Multi-scale phytoplankton dynamics in a coastal system of the Eastern English Channel: the Boulogne-sur-Mer coastal area

—

Reply to Referees' Comments

Kévin Robache, Zéline Hubert et al.

We would first like to thank Referee 1 (R1) and 2 (R2) for their comments and for their involvement in this review process. In the sections below, we have made some enhancements to better delineate between Referees' comments and our manuscript corrections. In this iteration, comments from Referees are highlighted in blue text, while our modifications within the manuscript are denoted in *green* text.

This response letter is structured into two parts: Section 1 addresses R1's comments, and Section 2 covers R2's comments:

Contents

1 Referee 1 comments (RC1)

1.1 General comments

The manuscript "Multi-scale phytoplankton dynamics in a coastal system of the Eastern English Channel: the Boulogne-sur-Mer coastal area" presents a nice dataset with very high frequency sampling of phytoplankton abundances which is novel and interesting. Below are some general comments while more detailed comments are included in the file enclosed.

We thank you for your evaluation of our study and the interest shown in the high-frequency phytoplankton dataset. Thank you for your comments and for the time you devoted to them. We appreciate the constructive feedback provided, both general and detailed, and have addressed each of them carefully in the revised manuscript.

Overall, the article is well written, but sometimes it is hard to follow as the authors switch from the results of one year to another. Like for example presenting raw data of phytoplankton abundances for year 2021 and then jumping to a cross-covariance analysis for year 2022. Then my suggestion to the authors will be to focus first on the results from 2021 and then move on to 2022. Presenting the same set of results and analysis for both years so readers can see both and compare them directly.

We understand the concern regarding the structure of the results section, particularly the transitions between years and the presentation order of analyses. However, we believe that presenting the results year by year may not be the most suitable approach, as the objective is not primarily to compare the two years, but rather to investigate the scales at which changes in phytoplankton dynamics may occur across the two deployments.

Nevertheless, in response to your comment, we have restructured the results section accordingly and now provide a more concise description of the findings for each year within their respective subsections.

For the analysis I suggest the authors to include other statistical analysis to see what is driving the abundances of different phytoplankton groups like for example a multivariate analysis like a redundancy analysis that will show how different environmental variables affect different phytoplankton groups. That way readers will see directly which phytoplankton groups behave similarly and what variables are driving the observed phytoplankton dynamics. Similarly, to assess changes in the composition of the phytoplankton community just analyzing the Shannon index is not enough since the index does not inform in changes in composition just changes in relative abundance distribution. To assess changes in community composition the authors may use ordination analysis like nMDS or a linear

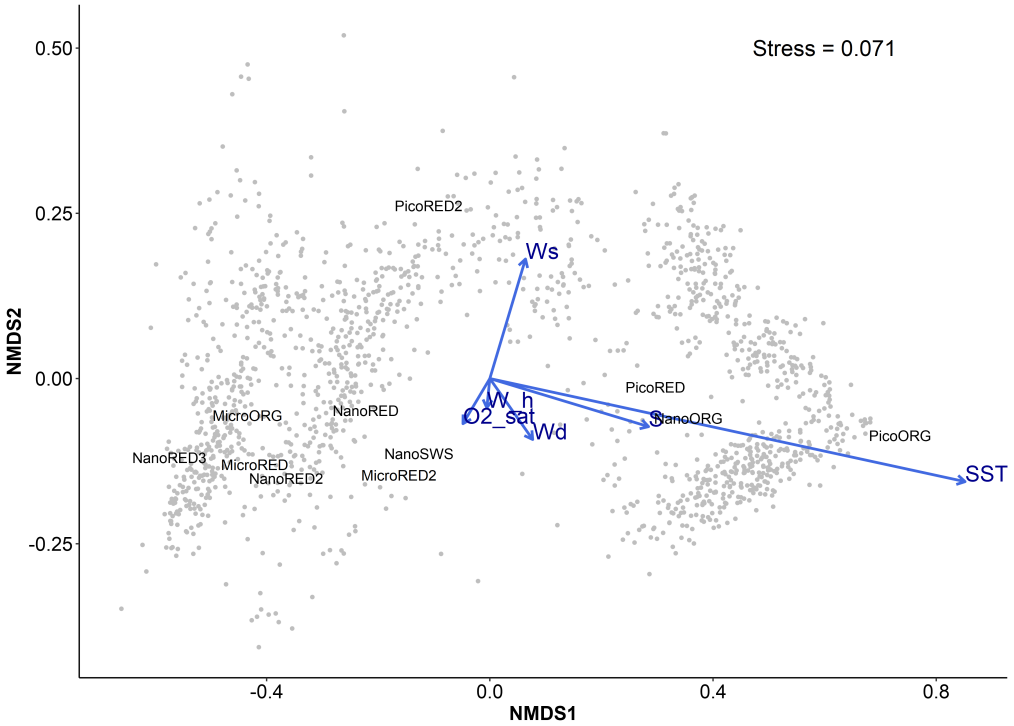

Figure A: nMDS of abundance data (Hellinger transformed) in relation to high-frequency environmental variables measured at the MAREL Carnot station. The stress value, below 0.1, is indicated. O2_sat refers to oxygen saturation (based on solubility), W_h to water height, Ws to wind speed, and Wd to wind direction.

discriminant analysis, which are classical analysis to study changes in the composition of microbial communities.

We agree that multivariate analyses such as RDA or ordination methods (e.g., nMDS, LDA) can provide valuable insight into the drivers of phytoplankton group dynamics and community composition. In the present manuscript, our main objective was to describe the non-linear high-frequency variability and event-scale dynamics of phytoplankton functional groups using a trait-based cytometric approach. While we did explore environmental drivers qualitatively and through targeted analyses (e.g., cross-covariance), we acknowledge the potential value of linear multivariate techniques to better identify environmental gradients and similarities among groups. However, in our case, we have too little data to fully explain phytoplankton dynamics across all considered scales using this type of analysis. For instance, nutrients and light are two key parameters required for such an approach, and we do not have them at high frequency. As a result, the linear analyses do not yield very conclusive results, as illustrated by the nMDS presented in Fig. A, which primarily reveals the seasonality of the data: pico-organisms and NanoORG cluster together in association with high SST values, reflecting their dominance during the summer period when sea surface temperatures are higher. We acknowledge this limitation and we suggest that future studies incorporating longer time series and more comprehensive abiotic datasets (e.g., nutrients, light, mixing) would benefit from such multivariate analyses to

further characterize drivers of PFG dynamics and community composition: "*light and mixing*" (l. 440).

In the methods section some important details are missing (see details in the enclosed file) so please include them. Especially the authors need to include a section regarding extreme events since there is established body of literature on this topic and right now it is impossible to say if the authors followed it. For example, they defined a marine heatwave (MHW) showing 90^{th} percentile of the data gathered during one sampling period and to define MHW 30 years of data previous to the analyzed period are needed to generate proper climatology and then establish the 90^{th} percentile as a threshold for MHW. Similar analysis should be used to establish extreme events regarding other variables. If these methodologies were not used (or cannot be used due to a lack of data) the authors cannot use the term extreme events or MHW since they clearly defined in the literature. Therefore, they should use a different term to refer these events like maybe "case studies".

We thank the reviewer for this important comment and the clarification regarding the formal definitions of extreme events, particularly marine heatwaves (MHWs). In response, we have expanded the Materials and Methods section to include more detail on how we identified and characterized these events: "*To assess the occurrence and intensity of extreme environmental events (e.g., marine heatwave, desalination, high wind storm), we used a long-term baseline derived from high-frequency MAREL Carnot surface measurements collected between 2004 and 2023. The probability density functions of the used parameters are shown in Fig. 4. Percentile thresholds (e.g., 90^{th}, 95^{th} and 99^{th} percentiles) were computed from this 20-year dataset and used to identify values considered anomalously high, as commonly done for time series extreme event detection (e.g., Camuffo et al., 2020; Jamous and Marsooli, 2023; Hemming et al., 2024). This method provides a robust site-specific context for interpreting "rare" events*" (l. 130).

