

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.
- 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.
- 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.
- 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).

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.*

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.

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.

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

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.

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 ?

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)

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

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?

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

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

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

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.

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)?

Line 171-172: the authors describe how light penetration is modeled 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)

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?

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

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.

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.

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

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

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).

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.

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.

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.

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.

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.

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.

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.

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.

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

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.

I suggest rephrasing line 345 and line 353-354.

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.

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 modeled temperature vertical profiles.

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

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 ?

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

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.

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.

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.*

Line 390: surface biomass accumulation or biomass accumulation ?

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

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

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

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)

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

Line 432: the author compares the 2nd modes of PP_{eu} and PP_{fd} , finding that these two modes are of different shape: PP_{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. $PP_{fd} > PP_{eu}$). However, with the column integrated equation of line 161 and assuming the euphotic depth to be deeper than the first optical depth then PP_{eu} necessarily sums PP_{fd} 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.

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

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.

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 ?

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.

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.

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.

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.

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

Technical corrections

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

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).

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

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

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

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

line 227: For all instead of From all

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

Line 247: induced by rather than induced to.

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*

Line 284: section 2.2.1 instead of section Data.

Line 290: a word seems missing being *their* and *derived*

Line 295: in correspondence with instead of in correspondence of

line 295: I suggest to rephrase as only over 1 % of the basin area

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

Line 331: spanning over

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

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