Dear Handling Topic Editor (Yuntao Zhou) and Editor-in-Chief:

Re: essd-2026-341

We sincerely appreciate your time and effort in reviewing our manuscript. Your critical comments and insightful suggestions have been invaluable in improving the quality and clarity of our work. We have carefully addressed all the concerns raised during this round of review. In the revised manuscript, all modifications made in response to the reviewers' comments are highlighted in **red** for easy identification. Additional experiments, analyses, or clarifications have been incorporated as suggested. Below, we provide point-by-point responses to the reviewers' specific questions. Should further revisions be required, we are committed to addressing them promptly.

Topic Editor

Following the second round of review, one reviewer is concerned about methodological transparency, the interpretation of the evaluation results, and data citation. I invite you to submit a further minor revision addressing these issues before a final decision is made. Please provide a point-by-point response to all remaining comments. In particular, expand the description of the ROMS simulation, clarify its role in training and evaluation, and explain how the horizontal derivatives in the advective terms are calculated. Revise the statement concerning resolved spatial scales and justify the use of 30-day averages, including the implications for the evaluation.

Respond: Thank you for managing the second round of review for our manuscript and for the opportunity to submit this minor revision. We greatly appreciate the constructive feedback, which has helped us further improve the methodological transparency of our work. Below, we provide our point-by-point response addressing the reviewer's remaining concerns:

1. Description of the ROMS simulation, use of 30-day averages, and its role in training and evaluation

As detailed in our direct response to Reviewer 3, we have added a more comprehensive description of the ROMS simulation. We have also clarified that the ROMS results were not

involved in model training but were used mainly as an additional source of evidence to further assess the effectiveness of the proposed method, particularly the comparison with the iterative approach. Since the ROMS-based analysis was intended only as a supplementary comparison rather than as the primary validation, we used 30-day averaged fields to reduce short-term variability and provide a more stable representation of the circulation patterns for comparison. The primary validation of the method was conducted using observational mooring data and OSCAR surface-current data. These points have now been clarified in the revised manuscript (please see **Lines 107–109**, **Lines 114–120, and Lines 311-312** in the revised manuscript).

2. Calculation of horizontal derivatives in the advective terms

We have clarified the methodology for this calculation. In our model, these derivatives are computed using automatic differentiation (AD), a built-in mechanism in modern deep learning frameworks. The AD recursively applies the chain rule through each differentiable operation in the neural network to obtain the derivatives of the predicted velocity fields with respect to the spatial coordinates (x, y), rather than relying on numerical finite differences. This explanation has been added to the revised manuscript (please see **Lines 212–215**).

3. Resolved spatial scales

In our study, the *in situ* mooring observations provide the primary benchmark for evaluating model performance. The ROMS simulation was included only as a supplementary validation. We agree with the reviewer that the use of 30-day averaged fields substantially suppresses submesoscale variability. To avoid overstating the resolved spatial scales, we have therefore revised it as "allowing the model to resolve mesoscale circulation features" (please see **Line 114**).

The geographical scope of the in-situ validation must be made explicit wherever the associated performance statistics are reported, including the abstract. Claims of improved performance should be limited to what the evaluation supports. Additional validation outside the tropics is encouraged where feasible; otherwise, explain the limitation and justify the adequacy of the existing evaluation for the revised claims and intended uses of the dataset. Please also specify and cite the OSCAR version actually used, justify its

suitability for the comparison, and consider the suggested references where relevant.

Respond: Thank you for your valuable feedback regarding the geographical scope of the validation and the data citations. We have addressed all of these points in the revised manuscript as detailed below:

1. Geographical Scope of In-Situ Validation and Performance Claims

We have explicitly clarified the geographical coverage of the in-situ mooring validation throughout the manuscript to ensure that all performance claims are strictly supported by the evaluation data. Specifically, we have noted that the dataset demonstrates good agreement with available mooring observations across the tropical Pacific, Atlantic, and Indian Oceans, while explicitly stating that its performance in other regions requires further validation in following works. We have also reviewed and adjusted the claims across the text and abstract to ensure that statements of improved performance are strictly confined to these evaluated tropical domains. **(please see Line 24, Lines 281–286 and Lines 303–305 in the revised manuscript)**

2. OSCAR Version and Citation

We have updated and corrected the citation and specifications for the OSCAR dataset used in the comparison **(please see Lines 250–251 and Line 263-265)**. The exact version, spatial resolution, and access details have been explicitly cited to ensure full reproducibility and to justify its role as a standard reference product for surface current comparisons.

