We thank the Editor and the three reviewers for their careful and constructive evaluation of our manuscript. We have addressed all comments thoroughly, which has led to a significantly improved and clarified version of the paper. Importantly, while the presentation and methodological details have been strengthened, the main results and conclusions remain unchanged. One notable addition is the extension of the analysis to two years of data, which further reinforces the robustness of the results. Answers to the reviewer comments are written **in blue** in the text.

First, we mention here that during the revision process, we chose to repeat the analysis using the recently released v3.0 of SWOT data instead of v2.0.1. We find that v3 improves eddy detection, notably by reducing spurious small eddies and yielding a more balanced distribution between cyclonic and anticyclonic structures, likely due to improved noise filtering. This update further increases the reliability of the study, while not affecting its main conclusions. A detailed comparison between data versions is beyond the scope of the present work. We note that the eddy detection database will be updated accordingly as future data versions become available.

Before addressing each comment separately, we give a common answer to both reviewer 1 and 3 about the inpainting method, as their comments address closely related concerns.

R1 comment: The method of filling using "biharmonic inpainting" seems to be uncertain to me. It is not well explained in the text but looking at Fig. 1 this "filling" is an extrapolation over a distance of ~50% of the original width of the satellite swath. I assume that this introduces a lot of uncertainty even though only eddies that are mostly outside of those extrapolated regions are considered. I strongly suggest the authors do some sensitivity testing with different extrapolation methods, or present results if testing was already done. Even better would be to take a dummy dataset, i.e. a gridded SSH dataset or model output, subsample some artificial SWOT swaths and then detect eddies in those and compare to an eddy detection performed on the original complete, gridded dataset.

R3 comment: The choice of the biharmonic inpainting method is critical, as it directly affects the reconstructed height fields and therefore the eddy detection.

Please justify the choice of this specific method, indicate whether alternative inpainting approaches were tested and discuss its potential impact on eddy properties (e.g., amplitude, radius, detection rate).

Indeed, this gap filling process may induce many erroneous closed contours, especially at the edges of the swaths where the field is extrapolated.

The inpainting method (ideally) or the eddies concerned by this bias should be validated. At least, this should be discussed in the article.

Several approaches can be considered to reconstruct missing sea surface height fields, ranging from interpolation-based methods to more advanced techniques such as variational approaches or machine learning models (e.g., neural networks such as U-Nets). In particular, data-driven methods have recently shown promising results for reconstructing complex ocean structures, for example the approach of Lenain et al. (2026) to infer velocity fields from SST, or U-Net architectures used to filter non-balanced signals in KaRIn data. Such approaches could, in principle, also be applied to gap filling.

In this study, however, we deliberately chose a biharmonic inpainting method, which relies on a well-defined physical constraint and produces a smooth and dynamically consistent reconstruction of the field (as a purely Stokes Flow). This choice was motivated by the need for a method that is robust, interpretable, and does not rely on training data or prior assumptions about the eddy field.

We acknowledge that this aspect of the methodology is critical. <u>Following the reviewers' remark, we have therefore added a dedicated validation and detailed description of the inpainting procedure in an Appendix, to better support the robustness of the approach.</u>

In summary, to validate the methodology, we used outputs from the GIGATL1 simulation (1 km horizontal resolution, 100 vertical levels), which fully resolves the mesoscale dynamics in the subpolar Atlantic, including in regions where the first baroclinic Rossby radius is O(10–15 km) (de Marez et al., 2025). We chose the same areas as in our study, the Labrador Sea.

The method is as follows. The SSH field (from the simulation) was first interpolated onto a 2-km grid to match SWOT's effective resolution. From these, we extracted N × 128 domain, which we refer to as the "*truth*" reference field. We then constructed synthetic SWOT-like swaths by introducing gaps corresponding to the nadir region and the swath edges, reproducing the exact data geometry encountered in the real SWOT observations. This produced datasets with the same grid and missing-data structure as those used in our detection method. These gapped fields were subsequently filled using the same biharmonic inpainting procedure as in the manuscript, yielding what we refer to as the "*swotlike*" fields. Eddy detection was then applied independently to the truth and swotlike fields. This procedure was repeated weekly over one year for the two synthetic passes, allowing a statistical comparison of eddy properties derived from complete versus inpainted data.

The comparison shows that the distributions of eddy amplitude and radius obtained from the truth and swotlike datasets are very similar. For the individual passes where detections do not perfectly coincide, the misdetection rate remains below ~5% over the entire swath. Therefore, over the annual sampling, the statistical distributions of eddy properties are not significantly altered by the inpainting procedure.

A trend nevertheless seems to appear: discrepancies (difference of the number of detected eddies using the two arrays) increase with eddy radius. This behavior is physically expected. Larger eddies have a broader spatial imprint and therefore intersect the gapped regions more frequently, making their reconstruction more sensitive to the inpainting. In contrast, eddies with radii smaller than ~15 km (i.e. diameters smaller than approximately half the swath width) remain mostly constrained by observed data and are only weakly affected by the extrapolation.

These additional diagnostics demonstrate that the biharmonic inpainting does not significantly bias the eddy statistics in the size range primarily analyzed in this study (O(15 km) radius). The method is robust for eddies whose characteristic scale is smaller than half the swath width, which corresponds precisely to the population of eddies that SWOT allows us to observe and that are the focus of this paper. We noted the limitations of our method to eddies with radius smaller than ~20 km in the methods section, see lines 141-144.

Reviewer #1

Review of "High-Latitude Eddy Statistics from SWOT assessed by in situ observations" by de Marez et al.

The authors use ungridded, along-track satellite altimetry data from SWOT to detect mesoscale eddies in the Labrador Sea and compare individual eddies and eddy statistics compiled over a full year to eddies detected from in-situ, shipboard ADCP measurements, as well as gridded, lower resolution altimetry data. The study confirms existing knowledge about various types of mesoscale eddies in the Labrador Sea but a longer study period would be necessary to really assess this with confidence. However, the main advancement of the study is the methodology of detecting eddies from along-track satellite altimetry, allowing to detect smaller features than from gridded altimetry products. Both, this new detection method and its application to SWOT data make this study a significant contribution to our knowledge of mesoscale eddies.

The manuscript is well structured and written and I only have a few, relatively minor comments.

