Responses to reviewers

We would like to thank the Editor and the reviewers for this second round of reviews and their constructive comments.

In the following document, as in the previous letter, reviewer comments are reproduced in black and our responses are given in blue. For transparency, each substantive modification is accompanied by a screenshot of the revised manuscript with track changes enabled. Additions and modifications are highlighted in red, whereas removed text appears as strikethrough text.

Response to report #1

Dear Editor, Thank you again for giving me the opportunity to review the revised manuscript by Gamot et al., entitled "META4.0: a new mesoscale eddy network atlas derived from altimetry". The authors have carefully addressed the comments raised in the previous round of review and have made substantial revisions to the manuscript. The revised version now provides a mathematical definition of the intersection-over-union (IoU)-based overlap metric, further explains the rationale for the 10% IoU threshold and the 7-day search window, expands the discussion of limitations associated with the effective spatial resolution of the altimetry product, and adds examples from dynamically distinct regions, including the Agulhas and Gulf Stream systems. The authors also clarify that the particle-advection experiments do not constitute a fully independent validation of the eddy detection and tracking method, but rather assess the consistency between the reconstructed networks and Lagrangian transport characteristics. These revisions have substantially improved the methodological transparency, accuracy of presentation, and overall completeness of the manuscript.

Overall, I believe that the authors have adequately addressed the major concerns raised in the previous round of review, and the manuscript is now largely suitable for publication. I therefore recommend acceptance subject to technical corrections. Nevertheless, I suggest that the authors further clarify two points in the final version. These comments mainly concern an accurate description of the applicability and limitations of the methodology and do not require new large-scale calculations or reconstruction of the entire data product in the present revision.

We thank the reviewer for the careful reading of the revised manuscript and for these constructive suggestions. Both points have been taken into account in the revised version.

Specific comments are as follows:

1. Parameter definitions are now clearer, but parameter robustness has not yet been directly demonstrated by sensitivity tests

I appreciate that the revised manuscript now provides the mathematical definition of IoU and gives a clearer explanation of the rationale for selecting the 10% IoU threshold and the 7-day search window. These revisions have satisfactorily addressed my previous concerns regarding unclear parameter definitions, meanings, and selection criteria. It should be noted, however, that explaining the rationale for parameter selection and quantitatively demonstrating parameter robustness are two different issues. The 10% IoU threshold and the 7-day search window directly affect network connectivity, trajectory reconnection, and the identification of merge/split events. Therefore, strictly speaking, the current manuscript has not yet demonstrated, through systematic parameter sensitivity tests, that the main statistical results are robust to variations in these empirical parameters. In particular, the potential risk of spurious eddies and false reconnections associated with the 7-day search window deserves further discussion. Although a relatively long temporal tolerance can help restore trajectory continuity after short-term detection gaps, it also enlarges the spatiotemporal search domain for candidate matches. If spurious eddies or features caused by altimetry mapping errors, noise, or short-lived anomalous structures occur during the intervening period, a subsequently detected structure that does not correspond to the same physical eddy may be reconnected to the original trajectory. This may generate spurious connections and, in turn, affect estimated trajectory lifetimes and the statistics of merge/split events. In this context, Faghmous et al. (2015), "A daily global mesoscale ocean eddy dataset from satellite altimetry", Scientific Data, 2, 150028, doi:10.1038/sdata.2015.28, provides a relevant discussion of spurious features and tracking uncertainty in automated eddy datasets. I therefore recommend that the authors explicitly discuss the potential risk of false reconnections introduced by the 7-day search window and state that this risk has not yet been quantitatively assessed through parameter sensitivity analyses. Given that the authors have now provided a reasonable explanation for the empirical basis of these parameters, and that a complete global sensitivity experiment would require substantial additional computation, I no longer consider new sensitivity tests to be a prerequisite for acceptance in the present round. However, I recommend adding a concise and explicit statement of this limitation in the Methods or Discussion. The manuscript should clarify that these parameters are empirically chosen and that systematic parameter sensitivity analyses have not yet been performed. In particular, while the 7-day search window may reduce trajectory fragmentation caused by short-term detection gaps, it may also increase the risk that spurious eddies or unrelated eddy structures are incorrectly reconnected into the same trajectory. Consequently, some network connections, trajectory lifetimes, and merge/split statistics may depend to some extent on the selected threshold and search window. I also suggest noting in the conclusions or outlook that future versions of the data product could further evaluate parameter robustness through dedicated sensitivity experiments. This clarification would prevent readers from interpreting the current parameter combination as rigorously optimized or uniquely determined.

