

Specific comments

1. Title: Given the context of the manuscript, I understand the authors' intended meaning for the phrase "seasonal enrichment of low-chlorophyll surface waters". But, I think it could be clearer. The word "enrichment" describing the "low-chlorophyll surface waters" could be interpreted to mean that horizontal stirring makes chlorophyll even lower in the region. "Seasonal" is also vague, considering that the study focuses on winter blooms. Some alternative suggestions are:
 - "Coastal to offshore submesoscale horizontal stirring enriches wintertime phytoplankton blooms in the oligotrophic Mediterranean Sea"
 - "Submesoscale horizontal stirring increases offshore chlorophyll concentrations during the winter bloom in the Eastern Mediterranean Sea"
2. The authors tackle the challenge of explaining satellite chlorophyll observations with the dynamics observed in a numerical model. To increase trust that they're not comparing "apples and oranges", I suggest that they include more information about the validation of the numerical model and how representative it may be of real-world physics observed in the system. A few sentences would be sufficient, potentially citing more of the findings of the original study by Solodoch et al. 2023 (by the way, I think this study is accidentally cited as two separate citations in this manuscript). Additionally, an explanation for why a numerical model, as opposed to observations, was needed to capture the submesoscale physics (e.g., satellite altimetry resolution is too coarse) would be welcome background information in the introduction. Is there an advantage to using the numerical model to study how the physics affect real chlorophyll blooms as opposed to high-frequency radar observations in the area? Would SWOT be useful to corroborate the results in the future?
3. Missing definitions:
 - Line 68: Can the authors describe and define how they compute the Chl gradients?
 - Line 72: Can the authors define surface vorticity and be more specific about how they're measuring it? E.g., what depth is "surface", and how are the values calculated from the model? ζ/f in Fig. 2 is a normalized relative vorticity and should be defined. Equations and more details would be welcome.
 - Line 74: I suggest the authors define "mesoscale" here, e.g. provide a length scale to distinguish from submesoscale.
 - Figures 1 & 5: Can the authors briefly describe how the 95% confidence intervals were computed?
 - Figure A4: Can the authors define horizontal divergence? How do they compute it from the model? An equation would be welcome.
4. The authors present their conclusions in rather qualitative ways, but a few quantitative statistics to support their findings would go far to strengthen the manuscript. Here are some cases where I feel this is relevant:
 - Abstract Line 11: "this submesoscale horizontal stirring mechanism is responsible for ~24% of the seasonal surface chlorophyll increase in the region." The authors mention this result once in the manuscript (Line 69), so it may be easy for the reader to miss. To be presented as a main result in the abstract, I believe it needs more quantitative development. This result is based on a 9-year chl timeseries (2010-2019) average, but the model timeframe is much shorter. This result doesn't account for variability in chl or submesoscale stirring from year to year. I suggest that the authors at least comment on

- the range of this value by quantifying the chl increase each year. It could also be mentioned in the discussion that there may be interannual variability in submesoscale stirring. Also, can the authors comment on whether the results are sensitive to increasing the “East” and “West” box sizes?
- Line 94-97: Can the authors provide the statistics for “little differences”, “substantially higher”, “similar values”?
 - Line 101: Can the authors provide the statistics for “substantially wider and more skewed”?
 - Figure A5: Can the authors provide the statistics for “good agreement” and “no significant difference”?
5. Figure 1, Panel (a):
- The circulation patterns are difficult to see in this figure. Can the arrows be plotted at a lower density and larger so that it is easier to see the major directions of flow?
 - It would be helpful for the reader if the boxes in panel (a) were colored in the same colors as panels (b) and (c), and labeled "West" and "East" directly above the boxes
 - I recommend showing the climatologies for spring and winter separately, such that panel (a) becomes two panels. That might help to show how the average circulation patterns change between the two seasons, which would supplement the narrative.
 - Can the authors comment on how the chlorophyll enrichment line was chosen and why they didn't compute the chlorophyll enrichment factor across the central diagonal of the box?
6. Figure 2:
- The difference in resolution (1000m and 300m) of different parts of the domain is mentioned for the first time in the caption here. Reading sequentially, this is confusing, given that the model setup is not described until much later in the text. This confusion would be alleviated if the Methods Section & Appendix A were moved before the Results.
 - Can the authors adjust the colorscale minimum so that more of the variability is visible in panel (a)? It currently feels like dynamics are hidden because it is all washed out.
 - For panels (b) and (d), can the authors either make the color scales the same, or comment in the caption on why they are different (e.g., “the limits of the colorbars are different in each panel in order to visualize the respective dynamics in the different seasons”)
7. Lines 95-99: The authors claim that the modeled vertical velocity, vertical mixing rate coefficients, and vertical gradients in nutrients “all have similar values in the upstream and downstream regions”. However, the captions of Figures A2 and A3 provide more context than this. I suggest including Figs A2 and A3 as panels (c) and (d) in Figure 3. Then, the details in the captions could be moved to the main text as well: “Contrary to the observations (Figs. 1- 3 in the main text), this would imply higher Chl concentrations and gradient magnitude should exist west of the Nile Delta if vertical transport processes dominated Chl distribution.” and “Contrary to the observations (Figs. 1- 3 in the main text), this would imply higher Chl concentrations and gradient magnitude should exist west of the Nile Delta if vertical mixing processes dominated Chl distribution.” These points are important in the logical process of ruling out potential mechanisms other than the

