

We thank the Editor and the two reviewers for their careful second review of our manuscript. We have addressed all comments thoroughly. Answers to the reviewer comments are written **in blue** in the text.

Reviewer #1

Second round of revisions of "High-Latitude Eddy Statistics from SWOT compared with in situ observations" by de Marez et al.

The authors responded satisfactorily to most of the major comments by all three reviewers and there remain only some minor issues that I will comment on in the following.

General Comments

Some mix up of different versions of the manuscript seems to have occurred. For example: In my first revision, I suggested renaming section 2.3 "Other datasets" instead of "Other dataset". The section is now still named "Other dataset" in the revised version but the tracked-changes version indicates that it has been changed to "Other dataset" from "Other datasets", even though it was never called "Other datasets" in the first version. There are several instances like this and I tried to indicate them in the minor comments for the changes initially suggested by me but I strongly suggest the authors go through the first revisions of all reviewers again and make sure that all the minor corrections that they said they implemented are actually included in the revised manuscript.

We acknowledge that we got mixed up, and we sent to review the wrong version of the manuscript that was not taken into account [some of the reviewers minor comments corrections](#) (but all the rest of the review, the major remarks). We indeed corrected your comments in the old version of the manuscript and not the newest... We are sorry about this, and we greatly thank the reviewer for their very cautious check.

Specific comments

II. 15-63: The introduction reads really well now!

I. 75: Please provide a DOI to a preprint or similar for the reference "Demol et al.". Citation from the submission guidelines for Ocean Science: "Works "submitted to", "in preparation", or "in review" can be cited upon submission with entry in the reference list, as long as these works are available to the reviewers (either on an external server or as review asset). Such works should not be cited in the final, accepted manuscript, unless published, accepted for publication, or available as preprint with a DOI." (<https://www.ocean-science.net/submission.html#references>) [The preprint of this specific submitted work is accessible online, we added both URL and DOI to the reference following the reviewer's comment.](#)

II. 110-112: I suspect there was some confusion about which datasets I was referring to in the first round of reviews where I commented that the datasets should be included in the list of references. I was referring to the OSI SAF and ETOPO datasets. Again, citation from the

submission guidelines: "Authors are requested to follow our data policy including [...] the proper citation of data sets in the text and the reference list including the persistent identifier." (<https://www.ocean-science.net/submission.html#assets>).

We now cite the datasets in the reference lists with all details, according to the OS guidelines.

II. 263-266: The authors use the instability along the West Greenland Current to explain the seasonal variability in their "West" area. As they pointed out in their response to the first round of revisions, the West Greenland Current is not in the the "West" area. Either the authors mislabeled "West" and "East" in Fig. 5 or they have to rethink their argumentation.

II. 272-274: These description fit with the column labeled "East" in Fig. 5 however, I strongly suspect that they refer to the "West" area in Fig. 4 as this is where velocities are weak.

We thank the reviewer for pointing this out. The "East" and "West" regions in Fig. 5 were not mislabeled. However, we agree that our previous interpretation invoking instabilities of the West Greenland Current (WGC) was misleading in the context of the defined regions, and we have therefore removed this argument from the revised manuscript.

Nevertheless, we still believe that the seasonal modulation of baroclinic instability is stronger in the western region than in the eastern one. In the eastern area, the detected eddies are predominantly generated by topographic interactions along the continental slope, and the West Greenland Current itself exhibits relatively weak seasonal variability. The Irminger Rings generated by instabilities of the WGC are primarily observed farther offshore in the basin interior, therefore described by the time series of the "central area". Conversely, in the western Labrador Sea, the pronounced seasonal cycle of mixed layer depth — partly influenced by sea-ice melt and freshwater input — likely drives stronger seasonal variations in eddy generation and mesoscale activity. We have clarified this interpretation in the revised text at lines 266-277.

II. 358-472: I again suggest that the authors include DOIs with all references where possible as this greatly simplifies access for the readers. Again citing from the submission guidelines for Ocean Science: "Please supply the full author list with last name followed by initials. After the list of authors, the complete reference title needs to be named: journal names should be abbreviated, followed by the volume number, the complete page numbers (first and last page), the digital object identifier (DOI), and the publication year."

(<https://www.ocean-science.net/submission.html#references>)

We added DOIs to all cited references following the reviewer's suggestion.

