

Dear Editor,

Thank you for inviting me to review this manuscript. This paper presents a new global mesoscale eddy network atlas, META4.0. Its main innovation lies in explicitly identifying eddy merging and splitting events on the basis of conventional single-trajectory eddy tracking, and in organizing related eddy trajectories into interconnected network structures. The authors present diagnostic tools for eddy networks, global statistical results, the spatiotemporal distribution of merging/splitting events, changes in eddy properties, qualitative validation with external data, and particle-advection-based coherence analysis.

Overall, this dataset has potential application value. However, the current manuscript still has several issues related to methodological definitions, parameter selection, independent validation, reproducibility information, and the interpretation of some figures and tables. In particular, the abstract is inconsistent with the validation presented in the main text; the definition of normalized overlap used for trajectory tracking is insufficiently clear; the 10% overlap threshold and the five clustering categories lack adequate objective justification; the external CHL validation is closer to a qualitative consistency check based on a small number of typical cases than to a truly independent validation of eddy boundaries; and some figures and case descriptions do not provide sufficient information for reproducibility and deeper interpretation.

For these reasons, I recommend that the manuscript undergo major revision before publication.

Major comments

1. The statements in the abstract regarding independent validation using SST and CHL are inconsistent with the main text

The abstract states that the study uses independent datasets, including sea surface temperature and chlorophyll concentration, to qualitatively validate selected events. However, in Section 3.3.5 of the main text, the external validation that is actually presented and discussed uses only CHL data, with no validation procedure or conclusions based on SST.

This creates an inconsistency between the abstract and the main text. I suggest that the authors either add validation results for typical merging/splitting events using SST data, including the data source, processing procedure, diagnostic criteria, and corresponding figures, or revise the abstract and related conclusions to avoid claiming that SST was used for qualitative validation.

2. Justify the use of 1/4° ADT data and discuss resolution sensitivity

This study uses global daily ADT data from the DT2021 reprocessing product at a spatial resolution of 1/4°. However, higher-resolution global L4 reprocessed products for sea level and derived variables are now available, such as the 1/8° × 1/8° product provided by Copernicus Marine. In addition, when discussing the spatial distribution of lonely eddies, the authors also note that some regions with a high occurrence of isolated eddies may be related to limitations in altimetric resolution.

Therefore, I suggest that the authors further justify why the 1/4° product is still used as the primary input dataset. Considering the large workload required to recompute a 30-year global atlas, the authors do not necessarily need to reconstruct the entire dataset. However, it would be useful to add a resolution-sensitivity experiment for representative regions or representative years. For example, several cases could be selected from western boundary current regions, the Southern Ocean, coastal regions, or equatorial regions, and the results obtained from the 1/4° and 1/8° products could be compared in terms of the number of detected eddies, the number of lonely eddies, the number of networks, the number of segments, and the number of merging/splitting events.

3. The area-overlap-based tracking method relies on a subjective threshold

The network grouping in this study mainly relies on the overlap between eddy polygons at adjacent time steps, with 10% set as the connection threshold. This threshold directly affects whether eddies are connected into the same network, and thus influences the number of networks, the number of segments, the number of merging/splitting events, and the resulting eddy lifetime statistics.

At present, the manuscript does not explain the rationale for choosing the 10% threshold, nor does it demonstrate the stability of the results under different threshold values. I suggest adding a threshold-sensitivity experiment.

In addition, I suggest that the authors compare the geometric-overlap tracking method used in this study with existing Lagrangian eddy tracking methods in the Discussion. The discussion could address the practicality of a geometric overlap threshold for constructing long-term global eddy networks, as well as its advantages and limitations relative to particle-advection methods in terms of physical constraints, computational cost, and parameter sensitivity. Relevant studies include Jones-Kellett A. E. and Follows M. J., “A Lagrangian coherent eddy atlas for biogeochemical applications in the North Pacific Subtropical Gyre,” *Earth System Science Data*, 2024, 16(3): 1475–1501; and Tian F., Zhao Y., Qin L., et al., “A Black Hole Eddy dataset of North Pacific Ocean based on satellite altimetry,” *Earth System Science Data*, 2025, 17(12): 7119–7145.

4. The definition of normalized overlap is insufficiently clear

The manuscript mentions normalized overlap around lines 125 and 135 and uses 10% as the network connection threshold, but it does not provide an explicit mathematical definition. It is therefore unclear how the overlap area is normalized. Different normalization schemes may substantially affect the identification of merging and splitting events.

I suggest that the authors provide a clear formula for normalized overlap before introducing this threshold, and explain how this definition affects the identification of the main eddy and secondary eddies in merging and splitting scenarios.