horizontal stirring in driving the winter bloom, so I do not think they should be buried in the Appendix.

8. The Lagrangian particle tracking demonstration is a nice visual to bolster the authors' arguments that submesoscale horizontal stirring from the coast to offshore is increased in the winter. However, I believe that providing more detail would improve the manuscript:
 - What is the sensitivity of the placement of the red box? How was that location chosen?
 - "About 40000 tracer particles were released uniformly" (Lines 209-210): Can the authors provide the exact amount? Does uniformly mean gridded? At what resolution were they seeded?
 - "Advection for 40 days during the winter and 33 days during the summer" (Line 211): Why not advect them for the same amount of time to be able to compare between the seasons? It feels particularly odd in Figure 5 a to have mismatching timescales provided
 - Are the particle distributions sensitive to the date of initialization?
 - Could the authors quantify what fraction of particles enter the "East" box of Figure 1 in each Lagrangian experiment? This would be a nice way to provide a quantitative assessment of Figure 4, and link directly to the region where chlorophyll has been measured. The "East" box could be plotted in Fig. 4 to complement such a calculation.
9. Figure 6 & Lines 142-148: The authors speculate that water particles accumulate on fronts, which move offshore, and then break up. However, the particles in Figure 4d-f appear to be transported by submesoscale instabilities and chaotic stirring, which need not necessarily be persistent fronts. Can the authors clarify their assumptions more and justify the inclusion of Figure 6? If the particles are in fact accumulating on fronts, perhaps plotting the particles with a color gradient in Figure 4 would make that clearer. Or, the authors could measure FTLE and FSLE and measure accumulation along those features. Alternatively, I think it would be justifiable to remove Figure 6 entirely since it is not currently particularly relevant to the key results, and the ideas presented could be left as discussion points.

Technical corrections

Lines 1-2: "The large seasonal increase in marine photosynthetic organisms - i.e., phytoplankton bloom - is a ubiquitous oceanic phenomenon..." to "The large seasonal increases in marine photosynthetic organisms - i.e., phytoplankton blooms - are a ubiquitous oceanic phenomenon..."

Line 3: Consider changing "and that supports the growth and development of larger organisms throughout the marine ecosystem" to "and supports the growth of larger marine organisms" to be more concise and avoid a run-on sentence.

Line 4-5: "front and filament circulation patterns..." to "fine-scale frontal and filamental circulations" (where frontal and filamental are adjectives describing the circulations) or to "fine-scale fronts and filaments" (where fronts and filaments are the nouns); I'm also suggesting to add the term "fine-scale" here for defining the submesoscale

Line 5: "characterizing the" to "characteristic of the"

Line 6: "are manifested by" to "manifest as"

Line 7: “are intensified by” to “are also intensified by”. Adding “also” here would make it clear that you are suggesting horizontal stirring is another mechanism besides vertical mixing that enhances chl, rather than the only mechanism.

Line 9: Consider using an alternative word to “interior” because that is often taken to mean the deep ocean. Maybe replace “in the sea interior” with “offshore” or “open sea”. The word “interior” is used several other times in this way (Lines 74, 83, 87, 99, 110, 138), which I suggest changing as well.

Line 10: “A comparison of” should point to two things. For example, “A comparison of the climatological circulation patterns and chlorophyll time series...” or , “A comparison of spring and winter chlorophyll indicates...”