Specifically, we now explain that we used a 20-year high-frequency dataset from the MAREL Carnot station (2004–2023) to establish a robust, site-specific climatology. Percentile thresholds (90^{th}, 95^{th}, and 99^{th}) were then computed to identify environmental anomalies. This approach is consistent with methodologies used in recent literature for detecting extreme events in time series. We therefore believe the terminology used (e.g., "marine heatwave" or "extreme event") is justified in our case, as it is grounded in a multi-decade local baseline, and not based solely on the two years of this study. This addition should clarify our methodological choices and align them with established practices in the field. Further details on the changes made in the manuscript to justify each point are provided in the following section of this letter (see Sect. 1.2).

Across the manuscript the authors used Julian Day (they start in January 1, 4713 BC) when they are referring to Ordinal day (between 001 and 365), please adjust this across the manuscript.

Thank you for pointing this out. We acknowledge the misuse of terminology. Throughout the manuscript, we have now replaced all incorrect mentions of "Julian Day" with "Ordinal Day".

It is unclear to me how the authors know which species dominated each phytoplankton group determined by the CytoSub since no phytoplankton samples were collected in parallel to calibrate the composition of each group by optical or genetic taxonomic analysis. This is not a trivial detail since across the manuscript the composition of the groups is considered as a given. Therefore, more details are needed regarding how the authors are sure on this and how it can be tested since for example some phytoplankton groups like diatoms and dinoflagellates are very difficult to distinguish based on their pigments as they have the a very similar mixture of pigments.

Thank you for this important comment. The dominant taxa associated with specific clusters—such as *Phaeocystis globosa* for NanoREDs or *Pseudo-nitzschia* for MicroRED2—were addressed through the imaging protocol ("Micro-Photos") which takes pictures randomly from a pre-defined area of the cytogram where micro- and nanoplankton are detected. This information has been made explicit in the manuscript. A detailed, point-by-point description of the changes can be found in Sect. 1.2.

In fact, I think no dinoflagellate is mentioned in the manuscript. Are there no dinoflagellates in the eastern English Channel? That would be strange feature (e.g. Widdicombe et al., 2010) and something that should be discussed in some detail.

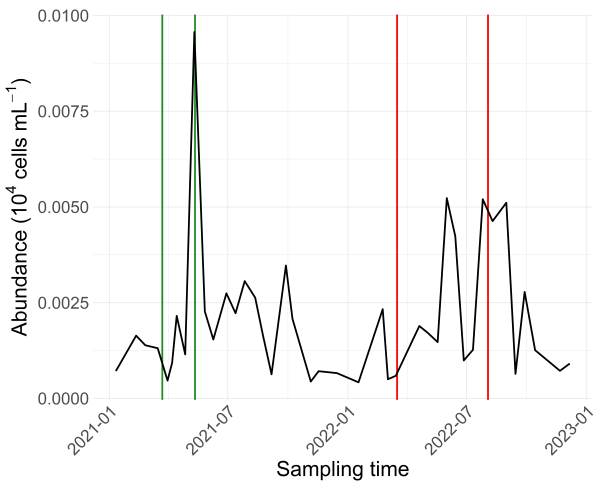

Figure B: Dinoflagellate abundances from the SNO PhytObs network (https://doi.org/10.17882/85178). Green lines indicate the 2021 cytometer deployment period, while red lines correspond to the 2022 deployment.

We agree that dinoflagellates are underrepresented in the discussion. This reflects their typically low abundance during the specific spring and early summer periods studied in this region, as also reported in previous studies in the eastern English Channel (e.g., Gómez and Souissi, 2007; Schapira et al., 2008; Lefebvre et al., 2011; Hubert et al., 2025).

Fig. B shows data from the SNO PhytObs network (https://doi.org/10.17882/85178) from Boulogne-sur-Mer. It is visible that dinoflagellate abundances remain low, even during our deployment periods ($< 0.01\ 10^4$ cells mL^{-1}).

The readability of the results section may improve if all the environmental data (including nutrients that now are only shown in the discussion section) are presented first as time series plots and then phytoplankton abundances, biomass and diversity are presented. It may be a good idea to include also a figure like figure 10 in the article for each sampling year. That way readers can easily see changes in total abundance and community composition before the analysis are shown.

Thanks for your comment. Following your advise, the times series used in the manuscript (SST, salinity, wind speed) are now presented in the Materials and Methods section (in the part on extreme events) as probability density functions: "*The probability density functions of the used parameters are shown in Fig. 4*" (l. 127).

The figure on nutrients (formerly Figure 11a) has also been splited and moved earlier in the text: "*The 2021 and 2022 data are presented in Fig. 2*" (l. 69-70).

Finally, we have added the stacked area figure as requested in Appendix 3 to avoid redundancy with Fig. 6 (former Fig. 4), which also provides this information: "*The relative abundances and FLR values for the two deployments are shown as stacked area plots in Fig. C1. The same patterns described previously are evident: a spring shift in the abundance time series (panel a) from picophytoplankton to nanophytoplankton, and in the FLR time series (panel b) from microphytoplankton to nanophytoplankton. The dominance of pico-organisms—particularly PicoORG—during summer is also observed in the 2022 data. Some extreme events are noticeable as well, such as the relative increase in MicroRED during the strong wind event of May 2021, visible as an expansion of the black area. Quick shifts, corresponding to high-frequency fluctuations, are also observable throughout the time series*" (l. 461-466).

In the discussion a paragraph is missing on how the data from the CytoSub compare to other methods as well as the limitation of this method. For example, the size range of the CytoSub leaves out the larger diatoms and dinoflagellates that may biased the results obtained towards shorter time scales

as smaller phytoplankton cells react in shorter time scales compare to larger cells. Similarly, some more context on how the dominant species for each group were established and how reliable these assignments of dominant species are for this area of the English Channel. These limitations are key to understand the reliability of the findings presented.

Thank you for your valuable comment. While we appreciate the importance of discussing the limitations of the methods used, we would like to clarify a few points where we have a slightly different opinion. Firstly, regarding the size range of particles measured by the CytoSub, it is important to note that this method is capable of acquiring particles of various sizes, including large diatoms and dinoflagellates. Previous studies (e.g., Pereira et al., 2017; Fragoso et al., 2019) have shown that the CytoSub can efficiently capture large sizes particles like ciliates and dinoflagellates. These references have been added in the text: "*capturing phytoplankton dynamics from picophytoplankton (1 µm) to microphytoplankton (800 µm width; Olson et al., 2003; Dubelaar et al., 2004; Pomati et al., 2013; Fontana and Pomati, 2014; Pereira et al., 2017; Fragoso et al., 2019; Louchart et al., 2020)*" (l. 39-41).

Furthermore, numerous comparisons have already been made with microscopy in the literature, yielding good results (e.g., McFarland et al., 2015). Regarding the identification of dominant species, these were determined using the "Micro-Photos" protocol, which associates each particle with detailed photographs. This protocol allows identification of dominant species or genera in the sample although this is not the main objective of this study.

In the manuscript the authors show how the LSP results changed drastically (almost opposite) in a few days within the same sampling period (see figure 8) so the readers may wonder how representative are the findings presented in the manuscript if the results can change so drastically in a few days. The authors should address this issue and state clearly how it affects their findings and conclusions.

Thank you for your thoughtful comment. We agree that the results shown in Fig. 10 (former Fig. 8) display significant changes in LSP over a short period of time. However, this is not related to the reliability of the analysis, but rather to the nonlinearity and temporal instability of phytoplankton dynamics. Indeed, the emergence or variation in the intensity of forcing effects on the time series of abundance and FLR is one of the key points we aimed to highlight in our study. This has been added in the manuscript: "This illustrate the emergence or variation in the intensity of forcing effects on the time series of abundance and FLR, highlighting the nonlinearity of phytoplankton dynamics over time" (l. 305-307).

