

EGUSPHERE-2025-5902

Point-by-point response to reviewers' comments

New insights into the primary production and the structure of the phytoplankton community in the South Indian Ocean using size fractionation experiments

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EGUSPHERE-2025-5902 Research article

Reviewer #2 – anonymous reviewer

We would like first to thank the reviewers for their relevant comments and suggestions which helped us to improve our manuscript. Reviewer comments are in black font, and responses are in blue font. Figures that have been modified in the revised manuscript were added to this document. Below are the authors' point-by-point responses to the reviewer's comments and questions.

General comments:

In this manuscript, Valentin Deteix and colleagues analyze the size structure of phytoplankton biomass and primary production in the Southern Indian Ocean during the austral summer of 2023. The authors partitioned the phytoplankton community into three size classes: picophytoplankton ($< 3 \mu\text{m}$), nanophytoplankton ($3\text{--}20 \mu\text{m}$), and microphytoplankton ($> 20 \mu\text{m}$). They then estimated the contribution of each size class to total Chlorophyll-a (TChl-a) and net primary production (NPP) across all stations analyzed. Furthermore, the authors estimated the contribution of major phytoplankton groups to Chl-a within each size class. Their findings indicate that TChl-a was primarily controlled by the nano- and microphytoplankton size classes, while integrated NPP was dominated by the microphytoplankton size class. Regarding phytoplankton group contributions to total Chl-a, the data revealed a latitudinal gradient consistent with previous reports in the Southern Ocean. These data are particularly interesting because phytoplankton studies often focus on specific groups rather than on the entire phytoplankton community. Moreover, the study area is an undersampled region of the global ocean, and therefore, the presented results are valuable and deserve to be published.

Overall, the manuscript is well-presented and reasonably well-written, the methodology is appropriate, and the figures are of good quality. However, several points require improvement. In general, the authors differentiate phytoplankton by size and major groups but scarcely mention the phytoplankton species or genera present in the region. Although their methodological approach does not allow for species-level identification, comparing their results with previously published information on the species present in the study area could aid in some of their interpretations.

Thank you for your comment. Following your recommendations, we have updated the discussion in several sections, by including phytoplankton genera that have previously been observed in the study area – and in other sectors of the Southern Ocean – and that are consistent with our results based on size-fractionated phytoplankton chemotaxonomic groups biomass.

Additionally, the description of the results, which often refers to stations numerically, is sometimes difficult to follow. It would be helpful to refer to the zonal systems where they are located, as this would improve the readability of the text. Below, I include a series of more specific points that the authors should consider for the next version of the manuscript.

Thank you for your comment. We have specified the hydrographic regions associated with the stations in the study area throughout the manuscript.

Specific comments:

Line 16. Could the authors specify the regions analyzed during the experiment?

The sentence has been rephrased to specify the biogeochemical regions analyzed in this study (modifications in bold):

“As part of the South Indian Ocean CARBOn fluxes from the surface to the mesopelagic twilight zone (SOCARB) project, the phytoplankton biomass and net primary production (NPP), along with the biomass of phytoplankton chemotaxonomic groups, were assessed **during late austral summer 2023 in contrasting biogeochemical areas: the oligotrophic subtropical waters of the South Indian Ocean, High Nutrient Low Chlorophyll (HNLC) waters, and the highly productive waters in the vicinity of Kerguelen Islands in the Southern Ocean.**”

Line 40. The term "irrelevant" is not the most appropriate here. Maybe "insufficient" or "too general"? Please rephrase.

The term “irrelevant” has been replaced with “insufficient”.

Line 49. Please replace the comma with "and" before dissolved inorganic phosphorous.

The comma before dissolved inorganic phosphorus has been replaced with “and”.

Line 58. What about silicate concentrations in the AZ?

Surface dissolved silicon concentrations in the AZ are usually $> 20 \mu\text{mol.L}^{-1}$ (Sarmiento et al., 2004). The following addition (in bold) is now included in the manuscript:

“The Polar Frontal Zone (PFZ), between the PF and the Subantarctic Front (SAF), and the Subantarctic Zone (SAZ), between the SAF and the STF, display high NOX and DIP concentrations but low dissolved silicon (DSi) concentrations (usually $< 5 \mu\text{mol L}^{-1}$), **in contrast to HNLC waters of the AZ (usually $> 20 \mu\text{mol.L}^{-1}$)**, resulting in High Nutrient Low Silicon Low Chlorophyll (HN-LSi-LC) conditions (Nelson et al., 2001; Sarmiento et al., 2004).”