General comments

1. In the introduction as well as the discussion, the authors highlight SWOT's potential ability to investigate eddies in "ice-influenced" regions or "seasonally ice covered zones where altimetric data were previously unusable". However, their analysis excludes eddies in areas with >15% sea ice cover, so that it seems that this aforementioned potential is purely hypothetic. I do not see the value in emphasizing this unless the authors show how a seasonal ice cover can inhibit eddy detection in lower resolution altmitry data even at times when no ice is present.

We thank the reviewer for this important remark and agree that the original wording could be misleading.

First, we clarify that by *ice-influenced regions* we refer to areas that are ice-free at the time of observation but located in the vicinity of the marginal ice zone (MIZ), where freshwater input and stratification associated with sea-ice melt can still significantly impact mesoscale dynamics. Similarly, *seasonally ice-covered regions* include areas that are ice-free during part of the year but remain dynamically influenced by prior sea-ice presence. For instance, regions north of Iceland (outside our study domain) exhibit strong modulation of the eddy field by freshwater input from sea-ice melt, as discussed in de Marez et al. 2025 (doi10.1029/2025JC022664). In our study area, some regions may also be indirectly influenced by such processes, although we do not investigate these mechanisms in detail here.

Second, we acknowledge that our current analysis excludes regions with more than 15% sea ice concentration and therefore does not directly demonstrate SWOT's capability in partially ice-covered conditions. Investigating eddy detection in such environments—potentially leveraging the sea-ice mask provided with SWOT data—is a natural extension of this work and is part of ongoing and future studies.

We agree that emphasizing this potential could be overstated in the context of the present analysis. To address the reviewer's concern, we have added a clarifying sentence in the Introduction to explicitly state this point and avoid ambiguity. "Furthermore, even in the absence of sea-ice, regions located near the MIZ remain dynamically influenced by freshwater input and stratification associated with ice melt \citep{de2025mesoscale}." see lines 41-42.

2. The method of filling using "biharmonic inpainting" seems to be uncertain to me. It is not well explained in the text but looking at Fig. 1 this "filling" is an extrapolation over a distance of ~50% of the original width of the satellite swath. I assume that this introduces a lot of uncertainty even though only eddies that are mostly outside of those extrapolated regions are considered. I strongly suggest the authors do some sensitivty testing with different extrapolation methods, or present results if testing was already done. Even better would be to take a dummy dataset, i.e. a gridded SSH dataset or model output, subsample some artificial SWOT swaths and then detect eddies in those and compare to an eddy detection performed on the original complete, gridded dataset.

See our common answer to R1 and R3 about this specific point and revised appendix.

Specific Comments

Title: This might be irrelevant as I am not a native english speaker but "assessed by in situ observations" seems to not convey what the authors want to say. I suggest replacing by "compared to in situ observations" or something similar. We modified the title following the reviewer suggestion.

l. 2: I suggest adding "in the ocean" after "momentum" in the introductory sentence. ok ll. 19-20: I find this slightly misleading as the automatic detection of eddies can be performed on mooring or ADCP data etc. What has been made possible through the gridded products is global detection of eddies. We modified the sentence and now refer to "global automatic detection".

ll. 23-24: I suggest rephrasing to "The ability of gridded SLA fields to represent the eddy field has previously been questioned, as they have largely distorted eddy characteristics" unless this distorts the intended meaning. ok

ll. 33-41: The authors mention two environments where eddies are abundant, they then go on to describe the processes in the MIZ in detail but do not describe anything about the boundary currents. I strongly suggest to add some information on what the eddies do in boundary currents. We added details on eddies generated by boundary currents. "In the boundary currents, mesoscale eddies are frequently generated through barotropic/baroclinic instability and can detach as coherent vortices---such as Irminger Rings from the West Greenland Current\citep{de2014two}---playing a key role in the lateral transport of heat, freshwater, and biogeochemical tracers into the basin interior." see lines 42-44.

Also see my General comment 1. We addressed comment 1 above.

ll. 69-75: It is not clear to me how the reduction of overall energy does not have implications for the presented analysis. I do believe this is true but the authors should rephrase this part and include an explanation of why it is true. We agree the sentence was not clear. We modify the paragraph accordingly:
"This method allows extraction of most of the balanced signal from the full SSH field, but it also reduces the overall energy, which can complicate interpretation at small scales where signal levels approach the noise floor \citep[see \textit{e.g.,}][]{callies_expectations_2019}. This effect primarily impacts the high-wavenumber part of the spectrum, where reduced energy can become comparable to instrumental noise. However, this limitation does not affect our analysis, as we focus on mesoscale structures whose signal amplitude remains well above the noise level \citep{chelton2024postlaunch}. Recent studies have indeed demonstrated SWOT's ability to robustly resolve mesoscale variability that was previously undetected in gridded products[...]" see lines 75-84.

l. 99: If possible, include those datasets in the references. We added mention in the code availability that these datasets are accessible upon request to authors.

ll. 120-128: The explanation of the padding and filling is not clear to me from the text alone. I suggest the authors rephrase this in order to avoid that the readers have to switch back between the text and Fig. 1 to understand the process. We modified the text in the hope it is now clearer to the reviewer. "We use the native 2-km resolution SWOT swaths, which are 69 pixels wide in the across-track direction. To enable efficient processing, each swath is first extended by padding its lateral edges with missing values (step #1 in Fig.~\ref{fig1}a). The padded swath is then subdivided into overlapping 128×128 pixel windows, with an overlap of 64 pixels between adjacent tiles (step #2 in Fig.~\ref{fig1}a). Each tile therefore contains the original data along with regions of missing values (NaNs), including the nadir gap. These gaps are then filled independently within each tile using a biharmonic inpainting method, resulting in complete 128×128 SLA images (step #3 in Fig.~\ref{fig1}a). Specifically, we use the \texttt{inpaint_biharmonic} function from the \texttt{skimage.restoration} Python module, which reconstructs missing values by solving the biharmonic equation ($\nabla^4 u = 0$) inside the gaps." see lines 133-137.

ll. 123-128: See General comment 2. We addressed general comment 2 above.

l. 168: Why was the SWOT-detected eddy from June 27 used while the one from June 25 would be closer in time to the ins situ-detected eddy on June 24? Is there an automatic algorithm that decides which of the duplicate detections to keep or is this chosen manually? I suggest the authors describe this process.