We thank the reviewer for this important remark. We agree that providing a rationale for parameter selection is not equivalent to demonstrating parameter robustness through dedicated sensitivity analyses. The 10\% IoU threshold and the 7-day search window were selected empirically based on previous experience with the META framework, existing tracking methodologies, and internal expertise. While these choices aim to balance trajectory continuity and the risk of incorrect associations, their influence on network topology, trajectory lifetimes, and merge/split statistics has not yet been systematically quantified through dedicated sensitivity experiments.

Following the reviewer's suggestion, we have added a statement in the manuscript explicitly acknowledging this limitation.

These tracking parameters were selected empirically based on extensive expert evaluation of trajectory continuity and network consistency. They build upon the methodology developed for previous versions of the META atlas Pegliasco et al. (2022), while being adjusted to improve the identification of merging and splitting events in the network framework. In particular, the temporal search window was extended from 5 days in META3.2 to 7 days in META4.0. Indeed, merging and splitting events are often associated with deformations of the eddy contours and rapid changes in their properties (see Section 3.3.3), which can increase the occurrence of temporary detection gaps. A longer temporal window therefore improves the reconnection of eddies belonging to the same network while remaining short enough to limit spurious associations. It should however be noted that the selected overlap threshold (10%) and temporal search window (7 days) are empirical choices and their influence on the resulting network statistics has not yet been systematically evaluated through dedicated sensitivity analyses. While the extended search window helps reduce trajectory fragmentation caused by temporary detection gaps, it may also increase the likelihood of reconnecting unrelated eddy structures, particularly in regions characterized by high eddy density, mapping uncertainties, or short-lived spurious detections. Consequently, some aspects of the reconstructed network topology, trajectory lifetimes, and merging/splitting statistics may depend on the selected parameter values.

It should be added that the complete workflow is implemented in the open-source `pyeddytracker` library, which allows users to modify the tracking parameters and generate customized eddy trajectories, networks, and associated statistics according to their specific applications.

2. The current tracking method remains partly empirical and subjective; its limitations should be stated more explicitly, and more objective Lagrangian tracking criteria should be prioritized in future work

I maintain my previous concern regarding the objectivity of the tracking method. The construction of META4.0 networks still relies primarily on geometric overlap of eddy contours, empirical thresholds, and trajectory reconnection within a finite temporal window. The revised manuscript now explains the origin and rationale of these rules more clearly and adds particle-advection experiments as a test of Lagrangian consistency, which are valuable improvements. At the same time, the authors correctly clarify that the particle-advection experiments are performed after the network has been constructed; their purpose is to evaluate the consistency between the identified networks and transport characteristics, rather than to perform independent tracking using Lagrangian criteria. From a methodological perspective, therefore, the identification of merge/split events still relies mainly on Eulerian geometric continuity and empirical rules rather than directly on constraints of material transport continuity or Lagrangian coherence. For some complex eddy interaction processes, contour overlap does not necessarily imply a physically meaningful merging or splitting event. I recommend that the authors make this limitation more explicit in the methodological discussion and conclusions, and avoid presenting the current geometric-overlap tracking results as a unique or fully objective identification of all physical merge/split processes. For future development of META data products, I still strongly encourage the authors to consider more objective tracking methods based on particle advection or other Lagrangian criteria for independent eddy tracking, or at least to use such approaches as a validation framework parallel to the current geometric-overlap method. For example, in representative ocean regions, eddy predecessors, successors, and merge/split relationships could be determined using particle-transport continuity, Lagrangian coherent structures, or particle-trajectory similarity, and then compared with the current META4.0 results. The studies of Jones-Kellett et al. (2024), Tian et al. (2025), and Tian et al. (2022) provide relevant methodological references. Because such analyses would involve a new tracking framework and substantial computational effort, I consider them more appropriate as a priority for future versions of the data product or for a dedicated study, rather than as additional analyses that must be completed before acceptance of the present manuscript. Overall, the revisions have satisfactorily addressed the principal issues raised in the previous round of review. The two points above are intended primarily to better delimit the applicability of the method and to ensure that the limitations of the data product are described accurately; they do not undermine the main results or the publication value of the dataset. I therefore recommend that the final version explicitly state that parameter robustness has not yet been systematically validated and that the current tracking method retains a degree of empiricism, while identifying more objective tracking approaches based on particle advection or other Lagrangian criteria as an important direction for future improvement of the data product. Subject to these technical textual corrections, I recommend acceptance of the manuscript for publication. Sincerely, Reviewer

We thank the reviewer for this comment. We agree that the identification of merging and splitting events in META4.0 relies on Eulerian geometric criteria. As a consequence, the reconstructed networks should not be interpreted as a unique or exhaustive description of all physical eddy interaction processes. Rather, the objective of META4.0 is to provide a consistent and reproducible framework for identifying and analysing potential eddy interactions at the global scale using the information directly available from satellite altimetry. The particle-advection experiments were therefore introduced as an assessment of the consistency between the reconstructed networks and Lagrangian transport characteristics, rather than as an independent tracking framework. We consider such approaches to be complementary to the present Eulerian framework rather than direct replacements, and agree that they represent promising directions for future validation of the META dataset.