Line 60. "SIO". Please reduce the use of acronyms such as this one. It will facilitate understanding of the text.

The acronym “SIO” has been removed from the manuscript.

Line 65. "Euphotic layer". Do the authors mean deeper layers of the euphotic zone, or subsurface layers of the photic zone? Please rephrase, as the upper surface layer (mentioned in the previous sentence) also belongs to the euphotic layer.

By "Euphotic layer", we mean the productive layer, which includes both subsurface and deeper layers of the euphotic zone. We have replaced the previous term "upper surface layer" with "surface layer", to avoid any confusion with the generic term "upper layer" and to stress the point that most previous data on phytoplankton biomass, composition and productivity in this area were sampled at ~10 m depth only.

Line 69. Please add "and" before Heard and McDonald Islands.

This has been added.

Line 74. Please provide background information about the differing roles of the different phytoplankton groups and size classes in the biological pump. So far, it is not clear in the text why it is important to differentiate between phytoplankton size classes.

In the initial manuscript, we provided some context in the first paragraph of the introduction section about the different roles of phytoplankton size structure in the biological pump, without going into extensive detail. To underline the importance of distinguishing between phytoplankton size classes, the following addition (in bold) has now been included in the first paragraph of the introduction:

"More specifically, the taxonomic composition and the size structure of phytoplankton communities can affect significantly the intensity and fate of NPP by controlling the photosynthetic CO₂ uptake efficiency (*e.g.* Cermeño et al., 2005), the transfer of NPP through either microbial trophic pathway or higher trophic levels (*e.g.* Marañón, 2009) and the carbon export and sequestration in the deep ocean (*e.g.* Guidi et al., 2009). **For instance, phytoplankton communities dominated by large cells are expected to contribute greatly to organic carbon export through their faster sinking velocity rates and more efficient transfer towards higher trophic levels compared to phytoplankton communities dominated by smaller cells (Legendre and Le Fèvre 1989; Wassmann 1998). Also, particulate organic carbon export may be enhanced when phytoplankton communities are dominated by biomineralizing organisms, as mineral ballast increases particle sinking rates (Armstrong et al., 2001; Klaas and Archer, 2002).**"

Line 79. "The long-term monitoring of oceanic CO₂ parameters in the SIO". Please provide references.

The following reference has been added in the manuscript and in the references section: Metzl, N., and Lo Monaco, C.: OISO-Océan Indien Service d'Observation, <https://doi.org/10.18142/228>, 1998.

Line 92. Could the authors relate these regimes to the stations shown in Figure 1? This could be done either here or later in this section.

Because Figure 1 provides an overview of surface chlorophyll *a* without surface nutrient data, these regimes cannot be directly related to the stations at this stage. The attribution of stations to these regimes is presented later, at the beginning of the Results section (section 3.1 and table 1).

Figure 1. Please identify the islands on the map.

The figure 1 has been updated following your recommendations.

