

General Comment

The manuscript assesses trend in EKE in the Mediterranean Sea over the 1993-2023 using gridded altimetry products (a two-satellite product and two all-satellite products). The authors examine both basin-wide and regional EKE changes, analyse differences between products, investigate eddy characteristics, validate trends against along-track data and explore the influence of sampling changes associated with the increasing number of altimeters.

The manuscript is clearly written and the results are clearly presented supported by thoughtful figures.

This work follows closely the recent global study by Barceló-Llull et al. (2025). The present manuscript applies a very similar methodological framework - contrasting two satellite and all-satellite altimetric products and analysing regional EKE trends — but within the specific context of the Mediterranean Sea. In doing so, it provides a valuable regional application that complement the global perspective.

I recommend its publication with minor revisions.

Barceló-Llull, B., Rosselló, P., Combes, V., Sánchez-Román, A., Pujol, M. I., and Pascual, A.: Kuroshio Extension and Gulf Stream dominate the Eddy Kinetic Energy intensification observed in the global ocean, Sci. Rep-UK, 15, <https://doi.org/10.1038/s41598-025-06149-9>, 2025.

Specific Comments

Line 92: For clarity and consistency with common altimetry literature, the authors may consider referring to this component as the cross-track velocity.

Lines 140-143: The study reports trends for the eastern Alboran gyre but no figure specifically highlights this sub-region. Including a small inset or zoomed panel for the eastern gyre would help readers better visualise these local trends.

Figure 3: Please defined “area-weighted mean”. A brief explanation of how the weighting is performed would improve clarity.

Figure 4: I suggest slightly rephrasing the caption for panel (a) for clarity. For example: “Time series of the difference between all-sat-glo and two-sat-glo EKE and the number of altimetry missions.”

Lines 186-188: “*Even though, the norther nlonian Sea ... from anticyclonic to cyclonic.*”
The sentence is not very clear. I suggest the following rephrasing:

“The northern Ionian Sea also shows negative trends, whereas the central Ionian displays positive trends, a pattern that may be linked to shifts in the basin’s circulation between anticyclonic and cyclonic states (Bessi eres et al., 2013; Kalimeris and Kassis, 2020).”

Table 1: The terms “mean of all trends” and “trend of the mean” should be defined.

Line 193: As mentioned in a previous comment, the term “area-weighted mean” has not been defined and should be clarified.

Line 200: The motivation for recomputing EKE using the CMEMS 1993–2012 reference period is not fully explained. It is a bit unclear to me what additional insight it provides beyond showing the sensitivity of trends to anomaly definition. The need to compute geostrophic anomalies with respect to the full time period is standard practice.

Figure 7(b): We observe a negative trend in the L3-ref date that does not appear in the L4 product. Could the authors provide an explanation for this discrepancy?

Line 211: I would suggest using the term “cross-track” instead of “perpendicular”.

Section 3.4: The manuscript reports statistics for “mesoscale eddies” in general, without distinguishing cyclonic vs anticyclonic. EKE contributions and dynamical behaviour can differ between cyclones and anticyclones. The authors could provide separate statistics if possible. Even a brief note would strengthen the interpretation.