Following the reviewer's suggestion, we have strengthened the discussion of these methodological limitations in the Discussion and Conclusions section.

Nevertheless, some limitations of the present framework should be acknowledged. The reconstruction of eddy networks relies on Eulerian geometric criteria based on contour overlap and temporal continuity, combined with empirically selected tracking parameters. Consequently, the identified merging and splitting events should not be interpreted as a unique or exhaustive representation of all physical eddy interaction processes. While the Lagrangian diagnostics provide additional evidence for the physical consistency of the reconstructed networks, they are performed *a posteriori* and do not constitute an independent tracking framework.

Future developments will include the application of the detection and tracking methods to next-generation sea surface height products, including datasets benefiting from the SWOT mission and forthcoming high-resolution altimetric products. In parallel, colocations with temperature and salinity Argo float profiles will allow the reconstruction of the three-dimensional structure of eddies, extending previous efforts that combine altimetry with in situ observations to better characterize mesoscale dynamics. These complementary observations will enrich the current datasets and enable a more complete characterization of eddy dynamical and biogeochemical impacts.

Particular attention will be devoted to the development of quantitative validation approaches for merging and splitting events based on independent observations, including biogeochemical tracers and in situ measurements. Such analyses will help confirm the physical realism of reconstructed eddy interactions and further improve the characterization of mesoscale connectivity. Complementary transport-based and Lagrangian diagnostics will also be investigated as additional tools to evaluate the consistency of reconstructed eddy interactions and to compare alternative representations of eddy connectivity.

Response to report #2

In this paper the authors present META4.0 the latest version of the META database suite for eddy detection from altimetry. The main novelty is that it accounts for the merging and splitting of eddies to build networks of interconnected eddies and the authors also provide a set of tools to analyse these networks.

I would first like to congratulate the authors on the effort they have made to address my previous comments as well as those of the other reviewers. The reviewing process is time-consuming and demanding so it is particularly appreciated when authors take the reviewers' comments seriously and make substantial efforts to improve the manuscript.

The paper is now much more readable and the technical sections and description of the data are much clearer. I understand the authors' choice to put more emphasis on their diagnostic tools and I have no particular concern about this.

One minor point regarding the presentation is that the quality of some of the figures is not optimal. However I believe this is understandable since I assume that these figures are essentially the direct output of the diagnostic tools developed to analyse the dataset. To make this clearer to the reader an explicit reference to the corresponding toolbox/diagnostic for each figure where relevant could be useful (e.g. "Replay segmentation" for Figure 2). This is only a general suggestion and in my opinion is not a requirement for publication.

Apart from this point I have no further general comments on the manuscript which I believe is now suitable for publication. I list below three additional comments following the authors' responses to my previous review that I consider worth addressing. However both should be relatively straightforward to take into account and I believe they could potentially be addressed by the authors and validated by the editor without requiring another round of review.

We thank the reviewer for its careful reading and time spent on the manuscript. Here are some responses elements for the three comments:

1. The Ierapetra example => Thank you for adding the Ierapetra example to the diagnostic section. I realise that my previous comment may not have been sufficiently clear. My point was that the Ierapetra anticyclonic mesoscale eddy is a rather persistent feature that remains most of the time in approximately the same location and therefore leaves a strong signature in the MDT field. As a consequence when the eddy temporarily drifts southward from its usual position, its temporary absence from its usual location can produce a negative anomaly in the SLA field. This anomaly may then be detected as a cyclonic eddy even though it actually results from the temporary displacement of an anticyclonic eddy. This distinction was the main point I wanted to highlight with the Ierapetra example.

We thank the reviewer for this clarification, and agree that this point deserves an explicit response.

The reviewer is correct that when eddies are identified from SLA, a quasi-permanent feature such as the Ierapetra anticyclone can produce an artificial cyclonic signal when the eddy temporarily shifts away from its usual position. This specific limitation of detection based on the SLA, with Ierapetra as the reference case, has been documented by Pegliasco et al. (2021).

However, from META3.2 version, eddy detection is performed on ADT rather than SLA. Because the MDT already contains the mean signature of the Ierapetra Eddy, a temporary displacement of the anticyclone does not automatically produce a cyclonic structure at its usual location, as may occur in detection on SLA field. We therefore believe that the specific sign-reversal mechanism described by the reviewer is mitigated when using ADT. Nevertheless, we cannot completely exclude that such situations may occasionally occur, and we will keep this point in mind for future investigations of the dataset.

