Response to reviews

Coastal Eddies as Vectors for Connectivity During the Summer in the Levantine Sea

We thank the Editor and both reviewers for taking the time to read our manuscript carefully and for their helpful comments. Their suggestions have helped us explain the observations more clearly, place our findings in the context of earlier work, and distinguish the processes supported by our measurements from those that remain possible explanations.

We have revised the text and figures accordingly and respond to each comment below. Referee 1's comments are shown in red and Referee 2's in blue, with our responses in black. Changes in the tracked manuscript follow the same colour convention. Line references refer to the revised tracked-changes manuscript.

Referee 1

Comment 1

L. 7, explicitly mention that you used an event-based approach, with three events.

Response

We thank the reviewer for this comment. We have revised the abstract to state explicitly that the study uses an event-based approach focusing on three coastal eddy events (L 7–9).

Comment 2

L.12, rephrase this sentence

Response

We thank the reviewer for this suggestion. We have rephrased this passage as two sentences to distinguish the biogeochemical observations from the circulation measurements and improve clarity. The revised text reads: (L 14–15).

"Cruise profiles reveal elevated subsurface concentrations of silica and inorganic particulate matter. HF radar measurements document anticyclonic circulation and offshore propagation of the coastal eddies."

Comment 3

L.49: please be more precise on mesoscale typical size, as it varies significantly across the Mediterranean Sea.

Response

We thank the reviewer for requesting this clarification. In the revised Introduction, we specify the typical range and regional variability of the first baroclinic Rossby radius of deformation, including smaller values over continental shelves and the estimate for the eastern Mediterranean. We distinguish this dynamical length scale from observed eddy diameters and place the smaller coastal eddies investigated here in the context of larger eddies documented in previous Mediterranean studies. This clarification is incorporated into the passage reorganized in response to Comment 7 (L 52–66).

Comment 4

L.53: Zodiatis et al (2023) A and B are the same article.

Response

We thank the reviewer for identifying this duplication. We have removed the duplicate reference and updated the corresponding citations (L 60; L 883–885).

Comment 5

L.66: Please describe more explicitly to which other coastal systems you refer, and why such comparison is useful.

Response

We thank the reviewer for this suggestion. We have revised the passage to identify the regions discussed and clarify the basis for comparison. Although Cornec et al. (2021) is a global analysis, it presents specific conclusions for stratified subtropical waters. We distinguish these findings from regional studies in the South China Sea and on the continental shelf adjacent to the East Australian Current, highlighting eddy effects on subsurface chlorophyll and nutrient distributions under stratified conditions (L 80–83).

Comment 6

L.146, typo in parenthesis

Response

We thank the reviewer for spotting this typographical error. We have corrected the parentheses around the citation (L 166).

Comment 7

L.163-174: This paragraph is quite long, and mostly introduce previous SSH-based studies. It should better fit in the introduction to explain your choice of chlorophyll remote-sensing data, prior to methods.

Response

We thank the reviewer for this suggestion. We have integrated the SSH-based literature and the rationale for using satellite chlorophyll imagery into the Introduction. The revised passage connects previous Mediterranean eddy research with the resolution and coastal limitations of altimetry, explaining our use of ocean-colour imagery complemented by HF radar observations. We have also consolidated the discussion of the remaining observational gap to avoid repetition and streamlined the opening of the Methods section (L 56–71; L 196–201).

Comment 8

L.272: The Rhodes gyre is not a cyclonic eddy, it is a gyre, which is much larger. + typo "eddies identified". If you want to discuss more about the Rhodes gyre, additional physical studies could be cited (e.g. Marullo et al, JGRO, 2003, Theocharis et al, Deep Sea Res, 1993, and maybe others)

Response

We thank the reviewer for this correction and the suggested references. We now identify the Rhodes Gyre as a larger-scale cyclonic circulation feature and have added Marullo et al. (2003) to support its physical characterization. Rephrasing the sentence also resolves the spacing error (L 292–294).

Comment 9

L.288: The mergings of coastal eddies coming from the Nile mouth and the Levantine coast to larger offshore anticyclones was already discussed by Barboni et al (2021).

Response

We thank the reviewer for highlighting this previous work. We have revised the passage to acknowledge both the formation and merging of boundary-current anticyclonic spirals illustrated through modeled vorticity fields by Verma et al. (2024), and the coastal detachment and subsequent merging with offshore anticyclones documented using SST imagery by Barboni et al. (2021). These references place our chlorophyll-based observation within the context of previously described physical processes (L 309–313).

Comment 10

L.334 and later: the 4 digits accuracy of "1,500 kg" is not associated with any uncertainty. Scientific formulation "1.5 x 10^3 kg" is expected.