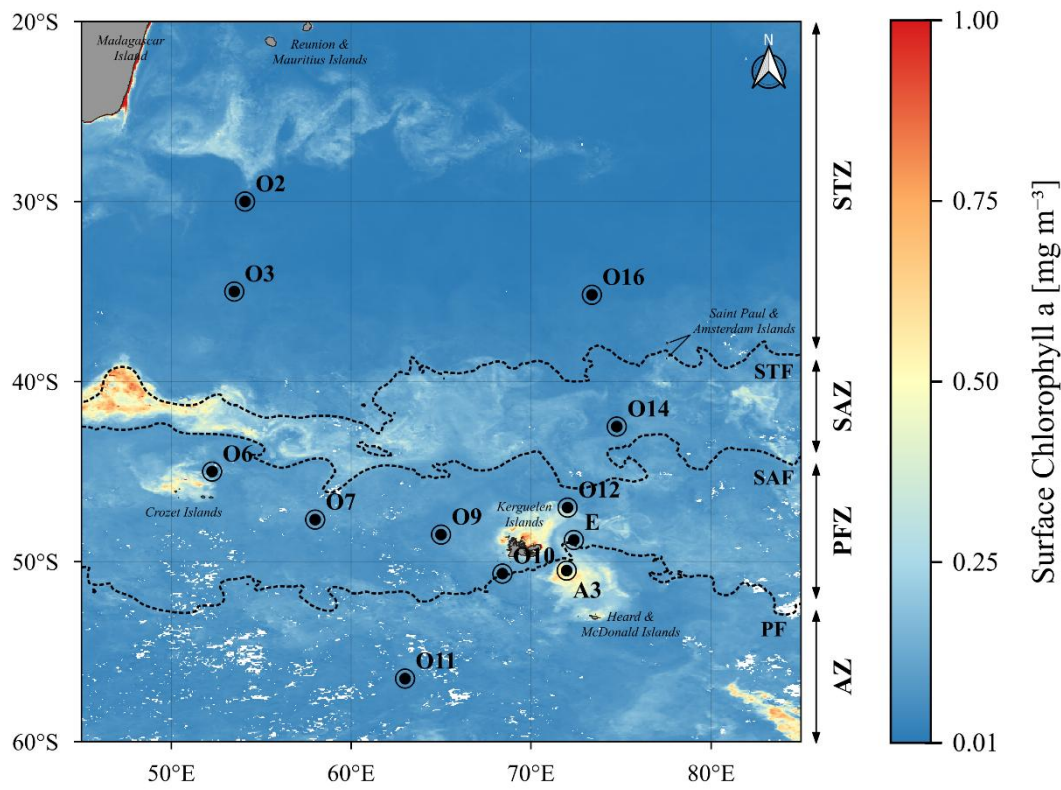


Figure 1: Map of the OISO33-SOCARB study area showing the location of the stations from this study, overlying the satellite-derived surface chlorophyll a concentration averaged over February 2023 (MODIS L3 product). The dotted lines indicate the positions of the main fronts determined from satellite-derived surface temperature averaged over February 2023 (CMEMS L4 product): STF, Subtropical Front (18°C); SAF, Subantarctic Front (13°C); PF, Polar Front (4.5°C).

Lines 99-103. So, this means that different depths were sampled at each station? Please clarify and provide specific depths somewhere in the text or as supplementary material.

That is correct. The specific depths are shown in the vertical profiles for each station displayed in Appendix A. Also, all data associated with this study – including the specific depths sampled for each station – will be available in the section “Data Availability” section through a DOI on the SEANOE database.

Line 200. Please write SML in full the first time it is mentioned in the text.

This has been taken into account.

Line 201. This information is very interesting, but it would be useful for the non-specialized reader if it were explained in further detail why values lower than the Redfield ratio are interpreted as N limitation. Please also extend your explanation to all the statements related to nutrients in this paragraph.

The paragraph, describing the distinct biogeochemical regions at the stations within the study area, has been revised to provide clarification on the relationship between the NO_x/DIP and DSi/NO_x ratios and associated nutrient limitations, while remaining concise. The terms “optimal” and “suboptimal” have been replaced to avoid any potential confusion. Modifications are indicated in bold:

“The study area can be further subdivided into distinct biogeochemical regions, with contrasting surface TChl_a and nutrient concentrations in the surface mixed layer (SML) (Table 1). In the STZ, stations O2, O3 and O16 exhibited LNLC conditions, with very low surface TChl_a. The NO_x/DIP ratios in the SML were notably lower than the Redfield ratio of 16/1 (Redfield, 1958), indicating **a relative deficiency of NO_x with respect to DIP for phytoplankton nutritional requirements, and thus suggesting** a potential NO_x limitation of the phytoplankton activity. Station O11 in the AZ featured HNLC conditions, with low surface TChl_a despite high macronutrient concentrations. The NO_x/DIP and DSi/NO_x ratios in the SML were close to **the Redfield and Brzezinski ratios** (Si/N for diatoms = 1.12 ± 0.33 , Brzezinski 1985), **indicating that NO_x, DIP and DSi were not limiting, and thus** suggesting a potential micronutrient limitation. Stations O6, O7, O9 **in the PFZ** and O10 **in the AZ** shared similar features with O11 **(AZ)** but exhibited lower surface DSi concentrations, leading to DSi/NO_x ratios **notably lower than the Brzezinski ratio**. These stations exhibited HN-LSi-LC conditions, indicating a potential (co-)limitation by Si (**Pondaven et al., 2000**). Station O14 stood out from the latter HN-LSi-LC stations, exhibiting lower NO_x, DIP and DSi concentrations in the SML along with NO_x/DIP and DSi/NO_x ratios **below the Redfield and Brzezinski ratios**. Stations O12, E and A3, located in the naturally Fe-fertilized Kerguelen bloom (Blain et al., 2008; Qu  rou   et al., 2015), exhibited the highest surface TChl_a and **a DSi/NO_x ratio in the SML lower than the Brzezinski ratio, indicating a potential (co-)limitation by Si**. These stations were grouped into a region hereafter referred to as “Kerguelen bloom” (KER), which differed from the offshore stations in the PFZ (O6, O7, O9) and AZ (O11, O10) (Table 1).”