The selection of the eddy detected on June 27 instead of June 25 is not based on proximity to the *in situ* observation, but results from our automated duplicate removal procedure. In that sense, the choice between the two detections is effectively arbitrary and does not reflect a manual selection.

Duplicates are removed using a physically based criterion (see also our response to Reviewer 3, comment #7): for each SWOT cycle, eddies whose centers are separated by less than their radius of maximum velocity Rmax and a fixed threshold of 10 km are considered duplicates. This threshold corresponds to the typical displacement of an eddy over one SWOT repeat cycle due to Rossby wave propagation.

When duplicates are identified, one eddy is retained without preferential selection (based on the order of passes within the cycle), ensuring that no systematic bias is introduced in the retained population. We have clarified this procedure in the revised manuscript to make the selection process fully explicit, see lines 165-169.

ll. 182-183: Are the "shallow eddies" here the ones that are detected in areas where the water depth is < 2000 m? If this is the case I suggest the authors rephrase this, as "shallow eddies detected above depth < 2000 m" gives the impression that the eddies themselves are above 2000 m in the water column. We remove the word "shallow" to avoid misunderstandings.

l. 188: Global datasets of mesoscale eddies (and their statistics) are available from lower resolution altimetry data, e.g. Ioannou et al. (2024), extending to similarly high latitudes. While the higher resolution of SWOT certainly makes the presented statistics more valuable, it is not the first time that those statistics are derived. We totally agree with the reviewer, and modified that sentence accordingly : "This provides a spatial statistical description of the regional mesoscale activity at such high latitude at an unprecedented resolution, significantly extending previous estimates derived from lower-resolution altimetry products."

ll. 198-199: I suggest the authors add a reference that supports their statement that "AEs are less susceptible to steering by background currents and less prone to instability-driven decay". We added reference to four classical articles that describe the asymmetry between C/A in geophysical flows (Polvani et al., 1994; Arai and Yamagata, 1994;Koszalka et al., 2009; Roullet and Klein, 2010).

ll. 204-205: The fact that the eddies are stronger near the slopes might also reflect the fact that those eddies are generated in those regions and are necessarily weaker away from them as they decay. We agree and modified the sentences accordingly: “Along the continental slopes, both cyclonic and anticyclonic eddies exhibit higher maximum velocities and elevated Rossby numbers. The enhanced intensity near the slopes likely reflects both active generation processes and stronger strain and frictional interactions with the underlying topography.” see lines 257-259.

Fig. 4: I suggest the authors use a plotting algorithm without interpolation such that the 2x1 degree boxes are visually identifiable on the maps. We modified Fig. 4 accordingly.

ll. 209-220: I suspect that the western and eastern regions got mixed up here as the West Greenland current is not in the western region and the eastern region is not the one with the weakest eddies. I suggest the authors check this and also make sure that the western and eastern region are correctly attributed to the columns in Fig. 5. We believe the confusion comes from the fact that we call West the Western Labrador Sea, while the West Greenland Current is actually on the Eastern side of the Labrador Sea, see Fig. 1 in Pacini et al. (2022, DOI:10.1175/JPO-D-20-0255.1).

ll. 214-215: Does the average westward propagation speed of Irminger Rings support the connection between a winter maximum of their generation in the east and the presented spring-autumn maximum in the central region? Unfortunately, using the present methodology we cannot perform eddy tracking and therefore we cannot infer propagation speed.

l. 227: I suggest removing "(surface-intensified)" as not all the types of eddies mentioned are surface-intensified (convective lenses). ok

l. 250: Not all datasets used in this study are mentioned here. If the code for the detection from SWOT data is available this should be added here as it is certainly of great interest to the community. We agree, and we will make this tool available as an available toolbox online soon, after the review process and feedback from the community.

References: In general, I suggest the authors add DOIs to all references. We leave this consideration to the publisher’s discretion.

Minor Comments

We corrected the minor comments accordingly.

l. 25 add a comma after "to be resolved, ..."

l. 34 "sea-ice melt generates"

l. 68: "preserving the balanced"

l. 86: is a duplicate of line 84

l. 99: "Other datasets"

l. 106: some formatting issue with "no—KaRIn—measurements"

l. 115: "MIOST" has not been introduced. MIOST is the dataset used in the paper cited in brackets, we clarified the sentence.

l. 149: I suggest replacing "in front of" with "using"

l. 160: "tracks"

l. 232: "detection"

l. 284: Use the final published version.

l. 341: citation has no journal

l. 346: Use the final published version.

References

Ioannou, A., Guez, L., Laxenaire, R., & Speich, S. (2024). Global Assessment of Mesoscale Eddies with TOEddies: Comparison Between Multiple Datasets and Colocation with In Situ Measurements. Remote Sensing, 16(22), 4336. https://doi.org/10.3390/rs16224336

Reviewer #2

Eddies are smaller in high latitudes due to the change of the deformation radius. The eddies in these latitudes are, however, important influencing deep-water formation, carbon uptake, and sea-ice melt. The manuscript shows that the new SWOT satellite enables robust, quantitative eddy detection in polar and subpolar oceans. I find the manuscript scientifically sound and well written. Only a few minor correction seems needed before publication.

(relatively major) comments

In Section 3.1 (methodology), the geostrophic balance is implicitly assumed. In scales much smaller than the deformation radius, it is not obvious that the velocity field is dominated by geostrophic flows. A short discussion to justify the geostrophic balance at these scales might be needed.

We thank the reviewer for this relevant comment. The assumption of geostrophic balance is indeed more robust at scales larger than the local deformation radius, and its validity can become less straightforward at smaller scales. However, mesoscale eddies—such as those considered in this study—are generally well approximated as geostrophic structures, although some ageostrophic contributions may exist, particularly for eddies with high Rossby number.

As shown in Ioannou et al. 2019, ageostrophic (cyclostrophic) corrections can be significant in some cases, especially for intense anticyclones, but the geostrophic approximation remains a robust first-order description for most mesoscale eddies. In this study, we therefore adopt this approximation as a first step, consistent with standard practices in altimetry-based eddy detection.