Response

We thank the reviewer for pointing out the implied numerical precision. We now report the chlorophyll mass estimates consistently in scientific notation with two significant figures throughout the manuscript (L 365–366; L 514; L 580).

Comment 11

L.393-394 (and more broadly Section 4.1): Previous studies already further documented the formation, detachment and drift offshore. You should discuss further on this particular aspect, more clearly highlightening your contribution. Gertman et al. (2010) showed the trapping of a buoy in such coastal eddy exporting waters offshore. Efrati et al (2013) already estimated the width, depth and flux associated with similar anticyclonic coastal detachment.

Response

We thank the reviewer for this suggestion. We have expanded Section 4.1 to acknowledge more explicitly the contributions of previous studies. Gertman et al. (2010) documented anticyclonic eddy detachment and offshore propagation using drifter trajectories, while Menna et al. (2012) characterized regional surface circulation and seasonal variability. Baaklini et al. (2024) and Akpınar (2024) examined coastal-water pinching-off events and seasonal frontal variability, respectively, and Estournel et al. (2021) and Verma et al. (2024) investigated three-dimensional circulation and boundary-current instabilities through numerical modeling. Efrati et al. (2013) combined satellite and shipboard observations to characterize the physical and biogeochemical properties of a coastal intrusion, identify its approximately 25 m vertical extent, and estimate its offshore volume flux. Building on these findings, our study extends the vertical characterization to approximately 200 m, identifies a shoaled DCM resembling the coastal shelf chlorophyll structure, and documents recurring vertical signatures across independent sampling events. HF-radar measurements of anticyclonic circulation, supported by depressed isopycnals, further link these biogeochemical signatures to transport by anticyclonic coastal eddies (L 435–442; L 458–480).

Comment 12

Section 4.1 or 4.2. At some point should be also discussed the fact that coastal mesoscale eddies are only anticyclones. Are cyclones also present? What is their impact?

Response

We thank the reviewer for raising this point. We have clarified the evidence for circulation polarity in our sampled events. Anticyclonic circulation is supported by HF-radar measurements in the 2020 event and depressed isopycnals in the primary glider case, while polarity was not independently established for every event. Drawing on Verma et al. (2024), we describe how concentrated anticyclonic vorticity in the summertime boundary current develops into anticyclonic spirals, with smaller cyclonic spirals occasionally forming at their peripheries through secondary instabilities. We acknowledge their potential contribution to coastal-water redistribution, while noting that their biogeochemical effects are not resolved by our observations (L 458–468).

Comment 13

L.477-471: Efrati et al (2013) already suggested that these local detachments could sustain plankton niches.

Response

We thank the reviewer for highlighting this earlier interpretation. We have revised Section 4.4 to explicitly acknowledge Efrati et al. (2013), who proposed that coastal intrusions bounded by transport barriers could sustain localized plankton niches. We also cite Moore et al. (2007), who identified modified shelf phytoplankton communities within offshore anticyclonic eddies. We clarify that our observations characterize physical and biogeochemical signatures, while plankton-community composition was not measured in this study (L 539–545).

Comment 14

L.481-484: this accumulation of citations is not linked to your previous results. These studies are further not on the same region, then without explanation the comparison seems off-topic.

Response

We thank the reviewer for pointing out the weak connection between these statements and our results. We have removed the broad statement about fronts, biological heterogeneity, and energy transfer, together with its citation cluster. We retain a shorter statement on potential effects of episodic coastal inputs on ecosystem structure, supported by Lévy et al. (2018) and Chenillat et al. (2024), following the more specific discussion of Efrati et al. (2013) and Moore et al. (2007) (L 545–549).

Referee 2 - Major comments

Comment 1

The word "coherent" as used in the manuscript is loaded and too strong (e.g. lines 304-307 and elsewhere). Transport can be due to stirring induced by mesoscale features, but also inside a material surface, such as at the core of an eddy. The evidence presented in the manuscript suggests that having rotational velocities greater than translation speed is sufficient to prove a coherent eddy exists. There is quite a bit of literature on this subject, such as the work of Haller and others (doi: 10.1146/annurev-fluid-010313-141322). If you want to claim that the eddies are coherent, your best approach would be to generate streamlines or advect particles within the velocity fields of the HF radar dataset, and show that the trajectories close on themselves.

