Response letter to the reviewers for: "Coastal-to-offshore submesoscale horizontal stirring enhances wintertime phytoplankton blooms in the ultra-oligotrophic Eastern Mediterranean Sea"

We thank the reviewer for the careful reading of the manuscript and for the constructive comments. In response, we have clarified the methodology, refined the presentation of the results, and added quantitative explanations where appropriate. All changes are detailed below. The reviewers' comments are in black, and the authors' responses are in dark red.

Minor Comments:

1. Lines 91-95: It is unclear how the authors are drawing these conclusions here. Are these findings all from Solodoch et al. 2023? The wording is vague, e.g., "good agreement", "appear consistently", "generally in agreement", "compare well", "fair agreement". I recommend briefly clarifying how these conclusions were drawn with more quantitative language.

We thank the reviewer for this comment. The validation of the circulation model is presented in detail in Solodoch et al. (2023), which is cited here as the primary reference for model realism and performance. We agree that the original wording in this manuscript was overly qualitative. To address this, we have revised the text to explicitly attribute the validation results to Solodoch et al. (2023), along with referencing specific figures from that paper and the validation of a variety of parameters. Additionally, we replaced qualitative expressions with specific, quantitative statements reported in that study where possible.

In particular, we now state that the domain-mean surface eddy kinetic energy differs from altimetry-derived estimates by approximately 0.002 $m2\ s^{-2}$ and from CMEMS reanalysis by approximately 0.005 $m2\ s^{-2}$, and note that spatial smoothing (to fall in line with available observation resolutions) improves agreement between modeled and observed EKE fields. We also clarify that modeled sea-surface temperature and salinity differences relative to satellite and reanalysis products fall within the statistical uncertainty of those datasets, and that the domain-mean mixed layer depth was almost identical to the reanalysis. Finally, we specify that the 300-m model frequency spectra are in fair agreement with observations from the DeepLev mooring and that it captures the observed wintertime submesoscale energization in the 1–5 day band above 500 m depth. Monthly mean velocities are in agreement with a shelf break-mooring (Rosentraub and Brenner 2007) (L 89-100).

We believe these revisions clarify the basis for our statements while appropriately referring the reader to Solodoch et al. (2023) for the full validation analysis.

2. Lines 162-165: "When referenced to the shared summer minimum, the wintertime increase in the East is ~24% larger than in the West…" I believe that it is important to make this finding crystal clear since it is included in the abstract as the main quantitative conclusion. Is the "shared summer minimum" the minimum mean Chl concentration of the two boxes, or is it the minimum from the unsmoothed daily Chl data? Is the 24% increase the mean result? Are 11.5 (Line 164) and 25% (Line 165) mean values? Where are the results of 24% compared to 25% drawn from? 25% and not 24% is referenced again in the Discussion (Line 258).

The authors thank you for this observation. The shared summer minimum has been better defined as "To compare the seasonal amplitude between regions, winter Chl concentrations were referenced to the common summer baseline defined by the mean summer Chl concentration. " (L 167), and we added another line for clarity regarding the averaging:" For each region, Chl values represent spatial averages over the respective boxes and temporal averages over the indicated seasons" (L 163).

The mismatched reports stem from a rounding inconsistency. The exact figures calculated from the 10-year time series are:
Winter mean concentrations (Jan–Mar):
East: 0.06665 mg/m³
West: 0.05974 mg/m³
East is 11.56% higher than West (reported as 11.6%)

Summer mean concentrations (Jul–Sep):
East: 0.02792 mg/m³
West: 0.02794 mg/m³
East is 0.09% lower than West (reported as 0.1%)

Winter increase in the East referenced to the summer baseline, is 24.76% (reported as 24.8%)

Interannual Variability:
Winter EN>WN % : mean = 11.529%, std = 8.886% (reported as 11.5% and 8.9%)
Extra EN rise % : mean = 24.953%, std = 22.471% (reported as 25.0% and 22.5%)

The ten-year mean winter difference between the east and west (11.56%) is different from the mean derived from the interannual variability (11.529%) because its calculated differently. The former is a percentage computed from **10-year mean values,** and the latter is the **mean of yearly percentages.** It's the same for the seasonal amplitude calculations and statistics. At some point, we must have rounded 24.7% down to 24% instead of 24.8%. We have now ensured that all statistics are reported consistently and that the correct numbers have been added to the abstract and the results section.

3. The authors clearly show that the submesoscale circulations increase offshore transport by comparing Lagrangian particle simulations in the 300m and 3km wintertime simulations. However, the 3km offshore particle transport is still larger than the summertime (Figures 4B/H, C/I, and 5A), indicating that mesoscale transport plays some role and should not be entirely discounted. Lines 233-235 would be an appropriate place to further clarify this, e.g. “mesoscale wintertime spirals”, with some mention in Lines 259-261. E.g., “the observed winter enhancement cannot be explained by local vertical processes, regional differences in nutricline structure, or mesoscale currents alone.”

The authors thank you for this comment. As suggested, we have added clarification that indeed mesoscale structures play a significant part in offshore transport. (L 239,267).

Technical Corrections:

1. Abstract Line 6: Recommend removing the word “also”, since that is interpreted to reference the previous sentence, which describes vertical submesoscale processes that are not the diagnosed mechanism of increasing chlorophyll in this case.
Thank you, the word “also” has been removed.
2. Line 18: Recommending not to start a new paragraph here to improve flow.
Thank you. The authors have implemented your suggestion for line 18.
3. Line 81: Readers may not be familiar with what a sigma level is. Please briefly define, e.g. “terrain-following depth levels”.
Thank you for pointing this out. The sentence has been rephrased and a definition added: ”...employing 80, 120, and 150 terrain-following (σ) vertical levels…” (L80).
4. Lines 138-139: Reference the dashed lines in Figure 1A, B to visually aid the reader, and describe in the caption of Figure 1 what the dashed line represents
Thank you. The authors have added a reference to Figure 1 at Line 142, and added a description of what the line represents in the caption for Figure 1.