We acknowledge that this represents a limitation, and more advanced processing of SWOT sea surface height data is currently ongoing. In particular, we plan to improve the methodology by starting from the unfiltered SLA, extracting the balanced signal following the approaches of Zhang et al. 2025 and Skinner et al. 2026, and then applying cyclostrophic corrections before performing eddy detection. This will allow us to extend the methodology toward submesoscale dynamics, particularly in tropical regions. However, for the mesoscale eddies considered here, we expect the impact of these corrections to be limited.

We added the following sentence to the manuscript to discuss this point.

"Eddies are assumed to be predominantly geostrophic, and although ageostrophic (cyclostrophic) corrections may become relevant for the most intense structures \citep[\textit{e.g.,}][]{ioannou2019cyclostrophic}, they are neglected here as a first-order approximation, following standard practice for mesoscale structures in altimetric data." see lines 149-151.

L.198, AE/CE asymmetry is mentioned here. Fig.3 of Chelton et al. (2011, https://doi.org/10.1016/j.pocean.2011.01.002) indicates that the asymmetry shows up after travelling > 2000 km. Is this the situation in the Labrador Sea (not obvious from Fig.4)? Or do you have any better reference? Later at L.211, it seems a local generation along the W.Greenland Current is suggested. Does this agree with the AE/CE asymmetry?

We agree that in Chelton et al. 2011 the asymmetry between anticyclonic and cyclonic eddies is largely attributed to long-distance propagation (>2000 km), which may not directly apply to the Labrador Sea where eddy lifetimes and propagation distances are more limited. In our case, we interpret the observed AE/CE asymmetry primarily as the result of the intrinsic asymmetry between cyclones and anticyclones in geophysical flows, rather than long-range propagation effects. To support this interpretation, we have now added several references (Polvani et al., 1994; Arai and Yamagata, 1994; Koszalka et al., 2009; Roullet and Klein, 2010), that highlight such asymmetries can emerge from local geophysical processes, independently of long-distance propagation.

minor comments

L.95 "from classical a" → from a classical ok

L.177 and Fig.3e, How is the Rossby number defined here? We use the standard definition for vortices R0=V/fR. We defined it in the caption of Fig. 3.

Fig.2 caption, 3 lines from the bottom. The subscript 1 and 4 are connected by a strange character. We corrected this.

L.250, the website shown at "data availability" section is not a link to the data. Maybe this one? https://doi.org/10.24400/527896/A01-2023.017. We corrected the data availability section.

Reviewer #3

General assessment

This manuscript presents a study using SWOT swath data to characterize mesoscale eddies in the Labrador Sea. The use of native-resolution SWOT observations combined with in situ SADCP data provides a promising framework to investigate mesoscale dynamics at high latitudes, where conventional altimetry has long been limited.

However, several aspects of the methodology, validation strategy, and interpretation of the results require clarification and strengthening.

Major comments

We thank the reviewer for these detailed comments, many of which relate to the eddy detection methodology itself. We acknowledge that several aspects of the implementation needed further explanation, and that some validations of the choices made were needed.

We want to mention that the primary objective of this study was not to develop or fine-tune a specific detection algorithm, but rather to apply an efficient and established framework to exploit the unprecedented resolution of SWOT observations. In that context, we deliberately relied on *pyeddytracker* with standard parameter choices, ensuring robustness, reproducibility, and consistency with existing studies.

We also emphasize that the workflow is designed to be modular: eddy detection is performed on independent 128x128 grids, making it straightforward to implement alternative detection methods or parameterizations if desired (which is part of on-going collaborations). This flexibility motivated our choice not to rely systematically on all built-in strategies, as the focus was on maintaining a simple and transparent framework.

That being said, we have nevertheless addressed all comments very carefully in the revised manuscript, and the reviewer can find our answers below.

1. Detection on SLA field

It has been previously assessed that the detection should preferably be done on ADT field rather than SLA field as in *Pegliasco, C., Chaigneau, A., Morrow, R., & Dumas, F. (2021). Detection and tracking of mesoscale eddies in the Mediterranean Sea: A comparison between the Sea Level Anomaly and the Absolute Dynamic Topography fields. Advances in Space Research, 68(2), 401-419.* Most recent global eddies atlas perform the detection on the ADT field as in ToEddies (*Ioannou, A., Guez, L., Laxenaire, R., & Speich, S. (2024). Global assessment of mesoscale eddies with TOEddies: comparison between multiple datasets and colocation with in situ measurements. Remote Sensing, 16(22), 4336.*) or META atlases

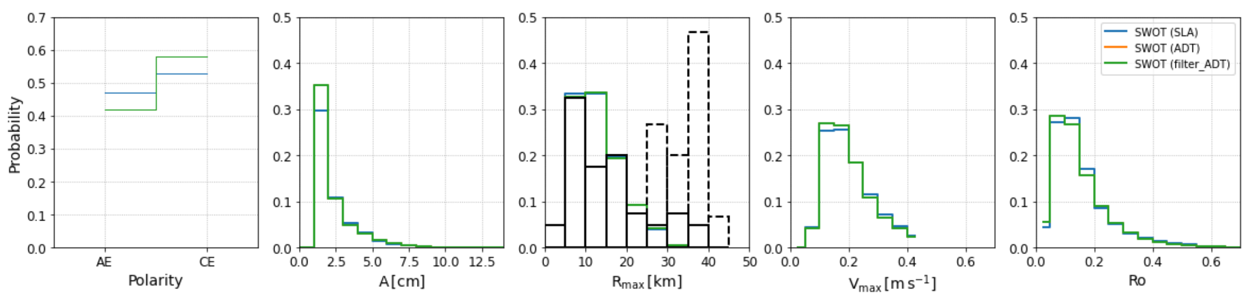
(*Gamot, J., Delepoulle, A., Nencioli, F., Pujol, M. I., & Dibarboure, G. (2026). META4. 0: a new mesoscale eddy network atlas derived from altimetry. Earth System Science Data Discussions, 2026, 1-31*). This can, for example, explain the AE/CE asymmetry.

I strongly recommend performing the detection on the ADT field as prescribed by previous studies.

We thank the reviewer for this important comment. We acknowledge that several studies recommend performing eddy detection on Absolute Dynamic Topography (ADT) rather than Sea Level Anomaly (SLA), as discussed in Pegliasco et al. 2021 and adopted in recent atlases such as Ioannou et al. 2024.