Response

We thank the reviewer for clarifying the distinction between rotational circulation and material coherence. Although the satellite chlorophyll snapshots qualitatively suggest a feature bounded by closed contours, the eddy is not continuously resolved within the HF-radar domain as it propagates. The observations sample both its periphery and core, but coverage of the complete feature is limited to a single approximately three-hour averaged field. Consequently, we cannot establish whether a material boundary retains the same water throughout propagation, as required to demonstrate material coherence in the sense discussed by Haller (2015). We have revised the terminology throughout the manuscript, removing claims of dynamically coherent eddies and describing the observed circulation and hydrographic signatures more explicitly. We retain the rotational-to-propagation speed comparison as evidence consistent with potential fluid trapping, following Chelton et al. (2011), while explicitly acknowledging that material retention during propagation was not directly assessed. We have also removed the statement that persistent closed circulation confirms coherence (L 19; L 92; L 99–101; L 235; L 317–324; L 336–347; L 419–430; L 450; L 535–538).

Comment 2

The text mentions that radial and azimuthal components of HF radar velocity were calculated. The figures shown in the text are quiver plots of the full velocity, as best I can tell. Please include a (supplemental) figure showing the azimuthal velocities to help your argument.

Response

We thank the reviewer for this helpful suggestion, which strengthens the interpretation of the observed circulation as anticyclonic. We have added Fig. A3, showing the azimuthal components of the HF-radar velocities alongside the corresponding Level-4 chlorophyll maps for 30 July–3 August 2020. We have also added a short interpretation in Section 3.2: within the portions of the chlorophyll feature sampled by HF radar, azimuthal velocities are predominantly negative, supporting anticyclonic circulation as the feature moves into the radar domain (L 281–285; L 330–332; Fig. A3).

Comment 3

Since there is quite a bit of hydrographic data presented here attempting to characterize the coastal productive water, I would suggest including a T-S diagram to help in doing so, perhaps scatter plots with the scatter points colored by a third property (Chl, Si, etc).

Response

We thank the reviewer for this suggestion. We examined T\S diagrams for the summer N800 observations and for the eddy and reference profiles from glider mission M141. We provide the M141

comparison below as Fig. R1, with observations coloured by chlorophyll concentration. The eddy observations include warmer surface waters, and elevated chlorophyll spans a broader temperature range than in the reference observations, where the enhancement is more narrowly concentrated. These patterns are consistent with the hydrographic structure and broadened, shoaled chlorophyll distribution already presented in Figs. 3 and 5 (L 345–360; L 361–366). We therefore consider the T\S comparison useful as a complementary visualization, but have retained the depth-resolved figures in the manuscript because they show these differences directly in their vertical context.

For N800, the monthly observations at a fixed offshore station contain both seasonal variability and the influence of episodic transport, rather than contemporaneous observations of distinct source and receiving waters. The T\S diagrams illustrate differences between months but do not provide a clearer separation of coastal and offshore waters than the temperature and salinity profiles already presented alongside PAR in Fig. A4. We have therefore retained those profiles, which preserve the depth and vertical extent of the hydrographic differences relevant to the chlorophyll structure.