Finally, something the authors should expand in the discussion/conclusion section is the fact that both deployments were shorter than one year so seasonality was not recorded in their dataset which probably biased the results towards shorter time scales. Besides, since the second deployment lasted double (6 months) than the first one (3 months) this feature should also be considered when comparing the results from each deployment.

We appreciate your suggestion, and we revised the manuscript to include a more thorough discussion on how the duration of the deployments and the absence of seasonality might have affected our results and conclusions: "*As previously noted, seasonal variations—which are essential for understanding phytoplankton dynamics—were not fully captured in this study due to its limited temporal coverage. This highlights the need for future research based on longer monitoring periods*" (l. 444-446).

1.2 Specific comments

1.2.1 Abstract

p. 1, l. 2: Please specify that this is in the English Channel so readers know where it is located

Done, "*eastern English Channel*" was added (l. 2-3).

p. 1, l. 6: not a genus, but a species

Done, "genus" was replaced by "*species*" (l. 6).

p. 1, l. 7: better to use fast or strong

Done, "high" was replaced by "*strong*" (l. 8).

p. 1, l. 10: does this refer to the 11 PFGs or total phytoplankton abundance please be specific so readers know it when reading the abstract.

Done, the term "*total*" was added (l. 10).

1.2.2 Introduction

p. 2, l. 20: the authors should define here what they understand as coastal zones since it can vary from one definition to another.

We thank the reviewer for this comment. We have clarified what we mean by coastal zones in the context of our work. Specifically, we refer to "*defined as transitional areas between marine and terrestrial environments*", in line with the definition provided by Crossland et al. (2005), which we explicitly cite (l. 20).

p. 2, l. 31–32: Light is one of the main drivers of phytoplankton dynamics so it seems rare that is not mentioned here.

This has been added: "*light*" (l. 32).

1.2.3 Materials and methods

p. 2, l. 48: Please include here the position of the station and its depth so readers have the actual numbers at hand when reading.

The location and area depth was added in the next sentence discussing the location of the station: "*50°44'25.8" N, 1°34'3.72" E*" (l. 54) and "*The depth of this area ranges between 5 and 16 m, with a mean depth of approximately 10 m (Halawi Ghosn et al., 2023)*" (l. 55-56).

p. 3, l. 56: Information of the instruments registering these variables is missing and should included in the manuscript so readers do not need to look for it elsewhere.

Informations about instruments was added to the text, according to Halawi Ghosn et al. (2023): "*with a multiparameter sensor (NKE MP6) and a static weather station (AirMAR 200WX)*" (l. 60).

p. 3, l. 59: Ok, but then you need to include here the position of the station used son readers can look how far it is from our study site. It would also good if the station used can be included in figure 1.

The location of the Météo France station in Boulogne-sur-Mer was added to the manuscript and to the Fig. 1: "*50°43'57" N, 1°35'59" E*" (l. 63).

p. 3, l. 59: Is it not possible to also have downward radiation from this station? If MAREL buoy does not register PAR it would ber very helpful to have an idea of the ammount of light reaching sea surface.

To our knowledge, radiation data are not available from the Boulogne-sur-mer Météo France station. Consequently, no PAR data could be included in this study.

p. 3, l. 62–64: Ok, but here you need to explicity say the frequency of the nutrient sampling, the depth and position of the 1 SRN-station. Similarly this station should also be shown in figure 1.

Spatial coordinates ("*50°43'90 N, 1°33'00 E*"), sampling frequency ("*fortnightly*") and measurement depth ("*0.5 to 1 m depth sampling*") have been added in the text (l. 67-68). It has also been added to the Fig. 1.

p. 4, l. 69–70: Since in these sytems conditions change abruptly in short spatial and temporal scales it is paramount to know the distance in space and time between the nutrient data and the data collected at MAREL buoy. Without this information it is impossible to know if macronutrient data are really representative of the dynamics registerd at MAREL.

The distance between the MAREL buoy and the most coastal SRN station selected for this study is 1.25 km. The GPS coordinates of each of the data sets used in this study was added as stipulated in the responses to the previous comments. It has been added to Fig. 1 caption: "*The MAREL Carnot, the Météo France (MF; 2.43 km from MAREL Carnot) and the SRN 'Boulogne 1' (BL1; 1.25 km from MAREL Carnot) stations are indicated*".

p. 5, Fig. 2: As there is quite a lot of overlap between the different groups (shown as ellipses in the plots), the authors should discuss how they dealt with cells that were in the overlapping areas. Especially as in some cases there were several groups in the same intersection between the ellipses.

Thank you for this useful comment. We acknowledge the apparent overlap between the different groups in the 2D projections shown as ellipses in the plots. To clusterise each cytogram, we used the exclusive set function of the clustering software, which ensures that each particle (or cell) is counted and assigned to a single cluster only, thereby avoiding any duplication in group assignment. Furthermore, while some overlap may be visible in specific combinations of axes, this is not the case across all dimensions. The ellipses in the plots are 2D projections of a multidimensional space, and additional combinations of axes (not all shown in the manuscript) can help to discriminate groups more clearly. We have added a clarification of this point in the caption of Fig. 3 of revised manuscript "*and assigned to a single cluster using an exclusive set function*".

p. 6, Table 1: Please include an example of which kind of picoeukaryotes would fit wihitn this group, because there can be several of them within this group.

As this method is ataxonomic, particularly for the smallest size classes where no photographs can be taken, it is not possible to provide definitive taxonomic identifications. However, based on size (< 3 µm) and fluorescence characteristics, the PicoRED group likely includes small picoeukaryotes such as *Micromonas* spp. or other similar prasinophytes. This has been added in the text: "*Yet, as our method was particularly atoxonomic for the smallest size classes where no photographs can be taken by the "Micro-Photos" protocol, it was not possible to provide definitive taxonomic identifications for the PicoRED groups. However, based on size (< 3 µm) and fluorescence characteristics, the PicoRED*

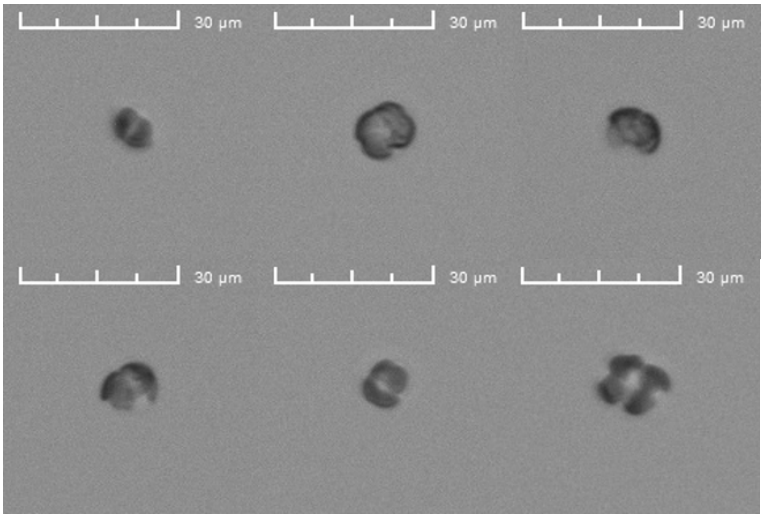

Figure C: Photograph of *P. globosa* acquired using the "Micro-Photos" protocol during deployment on MAREL Carnot.

groups likely includes small picoeukaryotes such as Micromonas spp. or other similar prasinophytes (Not et al., 2004; Masquelier et al., 2011)." (l. 116-120).

p. 6, Table 1: How do the authors know that this group mainly consists of *P. globlosa*? It should be celarly stated in the text.

The identification of this group as mainly consisting of *Phaeocystis globosa* is supported by several lines of evidence. First, previous work by Rutten et al. (2005) and Guiselin (2010) successfully identified and clustered *P. globosa* using flow cytometry in the same study area. Second, this species is known to dominate the nanophytoplankton community in the coastal zone of the study area up to 80 % during the study period (Breton et al., 2000, 2021).