Minor comments

We addressed all the following point.

- I. 2: see comment to I. 2 in first round of review ok
- I. 10: "full-calendar" should be "full calendar" ok
- II. 24-25: see comment to II. 23-24 in review 1 ok
- I. 26: see comment to I. 25 in review 1 ok
- I. 35: see comment to I. 36 in review 1 ok
- I. 74: see comment to I. 68 in review 1 ok
- I. 109: see comment to I. 99 in review 1 ok
- I. 116: see comment to I. 106 in review 1 ok
- I. 148: "detection" should be "detections" ok
- I. 185: see comment to I. 149 in review 1 ok
- I. 196: see comment to I. 160 in review 1 ok
- I. 281: see comment to I. 227 in review 1 ok
- I. 286: see comment to I. 232 in review 1 ok
- I. 330: "appears" instead of "appear" ok
- I. 386: see comment to I. 284 in review 1 ok
- I. 462: see comment to I. 341 in review 1 ok
- I. 467: see comment to I. 346 in review 1 ok

Reviewer #3

General comments

Most comments have been carefully addressed and justified, and I find the new manuscript improved. I still have one remark on the methodology and a few minor comments.

We thank the reviewer for these comments that again improved the manuscript quality.

Major comment

On the choice of SLA over ADT. I thank the authors for the sensitivity analysis, which is both interesting and reassuring for this specific study area. However, even if the statistics appear similar in this case, ADT remains the standard field recommended for eddy detection and should be preferred when possible. Regarding the justification provided:

"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."

this argument is not fully convincing and should be reconsidered. Most recent global eddy atlases (ToEddies, META4...) apply detection directly on the raw ADT field without any additional large-scale filtering.

As explained 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.], the ADT field is preconised in "semi-enclosed seas [...] and in any ocean region with strong geographical patterns", and suggest that "SLA should be preferred only in areas where the MDT is not reliable." The Labrador Sea appears to correspond to a zone where ADT should be preferred. Therefore, I strongly recommand to use ADT or at least to remove this specific argument and replace it with a more robust and accurate justification.

Following the reviewer remark, we have revised this concept in the manuscript.

As already mentioned, we performed the full two-year eddy detection using both SLA and ADT fields. We find that, in the Labrador Sea, the resulting eddy statistics and the validation against *in situ* observations are very similar for the two approaches. As an additional example not shown in the manuscript, for the anticyclonic eddy A1 sampled during the MSM129 cruise, both SLA and ADT detections yield nearly identical properties, with $V_{max}=0.4$ m/s in both cases, and $R_{max}=23.4$ km for SLA versus 24.3 km for ADT.

We fully acknowledge that ADT is commonly recommended for eddy detection, particularly in regions influenced by strong mean circulation patterns, as discussed in Pegliasco et al. 2021

and adopted in recent global atlases such as Ioannou et al. 2024 and Gamot et al. 2026. We also agree that a more detailed assessment of the MDT contribution and of the relative performance of SLA versus ADT in SWOT products is an important topic. However, we consider that such an analysis would require a dedicated study focused on the properties and uncertainties of SWOT MDT products, which is beyond the scope of the present paper. We also importantly note that most recent studies specifically investigating eddies from SWOT observations have relied on SLA fields (e.g., Han et al., 2026; Zhang et al., 2025; Zhu et al., 2025; Fu et al., 2026). To remain consistent with these studies we therefore chose to present in priority the SLA-based detections in the main manuscript. Nevertheless, to address the reviewer's concern, and because it may indeed benefit to the manuscript in the long term, and removes any doubts we now explicitly mention in the revised manuscript that both SLA- and ADT-based detections were performed, and we additionally provide ADT-based results in Figure 3, and we supply all statistics based on ADT in Appendix B for comparison purposes.

We modified the text accordingly at lines 180-186.

Minor comments

- The direct validation is based exclusively on four anticyclonic eddies. The authors should add an explicit sentence acknowledging that cyclone properties are validated only statistically. We added a sentence to acknowledge this point : "Also, note that this \textit{in situ} validation is based solely on anticyclonic cases; cyclonic eddy properties are validated statistically in the following."
- L.10: "distorded" -> "distorted" ok
- L.116: "no—KaRIn—measurements": please remove the "—" characters, as the phrasing is awkward. ok

Overall, I recommend acceptance after those points addressed.