3. Suggested References

We have carefully examined the references suggested by the reviewer and integrated the relevant citations into the revised manuscript to appropriately frame the methodology and contextualize our findings (**Red markings in the references**).

Reviewer 1

The revised manuscript has improved considerably, and the authors have made a serious effort to address the previous comments. In particular, the expanded mooring validation, the comparison with OSCAR, and the ablation results provide better support for the dataset. The method and data documentation are also clearer. No further major analysis seems necessary. The remaining point is mainly one of wording. Some statements appear

slightly broader than the validation results can support. For example, "robust accuracy across all three major oceans" may be too strong because the in-situ validation is mainly based on tropical mooring arrays. A more precise statement would be "good agreement with the available mooring observations across the tropical Pacific, Atlantic, and Indian Oceans." Similar expressions implying globally uniform or conclusive performance should also be checked and softened where appropriate, especially for high-latitude, polar, and coastal regions that are not directly covered by the current in-situ validation.

Respond:

Thank you for your positive feedback on our revisions and your suggestions regarding our wording. We have thoroughly reviewed and softened these broad expressions throughout the revised manuscript. They have been adjusted to more precise descriptions (e.g., **Line 284**: "good agreement with the available mooring observations across the tropical Pacific, Atlantic, and Indian Oceans"), and we have explicitly acknowledged the limitation of lacking direct in-situ validation in high-latitude, polar, and coastal regions. For the specific modifications, please refer to **Lines 281-286 and Lines 303-305** in the revised manuscript.

There is also a small but important consistency issue in the method description. The manuscript should consistently state that the data-loss target is the pseudo-label constructed from the geostrophic current plus the Ekman current. However, the sentence "Loss_data" represents the mean squared error between the predicted and geostrophic currents" describes "Loss_data" as the error between the prediction and the geostrophic current only. This should be revised to refer to the geostrophic-plus-Ekman pseudo-labels, so that the text is consistent with the equations and the training strategy.

Respond:

Thank you for pointing out this detail. We have corrected the statement to explicitly clarify that $Loss_{data}$ represents the mean squared error between the predicted currents and the pseudo-labels constructed from the geostrophic plus Ekman currents. This revision ensures that the text is fully consistent with the equations and our training strategy. For the specific modification, please refer to **Lines 187-189 in the revised manuscript**.

Reviewer 3

Referee Report

"Daily Global Sea Surface Ageostrophic Current Dataset from 1993-2023 via Physics-Informed Deep Learning"

G. Cui, K.-V. Yuen, Y. Chen, Z. Liu, D. Yang, G. Liu, and Z. Cai, Earth System Science Data, manuscript essd-2026-341 (version 2)

October 1, 2026.

Summary of the submission

The manuscript (Cui et al., 2026) presents a daily, global, 0.25° surface current dataset for 1993-2023. A physics-informed neural network (AC-PIDNN) adds Ekman and nonlinear advective corrections to altimetric geostrophic currents by minimizing the residual of a steady momentum balance among advection, Coriolis, pressure gradient, and wind terms (their Eqs. 1-2). The product is assessed through its momentum-balance residual, through comparison with tropical moored current observations and the OSCAR product, and through comparison with a regional ROMS simulation.

The comments below concern the description of the simulation used for evaluation, a claim made about its resolution, a missing methodological detail, the representativeness of the in situ validation, the comparison product, and prior work that should be cited. Line numbers refer to version 2 of the manuscript.