Line 13: The term “regulating” is unclear. I recommend removing it.

Line 15: “phytoplankton bloom” to “phytoplankton blooms”. In my opinion, this is pretty repetitive with the first line of the abstract, and you don’t need to define phytoplankton blooms twice. Maybe make more concise like: “Seasonal phytoplankton blooms occur worldwide, playing a critical role in...”

Lines 18-19: This sentence in its current placement jumps the gun a bit. I suggest moving it to the first line of the paragraph at Line 28. Then, the first sentence can be the opener for the paragraph that follows in Line 20.

Line 21: “a proxy to” to “a proxy for”

Lines 26-27: Citation needed for the statement: “in the oligotrophic nutrient-depleted subtropics Chl exhibits a moderate increase driven by enhanced vertical mixing during winter”

Line 40-41: “imagery of Chl concentration that allows monitoring seasonal changes” to “imagery of Chl concentration that allows the monitoring of seasonal changes”

Lines 40-42: The text alludes to a “high-resolution” model with the “same spatiotemporal scales” as the satellite data, without providing actual numbers. Can the authors briefly comment on the specific resolution of the satellite and numerical model here? I know there are more details at the end of the text, but it feels odd not to explicitly state the resolution up front.

Line 48: “the increase in nutrient availability resulting from it” to “the resulting increase in nutrient availability”

Lines 59-60: “Analysis of the large-scale spatial variations in Chl distribution reveals that the transition zone between coastal and pelagic waters varies between different parts of the EMS (Fig. 1a).” This sentence is vague and unclear. I’d suggest removing it entirely and starting the paragraph with the sentence “Focusing on the vicinity...”

The authors interchangeably use the terms “northeast”, “north-east”, and “east” (similarly for west). Consistent terms should be used to avoid confusion. In my opinion, sticking to “east” and “west” would be the clearest. Examples:

- Line 62: “the region to the north and to the east” to “the east region”
- Line 67: “one to the north-west and one to the north-east” to “one to the west and one to the east”; the terms used in this sentence should match the terms used in Figure 1
- Line 69: “north-eastern” to “eastern”
- Line 70: “north-western” to “western”
- Figure 1: “northeast” and “northwest” used in caption; I suggest changing to “east” and “west”, as they’re referred to in panels (b) and (c)

Line 77: The authors switch from EMS to “Levantine Basin” here. I suggest using EMS throughout, or be sure to explain/define the Levantine Basin.

Lines 85-87: “In contrast, the more uniform distribution of surface Chl during winter suggests that the transport barrier weakens substantially due to an increase in submesoscale activity...” A “uniform distribution of surface Chl” does not inherently suggest that “the transport barrier weakens substantially due to an increase in submesoscale activity”. I recommend rewording to something like this: “In contrast, the more uniform distribution of surface Chl during winter suggests that the transport barrier weakens substantially. Here we test the hypothesis that this is due to an increase in submesoscale activity...”

Line 88-89: “It is well documented that submesoscale currents are characterized by strong vertical motions that can amplify nutrient transport and consequently lead to phytoplankton blooms (Mahadevan, 2016; Lévy et al., 2018). To test whether the observed increase in open-sea Chl gradients in the region downstream from the Nile Delta is indeed driven by local effects of the submesoscale dynamics...” The combination of these two sentences leads the reader to believe you’re testing if the vertical motions of submesoscale currents increase chl, but you’re trying to argue that it’s the horizontal stirring. I suggest rewording.

Line 92: I suggest replacing “the two aforementioned open-sea regions” with “the east and west regions highlighted in Figure 1a”.

Figure 4: Can the authors add the phrase “horizontal resolution” to avoid confusion with depth? E.g. “Summer (300 m horizontal resolution)”

Line 120: “further quantified”, suggest removing “further” because you have not yet quantified the offshore transport at this point, only provide a qualitative analysis

Line 123: Suggest changing “changing” to “constrained”

Line 124: “in 3km winter” to “in the 3km winter”

Line 124: Suggest changing “changes” to “fluctuations”

Line 140: Add a comma between “bloom” and “it”

Figure 5: I suggest labeling the 5b y-axis as “Chlorophyll enrichment factor” instead of “Enrichment Value” to be consistent with the main text. Fig A6 is only referenced in the Fig 5 caption. I suggest referencing it in main text as well.

Line 149: “As previously shown” to “As shown in previous works”; the current wording could be interpreted to mean this result was shown earlier in the text