Line 209. What values are suboptimal and why? Please explain in further detail.

The terms “optimal” and “suboptimal” have been removed to avoid any confusion. Please refer to the previous comment for the revised paragraph, with modifications in bold.

Lines 247-249. "By normalizing NPP to TChl_a (NPPTChl_a, mgC mgTChl_a⁻¹ d⁻¹) which can reflect photosynthesis efficiency under given environmental conditions (e.g. light/nutrient availability; Cerme  o et al., 2005), NPPTChl_a TOTAL was maximal in the SML at all stations, except at O3 and O6 where it peaked below the SML, and decreased with depth (Fig. 2k-o; Fig. A3 in Appendix A)." Please rephrase this sentence.

The sentence has been rephrased to separate the computation and purpose of the parameter NPP^{TChl_a}, from the results. Modifications are indicated in bold.

“By normalizing NPP to TChl_a, **we calculated NPP^{TChl_a} (in mgC mgTChl_a⁻¹ d⁻¹)** which can reflect photosynthesis efficiency under given environmental conditions (e.g. light/nutrient availability; Cerme  o et al., 2005). NPP^{TChl_a}_{TOTAL} was maximal **in the first 50 m** at all stations, [...].”

Line 309. This is interesting because dinoflagellates are often relatively large. Do the authors have an idea of the species present in the study area? Could you refer to the existing bibliography to hypothesize possible components of this group of organisms within the study area?

The literature on the diversity and distribution of peridinin-containing dinoflagellates in this region remains scarce. We found some studies reporting some dinoflagellate genera potentially in the nano- size range such as *Amphidinium* (size range < 10-100 μm), *Gymnodinium* (size range < 5-200 μm), *Proto-peridinium* or *Prorocentrum* (size range 15- 200 μm) in the South Indian Ocean (Georges et al., 2014; Hörstmann et al., 2021; Sreerag et al., 2023). The following addition (in bold) has now been included in discussion, section 4.1.2, to explain the latter results:

Section 4.1.2: “For instance, diatoms and dinoflagellates, which are commonly associated with the microphytoplankton in these approaches, were also distributed in the pico- and nanophytoplankton (Fig. 4). **This likely reflects the presence of nanoplanktonic dinoflagellate genera such as *Amphidinium*, *Gymnodinium*, *Proto-peridinium* and *Prorocentrum* which have been reported in the Indian sector of the Southern Ocean (Georges et al., 2014; Hörstmann et al., 2021; Sreerag et al., 2023). Additionally, pico- and nanoplanktonic diatom genera such as *Minidiscus* and *Fragilariopsis*, as well as haptophytes, a eukaryotic picophytoplankton group genetically very close to diatoms and sharing a similar pigments composition (Guillou et al., 1999), have previously been observed in the South Indian and South Atlantic Oceans (Hinz et al., 2012; Leblanc et al., 2018; Nunes et al., 2019; Deteix et al., 2024). Also, the presence of haptophytes in the picophytoplankton could be attributed to some coccolithophore genera such as small *Emiliana*, and to other genera such as *Chrysochromulina* and *Phaeocystis* (Poulton et al., 2007; Hinz et al., 2012; Patil et al., 2017; Hörstmann et al., 2021). Notably, Nunes et al. (2019) have shown that phytoplankton functional types approaches based on bulk measurements predicted a high contribution of nano- and microphytoplankton in the Atlantic sector of the Southern Ocean, while the size fractionation approach indicated the dominance of picophytoplankton.”**

Lines 311-312. "As expected, cyanobacteria (*Prochlorococcus* and *Synechococcus*) biomass was mainly distributed in the picophytoplankton, but also surprisingly detected in the nano- and microphytoplankton." Could the authors provide a possible explanation for these results? Was sieving not effective? Were they attached to larger phytoplankton? Did they form colonies? Please provide possible explanations in the discussion section.

