

Overall quality and novelty

Deteix et al preprint's investigates spatial variability of phytoplankton community structure and net primary productivity across different zones and regions of the Indian Ocean using size-fractionated net primary production and size-fractionated pigment-based community composition. This is a powerful approach rarely used, in general and specifically in the Indian Ocean, which together with the adopted vertically-resolved strategy generates a novel dataset with potential to shed light on the relationship between phytoplankton community composition and net primary production. Yet, I find at times that the preprint does not fully exploit the potential of the data and lacks discussion of certain aspects, such as growth rates and C:Chla and reported size and taxonomic dependent differences, that are critical for the interpretation of the data and conclusions about the linkage between Chla biomass and NPP. While similar data from the Indian Ocean are scarce or absent (see Landry et al. 2022), studies containing bulk community, size- and taxon-specific data on Chla biomass, NPP and growth rates conducted in analogous oceanographic zones and biogeochemical conditions in other ocean basins are missing and improve the discussion.

Specific comments

1- The preprint builds on the analysis of the relationship between size-fractionated and taxon-specific Chla partition to assess the influence of phytoplankton size structure and composition on NPP. Yet, C:Chla and growth rates, and their size and taxonomic dependence can alter this relationship and the conclusions inferred. The influence of differences in C:Chla and intrinsic growth rates between phytoplankton size and taxonomic groups needs to be discussed with regards to the links between primary production and phytoplankton (Chla) biomass structure. Some studies investigating this bulk and group-specific variability that could help improve the discussion in this regard are listed below Strom et al. 1991; Landry et al. 1993; Arin et al. 2002; Beherenfeld et al. 2005; Sathyendranath et al. 2009; Latasa et al. 2014; Jakobsen and Markager 2016; Liefer et al. 2018; Marañón et al. 2021; Landry et al 2022; Gutierrez-Rodríguez et al. 2023; Yingling et al., 2025.

2. Results presented in figures and tables refer to stations, regions and zones (Figure 3 and 5) or zones (Tables 2 and 3) but results described throughout the text mostly refer to specific stations instead.

Figure 2, for instance, presents size-fractionated Chla biomass and NPP profiles for individual stations representative of different zones while the rest of stations are included as supplementary material. It is not obvious for the reader how representative these are or how marked the differences between zones are. I think showing the mean +/- error profiles estimated for stations of each zone would be useful to investigate the differences in the biomass and productivity structure in each zone, while leaving the individual stations profiles for supplementary material. Similarly, the starting sentence of the discussion “*At several offshore stations in the STZ (O16), SAZ (O14), PFZ (O6, O7, O9) and AZ (O11), NPPTChlaMICRO peaked below the SML while NPPTChlaNANO and NPPTChla PICO were minimal (Fig. 2k-o; Fig. A3)*”. It is not clear how representative this feature is of the region/zone. I think the message would be more clearly conveyed if the focus is placed on zones, pointing then to

specific stations to describe intra-zone or intra-region variability. This would also facilitate comparison with other studies and identification of shared patterns or differences with analogous zones and biogeochemical conditions in other basins.

3. In the discussion section 4.1.2. Global feature of phytoplankton biomass and productivity size structure across the study area – The authors indicate that their results of microphytoplankton being the dominant contributor to NPP while nano and micro dominated the TChla biomass are in agreement with previous studies. But they only refer to Froneman et al. 2001. Relevant studies investigating phytoplankton structure, growth and productivity across contrasting zones and regions are not mentioned. Examples of such studies include Boyd et al. 1999 JGR; Bradford and Grieve et al. 1999; and McKay et al. 2005 conducted in ST, SA and AZ of the SW Pacific. I would also refer to studies conducted around islands, plateaux and bathymetric features such as the Kerguelen, Crozet, Campbell plateaux (e.g. Irion et al. 2020 or Gutierrez-Rodriguez et al. 2020) with relevant data obtained in analogous productive and oligotrophic (HNLC and HNLC-LSi) conditions that could be useful to broaden the discussion.

4. I am concerned with the Spearman rank correlation approach adopted to explore the role of each size fraction and phytoplankton group in driving the NPP variability, particularly with regards to potential spurious correlation and covariation in phytoplankton groups's Chla biomass. I wonder whether multiple regression (e.g. $NPP_SF \sim group1 + group2, \dots +$ environmental variables) or the use of partial least squares could help handle these respective issues.

5. Pigment size-fractionated results. I found interesting and striking the significant contribution of picoplanktonic groups such as Syn, Prochl or green algae to the micro-size Chl a biomass as well as the dominant contribution of haptophytes in the picophytoplankton fraction, which yielded almost 3-fold higher contribution than Chlorophytes, which tend to co-dominate in similar open ocean waters (see SAWS region in Figure 7 of Nunes et al. 2019 mentioned in the preprint. Given the importance of these results, I recommend the authors to expand the discussion around these points and potential methodological bias. Given the scarcity of this type of data the study by Rodríguez et al., 2006, although from a different region and conditions, could add to this discussion.

6. It is unclear why in the 4.2 intrazonal variability Discussion section subsections include the Subtropical Zone and the Polar Frontal Zone but the Kerguelen bloom region instead of the Antarctic Zone, comprising both the KER and HNLC and HNLC-LSi regions as in Table 1.

7. Lines 449 – 455. If I understand correctly the authors here cite Schluter et al 2011 to point that opportunistic taxa belonging to nano and microphytoplankton are adapted to exploit excess nutrients in a way consistent with size-fractionated trends observed in richer AZ and KER regions. There have been recent studies pointing in this direction (e.g. Leblanc et al. 2018) or even that certain picophytoplankton specific groups such as prasinophytes (Barber and Hiscock 2006; Irion et al. 2020; Gutierrez-Rodriguez et al. 2023) often respond also to improved nutrient conditions in HNLC open ocean waters.

8. Section 4.2.1. Here the authors invoke the passage of a cyclonic eddy to explain increased productivity observed in ST O3 compared to O2 without concomitant shift in community

composition. Similar results have been reported in the NPSG (Landry et al. 2008; Beatty et al. 2025) in more recent and complementary studies that I find relevant for this discussion point.

9. I found the discussion point developed in the paragraph starting in line 402 of particular relevance and suggest including it as part of the abstract.

Minor comments

Line 114 – Please clarify what “shaken vigorously” means as it may affect protistan grazers to a greater extent than their phytoplankton prey and bias the NPP estimates.

Line 378 – Please indicate these stations belong to different zones.

Line 401 – Please indicate “bulk Chla biomass” here and throughout the document when referring to Chla-based phytoplankton biomass.

Line 406 – See Rodríguez et al. 2006 as another example of the size-fractionated pigment analysis approach.

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