Respond:

We would like to express our sincere gratitude for your time and effort in reviewing our manuscript. We appreciate your constructive comments regarding the description of the simulation data, the resolution claims, the methodological details, the representativeness of the in-situ validation, and the necessary citations. Your detailed and professional suggestions have been immensely helpful in improving the scientific rigor and overall quality of our paper.

We have provided detailed, point-by-point responses to all your concerns below and have

thoroughly updated the revised manuscript accordingly.

Comments

1. Section 2.2 (Simulation Data) is inadequate (ll. 107-118). The ROMS simulation is used as the "ground truth" for the method, yet it is described in a few lines that give only the domain, the grid spacing, and the averaging period. The following need to be answered in the text:

Respond:

Thank you for reviewer's careful comments. We have added a more comprehensive description of the ROMS simulation (**Lines 114-120**). We have also clarified that the ROMS results were not involved in model training but were used mainly as an additional source of evidence to further assess the effectiveness of the proposed method in the revised manuscript. Details are shown in the following responses.

• How was this ROMS simulation forced? Section 4.1.3 states that the simulation "neglects the Ekman component" (l. 303), which makes the nature of the surface forcing unclear.

Respond:

Thank you for pointing out this omission. In this simulation, the wind forcing was ignored and the surface forcing for this simulation is driven by the mean external volume flux prescribed at the southeastern and northeastern boundaries. Thus, we mention that it neglects the Ekman component. We make this clear in the **Lines 116-117** of the revised manuscript.

• Did it assimilate any observations? If so, which ones?

Respond:

Thank you for your question, and no observations were assimilated. (**Line 117**)

• How was the model spun up? • How many years were simulated?

Respond:

Thank you for your question. The model was initialized with horizontally uniform salinity and temperature fields, which were derived from the annual mean data of the World Ocean Atlas 2013 averaged over the North Pacific Ocean. Following initialization, the model underwent a spin-up period of 25 years to attain a quasi-steady state. The results from the final 5 years of the simulation were then utilized for our analysis. (**Lines 114-118**)

• Why are 30-day means used (l. 114)? Averaging over 30 days erases any trace of the submesoscale.

Respond:

Thank you for your insightful comment. In our study, the *in-situ* mooring observations already serve as the primary and solid benchmark for model validation. The ROMS results were used mainly as an additional source of evidence to further assess the effectiveness of the proposed method. We completely agree with your point that averaging over 30 days indeed erases submesoscale signals, and we have also removed the inaccurate claims about "resolving the submesoscale" in the revised manuscript to avoid any ambiguity (please see **Line 114 of the revised manuscript**).

• Is ROMS part of the training, or is it used only for evaluation?

Respond:

Thank you for your question. The ROMS results were not part of the training. They were used mainly as an additional source of evidence to further assess the effectiveness of the proposed method, particularly through comparison with the iterative approach. The primary validation of the method was conducted using observational mooring data and OSCAR surface-current data. We clarified this point in **Lines 107-109** and **Lines 311-312 of the revised manuscript**.

2. The statement that the simulation resolves the submesoscale is incorrect (l. 115). The manuscript states: "This high-resolution setup effectively resolves mesoscale and submesoscale oceanic dynamical processes." A 3 km grid does not resolve the submesoscale. Dong et al. (2020) estimate that resolving wintertime surface mixed layer eddies requires grid spacings of 1.9 km (50% of regions resolved) and 0.92 km (90%) globally, and finer still in summer (1.3 km and 0.55 km). Dong et al. (2021) estimate that beginning to resolve symmetric instability requires grid spacings of 24 m (50%) and 7.9 m (90%) in winter. A 3 km simulation is therefore mesoscale-resolving and, at best, submesoscale-permitting; and, as noted in Comment 1, the 30-day averaging removes

whatever submesoscale variability the model does permit. The sentence should be corrected.

Respond:

Thank you for your correction and for providing the valuable references (Dong et al., 2020, 2021). Following your suggestion, we have removed this inappropriate claim in the revised manuscript and corrected the description to accurately state that the simulation primarily resolves **mesoscale dynamical processes** (**Line 114 of the revised manuscript**).

