We thank Referee #3 for her helpful and important comments on the revised version of the manuscript. We have addressed all the comments to improve the paper. Our responses to questions are detailed as follows:

Review of manuscript egusphere-2024-3730 entitled " Geostrophic circulation and tidal effects in the Gulf of Gabès"

Main comment:

Within the manuscript the authors use a 30-years time series of altimetry data as well as a numerical model (all freely available from CMEMS, Copernicus Marine) to investigate the dynamics in the gulf of Gabès. This region is of particular interest since it is the area of exchange between Western and Eastern Mediterranean Sea water masses. The authors perform a climatological study of the geostrophic circulation and investigate the effect of tides leading to the generation of a cyclonic current. The effect of persistent Lagrangian structures (FTLE) on the phytoplankton bloom occurrence is also discussed.

The paper is detailed, well-written and well structured and I think provide a quite complete overview of the dynamics, as seen by altimetry (or limited only to geostrophic balance) of the area. In its current form the paper is very interesting but I think would benefit from few more information/analysis before it can be published. Therefore I would recommend to publish the manuscript after some major revision. Please find in the following my detailed comments.

Major comments:

Q1) Even though I am sure that this kind of climatological review is necessary for a good understanding of the studied area, it seems to me that the text lacks from any explanations about what this kind of analysis brings in terms of new knowledges. It stated several times the results agree with previous work but never what we are the additional information. For example in the Introduction and Conclusion, the authors may emphasize more on the novelty of their approach compared to previous studies. I really think this could boost the readers' interest.

R1) Done, see please lines: 53-58; Some efforts have been made to focus on the dynamics of offshore waters in the central Mediterranean Sea from satellite-derived products, i.e. the dynamics in Sicily Channel show multi-scale spatial and temporal variability (Menna et al, 2019). Nevertheless, a long term analysis for understudied regions like the coastal GG area can benefit an overview of: persistent Lagrangian structures, attracting and repelling coastal zones, trends and upwelling flow.

The geostrophic circulation from altimetry data for the three decades within the GG is characterized by strong seasonal and spatial variability where the dynamics varies differently in the three subareas (lines: 609-611).

Q2) One point that is not clearly stated in the entire text, although written on line 421, is that FTLEs are dynamical diagnostics allowing to identify frontal/stretching areas it cannot be used as a diagnostic of biogeochemical processes. They can explain the relative 2D horizontal dispersion/distribution of some biological quantities and thus provide some insights on potential vertical processes that may engender phytoplankton blooms (Lévy et al.,). I would like to draw the authors attention on the fact that throughout the text a confusion can arise especially in section 3.2.2 (see detailed comments). Also, the title of section 3.2.2 is a bit confusing tome. I would not talk about turbulence here for several reasons:

- FTLE are not a diagnostic of turbulence, especially when computed with low-resolution altimetry-derived (geostrophic) surface currents

- In the present study, the authors got interested in features detected by persistent FTLEs (a mean over a long time period) which means that the features discussed here occur at 1 temporal scales (years) that are way larger than turbulence (days) or even fine-scales (weeks-months).

I would thus recommend to modify the section title for something like "impact of tides on strain and effect on biogeochemical distribution" or even the authors may consider splitting tides and FTLEs into two different subsections.

R2) Done, see please lines: 532-534.

We totally agree with the proposition and we modified the section title accordingly (line 454).

Q3) The authors provide a quite complete overview of the dynamics in the Gulf of Gabès but never discuss the evolution of SSH (surface currents velocity or direction…) as monitored by the altimetry time series between 1993 to 2022. This could provide insights on the evolution of the regional dynamics (any trends ?) in the context of the climate change. In the discussion section these trends (if any?) could be discussed for future years evolution and potential impact on biology.

R3) We computed the daily mean speed and kinetic energy (KE) time series over 30 years (1993-2022) using altimetry data, as shown in Figure 4. The quantities are averaged over the larger GG box, indicated by the red rectangle in Figure 2. Higher speed and KE values are mostly observed in winter and fall, while lower values occur in spring and summer. This variability is likely strongly related to atmospheric forcing. In order to evaluate the evolution of regional dynamics over the decades, we computed the means of the two quantities separately for the three following periods: 1993-2002, 2003-2012 and 2013-2022. The mean speed increased over the decades, from 7.35 cm/s in 1993–2002, to 7.6 cm/s in 2003–2012, and 8.01 cm/s in 2013–2022. Similarly to the averaged speed, the Mean Kinetic Energy also increased by approximately 7 cm^2/s^2 from the beginning to the end of the considered period. See please lines: 324-334.

This study investigates sea surface height trends over the GG from 1993 to 2022, where the surface layer shows a speed trend of 0.033 cm/s and a KE trend of 0.34 cm^2/s^2 as shown in Figure 3. The evolution of regional dynamics, and the consequent potential impact on biogeochemical aspects, is certainly a highly interesting topic, worthy of further investigation in future studies (lines: 555-559).