In our study, SLA was initially chosen both for clarity in visualizing the eddy field and for consistency with previous work (e.g., Amores et al., 2018). We recognize that this choice may introduce biases, particularly in regions where the mean dynamic topography (MDT) contributes significantly to the signal.

To assess this, we repeated the full two-year detection using both SLA and ADT. We find that <u>the resulting statistics are very similar (see histograms below), which is the primary reason we retain SLA in this study.</u> Differences are mainly observed for smaller eddies and in regions close to boundary currents, where ADT tends to yield slightly more cyclonic detections. This behavior is consistent with the fact that, in the central Labrador Sea, MDT exhibits very weak “eddy-like” signals (on the order of 1–5 cm s^{-1}), so that most of the eddy variability is already captured in SLA, while differences are more pronounced in energetic boundary currents.



A more fundamental limitation arises from the use of swath-based data. Proper use of ADT requires removing the large-scale background signal, which is typically achieved through spatial filtering. (see e.g., *https://py-eddy-tracker.readthedocs.io/en/stable/python_module/02_eddy_identification/pet_sla_and_adt.html*). However, in our framework, the maximum resolvable scale is constrained by the swath size (here, about 250 km after padding), which limits the effectiveness of such filtering. In practice, this leads to suboptimal separation between large-scale and mesoscale signals when working directly on swath data, and therefore to less reliable ADT-based detections.

For these reasons, we consider that, in the present context, using SLA provides a more robust and consistent basis for eddy detection than applying ADT with imperfect large-scale filtering. While some misdetections may occur when using SLA, they are limited and do not affect the statistical results. A full comparison with gridded Level-4 ADT products (including SWOT) would be an interesting extension, but is beyond the scope of this study, which focuses on exploiting the native swath observations.

This choice and its implications have now been clarified in the revised manuscript at lines 181-184: “Detection is performed on SLA rather than Absolute Dynamic Topography (ADT) fields: while ADT is commonly used for eddy detection, tests performed here show very similar statistics between SLA and ADT. In the context of swath-based data the limited spatial extent prevents an effective removal of the large-scale signal required for robust ADT-based detection, making SLA a more consistent choice in this framework.”

2. Choice of reference altimetry product

The comparison with mapped altimetry relies on the CMEMS 1/4° product (as performed in Bendinger et al., 2025). However, more recent higher-resolution products (e.g., DUACS DT2024 1/8°) are available for this region. Given that eddy detectability is strongly resolution-dependent, this choice may artificially enhance the contrast between SWOT and CMEMS.

In addition, eddy detection strongly depends on the algorithm and parameter settings. The detection method applied to the CMEMS dataset should therefore be clearly specified, and the results interpreted in the light of these methodological choices.

Please justify the use of the 1/4° product, specify the exact CMEMS product and detection configuration used, and discuss whether using higher-resolution products (e.g., 1/8°) would affect the conclusions. The use of the last altimetry product should be considered in order to improve the quality of the comparisons.

The eddy detection applied to the CMEMS product follows exactly the methodology described in Bendinger et al. 2025 (Section 3e), using the AMEDA algorithm (Angular Momentum Eddy Detection and Tracking Algorithm; Le Vu et al. 2018). Importantly, in the present study, we did not perform the detection ourselves but directly used the eddy dataset produced in Bendinger et al. 2025. See lines 103-104.

Regarding the choice of the 1/4° CMEMS product, we acknowledge that higher-resolution products such as the DUACS DT2024 1/8° fields are now available and could improve the representation of smaller-scale structures. However, we did not rerun the detection on these newer products, as the objective here is not to provide an exhaustive intercomparison between altimetry products, but rather to contrast SWOT observations with a widely used, standard mapped altimetry reference. The methodology and configuration used in Bendinger et al. 2025 were considered sufficiently robust for this purpose.

Importantly, given the typical size of the eddies analyzed in this study, we do not expect that increasing the resolution from 1/4° to 1/8° would qualitatively alter the main conclusions, although it would likely improve the detection of smaller and weaker structures and thus refine the comparison. We discuss this point in the methods section, see lines 106-108.

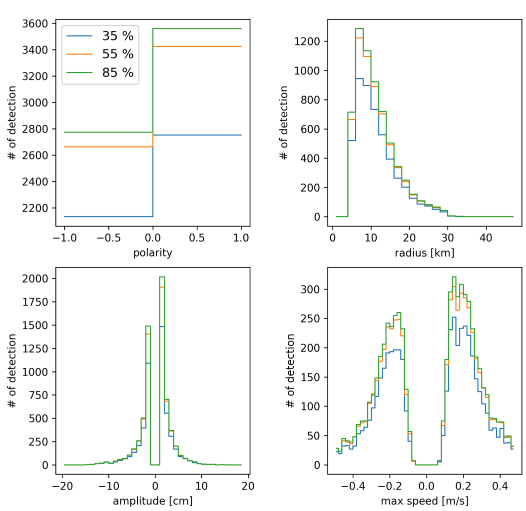
3. Missing key parameter: shape error

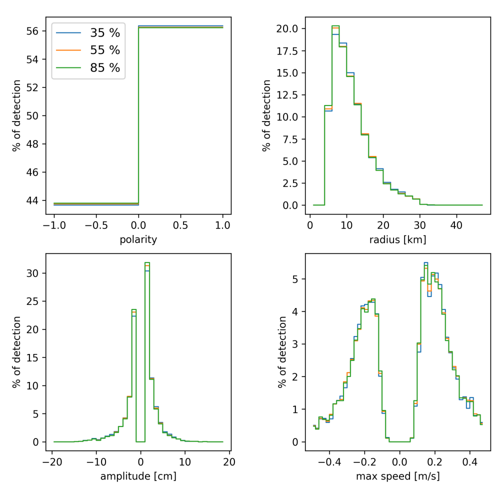
The shape error parameter in pyeddytracker is not documented. This parameter strongly controls the acceptable deviation from circularity, and the sensitivity of detection to noise and interpolation artifacts.

Please provide and justify the value used, and indicate whether sensitivity tests to this parameter were performed. This would enhance the reproducibility of the method.

We agree with the reviewer that this parameter indeed plays a key role in the eddy detection algorithm. We performed before the initial submission a dedicated sensitivity analysis based on the 2024 dataset, exploring a range of shape error thresholds (35%, 55%, and 85%). Decreasing the shape error parameter leads to a reduction in the total number of detected eddies (4884, 6087, and 6331 detections for 35%, 55%, and 85%, respectively), reflecting the stricter constraint on eddy geometry.