Finally, this identification was further confirmed in our study by images acquired directly from the imaging flow cytometer, which allowed visual verification of cell morphology consistent with *P. globosa* (Fig. C). This was precised more explicitely in the text: "*based on photo identification*" (l. 114-115).

p. 6, Table 1: Since dinoflagellates and diatoms share the photosynthetic pigments, I am wondering how the authors know that this groups do not include dinoflagellates and are mostly comprise of diatoms. This should be clearly explained in the text.

We thank the reviewer for this insightful comment. We agree that dinoflagellates and diatoms can share similar photosynthetic pigment signatures, making it challenging to distinguish them based solely on fluorescence properties. However, in our study, the group in question was characterized not only by its pigment fluorescence but also by specific cytometric parameters such as side scatter (SWS) and morphological traits observable through imaging flow cytometry. In particular, the cells within this group

exhibited high SWS and distinct elongated or chain-like shapes that are characteristic of diatoms. Furthermore, no typical morphological features of dinoflagellates—such as cingulum or flagella—were observed in the cytometric images. These visual observations, combined with the known seasonal dominance of diatoms over dinoflagellates in the study area during the sampling period, strongly support our interpretation that this group is predominantly composed of diatoms. This has been added in the text: "*as autotrophic dinoflagellates occur at very low abundance in the EEC during this period; Gómez and Souissi, 2007; Schapira et al., 2008; Lefebvre et al., 2011; Hubert et al., 2025*" (l. 218-220).

p. 7, l. 110: This part of the methods section is clear, but the details of how this analysis was carried out are missing. Did the authors code it themselves, if so, in what language, if not, what environment was used (R, MatLab...) and what specific function/package/toolbox was used.
The R environment was used, along with the 'EMD' and 'lomb' packages. This information is available in the corresponding section (*Code and data availability*).

p. 8, l. 139: Why is a binary logarithm used when a natural logarithm is normally used to calculate H? This should be explained since this H values will not be comparable with other studies if the logarithm used is different.
There is no mathematical standard for the logarithmic base used in the formula. Base 2 was chosen here to express the measure in bits. Moreover, our aim was to make our results comparable to those of previous studies conducted in ecology using functional traits (Sun and Wang, 2021). This has been added in the text: "*The binary logarithm was used in Eq. (4) to express the measure in bits and to ensure comparability with previous ecological studies based on functional traits (e.g., Sun and Wang, 2021).*" (l. 171-172).

1.2.4 Results

p. 8, l. 158: I think the authors here use Jd as the Julian day. However, 82 is not the Julian day, but the Ordinal day/date, i.e. the number of days since the beginning of a given year, i.e. between 001 and 365.
Thank you for your comment. You are correct that the term used refers to Ordinal days rather than Julian days. We have now corrected this throughout the manuscript to reflect the appropriate terminology.

p. 9, l. 174: Since this is the first time the name of the species appears in the main text of the

article it should not be shortened here. If the authors did not test in any way that this bloom was dominated by this species, I suggest that they avoid mentioning it here. Instead, this should be part of the discussion and the authors should be more careful about stating which species dominates a PFG if it has not been tested.

Thank you for this comment. We now ensure that the full species name *Phaeocystis globosa* appears in full at its first mention in the main text as required. The sentence was repositioned within the text: "*This phase corresponded to the onset of the P. globosa bloom, a phenomenon typical of the EEC (Breton et al., 2000, 2021, 2022) and frequently monitored due to its classification as a HAB (Lefebvre and Dezécache, 2020)*" (l. 330-331).

Regarding the concern about species-level identification, we agree that caution is necessary when attributing PFG dominance to a specific species. However, in this case, we believe such attribution is justified and has now been supported by multiple lines of evidence, as detailed in the manuscript and in this letter (comment p.6 Table 1 for example). Specifically, previous work in the same region identified *P. globosa* using flow cytometry, it is known to dominate the nanophytoplankton community during the study period, and we confirmed its presence in our samples through imaging flow cytometry (Fig. C).

p. 9, l. 188: This may be partly because the deployment dates were different for each year, so the seasonality recorded was different for each year. Therefore this should be taken into consideration when comparing the results of each year.

Thank you for this comment. Here, we used a representation in Ordinal days to allow the phytoplankton dynamics of both years to be overlaid. The phenologies observed in 2021 and 2022 are indeed different, even for the Ordinal days covered by both study years (see, for example, the dynamics of NanoRED3 in 2021 and 2022 in Fig. 5).

p. 9, l. 197: There is a large body of literature on extreme events with clear methodologies on how to define and describe them. For example, there is a clear definition of a marine heat wave, which requires comparing data to a 30-year climatology (see Hobday et al., 2016). These details are missing in the methods section, so it is unclear whether these events are really extreme events such as a MHW or not. If the authors want to include this nomenclature and results, they should include it in the methods section and follow it.

Thank you for raising this important point. We agree that the definition of extreme events should follow established methodologies based on long-term climatological baselines. In our analysis, we es-

timated percentiles from a high-frequency, 20-year SST dataset (MAREL Carnot station, from 2004 to 2023; $n = 339,137$ observations), which we used as a reference to assess percentiles and characterize extreme events. While this information is currently stated in the figure legend and results part, we recognize the need to explicitly include and justify this approach in the Methods section. We have therefore revised the Methods to clarify the climatological baseline used and better align our terminology with established definitions where appropriate: "*To assess the occurrence and intensity of extreme environmental events (e.g., marine heatwave, desalination, high wind storm), we used a long-term baseline derived from high-frequency MAREL Carnot surface measurements collected between 2004 and 2023. The probability density functions of the used parameters are shown in Fig. 4. Percentile thresholds (e.g., 90^{th}, 95^{th} and 99^{th} percentiles) were computed from this 20-year dataset and used to identify values considered anomalously high, as commonly done for time series extreme event detection (e.g., Camuffo et al., 2020; Jamous and Marsooli, 2023; Hemming et al., 2024). This method provides a robust site-specific context for interpreting "rare" events*" (l. 125-130).

We also modified the Results part to better justify the event characterization: "*from 2004 to 2023, $n = 325,699$ observations*" (l. 232); "*from 2004 to 2023, $n = 151,663$ observations*" (l. 239); and "*estimated from MAREL Carnot data from 2004 to 2023, $n = 339,137$ observations*" (l. 255).

p. 10, Figure 3: The meaning of AU should be explain in the figure legend.

We have now added a clarification in the figure caption to indicate that "AU" stands for arbitrary units. This unit is commonly used in flow cytometry to represent relative fluorescence or scatter intensity. This has been added in the caption.

p. 10, Figure 3 caption: Ordinal day, please adjust this thorought the text.

We have revised the manuscript to ensure consistent use of the term Ordinal day throughout the text.

p. 10, Figure 3 caption: Looking at the figure, there seems to be a data gap for the year 2022 (between 150 and 160 approximately) that is not explained in the manuscript, so it should be explained in the methods section.

Thank you for your careful observation. The data gap observed for the year 2022 (between approximately days 150 and 160) is due to missing data due to technical issues. We have now added this explanation at the beginning of the Results section of the revised manuscript to ensure clarity regarding this gap: "*First, it is important to note that these time series are incomplete. This results from technical failures that occurred during the deployments*" (l. 179-180).

p. 10, l. 201: The details of the reference climatology for salinity that informs this 5^{th} percentile is missing, so without this information it is impossible to assess the validity of the extreme event.

We agree that the validity of an extreme event threshold depends on a well-defined reference climatology. In our case, the 5^{th} percentile for salinity was calculated using the long-term MAREL Carnot dataset (2004–2023), the same reference used for temperature-based anomalies. We have now clarified this in the Methods section to ensure transparency regarding the baseline used for salinity percentiles: "*from 2004 to 2023, $n = 325,699$ observations*" (l. 232).

p. 10, l. 201: Same comment applies to all the climatologies used to infer the presence of extreme events.