Thank you for this comment. The sentence you cited in lines 311-312 was discussed in section 4.1.2. We stated that it is possible that the 3 μm and 20 μm pore sizes from the filter cartridges may retain a part of picophytoplankton < 3 μm such as *Prochlorococcus* and *Synechococcus*. The following modifications (in bold) are now included in the manuscript:

Section 4.1.2: “Furthermore, **our findings revealed that *Prochlorococcus*, *Synechococcus* and chlorophytes** were mainly distributed in the picophytoplankton, but were also detected in the nano- and microphytoplankton (Fig. 4a-b). This result, also reported in previous studies (**Rodríguez et al., 2006; Nunes et al., 2019**), can be explained by the size fractionation methodology, as the 3 μm and 20 μm pore sizes may retain a part of these organisms **due to aggregation and/or adhesion**. In addition, **the attribution of pigments like zeaxanthin – associated with *Synechococcus* in this study – to larger size classes may be influenced by the presence of this pigment in nanophytoplankton (e.g. UCYN-B; UCYN-A in symbiosis within nanophytoplankton haptophytes) and microphytoplankton (e.g.**

Trichodesmium spp.; diatom diazotroph associations) previously detected in the STZ of the South Indian Ocean (Metzl et al., 2022; Chowdhury et al., 2024) or in some diatoms under high irradiance (Lohr and Wilhelm, 1999).-Thus, our result underline the importance of interpreting phytoplankton size structure data in the context of methodological constraints. The presence of picophytoplankton groups in larger size classes and the dominance of haptophytes in the picophytoplankton underscore the need for complementary validation using microscopy, flow cytometry or molecular techniques.”

Line 369. The title is vague; please rephrase.

The title of section 4.1 has been rephrased as follow: “**Analyzing the interplay between phytoplankton biomass and productivity in relation to size structure**”

Line 378. Could you please associate these stations with environments/zonal systems?

The stations cited were associated with their respective zone in parentheses as follow (addition in bold): “However, haptophytes were the main contributor to TChl_a_{MICRO} at O14 (**SAZ**) and O7 (**PFZ**), where NPP^{TChl_a}_{MICRO} peaked.”

Line 390. The term "global" is misleading; please rephrase.

The terms “global features” from the title of section 4.1.2. have been replaced by “overall patterns”.

Line 392. I would suggest using the term "similar" instead of "are in agreement," as the regions are not directly comparable. The Atlantic and Indian oceans exhibit different physical and chemical distributions, which hamper direct comparisons between regions.

Thank you for this comment. The term “are in agreement with” has been replaced by “are similar with” following your suggestion.

Lines 417-420. The relationships between pigments and phytoplankton groups should be presented in the Materials and Methods.

Thank you for this comment. We have added a new table in section 2.4 of the Materials and Methods, specifying the assignment of pigments to the corresponding phytoplankton chemotaxonomic groups in the phytoclass algorithm.

Table 1: Phytoplankton chemotaxonomic groups and associated pigments computed with phytoclass in this study.

Phytoplankton group	Pigments used for phytoclass in this study
Diatom	Chl _a ; Fuco
Haptophytes	Chl _a ; But-fuco; Fuco; Hex-fuco
Cryptophytes	Chl _a ; Allo

Dinoflagellates	Chla; Peri
Chlorophytes	Chla; Chlb; Lut; Neo; Viola; Zea
Pelagophytes	Chla; But-fuco; Fuco
<i>Synechococcus</i>	Chla; Zea
<i>Prochlorococcus</i>	DVChla

Line 419. Please, do not capitalize "diatoms."

This has been corrected for every phytoplankton group (except *Prochlorococcus* and *Synechococcus* as they refer to genera).

Line 421. "Inter-zonal spatial variability of phytoplankton biomass..." The title of this section is unnecessarily complex. Why not simply "Geographical distribution of phytoplankton biomass"?

The title for section 4.1.3 has been corrected following your suggestion (modifications in bold):
“**Geographical distribution** of phytoplankton biomass and productivity size structures in relation with environmental factors”

Line 424. Please provide references documenting similar changes in the size and/or composition of phytoplankton assemblages between the STZ and the Southern Ocean.

Thank you for this comment. We have provided references documenting similar changes in the size structure of phytoplankton biomass and productivity between the STZ and the Southern Ocean. The following modifications (in bold) are now included in the manuscript:

Section 4.1.3: “The size structures of integrated biomass and primary production clearly shifted between the oligotrophic subtropical waters and the Southern Ocean waters, **consistent with previous studies conducted in the Indian and Atlantic sectors of the Southern Ocean (Froneman et al., 2001; Mishra et al., 2017, 2020).**”

Lines 425-427. This part of the discussion is unclear. The authors list the main factors controlling the spatial distribution of phytoplankton, but in the second sentence, other parameters are addressed. I would suggest that the authors commence with their findings, then compare results with studies in the literature that found similar results, then with studies that do not match (if such studies exist), and then reach a conclusion based on all the evidence presented.

