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Peer Review File

The three-dimensional structure of fine-scale, vertical velocities in the Southern Ocean inferred from space

Corresponding Author: Dr Andrew Thompson

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Thompson,

Please accept our apologies for the delay in sending a decision on your manuscript. Your manuscript titled "The three-dimensional structure of fine-scale, vertical velocities inferred from SWOT and glider observations" has now been seen by 2 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file. In addition to this, to be considered for publication, please ensure that the following editorial thresholds are addressed:
- perform an independent verification of the GEM reconstruction method,
- justify the small Rossby number assumption used in the QG-omega reconstruction of vertical velocities.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Jennifer Veitch, PhD

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of "The three-dimensional structure of fine-scale, vertical velocities inferred from SWOT and glider observations" by Thompson et al.

Upper ocean dynamics at lateral scales down to 10-100km form a rich tapestry of eddies, fronts, and filaments that play a crucial role in ocean biogeochemistry, productivity, atmosphere-ocean interactions, and marine ecosystems. This dynamic regime, which encompasses submesoscales and small mesoscales, are characterised by sharp fronts and strong vertical velocities that are organised and modulated by the mesoscale eddy field. Improved quantification of the vertical velocity field at these scales is important for understanding their impact on ocean circulation, climate, and ecosystems.

This study presents a novel approach for reconstructing upper ocean vertical velocity using SWOT sea surface height and (horizontal) velocity data in concert with in situ measurements of density from observations collected by gliders during the FOCUS field campaign, a research cruise that was part of the international SWOT regional calibration and validation efforts. The method relies on inversion of the quasi-geostrophic omega equation to derive vertical velocities arising from an imbalance between the geostrophic shear and density (buoyancy) gradients. The key innovation is the use of glider measurements to derive an empirical relationship between absolute dynamic topography and potential density, thereby allowing the reconstruction of high resolution vertical velocity fields from SWOT sea-surface height maps.

The study is an exciting application of in situ and SWOT data that has the potential to unveil the vertical velocity field in the upper ocean. However, it relies on several key assumptions that raise some questions for me:

(1) GEM method for subsurface potential density reconstruction:

The relationship between empirical sea-surface height and potential density is based on a gravest empirical mode (GEM) method that has been used successfully in earlier Southern Ocean studies. The GEM method exploits the equivalent barotropic structure of the upper ocean in this region, and is validated in this study using a numerical model. The empirical relationship is derived by comparing subsurface glider measurements with traditional gridded DUACS absolute dynamic topography.

(a) I wonder if biases might arise if there is insufficient sampling by the gliders. This is partially addressed in Supp. Fig. 2 which describes the criteria used to retain glider profiles for each ADT bin. How does the removal of glider profiles shown in Supp. Fig. 2 impact the results? Can you estimate the potential error arising from the use of relatively few glider profiles at more extreme ADT bins?

(b) As discussed in section 2.2, a potential source of uncertainty could occur if ADT gradients are displaced or smeared out due to interpolation errors. This is particularly relevant near regions of strong SSH gradients, such as the Polar Front, which is also where we expect to see strong vertical velocities. Are there issue with applying a GEM method derived from low-resolution DUACS ADT with high-resolution SWOT SSH maps, given the differences in spatial sampling between these two data sources?

(c) The GEM reconstruction is validated by reconstructing the potential density from SSH along the path of one of the gliders and comparing this with the true potential density (Fig 3c,d). It wasn't clear to me if the data from this glider path were used in the deriving the SSH-potential density relation that was used to reconstruct Fig 3c. If so, it would be better to withhold some of the glider data from the GEM reconstruction as an independent verification of the method. Alternatively, if there were any other independent density measurements (Argo floats, CTD profiles) collected from the region during the campaign, this would give further confidence in the method.