We now clarify in the Methods section that all percentile-based thresholds used to characterize extreme events (for temperature, salinity and wind) were derived from the same long-term reference dataset: MAREL Carnot surface measurements from 2004 to 2023. This ensures consistency across variables and allows reproducibility of the climatological baselines used: "*we used a long-term baseline derived from high-frequency MAREL Carnot surface measurements collected between 2004 and 2023*" (l. 126-127).

p. 10, l. 205: Wind direction is also very important to assess the effect of wind on phytoplankton dynamics so the direction of the wind should also be included here.

Thank you for this suggestion. We agree that wind direction is a key factor influencing phytoplankton dynamics, particularly in coastal systems. While our initial focus was on wind speed due to its role in mixing, we have now included information on wind direction data in the Results section to provide a more complete picture of wind-driven processes during the study period: "*coming from the south-west, with a direction between 200° and 260° relative to true north; see Appendix D)*" (l. 237-238) and "*However, the data recorded before and after the cutoff show a change in wind direction at the peak, with winds shifting from south-western to north-eastern direction for about one day (see Appendix D)*" (l. 247-248).

We also added a figure in Appendix section (Fig. D1) to provide a visualization of this event: "*The wind directions observed during the two strong wind events (see Fig. 7) are shown in Fig. D1. As previously mentioned, the 2021 event was characterized by strong southwesterly winds (ranging from 200° to 260°). In 2022, the event initially featured southwesterly winds (although PFG data were not available for this period), followed by northeasterly winds during the peak in abundance highlighted in*

Fig. 7. Unfortunately, wind data were not available precisely during this abundance peak" (l. 468-471).

p. 12, Figure 5: Please include the year on the x-axis, not just the day and month, so it is easier to know which year each figure corresponds to. The figure in the right column contains a dashed black line which I assume represents the SST, salinity or wind data, but this is not stated anywhere in the figure or legend. Please include this information somewhere.

Thank you for this helpful comment. We have addressed both aspects as follows (see Fig. D, corresponding to Fig. 7 in the manuscript):

- x-axis labeling: We agree that including the year on the x-axis improves clarity. We have now modified the x-axis labels in the relevant figures to include the year, so that the temporal context of each dataset is immediately apparent.
- Dashed black line identification: Thank you for pointing this out. The dashed black line in the right-hand figure represents the parameter specified each time on the right-y-axis. We have now specified this the corresponding legend to avoid any ambiguity.

p. 12, Figure 5 caption: This is not meaningful at all, as the percentile is only calculated for the sampling period, and then the percentile is more representative of seasonality (warmer temperatures in August) than a MHW.

Thank you for this comment. We would like to clarify that the percentile was not calculated based solely on the sampling period. As stated in the Materials and Methods section, the percentile threshold was derived from a almost 20-year historical dataset, which provides a robust climatological baseline. Therefore, the identified anomalies are not simply reflective of seasonal variations, but are assessed relative to long-term conditions, in line with standard methodologies used for marine heatwave detection. We have made this point more explicit in the revised manuscript to avoid any potential misunderstanding: "*To assess the occurrence and intensity of extreme environmental events (e.g., marine heatwave, desalination, high wind storm), we used a long-term baseline derived from high-frequency MAREL Carnot surface measurements collected between 2004 and 2023*" (l. 125-127).

p. 13, l. 221: Yes, but in order to use this methodology you need to compare your data to a 30-year climatology prior to the period being studied, so in this case you need SST data from 1990 to 2020 to apply the reference methodology. Is this the case?

While we acknowledge that the original definition of marine heatwaves by Hobday et al. (2016) is based on a 30-year historical baseline, in our case, the analysis was conducted using almost 20 years

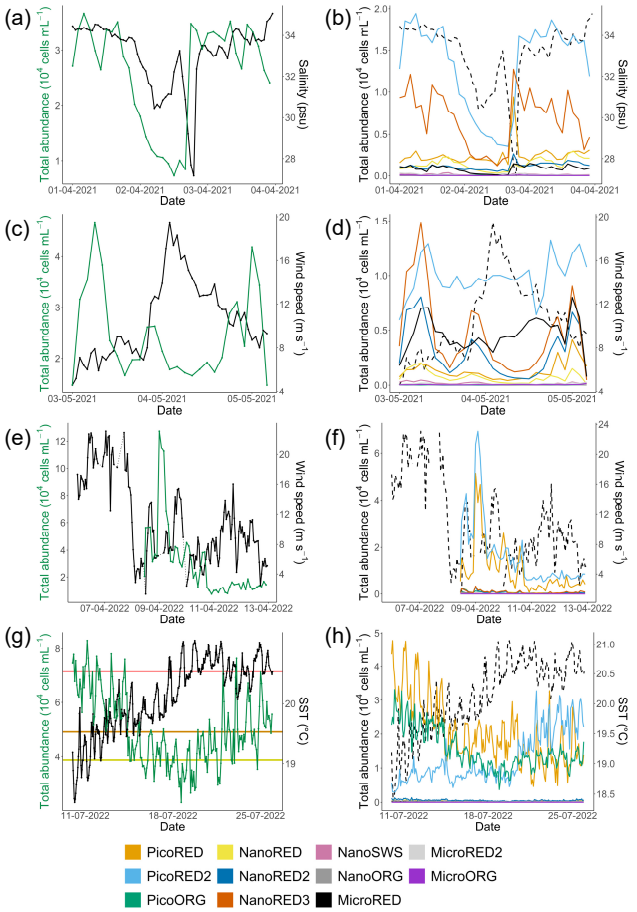

Figure D: Examples of extreme events recorded on MAREL Carnot station during the study: (a,b) a desalination event in April 2021, (c,d) a high wind speed period in May 2021, (e,f) a strong wind period in April 2022 and (g,h) a high SST event (heatwave) in July 2022. Each of the corresponding parameters is shown on the second y-axis (right axis) as a solid black line in the (a,c,e,g) left-hand figures and as a dotted line in the (b,d,f,h) right-hand figures. The absolute total abundance are given in the left-column (a,c,e,g; green line) and the absolute abundance for each phytoplankton group are given in the right-column (b,d,f,h; colored lines). The three horizontal lines of the figure (g) represent the 90 % (yellow), 95 % (orange) and 99 % (red) percentile of the SST observations of the MAREL Carnot station between March 24$^{\text{th}}$ 2004 and September 30$^{\text{th}}$ 2023. The abiotic data (salinity, SST, wind speed) presented here comes from (a,b,e,f,g,h) MAREL Carnot station and (c,d) Météo France (Boulogne-sur-Mer station).

of high-frequency SST data from MAREL Carnot station (2004–2023, corresponing to 339,137 observations), due to data availability for this specific region. However, previous studies (Simon et al., 2023) have highlighted the intense activity of marine heatwaves in our study area in 2022 (during our deployment), further supporting the reliability of the designation applied to this event. This has been added in the manuscript: "*We did not have 30 years of data as recommended to define the marine heatwave threshold, but this event qualification was supported by other studies in the same study location showing an intense activity of marine heatwaves during summer 2022 in the English Channel (Simon et al., 2023).*" (l. 256-258).

p. 13, l. 234-235: Please, also include here the time in hours or days of these IMf so we can compare with the previous ones.

We have now added the mean period of each IMF cited in the text: "*The third IMF, C_3, corresponds to two tidal cycles ($\overline{T_3} = 1.07$ days), while the fourth and fifth IMFs, C_4 and C_5, are likely related to tidal cycle impacts at different timescales ($\overline{T_4} = 2.09$ days and $\overline{T_5} = 4.40$ days)*" (l. 273-275).

p. 13, l. 244: Looking at the figure, I assume that VLost represents the cumulative variance lost from the longest IMF to the shortest IMF, because otherwise the variance lost will be more than 100%. This is not explained anywhere in the manuscript or in the figure.