Thank you for this comment. We have revised the PCA to better take into account the relevant environmental variables with ecological significance. Note that with the corrected PCA does not alter the main results and the conclusions. We have also revised the corresponding text in the Material and Methods and the Discussion following your recommendations. The following additions and modifications (in bold) have now been included in the manuscript:

Section 2.6: “Principal component analysis (PCA) was performed on the volumetric dataset (n = 72) to **explore the relationships between** environmental parameters (explanatory variables) **and net primary production as well as** phytoplankton chemotaxonomic groups **biomass** (supplementary

descriptors). **The initial** explanatory variables were **potential temperature**, **salinity**, σ , dissolved oxygen, PAR, DIC, NO_x, DIP, DSi, Z_{SML} and Z_{EL0.01%}. **Prior to the analysis, explanatory variables and supplementary descriptors were standardized** (`vegan::deconstand()` function). Furthermore, collinearity among explanatory variables was assessed using a Spearman correlogram (Fig. S2). **Potential temperature was strongly correlated with σ ($\rho = -0.98$), dissolved oxygen ($\rho = -0.92$) and DIC ($\rho = -0.92$); among these variables, potential temperature was retained, as it is a key driver of water mass structure and biological activity. NO_x and DIP were also highly correlated ($\rho = 0.98$), and only NO_x was retained. After this selection, potential temperature and salinity displayed a variance inflation factor (VIF) > 20 (`vegan::vif.cca()` function); salinity was discarded in favour of temperature. Final explanatory variables were potential temperature, PAR, NO_x, DSi, Z_{SML} and Z_{EL0.01%}. All variables displayed VIF values < 10, except for NO_x (VIF=14).**

All statistical analyses were conducted in the programming environment R 4.4.2 (R Core Team 2024). The package tidyverse (v2.0.0; Wickam et al., 2019) was used for data manipulation; oce (v1.8.3; Kelley & Richards 2024) for trapezoidal integration computations; stats (v4.4.2; R Core Team 2024), rstatix (v0.7.2; Kassambra 2023) and corrrplot (v0.95; Wei and Simko 2024) for statistical analyses; FactoMineR (v2.12; Lê et al. 2008) and vegan (v2.6.10; Oksanen et al. 2025) for multivariate analyses.”

Section 4.1.3: “Result from **the** multivariate analysis showed that **potential temperature (theta)** and **NO_x concentration (and DIP due to its strong correlation with NO_x, see section 2.6)** were the major factors **driving** the spatial variability between the different zones (Fig. 6). Indeed, temperature and NO_x concentration are **recognized as key** factors to shape phytoplankton biomass and productivity size structures, with picophytoplankton usually **prevalent** in warm and oligotrophic waters (Marañón, 2009; Hörstmann et al., 2021; Berthelot et al., 2025).”

