

General Comment

This study quantifies changes in surface phytoplankton biomass and community composition over fronts in the Mediterranean Sea. The authors find that biomass is consistently enhanced over fronts and diatoms are favored, but with regional and seasonal variability such that the strongest effects were found in wintertime and oligotrophic regions. Although focusing on a different region, these results and SST-frontal identification methods closely mirror those of the North Atlantic region presented in previous studies by the authors (Lévy et al. 2025 and Haëck et al., 2023).

From my perspective, the most interesting and novel component of this study is comparing whether phytoplankton have a stronger response to SST-derived, density-derived, or FSLE-derived fronts. Unfortunately, this is the component of the paper for which I have the most methodological concerns. I believe the authors would produce a more convincing analysis if they calculated FSLE independently rather than relying on existing datasets so that they could conduct controlled tests, changing only one factor at a time (e.g., input velocity grid resolution, initial Lagrangian particle separation, final particle separation). I have many other concerns regarding the interpretation of FSLE, listed in the comments attached.

I believe that this study has potential, but the manuscript needs considerable improvements. Therefore, I cannot recommend this pre-print for publication primarily due to its incremental novelty from the authors' previous work, insufficient methodological descriptions (Major Comment 1), and major methodological concerns (Major Comment 2). The manuscript would also benefit from English language editing, and to that end I provide many recommendations in the Technical Comments attached.

Major Comments

1. Methodological details are unclear or neglected in many instances:
 - Lines 92-93: Can the authors clarify if the spatial interpolation of the SST over cloud-covered regions is part of the product, or if they did it themselves? If the interpolation was done by the authors, what percentage of the pixels were interpolated?
 - Lines 100-101: Can the authors clarify if they are applying the “multivariate optimal interpolation algorithm”, or if this was done previously by Sammartino et al.? Either way, an explanation is needed to describe what this means. It is also unclear why the authors describe a depth-resolved machine learning algorithm to derive density, if they only end up using surface data from satellite and in situ data.
 - Lines 104-112: I recommend providing an equation for FSLE in the paper. Additionally, these methods are hard to follow when the text switches back and forth with some numbers provided as decimals and others as fractions. If I understand correctly, the input velocity fields are coarser for the AVISO product (1/4 degree) compared to the 4DMED-SEA product (1/24 degree), and the choices for the Lagrangian particle initializations are also different. For AVISO, the particles are seeded at 1/50 (0.02) degree and FSLE is calculated as the time at which the particles spread 30 times the initialization (0.6 degree). The 4DMED-SEA product initializes at 1/24 degree, and calculates FSLE at the time at which particles spread 30 times the initialization (30/24 degree). Wouldn't this result in the detection of entirely different sized fronts, such that the ones in 4DMED-SEA are much larger? Do both datasets use the same interpolation schemes and timestepping methods? Is 4DMED-SEA also running particle trajectories backward-in-time? These choices all

- could affect the conclusion of which front identification method best highlights the fronts that result in a biological response.
- Lines 117-123: Can the authors comment on if the phytoplankton algorithm was validated in the Mediterranean Sea? Does it work effectively in coastal, oligotrophic, and eutrophic systems? I find it curious that the derived community structures look very similar between the oligotrophic and eutrophic regions (Fig 1c).
 - Lines 134-137: How was mixed layer depth calculated? How was the clustering “simplified”?
 - Lines 157-158: Can the authors clarify how “all fronts are projected onto a common grid”?
 - Line 170-171: “the 7x7 pixels window was the most optimized choice for our study”; what was the logic to draw this conclusion?
 - Line 174: What does the “L2 norm of the difference” mean here?
 - Line 177-178 & 197-200: At first, the authors claim that HI index > 10 is used to delineate the fronts from the background circulations, but then give further explanation later. I think these sections should be combined into one coherent thought and provide a quantitative assessment of the Chl-a over the strong and weak fronts (“close to” is vague). Can one standalone pixel be considered a front by the HI definition? What is the minimum size of the fronts?
 - Lines 180-184: Line 180: If using backward-in-time trajectories, doesn’t the FSLE identify convergence, not stretching? I suggest clarifying if the previous studies cited used this FSLE threshold in the same domain as this study. What are the units of this threshold (1/time)? What sensitivity tests were conducted? What was the logic to choose an “optimal value”? As with the HI, can one standalone pixel of high FSLE be considered a front? If so, this may be very sensitive to numerical instability (for example, see Reijnders et al. <https://doi.org/10.1029/2025MS005470>)
 - Lines 193-194: How is “total chl-a” defined? Regional average? Seasonal average? Whole domain?
 - Line 205: How big are the sample pools from which the draw of 300 comes from? How was 300 chosen? A bootstrapping analysis would strengthen the statistical analysis here (e.g., redrawing 300 samples many times and computing a confidence interval for the statistic).
 - Line 211-213: How was the phytoplankton group anomaly computed? Same as chl-a? Is the median used for chl-a, but mean for the groups?
2. The way the FSLE is applied to define fronts from a single threshold is concerning to me especially given that the density of occurrence is 100% in parts of the Alboran region, and generally much higher than the HI methods throughout the entire domain (Figure 2d). As mentioned in my earlier comment, I believe that the AVISO and 4DMED datasets are detecting differently sized fronts. Using a single threshold for defining fronts from the FSLE throughout the entire domain and for both datasets is also not sufficiently justified beyond citing previous papers and claiming a sensitivity test was conducted without showing those results. This is especially important given that one of the major conclusions of this paper is that the HI method is superior to the FSLE method in identifying fronts with biological reactions. Such a claim goes against previous studies of the region that suggest using both together (Sudre et al. 2023: <https://www.nature.com/articles/s41597-023-02615-z>; d’Ovidio et al. 2009: <https://doi.org/10.1016/j.dsr.2008.07.014> (a highly relevant study that is not cited or discussed in this paper)). The authors speculate that the FSLE is not correlated with the biological response because of the “effective resolution” (Line 256, 305), without much

