods section now explicitly clarifies that both CHL and PSC are surface quantities derived from satellite observations, while PP is depth-integrated over the full euphotic zone. The high correlation between PSC and CHL is expected by design, as PSC are derived directly from CHL using the algorithm of Di Cicco et al. (2025), and this is clearly stated in the relevant section of the revised manuscript

Line 504, I think that what has been quantified is that the considered PP model has chlorophyll as the main driver. This point could be discussed in regard to the used PP model.

Author's reply: "Summury and Conclusions" are completely rewritten and this sentence it is not recognized.

Line 508, I think that the resilience of Mediterranean sea primary production was not shown by the results.

Author's reply: "Summury and Conclusions" are completely rewritten and this sentence it is not recognized.

Technical corrections

Line 126 The reference to the product could be recalled here (product ref no. 1).

Author's reply: Done

Line 130: I suggest rephrasing the sentence as follow:

To assess the role of the ocean dynamics onto the biological compartment as described by Chl, PSC and PP, we use the remotely sensed Sea Surface Temperature (SST) and modeled Mixed Layer Depth

(MLD) from the Mediterranean component of the Copernicus Monitoring Forecasting Centre (MFC).

Author's reply: "Method" section are completely rewritten and this sentence it is not recognized.

line 135: please rephrase Data used in this work are mostly directly

Author's reply: "Method" section are completely rewritten and this sentence it is not recognized.

Line 148: i suggest rephrasing to All products derive from Mediterranean-specific processing chains and are composed of

Author's reply: "Method" section are completely rewritten and this sentence it is not recognized.

line 211: to evaluate the role of water temperature instead of to evaluate water temperature role

Author's reply: "Method" section are completely rewritten and this sentence it is not recognized.

line 212: product ref. no. 2 instead of SST dataset. Same comment for the MLD dataset.

Author's reply: Done

line 227: For all instead of From all

Author's reply: Done

line 247: I suggest to use average field and anomalies instead of the rule and exception.

Author's reply: Sentence deleted

Line 247: induced by rather than induced to.

Author's reply: Sentence rephrased.

Line 287: I suggest rephrasing when it comes to look into the various variables space time variability to when investigating space time variability of the variables through section

Author's reply: Sentence rephrased.

Line 284: section 2.2.1 instead of section Data.

Author's reply: Sentence rephrased.

Line 290: a word seems missing being their and derived

Author's reply: rephrased

Line 295: in correspondence with instead of in correspondence of line 295: I suggest to rephrase as only over 1 % of the basin area

Author's reply: Done

Line 318: apart from what is shown in the SST filed

Author's reply: Done

Line 331: spanning over

Author's reply: Done

Line 425: Similarly to phytoplankton biomass instead of As of the phytoplankton biomass

Author's reply: Sentence rephrased.