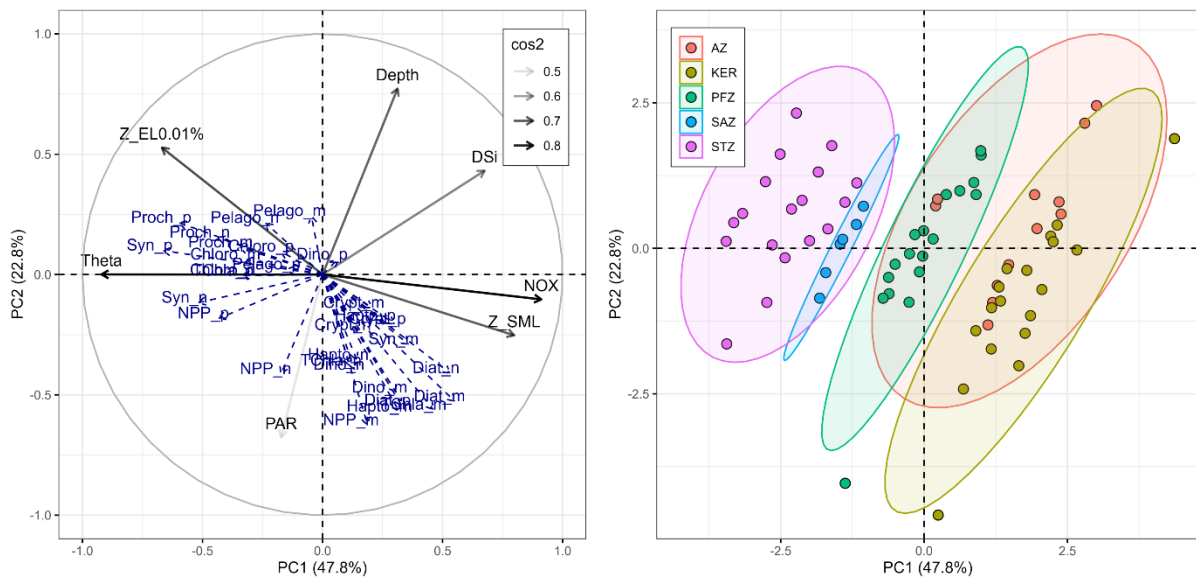


Figure 1: Principal Component Analysis illustrating the relationships between explanatory variables and supplementary descriptors across the global study area. The first principal component (PC1) axis explains 47.8% of the variance and the second principal component (PC2) axis explains 22.8% of the variance. On the left panel, the black arrows indicate the explanatory variables (environmental factors) with their transparency defined by their cos²: the better the variables are well represented by the principal components, the higher the cos². The blue arrows show the supplementary descriptors: NPP, TChl_a and

phytoplankton chemotaxonomic groups biomass in each size class. On the right panel, the colour of each point represents the zone of each sample (n = 72).

Line 426. Is salinity an important factor determining phytoplankton distribution in the study area, or is it just a spurious correlation? Please provide references that support the influence of salinity on the distribution of phytoplankton groups in environments similar to those analyzed here.

Thank you for this comment. Indeed, temperature and NO_x concentrations are the most important factors determining phytoplankton distribution in our study area. Temperature and salinity were significantly correlated ($\rho = 0.45$, $p < 0.05$), and this correlation was not spurious. As mentioned in the previous comment: “potential temperature and salinity displayed a variance inflation factor > 20; salinity was discarded in favour of temperature”. Consequently, we removed the term “salinity” from the sentence.

Line 449. "Phytoplankton chemotaxonomic groups biomass". How similar or different are the STZ of the Atlantic and Indian oceans in terms of physical and chemical parameter distributions? This should be mentioned somewhere in the text, as comparisons with the Atlantic sector are frequent.

The following addition (in bold) has now been included in the manuscript:

“Nevertheless, our results **were similar to those of Froneman et al. (2001) in the South Atlantic STZ, which reported a TChla size structure driven by pico- ($49 \pm 10\%$) and nanophytoplankton ($39 \pm 6\%$), but differed from other studies conducted in the northern and southern subtropical Atlantic**, where picophytoplankton accounted for 60–75% of TChla_{TOTAL} (Marañón et al., 2001; Morán et al., 2004). **As temperature and NO_x concentrations were similar between our study and the latter, the differences in TChla size structure between the Atlantic and the Indian basins may be attributed to factors** such as regional-scale hydrodynamics and/or atmospheric inputs (Marañón, 2009).”

Line 452. It would be interesting for the reader to mention some possible species that account for much of the biomass of the phytoplankton groups addressed in the discussion. This information would strengthen the interpretations. For example, there are several studies by Shramik Patil et al. that documented coccolithophore species distributions in this region, and there must be papers documenting diatom assemblages as well. Also, it is interesting that diatoms account for a substantial fraction of the nanoplankton. Diatom species are often larger than 20 micrometers, so the number of species smaller than 20 micrometers must be quite limited. The authors should revise the available bibliography in the study region to put their results into a broader context.

Thank you for this comment. Complementary to the updates made in section 4.1.2 following a previous comment from the anonymous reviewer (indicating line 309), we have now mentioned some possible genera that may account for a large part of the biomass of the phytoplankton groups addressed in section 4.1.3. References including studies by Shramik Patil *et al.* have been added to support these possible taxa. The following addition (in bold) has now been included in the manuscript:

Section 4.1.3: “Possible species that may account for much of the biomass of these phytoplankton chemotaxonomic groups include: *Synechococcus* and *Prochlorococcus* for cyanobacteria; *Pelagomonas*, *Micromonas* for pelagophytes; *Chloroparvula*, *Chloropicon* for chlorophytes;”

Chaetoceros, *Corethron*, *Coscinodiscus*, *Eucampia*, *Fragilariopsis*, *Thalassiosira* for diatoms; *Gephyrocapsa*, *Chrysochromulina*, *Phaeocystis* for haptophytes; and, *Amphidinium*, *Gymnodinium*, *Protoperidinium*, *Prorocentrum* for dinoflagellates (Armand et al., 2008; Lasbleiz et al., 2016; Patil et al., 2017; Irion et al., 2020; Hörstmann et al., 2021; Sreerag et al., 2023, 2025 ; Thyssen et al. 2024)”

Lines 481-482. Please explain in further detail the findings of Lamont and Barlow mentioned here.