Detailed minor comments:

L 15: "biogeochemical processes": I would rather use "biogeochemical dispersion".

Done, Line 19.

L 24: "richest" for the Mediterranean Sea yes but it is relative for other "rich" places in the world ocean. Maybe the authors can cite some references here.

Done, see please line 31.

L 35: "One of them … southward ()." I could not understand this sentence, please rephrase.

Done, see please line 45.

L 44: “spatial-temporal” change for “spatio-temporal”

Ok, line 60.

L 53: “exert” not sure if it is correct in English, “act” ?

Ok, line 72.

L 163: You can also cite other types of applications such as: d’Ovidio et al. (2010), Rousselet et al. (2025).

Done, line 204.

L169-171: I totally agree with these statements, however I don’t see how in this study these gaps are leveraged ? Please maybe add a comment in the text.

This study seeks to address this gap by computing λt specifically for LCS analysis in these areas. The implementation of FTLE using particle trajectories with increasing resolution (Onu, et al, 2015) in the GG could bring new insight into how coastal features impact biology. The use of FTLE in coastal areas is reliable to detect LCS (Peng et al, 2024). See please lines: 215-217.

L 180: forward in time.

Done, line 227.

L178-192: I do not understand for how long are the particle trajectories advected to computed FTLE ?

The particle trajectories are daily advected and then averaged seasonally over a 30-year period (lines: 231-232).

Figure 1: I think only two panels would be sufficient (either 2D or 3D bathymetry).

Done,

L 207: Even though I agree with the theory, some subareas are very coastal and we know that altimetry is not really reliable there, so maybe the authors can justify the use of altimetry data in such coastal zones.

Yes, the use of the altimetry data in very coastal areas can be limited by the lower spatial resolution of the data. Due to the lack of high resolution long term datasets availability in the GG we use altimetry data in its coastal areas. Whereas, altimetry analysis could help to overview long term kinematic properties in the coastal regions (Rinivasan and Tsontos, 2023) (lines: 281-282).

L 234-235: This is related to the major comment 2). Mean FTLE averaged over 30-year altimetry cannot be used to investigate chaotic turbulence since it is detecting large scale persistent (permanent) features. However I agree that such diagnostic is comparable to a mean concentration of Chl-a, I am just concerned by the sentence and reference to “chaotic turbulence”.

Our intention was to highlight the spatial patterns of stirring as inferred from FTLE, so we agree that the term “chaotic turbulence” for a 30-year mean FTLE analysis is not proper, thus we modified “chaotic turbulence” to “GG dynamics” (see please line: 315).

L 259: “several cyclonic eddies”. Again here can we rather talk about “permanent/recurrent eddies” or even “gyres” ?

Done, see please line 355.

Figure 4: In the caption please specify that the quantities are mean over each boxes.

Done, please note that Figure 4 has become Figure 5 in the revised version of the manuscript.

L 286-287: “the model results” at the surface. No comparison are performed on the vertical. Also is the model assimilating any observations ? Because if the model is assimilating satellite data then the agreement between the model and observations is obvious and I think this part should be removed.

Yes, the model is assimilating satellite data. We removed this part and the old Figure 5.

L 389-390: I don’t understand how the comparison between Chl-a and FTLE can “provide insights into the time lag” ?

The sentence is removed.

L 391: “biogeochemical processes”. I would change processes for “dispersion” since the biogeochemical processes are never really discussed (which one ? How?…)

Done, we modified the sentences, see please lines: 494-496.

L 414-415: I am not sure about this statement because many FTLE occurrences are not linked with any phytoplankton bloom (or more specifically high concentration of Chl-a). The authors should clarify or explain.

Yes, we agree that some FTLE/FSLE occurrences are not usually linked with phytoplankton bloom. But in some cases of geostrophic currents FTLE/FSLE can show fronts producing Chl-a filaments controlling phytoplankton bloom. See please for more details (Lehahn et al, 2007; Guinder et al, 2025). See please lines: 522-524.

L 437-439: Here I would link this to a dynamical process: “FTLE” act as barriers to offshore transport.

Done, see please line 552.

L 443-444: This statement is redundant, please remove or move to methods.

Ok, the statement is removed.

References: d’Ovidio, F., De Monte, S., Alvain, S., Dandonneau, Y. and Lévy, M., 2010. Fluid dynamical niches of phytoplankton types. Proceedings of the National Academy of Sciences, 107(43), pp.18366-18370.

Louise Rousselet, Francesco d'Ovidio, Lloyd Izard, Alice Della Penna, Anne Petrenko, et al.. A Software Package for an Adaptive Satellite-based Sampling for Oceanographic cruises (SPASSOv2.0): tracking fine scale features for physical and biogeochemical studies. 2024. ⟨hal-04705438⟩