This is intrinsically due to the definition of V_{lost} given by Eq. (3). However, the term "*cumulative*" (l. 154) was added in the corresponding section of the Materials and Methods.

p. 14, Figure 6: Please clearly indicate in the figure what is shown on each axis for the figures to the left and right of the colleague. Units are missing in the figure and the legend.

We have updated the figure and its legend to clearly indicate what is shown on each axis for both the left and right panels, and we have added all missing units to ensure clarity and completeness.

p. 14, Figure 6 caption: Ordinal day

The caption has been corrected to use "Ordinal day" as suggested: "*The EMD x-axis represents the Ordinal days and the y-axis represents the abundance values (10^4 cells mL^{-1})*".

p. 15, Figure 7: Why data from 2022 are not presented also in figure 6 and 7 or in similar figures? It seems quite arbitrary.

Thank you for this comment. We agree that the selection of 2021 data in Figs. 6 and 7 is somewhat arbitrary. Our intention was to provide a representative example to illustrate key patterns, while keeping the figures clear and readable. All relevant data, including from 2022, are fully presented in the accompanying summary table (Table 2), which allows for direct comparison across years. We added this information in the text: "*The results obtained with EMD-LSP analysis of the 2021 abundance time series were here arbitrary chosen to be presented in detail, to provide an example of the EMD-LSP analysis results interpretation. Nevertheless, all results for the other time series are presented in Table 2*" (l. 266-268).

p. 15, Figure 8: The previous figures are focused on 2021 data then why figure 8 only shows re-

sults from 2022. It makes the manuscript quite hard to follow. Please keep a clear thread for the readers in the results section.

Thank you for this observation. As noted in our previous response, the selection of specific years in Figs. 8 to 10 (former Figs. 6 to 8) was intended to illustrate representative examples rather than to provide an exhaustive year-by-year comparison. Yet, all complete results are available in the accompanying summary table (Table 2) for consistent comparison. Indeed, the results presented in Fig. 8 are also provided in the Table, and the results for the other used time series (FLR, Shannon index and abundance for the two years) are also provided.

p. 15, Figure 8 caption: Here the authors show that the results of the main analysis of the article do not hold for an entire sample period (year 2022), so I wonder how representative the results shown in Figure 6 are for the entire period 2021. Similarly, the authors should clearly address why these changes in the results obtained from the same analysis do not reduce the representativeness of their results and conclusions if the results obtained change so drastically depending on the time period analysed within a few weeks.

What we are studying with the EMD-LSP analysis is a global dynamics. The EMD is capable of distinguishing modes in the signal that are irregular (modes whose influence is nonlinear). These changes in dynamics are thus accounted for in the analyses. Here, in Fig. 10 (formerly Fig. 8), we separate the 2022 series and use only the LSP (without the EMD) to illustrate these changes in phytoplankton dynamics, highlighting the nonlinearity of phytoplankton dynamics over time. This has been added in the text: "*This illustrate the emergence or variation in the intensity of forcing effects on the time series of abundance and FLR, highlighting the nonlinearity of phytoplankton dynamics over time*" (l. 305-307).

p. 16, Table 2 caption: cummulative?

As explained before, V_{lost} is cumulative due to its definition.

p. 16, Table 2: How this term was calculated is not explained in the text.

Thank you for pointing this out. The calculation of the term in question, specifically the Shannon entropy $\exp(H')$, is described in detail in the Methods section under "Shannon diversity index H'" (Section 2.3.3). It is applied for FLR in the same way than for abundance data. It has been added in the text: "*or relative FLR in our case*" (l. 162).

p. 18, l. 269: How are the authors sure that the bloom consists of *P. globosa*?

As previously mentioned, the identification of the bloom as *P. globosa* is supported by earlier studies in the same area using flow cytometry (Guiselin, 2010), the known seasonal dominance of this species in the region (Breton et al., 2000, 2021), and visual confirmation of cell morphology from imaging flow cytometry conducted in our study.

1.2.5 Discussions

p. 18, l. 291: Light conditions are never shown in the article neither tested.

Thank you for this comment. Here the goal was to provide a context based on the literature of the area. We made it more explicit changing the sentence as follow: "*The P. globosa spring bloom is known to occur when abiotic conditions become favorable, such as nutrient availability and light conditions according to the literature*" (l. 335-336).

p. 18, l. 295: Again how do the authors know that this species was dominating this group?

The identification of this group as *Pseudo-Nitzschia* is based on seasonal occurrence patterns previously documented in the study area (Delegrange et al., 2018), combined with visual confirmation via the "Micro-Photos" protocol, which enabled us to observe morphological features consistent with this taxon (see Fig. E). While we acknowledge the limitations of optical identification alone, the convergence of temporal context and imaging supports *Pseudo-Nitzschia* as the dominant taxon within this group during the sampling period. Nevertheless, we adjusted the text to add more examples of possible species, and replaced the term "likely" that was to determinative: "*e.g., P. delicatissima, P. pungens, and P. seriata in this season*" (l. 341-342).

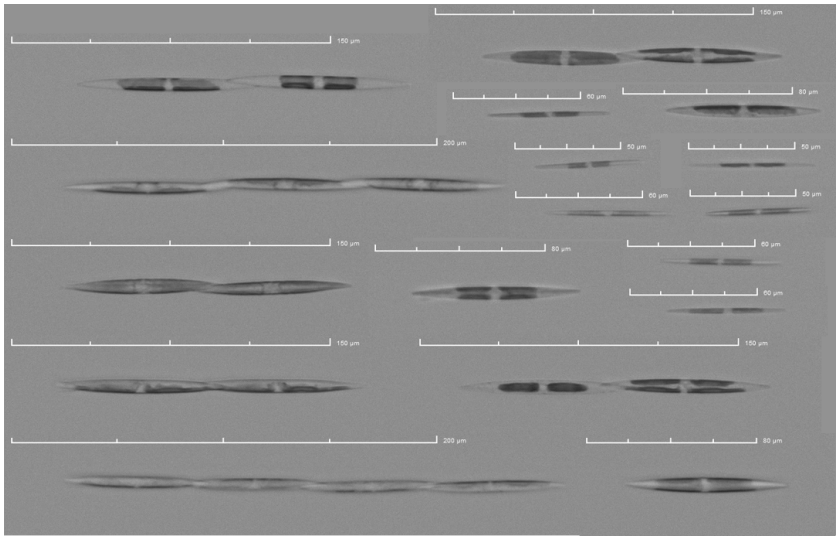

Figure E: Photograph of *Pseudo-Nitzschia* acquired using the "Micro-Photos" protocol during deployment on MAREL Carnot.

p. 18, l. 296: What was confirmed by this protocol? Because this protocol is not able to confirm the taxonomic composition of the group at a species or complex level.

As stated in the Materials and Methods section, the "Micro-Photos" protocol allows for the acquisition of images of some randomly selected particles from a pre-defined area of the cytogram where micro- and nanoplankton are detected. While this protocol does not allow for a definitive confirmation of the taxonomic composition at the species or complex level, it can suggest a possible composition of dominant groups—particularly within the Micro and Nano PFGs clusters based on visual characteristics observed in the images. Moreover, this information is supported by the analysis of pulse shapes, which provide an *in silico* image of the cells and colonies for all detected clusters, including those for which actual images are available. This information has been added in the text: "*While not allowing for a definitive taxonomic identification at the species or complex level, these images can suggest a possible composition of dominant groups—particularly for micro- and nano- PFGs clusters. This visual information is further supported by the analysis of pulse shapes, which provide an in silico image of cells and colonies across all clusters*" (l. 105-108).

p. 19, Fig. 11: Why are these data only displayed for 2022? In b the $NO_2^- + NO_3^-$ line is missing in the first days shown.