(2) Vertical velocity reconstruction:

The use of the QG omega equation to reconstruct vertical velocities from subsurface velocity and buoyancy (density) fields has been demonstrated in several previous studies (which are appropriately cited here). The method has some well-known limitations, in particular the small-Rossby number limit of QG theory and the method used for the inversion of the omega equation (Equations 1, 2). The method is well-described in this study, and validated using the numerical model. However, I have some (possibly naive) questions that might help further clarify the methodology:

(a) If I am following the method correctly, the "forcing" on the right-hand side of the QG omega equation involves gradients in the subsurface buoyancy field and the subsurface horizontal velocity field (Equation 2). In turn, the subsurface velocity field is derived from the buoyancy field (and the surface SWOT velocities) using the thermal wind equation (5). Finally, the buoyancy field itself is derived from the SWOT SSH field using the GEM method. Is there a danger of "double dipping" here, since the subsurface buoyancy fields and velocity fields appearing in Equation (2) are not in fact independent? Might errors in the SWOT data amplify as successive gradients are applied? Is this why the Gaussian filter is applied after each spatial derivative?

(b) Vertical velocity could also be calculated directly from the horizontal velocity field at each depth using the 3D incompressibility condition. How does this reconstruction compare with the omega equation?

(c) Why was a Dirichlet boundary condition applied to at z = -2000m, rather than directly applying a Neumann condition at z = - 1000m, if this is the desired result? Why are periodic boundary conditions applied in both the across-swath and along-swath directions? Is the curvature of the domain significant enough that the periodic box is not exactly rectangular?

(d) Velocity distributions: The lack of a persistent skewness in the vertical velocity is curious. Was the same thing observed in the numerical model? Even if the vertical velocity distribution is symmetric, there could still be a net vertical transport if there are correlations between the vertical velocity field and tracers such as heat. It is pointed out in the Discussion section that this would involve separate measurements of (e.g.) temperature or salinity, which are not available everywhere. However, it should be possible to do this analysis along the glider paths, which may give some indication if there is a correlation between vertical velocity and temperature/salinity. Likewise, this calculation should be possible in the numerical model.

Minor points:

L47 and elsewhere: Not sure what the convention is, but it seems strange to me to use a superscript for in-line citations.

L169: Again, this is a convention issue, but I don't usually see punctuation (:) before an equation.

L239: remove extra parenthesis

L387: Please provide reference or further information for the statement "the velocity field has strong agreement with..."

Equation 6: What are the typical Ro and Ri values here?

L404: remove period at start of line.

L427: Please provide reference or further information regarding the "separate study". Is this planned future work?

S. Keating, UNSW Sydney

Reviewer #2 (Remarks to the Author):

This article estimates vertical velocities with a new approach: exploit the gravest empirical mode method (GEM) to establish a SSH-sigma0 relationship using glider data, which is then used to produce profiles within the high resolution SWOT swaths. Additionally, a 3D interior velocity field is constructed from surface velocities and thermal wind, which indicate that large Rossby numbers are present well below the mixed layer. The availability of 3D velocity and density fields then allow the use of the QG-omega equation to infer vertical velocities.
The method is validated with a numerical model. High vertical velocities are found at the periphery of an eddy with high vertical coherence down to 1000m.

The method seems sound and superior to SQG. It has high potential to be applied to further high resolution satellite (SWOT) data, given the availability of concurrent in situ below surface T,S data. One caveat here seems to have been that the SSH-sigma0 relationship could not be established here from the SWOT data directly due to insufficient glider data beneath the swaths. Another caveat seems to be that QG-omega assumes low Ro, however large vertical velocities are found and high Rossby numbers, which appears to be a contradiction? Also, it should be stressed more, what is the significant new result here apart from the new method, since these large vertical velocities at the peripheries of eddies have been found before (and also its vertical coherence down to large depths?).

Abstract:
Specify what is novel in the method exactly.
Set the number of 150m/day in perspective. What do you compare this number to? Is this number a novel result?

Introduction:
The terms mesoscale and submesoscale are mentioned multiple times in the introduction. Do you define these terms here with respect to scale or dynamical regime (Rossby number)? This seems relevant since QG-omega assumes low Rossby numbers.

line 79: what do you mean by "local" at this point?