support (no citations, e.g., Yang 2025: <https://link.springer.com/article/10.1007/s41064-024-00318-7>).

Minor Comments

1. The title is misleading. This study specifically focuses on regions within the Mediterranean Sea, whereas the phrase “over oceanic regions” suggests a systematic study of a wider domain. Additionally, while I believe the authors are using the phrase “trophic” in reference to eutrophic versus oligotrophic, I believe that word is more commonly associated with the food web (trophic transfer), which may lead readers to believe that multiple trophic levels (e.g., zooplankton) are studied.
2. The authors state that their focus is on “fine scale fronts” (Line 59), without specifically defining what they mean by “fine scale”. For example, what is the specific length scale of interest? In Line 33, the authors state “Their spatial scale spans a large range, from a few hundred meters to hundreds of kilometers in length.” Is this entire range considered fine scale in this study? If not, I suggest the authors clearly articulate why their focus is on “fine scale” fronts, rather than all fronts.
3. It is assumed that readers are familiar with the currents within the sub-regions shown in Figure 1. I suggest providing a figure or subplot that shows the currents and gyres referenced throughout the paper.
4. It would be useful for readers to somewhere (maybe in the intro) provide context & citations regarding the traits of gleaners and opportunists, e.g., “larger phytoplankton” (Line 326) that are “fast-growing” (line 334) refers to diatoms, and “oligotrophic-adapted organisms” (line 328) refers to the prokaryotes.
5. I do not understand how the last paragraph (Lines 364-369) is relevant or logical, since frontal occurrence (Xing et al. 2024) and chl-a (Yang et al. 2023) in the Mediterranean Sea have not been projected to change.
6. The software used to conduct the study was not provided by the authors.

Technical Corrections

- Incorrect usage of “groups” versus “group” in many cases: Suggest changing “phytoplankton groups data” to “phytoplankton groups” or “phytoplankton group data” (e.g., Lines 7, 271, Fig 1 caption); “phytoplankton groups mosaic” to “phytoplankton group mosaic” (Line 154); “groups” to “group” (Line 211, 212, 213, 275)
- Citations needed to support the sentence in Lines 22-23 & the sentence in Line 33
- Line 27: the word “constitute” is not appropriate here
- The sentence in Lines 36-37 is incoherent; suggest re-writing
- Inappropriate use of the word “besides”, suggest removing it in Lines 40, 127, 282
- Line 45: “lagrangian” should be capitalized
- Line 54: Suggest changing “Advances in observational and detectable methods has significantly improved...” to “Advances in observation and detection methods have significantly improved...”
- “fine scale” and “fine-scale” are used interchangeably, suggest remaining consistent throughout

- The content in Lines 54 – 74 is strangely organized, and the flow of thoughts is difficult to follow:
 - The sentence in Line 57 feels out of place here, and repetitive with the content in Lines 68-71
 - Suggest moving the sentences in Line 61-66 to the end of the paragraph
- Line 72: I believe “lying” should be “relying”
- The sentence in Lines 97-99 is incoherent and a run-on sentence; suggest re-writing
- Line 122: I believe “their” should be “they”
- Fadida et al. 2026 (<https://doi.org/10.5194/os-22-329-2026>) is a regional-specific and relevant study to cite within lines 129-131
- Line 145: “green region” appears to be cyan in color; recommend using the regional name rather than the color in this description for the reader
- When discussing the “other eukaryote” groups, it would be helpful for the readers to remind that this means “non-diatom eukaryotes” or “diatom-excluded eukaryotes” (e.g., Lines 152, 273)
- Recommend changing “implies to compute” to “computes” (Line 161) and “to assume” to “assumes” (Line 162).
- Recommend removing the sentence in Lines 163-164 because they are confusing in this spot, and repetitive with sentence at Line 172. The sentence in line 164-165 is also repetitive with the second sentence of the same paragraph.
- Line 167: I believe “28 km square window” should be “28 km x 28km square window”
- Line 197: Recommend changing “make a difference” to “differentiated”
- Line 220: Recommend changing “fronts with SST” to “fronts detected with SST”
- Line 226: “bassin” typo
- Line 229: “associated to finer currents” to “associated with finer currents”
- Figure 3 & 4 captions: “ans” typo
- Line 247: “fronts effect” to “frontal effect”
- Line 299: “more than 60%” of chl-a? “front scale” to “frontal scale”.