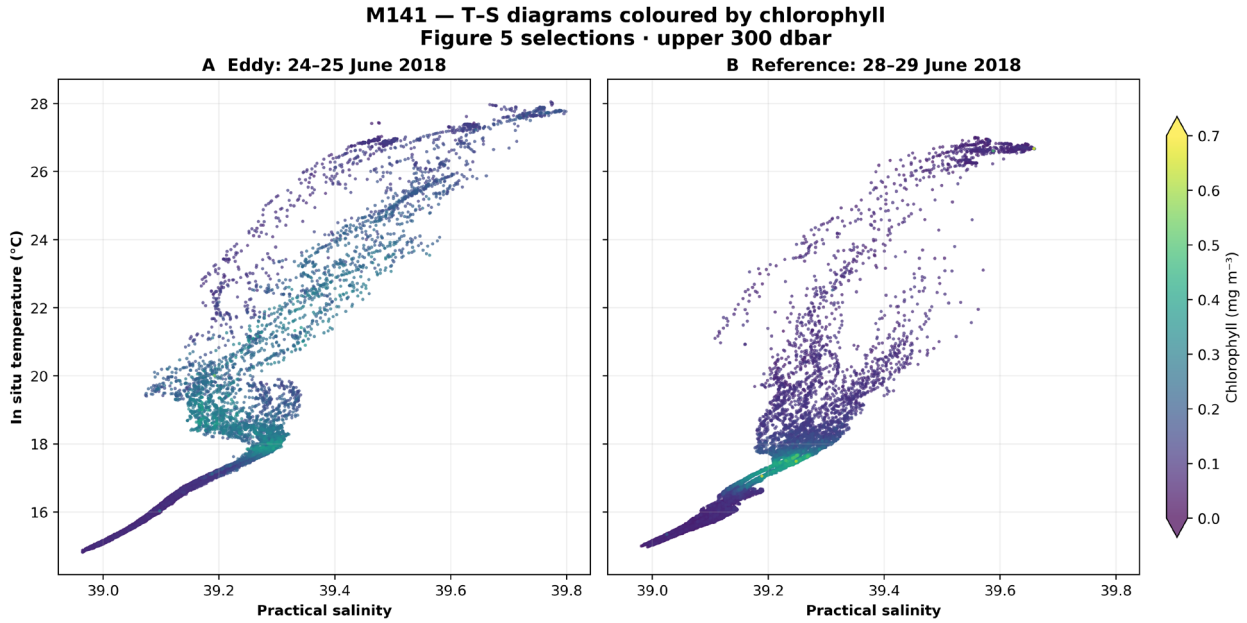

Figure R1. Temperature–salinity diagrams coloured by chlorophyll concentration for (A) eddy and (B) reference observations from glider mission M141. The selections match those used for Fig. 5, excluding the five casts recorded landward of the 700 m isobath. Observations are restricted to the upper 300 dbar.

Comment 4

Lines 426-432: I appreciate you postulate a hypothesis here, it is important to be thinking through what mechanisms might be at work. However, it requires more fleshing out, namely how would the proposed re-suspension impact the profiles seen in N800 station data (e.g. where would you expect the min/max of different profiles to change? Perhaps start with the zero-order hypothesis that the coastal water is simply advected, without further modification by the coastal eddy.

Response

We thank the reviewer for encouraging a clearer distinction between the baseline explanation and additional possible mechanisms. We have revised Section 4.2 to state that advection of preconditioned coastal water provides a plausible explanation for the N800 anomalies without requiring further modification during transport. We retain resuspension during eddy formation as a possible additional contributor and explain its expected signature: enhanced particulate Fe and turbidity within the transported layer, without necessarily producing a corresponding chlorophyll maximum. We also retain the possibility of entraining sediment-associated photosynthetic microorganisms and subsequent nutrient-

supported growth. The revised passage cites relevant studies and explicitly acknowledges that our measurements are insufficient to distinguish these contributions from transport of material already present in coastal waters (L 480–487).

Referee 2 - Minor comments

Comment 1

Line 65: Define DCM

Response

We thank the reviewer for noting this omission. We now define the deep chlorophyll maximum (DCM) at its first occurrence in the Introduction as a subsurface layer characterized by a local maximum in chlorophyll-a concentration (L 78–79).

Comment 2

What is the domain over which the mean chlorophyll value is calculated in equation (1)?

Response

We thank the reviewer for requesting this clarification. We have specified that the background chlorophyll concentration is calculated from valid pixels seaward of the 700 m isobath within 33.0–35.5°E and 31.0–33.7°N (L 220–224).

Comment 3

Line 272: include space between "eddiesidentified"

Response

We thank the reviewer for spotting this typographical error. The affected sentence has been rephrased in response to Referee 1, resolving the missing space (L 292–295).

Comment 4

Can you include a timeseries of the Area of the eddies, to see how good the continuity is in the dataset?

Response

We thank the reviewer for suggesting this continuity check. We have added daily offshore feature areas derived independently from L3 and L4 chlorophyll products to Table A2 for the five consecutive days of the HF-radar case. Both products show a progressive increase in detected area, from approximately 607 to 1364–1368 km², with close agreement between products. These values represent only the chlorophyll feature seaward of the 700 m isobath; their evolution therefore reflects changes in the detected offshore extent, including movement across this boundary, rather than changes in total eddy area.

Comment 5

Figure 3G,H, 4G. Please indicate along the timeseries the 700m isobath, and also which profiles are used for the inside/outside characterisation.

Response

We thank the reviewer for this helpful suggestion. We have revised Figs. 3G,H and 4G to mark the periods used for the eddy and reference profile calculations with dashed white and black rectangles,

respectively. Dotted yellow lines indicate the 700 m isobath crossings. The M164 time axis has also been extended through 3 July to include the full 15-cast reference selection. For M141, the glider briefly crossed approximately 1.3 km landward of the 700 m isobath during its eastward transect; the five profiles recorded landward of this boundary have been excluded from the calculations. We have clarified this selection in Section 2.2.1, "Estimation of chlorophyll redistribution." (L 240–245; Figs. 3–4).

Comment 6

Lines 359-361. Iron concentrations are usually reported in nM.

Response

We thank the reviewer for pointing this out. We now report particulate Fe concentrations in nM throughout the relevant Results passage and have updated the units and numerical values in Fig. 7E accordingly (L 389–395; Fig. 7E).

Comment 7

Line 366: Nitracline - presumably of silica?

Response

We thank the reviewer for highlighting this ambiguity. We have removed the reference to the nutricline and revised the passage to describe the observed chlorophyll profiles directly, emphasizing their broader vertical distribution and shallower enhancement in July and September (L 396–405).