3. How are the derivatives in the advection terms calculated (Eqs. 1 and 2, ll. 123-125)? The terms ADV_x = u∂u/∂x + v∂u/∂y and ADV_y = u∂v/∂x + v∂v/∂y require horizontal derivatives of the predicted velocity, but the manuscript does not say how these are evaluated; for example, by automatic differentiation of the network outputs with respect to the input coordinates (x, y), or by finite differences on the 0.25° grid. Because the advective component is the principal new quantity in the dataset, and it is defined entirely through these derivatives, the method of calculation must be stated.

Respond:

Thank you for your careful reading and professional suggestion. In our model, the derivatives in the advection terms are calculated by automatic differentiation of the network outputs with respect to the input coordinates (x, y). Automatic differentiation leverages the continuous differentiability of neural networks, allowing for a more precise calculation of the velocity gradients. We sincerely appreciate you pointing out the omission of this important detail, and we have explicitly stated this method of calculation in the **Lines 213-215 of the revised manuscript**.

4. The in situ validation is biased toward the regime where the advective and Ekman effects are large (Section 4.1.2, ll. 254-298). The in situ comparison uses only the TAO, PIRATA, and RAMA tropical moored arrays, all within roughly ±25° of the equator, and the manuscript itself reports that the largest errors are concentrated within about ±5°. Testing against near-equatorial observations biases the assessment toward conditions in

which advection and Ekman effects are large relative to the geostrophic flow, particularly in the meridional currents. The improvement reported over geostrophic and Ekman-corrected currents is therefore not representative of the global dataset, most of which lies outside the region sampled. This limitation should be stated wherever the error statistics are quoted, including the abstract, and the validation should be extended to observations outside the tropics if possible (e.g., surface drifters, which the manuscript already mentions in its introduction).

Respond:

Thank you for this insightful comment. We agree that the current in situ validation, which relies on the TAO, PIRATA, and RAMA moored arrays, is confined to the tropical regions. Thus, the reported error statistics primarily reflect the model's performance in low-latitude areas and are not fully representative of the global dataset.

Following your suggestions, we have explicitly stated this spatial limitation in the revised manuscript (**Line 24** in abstract and **Lines 281-286**). We have also noted that future work should extend the validation to additional observational datasets, particularly observations outside the tropical regions, to provide a more comprehensive assessment of the model's performance.

5. Product for comparison: OSCAR. The manuscript describes the comparison product only as "the existing OSCAR surface current product (1993-2022)" with a link to the ESR web site (l. 257); no version is given and the product is not in the reference list. The comparison should be made with, and the manuscript should cite, the Ocean Surface Current Analyses Real-time (OSCAR) Surface Currents Final 0.25 Degree (Version 2.0) product (ESR and Dohan, 2021):

ESR; Dohan, Kathleen. 2021. Ocean Surface Current Analyses Real-time (OSCAR) Surface Currents Final 0.25 Degree (Version 2.0). Ver. 2.0. PO.DAAC, CA, USA. Dataset accessed [YYYY-MM-DD] at doi:10.5067/OSCAR-25F20.

Respond:

Thank you for your careful correction. We indeed utilized the Ocean Surface Current Analyses Real-time (OSCAR) Surface Currents Final 0.25 Degree (Version 2.0) product in our

study. We have updated the download link in **Line 263-265** and the in-text citation in **Line 251** of the revised manuscript to explicitly state the specific data version. Additionally, we have formally added the suggested reference (ESR and Dohan, 2021) to the reference list.

6. References that should be cited.

Wunsch and Gaposchkin (1980), on the use of satellite altimetry to determine the general circulation of the oceans, where the manuscript introduces geostrophic currents derived from sea surface height (ll. 35-38).

Gold (1908), for the gradient wind relation. The manuscript states that the iterative solution of the gradient wind equation "was first developed in atmospheric science" (ll. 62-64) but supports this only with recent oceanographic papers.