However, the key result of this analysis was that while the **absolute number of detections varies**, the **normalized statistical distributions** (i.e., distributions expressed as a percentage of the total number of detections) **remain remarkably consistent across all tested values** (see new figures). This indicates that the shape error parameter primarily affects the detection count, but does not significantly bias the underlying statistical properties of the eddy population.





We therefore concluded that (i) higher shape error thresholds may introduce additional detections, likely corresponding to more irregular but still physically meaningful structures, and (ii) the main statistical results presented in the manuscript are robust to the choice of this parameter.

Following the reviewer's suggestion, this sensitivity analysis and its implications have now been discussed in the method section, see lines 145-148.

4. Value of the amplitude threshold

The amplitude threshold parameter in the detection procedure is set to 1 cm, while in other and **global** eddy atlases it is set to 0.4 cm (META), 0.25 cm (TIAN) and 0.1 cm (ToEddies) (as reported for instance in "Ioannou, A.; Guez, L.; Laxenaire, R.; Speich, S. Global Assessment of Mesoscale Eddies with TOEddies: Comparison Between Multiple Datasets and Colocation with In Situ Measurements. Remote Sens. **2024**, 16, 4336. https://doi.org/10.3390/rs16224336"). For submesoscale eddies, this amplitude threshold appears to be a bit high and may affect the detection rate.

I recommend using an amplitude threshold closer than the ones used in other atlases (1 to a few millimeters appears to be more appropriate for high-latitude eddies).

In this study, we chose a relatively conservative value of 1 cm, primarily to limit the risk of spurious detections associated with potential noise in the SWOT sea surface height fields. At the time of the analysis, uncertainties on the effective noise level at the smallest resolved scales led us to favor robustness over exhaustivity.

We agree that this threshold is higher than those used in existing eddy atlases and may therefore exclude part of the weakest, small-amplitude structures, particularly at the 'small' mesoscale. In practice, this mainly reduces the number of detected eddies—especially the smallest ones—without significantly altering the statistical distributions, and therefore does not affect the main conclusions.

Estimates by Dudley Chelton (DOI: 10.1175/JTECH-D-24-0035.1) indicate that SWOT noise is likely below ~1 cm, and we therefore agree with the reviewer that lower thresholds could be considered to extend the detection toward smaller eddies, but in our thoughts, only once the community has found an appropriate way to extract the balanced motions.

Here, we deliberately adopt a cautious approach to ensure the robustness of the detections in our analysis.

This choice is now clarified in the revised manuscript, and we note that future work could explore lower amplitude thresholds to include weaker and smaller-scale structures. See lines 156-160.

5. Justification of tiling strategy

The choice to divide SWOT swaths into overlapping 128×128 tiles is not justified, while pyeddytracker can operate directly on full grids. This tiling strategy may introduce artificial boundary effects, and additional complexity in the merging procedure.

Please justify why detection on the full swaths was not used and discuss the potential impact of tiling on detection results.

The tiling strategy was primarily motivated by computational constraints associated with the inpainting step, which is computationally intensive on full SWOT swaths, and in practice, not doable. Dividing the data into smaller tiles was therefore necessary to keep processing time tractable and to enable systematic testing of several inpainting methods.

The choice of 128×128 tiles is somewhat arbitrary, but was selected as a practical compromise: it is sufficiently large (on the order of twice the swath width) to allow robust eddy detection, including structures located near the swath edges, while remaining computationally efficient. Because detection is performed independently on each tile and all boundary regions are redundantly sampled through overlaps, and because we took good care of avoiding double detections in overlapping areas this approach does not introduce biases or artificial boundary effects in the final detection set. We justified the tiling procedure at line 137.

6. Inpainting method

The choice of the biharmonic inpainting method is critical, as it directly affects the reconstructed height fields and therefore the eddy detection.

Please justify the choice of this specific method, indicate whether alternative inpainting approaches were tested and discuss its potential impact on eddy properties (e.g., amplitude, radius, detection rate).

Indeed, this gap filling process may induce many erroneous closed contours, especially at the edges of the swaths where the field is extrapolated.

The inpainting method (ideally) or the eddies concerned by this bias should be validated. At least, this should be discussed in the article.

See our common answer to R1 and R3 about this specific point and revised appendix.

7. Duplicate removal methodology

The duplicate removal procedure is custom-built, with several criteria, whereas pyeddytracker provides a native eddy matching algorithm based notably on an overlap score threshold. In addition, it is unclear which criteria are used to select one eddy among duplicates (both between overlapping tiles and between swaths within the same cycle), and how this choice may affect eddy characteristics.

Please clarify why the built-in matching routine was not used, and describe the selection criteria for retained eddies, and discuss the sensitivity of the results to these choices.

Moreover, Figure 1 does not clearly illustrate duplicate detections before and after merging, particularly for overlapping tiles and multiple swaths within a cycle.

Please include a concrete example showing duplicated eddies, and how they are identified and removed in the final detection.

The duplicate removal procedure implemented in this study is physically based. For each SWOT cycle, duplicates are identified as eddies whose centers are separated by a distance smaller than their radius of maximum velocity Rmax and a fixed threshold of 10 km. This value corresponds to the typical displacement of an eddy advected over one SWOT repeat cycle.

When duplicates are identified (either from overlapping tiles or multiple swaths within a cycle), one eddy is retained without preferential selection—specifically, the first occurrence in the sequence of passes is kept. This ensures that the procedure does not introduce systematic biases in eddy characteristics. A similar point is raised in Reviewer 1's comment (l.168), where the choice between the two close detections shown in Fig. 2 is effectively arbitrary and governed by this automated procedure.

To clarify this process, we now describe a concrete example in the manuscript. The anticyclonic eddy located near 60°N, 54°W (left panel of Fig. 1b) is detected in multiple passes but retained only once after merging. See lines 167-169.

8. Biases introduced by gap-related filtering criteria

The exclusion of eddies with more than 40–60% of their contour located in missing data regions likely introduces a systematic detection bias. Indeed, this approach tends to remove eddies crossing the nadir gap, remove large eddies spanning swaths and favor small, compact eddies fully contained within a single swath. This creates a structural asymmetry with altimetry (e.g., CMEMS) products, which tend to detect larger and smoother eddies due to their lower resolution.