5. Table 1 lacks specific information needed to reproduce the representative network case

Section 3.1 introduces a representative anticyclonic network case and reports its number of observations, number of segments, number of merging/splitting events, lifetime, and longitude–latitude range. However, the manuscript does not provide the specific start and end dates of this case.

For a dataset paper, representative cases should be as reproducible as possible. I suggest that the authors add the start and end dates, as well as the specific data files corresponding to Figures 2–4. This would help readers verify the exploratory tools proposed by the authors.

6. Add local zoom-in panels for key interaction nodes in Figure 4

Figure 4 shows the temporal evolution of effective radius and shape error within the network, using marker size and color to represent eddy properties. Based on this figure, the authors interpret phenomena such as an increase in radius after merging, an enlarged radius before splitting, and elevated shape error near interaction events.

However, the current figure contains relatively dense data points, and the local changes before and after key merging and splitting events are not sufficiently intuitive. I suggest adding local zoom-in panels to Figure 4 to show the changes in eddy properties before and after typical merging and splitting events. This would provide more direct support for the interpretation that eddy interactions are accompanied by structural reorganization and morphological deformation.

7. Report network characteristics separately for cyclonic and anticyclonic eddies in Table 2

The manuscript states that network construction is performed separately for anticyclonic and cyclonic eddies. However, Table 2 only provides overall statistics for the META4.0-Networks dataset.

Since the construction procedure is polarity-specific, I suggest that Table 2 report statistics separately for anticyclonic and cyclonic eddies. These should include the number of observations, networks, segments, merging events, splitting events, and lonely eddies for each polarity, as well as the mean segment lifetime and network lifetime. This would help readers assess whether the two eddy polarities exhibit systematic differences in network complexity and interaction frequency.

8. The reason why segment lifetimes in Figure 9a are longer than META3.2 single trajectories is insufficiently explained

The central improvement of this study is that the network formalism connects multiple segments before and after merging and splitting events, thereby reducing artificial births/deaths in traditional single-trajectory methods and extending the effective lifetime of eddy systems. This conclusion is clearly reflected in Figure 9b.

However, Figure 9a shows that the lifetime distribution of META4.0 network segments is also longer than that of META3.2 single trajectories. This result is not intuitive. I suggest that the authors further explain why the segment lifetimes in Figure 9a are longer.

9. The diagnostic criteria for CHL-based external validation in Section 3.3.5 are unclear

Section 3.3.5 uses CHL fields to qualitatively validate merging and splitting detections, and Figures 18 and 19 show ADT and CHL background fields. The authors argue that these examples indicate that the merging/splitting events detected from altimetry can also be observed in an independent field.

However, the manuscript does not currently explain how CHL is used to diagnose interaction events. The eddy contours shown on the ADT and CHL panels in Figures 18 and 19 appear to be identical, suggesting that these contours are likely still eddy boundaries detected from ADT and merely overlaid on the CHL background field. If this is the case, CHL does not independently identify the same eddy boundaries, but instead provides a tracer-background response that is consistent with the ADT-based detection results.

I suggest that the authors clearly state whether all eddy contours in the figures come from ADT/META4.0, whether CHL data participate in boundary identification, or whether CHL is used only as a background tracer field. The authors should also specify the concrete criteria by which CHL is considered to support the merging/splitting detections.

In addition, the information in the corresponding network timelines in Figures 18 and 19 is rather crowded. I suggest retaining only the segments involved in the current interaction to improve readability.

Minor comments

1. The two subpanels in Figure 10 are not fully aligned.

2. It is reasonable to use normalized radius and normalized shape error in Figures 13 and 14 to analyze life-cycle stages before and after interactions. However, the normalized results cannot directly reflect absolute scale changes between parent eddies and resulting eddies. I suggest that the authors add corresponding statistics and figures for absolute effective radius and absolute shape error to support the statement that “merging leads to larger structures and splitting to smaller ones.”

3. In the multi-panel composites of Figures 18 and 19, some subpanels are slightly misaligned, and the overall layout is not sufficiently tidy.

4. In Figure 20, the colorbar labels in some subpanels are partially obscured or not fully displayed, and there are also alignment issues among different subpanels.

5. French labels appear in Figure 23b and should be unified into English.

In summary, I consider the META4.0 eddy network dataset proposed in this manuscript to have scientific value and application potential. However, the current version still requires further improvement in several respects, including key methodological definitions, parameter selection, robustness of results, sufficiency of external validation, and reproducibility information for representative cases. Therefore, I recommend that the manuscript be considered for acceptance only after major revision. I hope the authors will further strengthen the transparency of the methodology, the robustness testing of parameters, and the experimental framework, thereby improving the reliability, reproducibility, and practical value of the dataset. Thank you again for inviting me to review this manuscript. I also hope that the comments above will be helpful for the authors in revising and improving the paper.

Sincerely,

Reviewer