These data are shown only for 2022, as the succession between the NanoRED3 and NanoRED2 groups could not be fully observed in 2021 due to the earlier termination of the deployment that year. Therefore, only 2022 data are presented here to illustrate the potential link between nutrient dynamics and this succession.

In the former Fig. 11b, $NO_2^- + NO_3^-$ was not visible during the first days because the concentrations are very high and compress the rest of the data range (see Fig. F, top panel), making interpretation difficult. We so reproduced the figure with a y-axis $\log_{10}$ representation (see Fig. F, bottom panel): "*The logarithmic scale for the y axis was chosen for visualization purposes*" (Fig. 13 caption).

p. 21, l. 349: This was not tested as the composition was not taken into account in the analysis. The H index may be the same for communities with very different community composition as long as the relative abundance is similarly distributed. In order to test this, a different analysis should be carried out where the composition of the community is taken into account.

Thank you for this comment. We agree with the statement and have therefore replaced the term "composition" with "*assemblage*", which seems more appropriate for what we intended to convey.

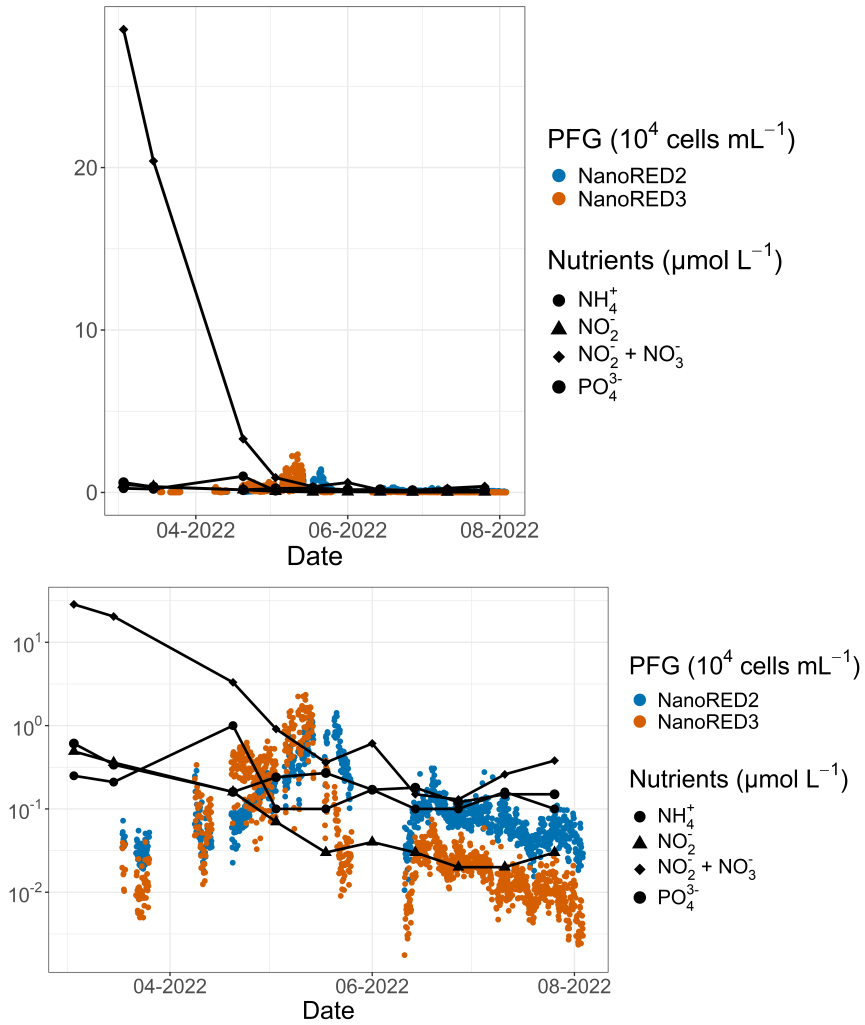

Figure F: *Top panel:* Reproduction of the former Fig. 11b when conserving all the values. *Bottom panel:* New version of the figure, with a log-y axis (Fig. 13 in the revised manuscript).

Indeed, since all 11 PFGs are consistently included in the calculation of the Shannon index, any variation reflects a change in the assemblage of these groups rather than their composition, as previously stated. This change was made for all occurrences of "community composition" in the manuscript.

p. 21, l. 349: Only changes in community diversity were analyzed so please adjust the analysis or just focus on this part.

Please refer to the previous comment answer.

p. 21, l. 354–356: This sentence is confusing please rephrase it and extend it so readers have a clear idea of the reasons for such a strong and abrupt change.

The sentence has been clarified: "*This change could be attributed to differences in the life cycles of the dominant picoorganisms, which became more prevalent than the nano- and microphytoplankton that dominated during the spring bloom. Indeed, during this period, the community was largely composed*

of picoeukaryotes (PicoRED) and cyanobacteria (PicoORG) which are known for their rapid growth rates, short generation times, and ability to quickly respond to changes in environmental conditions (Agawin et al., 2000; Worden et al., 2004)" (l. 404-409).

p. 21, l. 361–363: This seems like a probable explanation so I suggest the authors to test it by seeing the correlation between FWS and abundance to see if the have a negative and significant correlation.

The Pearson correlation coefficient have been estimated, and the following sentence has been added: "*We found a strong negative Pearson correlation between abundance and PicoORG FWS per cell ($r = -0.84$, p value < 0.01), and between abundance and PicoRED FWS per cell ($r = -0.78$, p value < 0.01) during this period, supporting this hypothesis*" (l. 415-417).

1.2.6 Conclusions

p. 22, l. 378–382: I suggest the authors to highlight here the fact that most of the variance was explained by very-short time scales (hours) which is more novel than phytoplankton changes in scale of days.

Thank you for this helpful suggestion. We agree that the fact most of the variance is explained by very short time scales (hours) is an important finding. We have now highlighted this point in the text to emphasize its significance in relation to longer time-scale changes in phytoplankton dynamics: "*Our findings reveal that much of the variability in these time series occurs at short time scales, particularly on the order of a few hours*" (l. 434-435).

p. 22, l. 383–388: This is a key point since the deployments were shorter than a year so the variance explained by seasonal changes is missing and they may represent a very important fraction of the phytoplankton dynamics of this area.

Thank you for raising this point. As mentioned in the conclusion, we acknowledge that the study's limited duration, constrained by the system's design, does not capture the full seasonal cycle, and seasonal variations, potentially significant for phytoplankton dynamics, were not fully explored. We emphasize the need for longer-term monitoring to better assess these seasonal changes and their implications for phytoplankton dynamics in the context of global change and anthropogenic pressures. This following sentence was added: "*As previously noted, seasonal variations—which are essential for understanding phytoplankton dynamics—were not fully captured in this study due to its limited temporal coverage. This highlights the need for future research based on longer monitoring periods.*" (l. 444-446).

1.2.7 Appendix

p. 23, Fig. A1: In many cases in this type of figures the limit for the significant covariance is shown in the figure.

In general, this kind of significance threshold is shown for cross-correlation functions. The cross-correlation function $C(\tau)$ of two zero-mean time series $X(t)$ and $Y(t)$ is obtained as follow:

$$C_{X,Y}(\tau) = \langle X(t)Y(t+\tau)\rangle$$

where $\langle\cdot\rangle$ mean statistical average. The cross-covariance function is defined as:

$$\gamma_{X,Y} = C(\tau) \cdot \sigma(X)\sigma(Y)$$

Unlike the cross-correlation function, the amplitude of the cross-covariance depends on the variances of X and Y, making the definition of significance thresholds more complex. In such cases, empirical approaches (e.g., permutation tests or surrogate data) are commonly used to assess statistical significance. But here it is beyond the scope of our analysis, as we used it not to study directly the numerical values of $\gamma_{X,Y}$ but only its dynamics in time (especially the 12-hours periodicity).