Please explicitly acknowledge this bias, and assess its potential impact on the statistics presented in Figures 3, 4, and 5.

The impact of gap-related filtering criteria has been assessed through sensitivity tests (similar to those discussed in our response to comment #3), in which we varied both the central tolerance and the percentage of missing data allowed within eddy contours.

As expected, relaxing these criteria increases the number of detections, but has only a limited impact on the normalized statistical distributions (see figures). However, in the present case, eddies detected in the gap-filled regions when considering large tolerance result from the inpainting procedure and are therefore not purely observational. When the tolerance is too permissive, a fraction of the additional detected eddies clearly originates from these reconstructed areas and is thus likely spurious (see our response regarding the inpainting method).

For this reason, we deliberately adopted conservative thresholds. It minimizes the inclusion of artificial detections and ensures that the retained structures are robust. Sensitivity tests are mentioned in the manuscript at lines 155-160.

9. Validation strategy

The validation is interesting but remains largely qualitative:

- Limited sample size: only 4 eddies are used for direct comparison;
- No quantitative metrics (e.g., bias, standard deviation, RMSE, correlation…) ;
- Temporal mismatch between SWOT and ADCP observations

Please include simple quantitative metrics, discuss the uncertainty associated with temporal mismatch, and moderate statements such as "excellent agreement" in the light of these limitations.

We thank the reviewer for this comment. We agree that quantitative metrics (e.g., RMSE, correlation) would have strengthened the validation. However, they are difficult to perform in our case due to the limited number of collocated eddies and, more importantly, the temporal mismatch between SWOT snapshots and ADCP measurements.

For this reason, we chose to rely on a qualitative, process-oriented comparison for individual cases, and to base the robustness of our results primarily on the statistical analysis over the full Labrador Sea.

In addition, following the reviewer's suggestion, we have included simple quantitative comparisons in the revised manuscript by reporting the distributions (mean and standard deviation) of eddy radius and velocity for SADCP and SWOT detections. These show consistent ranges and comparable statistics between the two datasets, while highlighting the clear differences with CMEMS gridded altimetry, which exhibits larger and weaker eddies due to its coarser resolution, see lines

We have moderated statements such as "excellent agreement" and clarified these limitations in the revised manuscript: "Note that given the limited number of colocated cases, this agreement should be interpreted qualitatively." See lines 221-227.

10. Missing visual intercomparison (SWOT vs ADCP vs CMEMS)

The manuscript lacks direct visual comparisons of the same eddies across datasets.

Please include case(s) study showing matching and mismatching eddies from the several datasets.

Suggestion: As mentioned above, pyeddytracker includes built-in matching tools that allow for systematic comparisons between datasets. These tools can be used, for instance, to identify eddies detected in multiple datasets in front of those detected in only one dataset, and to compare their spatial distribution and dynamical characteristics.

Such an analysis would provide a more quantitative assessment of the similarities and differences between SWOT, CMEMS, and in situ detections, and would help better identify the respective strengths and limitations of each dataset.

We thank the reviewer for this suggestion which led to a very nice revised version of Fig. 2.

We would like to emphasize that a detailed intercomparison with CMEMS products is not the primary objective of this study. It is well established that in regions where the first baroclinic Rossby radius of deformation is small, mapped altimetry products such as CMEMS have intrinsic limitations in resolving mesoscale and submesoscale eddies. In that context, the differences between SWOT and CMEMS are expected and do not constitute a central result of the paper.

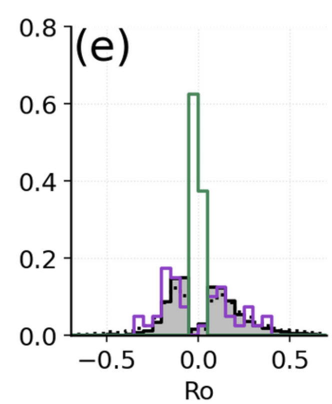
That said, we agree that a visual comparison can help the reader better appreciate the capabilities of SWOT. Following the reviewer's suggestion, we have added to Figure 2 a comparison for case A1 including both CMEMS 1/4° and 1/8° products. This example clearly illustrates the improved resolution of SWOT relative to conventional altimetry, and highlights that *in situ* observations provide a more appropriate reference for evaluating eddy structures at these scales.

We chose not to include additional CMEMS examples, as they lead to similar conclusions and would not provide further insight. We added a description to the revised Fig. 2 and CMEMS detection at lines 211-214.

11. Missing of CMEMS curves in Fig. 3 (panels a & e)

CMEMS results are discussed but not shown in Figure 3 for eddy polarity and Rossby number.

Please include these curves or explain their absence.



Concerning panel a, it was an oversight on our part, we added it. For panel e, is was because the value of Ro is way out of the y-range for the plot (see attached histogram).

We nevertheless chose to add it following the reviewer's comment, but keeping the same y-range and acknowledging the value in the caption.