The sentence has been completed to include the findings of Lamont and Barlow (2017). The following addition (in bold) has now been included in the manuscript:

“These hydrographic and biogeochemical features are consistent with previous observations of cyclonic eddies in the Mozambique Channel and Basin, **which are characterized by a shallower Z_{SML} and nitracline , and a deeper euphotic zone** (Lamont and Barlow, 2017).”

Line 482. It is the first time the authors refer to NPP fluxes; please explain the units when presenting this parameter either in the Introduction or Materials and Methods.

In marine biogeochemistry, the net primary production (NPP) represents the net flux of organic carbon synthesized by phytoplankton through photosynthesis. As defined in this manuscript, NPP is expressed in units of mass per unit volume (or per unit area for integrated data) per unit time, which is characteristic of a flux. For volumetric data, units are $\text{mgC m}^{-3} \text{d}^{-1}$; for integrated data units are $\text{mgC m}^{-2} \text{d}^{-1}$.

To facilitate the reading with a vocabulary coherent throughout the manuscript, the following sentences have been included in the Material and Methods section:

- Section 2.2: “Volumetric NPP is expressed as a flux in $\text{mgC m}^{-3} \text{d}^{-1}$.”
- Section 2.3: “Volumetric pigment concentrations are expressed as stocks in mg m^{-3} .”
- Section 2.6: “Integrated NPP are expressed thereafter in $\text{mgC m}^{-2} \text{d}^{-1}$, while integrated pigment concentrations and integrated biomass of phytoplankton chemotaxonomic groups are expressed in mg m^{-2} .”

Line 511. Which value range is considered suboptimal and why?

According to the previous reviewer’s comments addressed in section 3.1, the term “suboptimal” has been removed to avoid any confusion. The following addition (in bold) has now been included in the manuscript: “First, A3 and O12 exhibited DSi/NO_x ratios in the SML **lower than the Brzezinski ratio** (Table 1), [...]”

Line 512. "Si availability hypothesis" — shouldn't this be "Si limitation" instead?

“The Si availability hypothesis” refers to one of the potential factors which could explain the variability in integrated NPP in iron-fertilized areas, listed in the previous sentence. For a better clarity, the following modification (in bold) has now been included in the manuscript: “[...], indicating a potential Si limitation; **this indicates that Si availability does not explain the observed difference in $\text{NPP}_{\text{TOTAL}}$** .”

Line 512. Please replace "noticeable shift" with "noticeable difference."

The term "noticeable shift" has been replaced with "noticeable difference".

Lines 517-518. Please rephrase this sentence, as you indicate the purpose of the ratio twice.

From this sentence, we removed "as an indicator of the mixing processes in the water column (Moline, 1998)" to avoid repetition in the next sentence. The sentences are now as follow:

"Third, to investigate the light-mixing regime, we computed the ratio of diadinoxanthin (DD) and diatoxanthin (DT) concentrations to TChl a ((DD+DT):TChl a). Although DD and DT have limited chemotaxonomic values, they have a photoprotective role, with concentrations that respond rapidly to changes in irradiance (Demers et al., 1991). Because most phytoplankton contain these pigments, the (DD+DT):TChl a ratio provides useful information on the vertical mixing rates in the water column along with the light regime (Moline, 1998).

Line 567. Please do not capitalize "chlorophytes, dinoflagellates and cryptophytes."

This has been corrected for all phytoplankton groups throughout the manuscript (except for *Prochlorococcus* and *Synechococcus* as they refer to genera).

Additional minor changes have also been made in addition to those requested by the reviewers, as detailed below:

- Section 2.3: Typo error in pigment listing has been corrected: prasinoxanthin has been replaced by lutein. The following modification (in bold) has now been added to the manuscript: "In this study, the following eleven accessory pigments were further used to study the TChl a biomass of the phytoplankton chemotaxonomic groups: fucoxanthin (Fuco), peridinin (Peri), 19'-hexanoyloxyfucoxanthin (Hex-fuco), 19'-butanoyloxyfucoxanthin (But-fuco), alloxanthin (Allo), chlorophyll b (Chl b), zeaxanthin (Zea), neoxanthin (Neo), **lutein (Lut)**, violaxanthin (Viola), and DVChl a ."

- Table 1:

- Surface TChl a at O2: ~~0.07 mg m⁻³~~ → 0.08 mg m⁻³
- Surface TChl a at O12: ~~0.71 mg m⁻³~~ → 0.59 mg m⁻³

- Table S2 (in supplement): NPP values have been correctly reordered according to their respective station.

-Figure S5 (in supplement): ~~mg·m⁻²~~ → mg m⁻³

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