2. Reference to Chelton et al. (2011) and the Okubo–Weiss parameter => You have kept the following sentence: "Unlike early approaches such as Chelton et al. (2011), which relied on the Okubo–Weiss parameter to isolate rotational structures from a non-rotative background, spatial filtering to the ADT fields is no longer applied." When I read this sentence I understand it as stating that Chelton et al. (2011) relied on the Okubo–Weiss parameter. However I checked Chelton et al. (2011) again and their paper explicitly states that their new eddy identification procedure was based on SSH and was designed to avoid the problems associated with the Okubo–Weiss approach: "A new eddy identification procedure was developed for this study. As described in Appendix B.2, this procedure is based on defining the eddies in terms of SSH, thus obviating the need to differentiate the SSH fields and avoiding the associated deleterious effects of noise that are problematic in the procedure based on products of second derivatives of SSH in the Okubo-Weiss parameter (see Appendix B.1) that has been used in numerous previous studies, including Chelton et al. (2007)." I therefore suggest either replacing "Chelton et al. (2011)" with "Chelton et al. (2007)" in this sentence or slightly rephrasing the sentence to make the distinction clear. If you do not agree with this interpretation please clarify the intended meaning of the sentence.

We thank the reviewer for pointing this out. We made the following modification:

> Unlike early approaches such as ~~(Chelton et al, 2011)~~ Chelton et al. (2007), which relied on the Okubo–Weiss parameter to isolate rotational structures from a non-rotative background, spatial filtering to the ADT fields is no longer applied. Filtering was originally introduced to reduce large-scale gradients and noise that affected vorticity-based methods. However, the contour-based approach adopted here is inherently less sensitive to background flow variations, as it directly identifies closed SSH

3. Tracking and splitting events => Regarding my previous question about the tracking procedure, I understand your explanation that the algorithm searches sequentially from day +1 to day +7 and stops as soon as a valid association is found. I would nevertheless like to mention one point based on my own experience when working on splitting events in eddy detection. I encountered cases where a large eddy E0 was associated with a smaller eddy E1 at dt+1 while another smaller eddy E2 could appear at dt+X (1 < X =<7) and also satisfy the corresponding overlap criterion. If I understand correctly, E2 would not be identified as the result of a merging event using your method. To address this issue I performed a second pass in which I searched backwards for a parent eddy for eddies that did not have one in the initial pass (e.. backward tracking of E2). I am not suggesting that this necessarily needs to be implemented in the current version of META4.0. However in my experience this additional pass was not particularly time-consuming and was quite efficient at identifying such cases. It might therefore be a relatively inexpensive feature for the authors to test and potentially consider for a future release.

We thank the reviewer for this question. We realize that our previous answer may have been misleading because we were referring to the single-trajectory tracking procedure rather than to the networking procedure used in META4.0...

In the networking procedure, the algorithm does not stop when it finds the first valid association. For each observation at day *i*, all observations in the subsequent time window (e.g., *i+1* to *i+7*) are indeed considered. Candidate pairs are first identified using a bounding-box intersection, and the actual contour overlap is then computed only for these candidates. All pairs satisfying the overlap criterion are retained, rather than stopping at the first valid match.

Therefore, in the example you describe, if a large eddy E0 at *t* overlaps with E1 at *t+1* and with E2 at *t+X*, with $1 < X \leq 7$, both associations E0–E1 and E0–E2 are retained during the grouping step. The subsequent segmentation of the resulting network can therefore identify the corresponding splitting event (1 eddy leads to 2 eddies in the following days). In other words, E2 is not forgotten simply because

E1 was already associated with E0. Consequently, we agree that a backward-search pass could be an interesting alternative or additional strategy in a different tracking framework, thank you for the suggestion. However, for the current META4.0 networking approach, we do not think it is necessary to recover the type of case you describe, since all valid forward associations within the networking window are already retained and the subsequent network segmentation explicitly handles branching events.

Regarding the computational cost question which was part of your first comment: although the networking step examines all temporal pairs within the specified window, it does not perform the computationally more expensive polygon-overlap calculation for every possible pair of eddies. A bounding-box intersection is first used to identify geometrically possible candidates, and the detailed contour-overlap calculation is performed only for these candidates. The observations are also buffered to avoid repeatedly loading the same files. Thus the procedure remains largely computationally manageable. Please note that the networking code is fully open source. If you would like more details regarding the implementation of this procedure or specific coding choices, the corresponding routines can be found in py-eddy-tracker/src/py_eddy_tracker/observations/network.py at master · AntSimi/py-eddy-tracker · GitHub. Alternatively, please do not hesitate to contact us directly.

We apologize for the confusion in our previous response, which resulted from conflating the single-trajectory overlap tracking procedure with the new networking procedure.

Thank you again for carefully considering my previous comments and congratulations to the authors on this work.

Sincerely,

Rémi Laxenaire

Sincerely,

The authors