12. SWOT Cal/Val phase validation

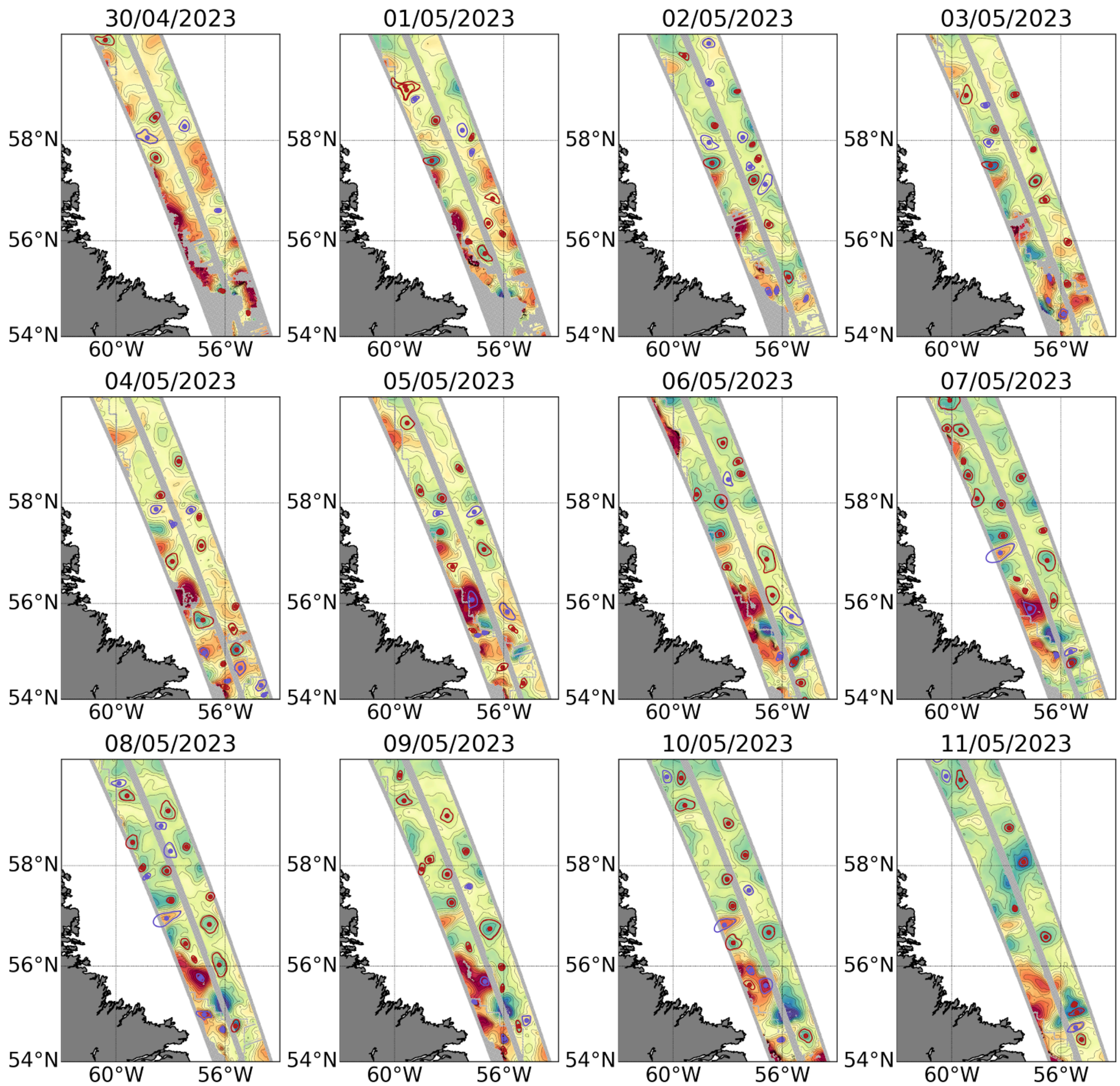
The SWOT Cal/Val phase provides higher temporal sampling, which offers an opportunity to assess whether the same eddy is consistently detected across successive passes and evaluate the sensitivity of the detection to variations in SWOT sampling (e.g., swath geometry and data gaps).

Please explain why this dataset was not used for validation or for assessing detection robustness, and eventually use it to complete the validation.

We thank the reviewer for this valuable suggestion. Following this comment, we performed eddy detection on SWOT passes acquired during the Cal/Val phase in the Labrador Sea. In particular, pass #20 provides a relevant test case. We analyzed consecutive days detection, for example 16 consecutive days in May (in the figure attached) and we observed a good overall consistency in eddy detection from one day to the next.

While some eddies are intermittently missed, this behavior is inherent to any detection method applied to evolving structures and varying sampling conditions. We expect this effect to be further reduced in the science phase, where swath overlap and the analysis of full swot cycles mitigate such intermittent misdetections.

At this stage, we have not implemented a quantitative metric to systematically assess detection consistency, and we consider this as a perspective for future work. Given the exploratory nature of this analysis, we chose not to include the figure in the manuscript. However, we now explicitly mention in the Methods section that detection consistency was qualitatively assessed using Cal/Val data, see lines 178-180.



13. Interpretation of seasonal results

The seasonal variability analysis is based on a single year (2024).

Please explicitly acknowledge this limitation and interpret the results accordingly.

In the revised manuscript, we have extended the analysis to include <u>two years of data</u>, rather than a single year. This significantly strengthens the robustness of the seasonal variability assessment.

14. Suggestion on the dataset availability

Would it be possible to share publicly the eddy detection dataset? This would allow users to perform comparison, validation and statistics with other atlases or personal data.

We chose to wait until the end of the review process to finalize the dataset. We confirm that the dataset will be made publicly available upon publication via a GitHub repository. Details and access information will be provided in the last version of the manuscript.

Minor Comments

General: The statement "first statistical description" should be nuanced. Previous studies based on in situ observations and numerical models have already provided partial statistical characterizations of eddies in the Labrador Sea.

Please clarify that the novelty of this work lies in providing a SWOT-based, high-resolution, etc statistical description, rather than the first statistical description overall.

We totally agree with the reviewer, and modified the mentions to "a first statistical description" accordingly, rather saying that we provide : "a spatial statistical description of the regional mesoscale activity at high latitude at an unprecedented resolution, significantly extending previous estimates derived from lower-resolution altimetry products." see lines 239-240.

Note that I am not an English native speaker so some of the following remarks can be irrelevant:

We addressed the english language corrections accordingly.

l.34 : generateS

l.60 : "see some background in e.g., Morrow et al., 2019" to "see, e.g., Morrow et al. (2019) for background" or something similar (for the use of e.g.)

l.71 : "this has no implications for our analysis" to "this is not expected to significantly affect our analysis" or something similar to nuance the sentence.

l.78 : which represents

l.124 : "the latter reconstructs"

l.135 : “sensitivity tests to this parameters were done”: “sensitivity tests for these parameters were performed” ?

l.136 : WAS obtained

Figure 2 : the units in the table should be given. They are given in the caption

Conclusions

In summary, this study is promising and addresses a relevant topic with a novel dataset. The approach has strong potential, and the results are encouraging. However, the current version of the manuscript lacks sufficient methodological justification and quantitative validation to fully support the conclusions.

I recommend major revisions, with particular attention to:

- **performing the detection on the ADT field rather than the SLA;**
- clarifying and justifying methodological choice;
- strengthening the validation and results;
- and moderating some interpretations in light of the current limitations.

Addressing these points would significantly improve the robustness and impact of the study.