The experimental multimission gridded Level-4 sea surface heights and velocities that include SWOT (MIOST) (CNES and CLS, 2025), as a current altimetric height and velocity product relevant to the data described in Section 2.1.

Respond:

Thank you for recommending these highly valuable references. We have incorporated all of them into the revised manuscript to strengthen our literature review and background context:

- The reference to Wunsch and Gaposchkin (1980) regarding the use of satellite altimetry to determine general ocean circulation has been added to **Lines 36-37** of the revised manuscript.
- The citation for Gold (1908) regarding the gradient wind relation has been added to **Lines 62-64** of the revised manuscript.
- The reference for the experimental multimission gridded Level-4 sea surface heights and velocities, which includes SWOT (CNES and CLS, 2025), has been added to **Lines 94-95** of the revised manuscript as a highly relevant and up-to-date altimetric product.

Web sources

Source	Link
OSCAR Final 0.25 Degree, Version 2.0 (PO.DAAC)	https://doi.org/10.5067/OSCAR-25F20
Wunsch and Gaposchkin (1980)	https://doi.org/10.1029/RG018i004p00725
CNES/CLS MIOST with SWOT Level-4 (AVISO+)	https://doi.org/10.24400/527896/a01-2025.001
Gold (1908)	https://doi.org/10.1098/rspa.1908.0038
Dong et al. (2020), submesoscale baroclinic instability	https://doi.org/10.1175/JPO-D-20-0043.1
Dong et al. (2021), symmetric instability	https://doi.org/10.1175/JPO-D-20-0159.1

References

CNES and CLS. Experimental Multimission Gridded (MIOST with SWOT) Level-4 Sea Surface Heights and Velocities. Processed by SSALTO/DUACS and distributed by AVISO+, supported by CNES, 2025. doi:10.24400/527896/a01-2025.001.

Guangxi Cui, Ka-Veng Yuen, Ying Chen, Zhiqiang Liu, Dingqi Yang, Guanliang Liu, and Zhongya Cai. Daily Global Sea Surface Ageostrophic Current Dataset from 1993-2023 via Physics-Informed Deep Learning. Manuscript essd-2026-341, version 2, submitted to Earth System Science Data. File essd-2026-341-manuscript-version2.pdf, retrieved 2026-10-01, 2026.

Jihai Dong, Baylor Fox-Kemper, Hong Zhang, and Changming Dong. The Scale of Submesoscale Baroclinic Instability Globally. Journal of Physical Oceanography, 50(9):2649-2667, 2020. doi:10.1175/JPO-D-20-0043.1.

Jihai Dong, Baylor Fox-Kemper, Hong Zhang, and Changming Dong. The Scale and Activity of Symmetric Instability Estimated from a Global Submesoscale-Permitting Ocean Model. Journal of Physical Oceanography, 51(5):1655-1670, 2021. doi:10.1175/JPO-D-20-0159.1.

ESR and Kathleen Dohan. Ocean Surface Current Analyses Real-time (OSCAR) Surface Currents Final 0.25 Degree (Version 2.0). NASA Physical Oceanography Distributed Active Archive Center (PO.DAAC), CA, USA, 2021. doi:10.5067/OSCAR-25F20.

E. Gold. The Relation between Wind Velocity at 1000 Metres Altitude and the Surface Pressure Distribution. Proceedings of the Royal Society of London. Series A, 80(540):436-443, 1908. doi:10.1098/rspa.1908.0038.

Carl Wunsch and E. M. Gaposchkin. On Using Satellite Altimetry to Determine the General Circulation of the Oceans with Application to Geoid Improvement. Reviews of Geophysics, 18(4):725-745, 1980. doi:10.1029/RG018i004p00725.

Respond:

Thank you for providing this detailed supplementary information. These highly valuable references pointed out the omissions in our original manuscript and helped us effectively perfect our discussions. We have fully added all the listed references to the bibliography of the revised manuscript and accurately cited them in the corresponding sections. Please refer to the **red-highlighted text in the reference of revised manuscript** list for these additions.