2 Referee 2 comments (RC2)

2.1 General comments

This study uses automated flow cytometer data to study the phytoplankton dynamics in a coastal system of the Eastern English Channel during spring in two consecutive years and summer in one year. The authors are able to retrieve from these high temporally resolved data information on the abundance and red fluorescence of 11 phytoplankton functional groups (PFGs). These data are used to investigate these groups' succession and their relation to four extreme events (heat wave, desalination, storms). The study reads very well, the methodology is mostly well described and the findings are nicely illustrated and discussed. Overall, I have just a few comments and suggestions for improving the manuscript. I think after applying these revisions the manuscript is ready for acceptance in Ocean Science.

Thank you for your positive feedback and thoughtful comments. We are glad to see that the study is well-received, and we appreciate your recognition of the methodology and findings. We will carefully address the revisions you suggested and are confident that they will further strengthen the manuscript.

2.2 Specific comments

1. The study uses many abbreviations which are always introduced and explained, still it could be helpful for reader to be able to look these easily up in a list of abbreviations.

Thank you for your comment. A glossary with all the abbreviations in the paper has been added as an appendix (see Table 1, corresponding to Appendix A1): "*The definition of the abbreviations used all along this article are provided in Table A1*" (l. 454).

Table 1: Glossary.

Abbreviations	**Definition**
EEC	Eastern English Channel
EMD	Empirical Mode Decomposition
FLO	Orange Fluorescence
FLR	Red Fluorescence
FLY	Yellow Fluorescence
FWS	ForWard Scatter
HF	High Frequency
IMF	Intrinsic Mode Function
LSP	Lomb-Scargle Periodogram
PFG	Phytoplankton Functional Group
ROFI	Region Of Freshwater Influence
SST	Sea Surface Temperature
SWS	SideWard Scatter

2. Although all the relevant protocols for the analysis of the flowcytometer data to PFG abundance and PFG red fluorescence are referenced it would be helpful to briefly summarize the steps how the PFG abundance data and red fluorescence data are converted into relative values (as presented in Figure 4).

Thank you for your comment. Relative values are estimated in the conventional way: the value (abundance or FLR) of each group is divided by the total sum across all groups. This process has now been specified in the caption of Fig. 6 (former Fig. 4) for clarity: "*The relative values of each group are computed as the ratio of the group's value to the total sum across all groups*" (Fig. 6 caption).

3. Table 1: I suggest to add the size ranges of each group to the table.

The size range for each group was added in new column to Table 1, following the theoretical size class provided by Thyssen et al. (2022): "*Thyssen et al. (2022) optical group common vocabulary equivalents and associated size range are also provided.*" (Table 1 caption).

4. Lines 164-165 should be moved to the discussion.

Thank you for your suggestion. The sentence has been moved to the first paragraph of the "Weekly-scale dynamics" section in the discussion.

5. Fig.3-5 add the months under the Julian days for better understanding.

Purple (or gray) lines indicating the months have been added for Figs. 5, 6, 8 and 11 (former Figs. 3, 4, 6, and 9) but not for Fig. 7 (former Fig. 5) as the x axis is not in Ordinal day.

6. Line 179 the 2nd bracket needs to be revised.

The bracket "[" have been corrected to ")", indicating a semi-enclosed interval.

7. Line 180: change sentence to "In addition, the PicoORG group contributed on average with 7 % between March 23$^{\text{rd}}$ and March 27$^{\text{th}}$ ($Jd = 82$ and 86) to the total abundance."

Thank you for your suggestion. The sentence have been changed as proposed (l. 209-210).

8. Comparison of Fig.3 and Fig.4: please comment also on the correspondence of the groups' abundances to red fluorescence. Explain further the relationship not only to phytoplankton composition but also physiological state?

Thank you for your comment. We have added a some sentences of the correspondence between the groups' abundances and red fluorescence in the manuscript. The fluorescence data reflects not only the presence and abundance of specific groups but also their metabolic activity and health, offering a more comprehensive view of their physiological state during the study period. This relationship is now further explored in the revised manuscript: "*This pattern may correspond to the initial diatom bloom, where the high FLR values reflect the metabolic activity of these cells. These high FLR per cell values may also reflect photo-acclimation to low light at the beginning of the blooming period (Houliez et al., 2013)*" (l. 195-197); "*signaling shifts in both community assemblage and physiological state*" (l. 198-199); "*and the associated increase in FLR indicates higher physiological activity during this phase*" (l. 201-202); "*The rise in FLR from NanoRED2 suggests enhanced metabolic processes and productivity in this group*" (l. 204-205).

9. Line 213: I think it should read "Unfortunately, PFG data ..." not wind data"?

Only the wind data were not available on April 9$^{\text{th}}$. In fact, looking at Fig. 7e (former Fig. 5e), we can notice that the black line is not complete (dashed line) during the high abundance peak (higher

than $12{\cdot}10^4$ cells mL^{-1}).

10. Figure 6 caption: change "residue" to "residual" and add here 10^4 so it reads: "the the *y*-axis represents the abundance values (in 10^4 cells mL^{-1})."!

This has been corrected: "*Decomposition of the 2021 total phytoplankton abundance data using the Empirical Mode Decomposition (EMD; left part) and the associated Lomb-Scargle Periodograms (LSP; right part) for the raw time series $X(t)$, for each IMF $C_i(t)$ and for the residual $r_n(t)$. The EMD x-axis represents the Ordinal days and the y-axis represents the abundance values (10^4 cells mL^{-1}). The LSP x-axis represents the decimal logarithm ($\log_{10}$) of the frequency (in h^{-1}) and the y-axis represents the normalized power. The gray dashed lines (left column) represent the beginning of April and May 2021*" (Fig. 8 caption, former Fig. 6).

11. Figure 7 caption: change to "... The black dashed lines represent the estimated linear regression."

This has been done: "*The black dashed lines represent the estimated linear regressions*" (Fig. 9 caption, former Fig. 7).

12. Lines 260-263: this text concerns what is presented in Figure 8b (reference is missing!).

The reference has been added: "*A new peak appeared at 24 hours, highlighting the changing temporal variability of the environmental forcing on phytoplankton communities (Fig. 10b).*" (l. 304-305).

13. Line 269: it is unclear to which event (to which figure) this sentence links to – maybe this should be moved to the discussion?

We chose to retain this sentence in the Results section, as it presents a direct observation. However, it has been revised to clarify the event it refers to, now explicitly mentioning the corresponding figures and the cytometric groups (PFGs) involved, to improve clarity for the reader: "*In contrast, $H'_{abundance}$ peaked during the NanoREDs bloom (Fig. 11a), while H'_{FLR} showed an opposite trend (Fig. 11b)*" (l. 313).

14. Figure 11a) shows for many nutrients the time series data, but only PO_4^{3-} is discussed – why?

Thank you for the remark. While our interpretation focused on PO_4^{3-} due to its commonly limiting role in phytoplankton blooms in coastal waters, we agree that a broader discussion of the other nutrients shown in Fig. 2 (former Fig. 11a) is relevant. We have therefore expanded the corresponding

paragraph to include references to silicate, nitrite and nitrate which also influence bloom dynamics and community composition and ammonium for the post-bloom: "*This difference in bloom intensity is also reflected in the post-bloom phase, with higher ammonium concentrations in 2021 suggesting more intense remineralisation compared to 2022*" (l. 346-347) and "*Moreover, limiting nutrients (*PO_4^{3-}*,* $Si(OH)_4$*,* NO_3^-*, and* NO_2^-*), were rapidly depleted at the onset of the bloom, consistent with the growth of diatoms (MicroRED2) and haptophytes (NanoREDs)*" (l. 349-350).

15. Line 309-311: can you add the reference to a figure at which this event is visible.

Thank you for the suggestion. A reference to the relevant figure illustrating this event has been added in the sentence ("*see Figs. 5 and 6*"; l. 360).

16. Line 368: I suggest to change "picoorganisms" to "picophytoplankton".

This has been done: "*monitoring the long-term high-frequency dynamics of picophytoplankton*" (l. 423).

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