Fig 1: bathymetry contour levels are barely visible. Add map of greater area to show where the location is.

Use same color scale for Fig. 2c and 3c.

Fig.4: Magnitude of Rossby number is hard to decipher. It seems well above 0.1 in the frontal regions and this is precisely where the vertical velocity estimated from the omega equation requiring Ro<<1 is large?

Fig. 7: What are the Rossby numbers in the model?

Discussion:
what is the difference in vertical velocity magnitude from SWOT versus the traditional SSH DUACS,

stress the benefits/novelty of SWOT in terms of the velocities
line 258: so these previous studies did not provide the 3D structure, or not with that resolution? Stress here what distinguishes this study from the others

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Version 1:

Decision Letter:

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Dear Dr Thompson,

Your manuscript titled "The three-dimensional structure of fine-scale, vertical velocities inferred from SWOT and glider observations" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Alice Drinkwater, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I thank the authors for their thoughtful and detailed responses to my comments in their manuscript. I am more than satisfied that they have addressed all of my questions and am happy to recommend this manuscript for publication. Well done Andy and colleagues on an exciting and impactful study!

S.Keating, UNSW Sydney

Reviewer #2 (Remarks to the Author):

Thank you for addressing all comments. I just have one minor request: you mentioned studies that show that "QG dynamics are often robust outside of the Ro << 1 regime". Please include these citations in your manuscript.

The three-dimensional structure of fine-scale, vertical velocities inferred from SWOT and glider observations

Reply to Reviewer 1
February 2026

This study presents a novel approach for reconstructing upper ocean vertical velocity using SWOT sea surface height and (horizontal) velocity data in concert with *in situ* measurements of density from observations collected by gliders during the FOCUS field campaign, a research cruise that was part of the international SWOT regional calibration and validation efforts. The method relies on inversion of the quasi-geostrophic omega equation to derive vertical velocities arising from an imbalance between the geostrophic shear and density (buoyancy) gradients. The key innovation is the use of glider measurements to derive an empirical relationship between absolute dynamic topography and potential density, thereby allowing the reconstruction of high resolution vertical velocity fields from SWOT sea-surface height maps.

The study is an exciting application of *in situ* and SWOT data that has the potential to unveil the vertical velocity field in the upper ocean. However, it relies on several key assumptions that raise some questions for me.

We thank the reviewer (thanks, Shane!) for their time reviewing this manuscript, the positive assessment of the analysis, and the constructive comments regarding the reconstruction method. During the course of our study, we considered many of the points raised in the review. Responding to these comments has, we feel, strengthened our discussion of the QG-omega method as well as its benefits and limitations. Below, please find replies to each specific point in the review. Our replies are presented in blue and changes made to the text are presented in red.

(1) GEM method for subsurface potential density reconstruction.

The relationship between empirical sea-surface height and potential density is based on a gravest empirical mode (GEM) method that has been used successfully in earlier Southern Ocean studies. The GEM method exploits the equivalent barotropic structure of the upper ocean in this region, and is validated in this study using a numerical model. The empirical relationship is derived by comparing subsurface glider measurements with traditional gridded DUACS absolute dynamic topography.

(a) I wonder if biases might arise if there is insufficient sampling by the gliders. This is partially addressed in Supp. Fig. 2, which describes the criteria used to retain glider profiles for each ADT bin. How does the removal of glider profiles shown in Supp. Fig. 2 impact the results? Can you estimate the potential error arising from the use of relatively few glider profiles at more extreme ADT bins?

We agree that a potential limitation of the reconstruction is the application of a "local" Sat-GEM that is applicable to the region that was sampled by the glider. In the case of the FOCUS study, we are able to provide a reconstruction over a relatively large spatial area, in particular a large meridional extent, because the glider sampled SSH values and associated water masses located in frontal zones both north and south of the Polar Front (PF). As SSH is relatively uniform in the frontal zones (the "steps," rather than the "risers" of a steppy SSH distribution), glider coverage is robust. As noted by the reviewer, the SSH coverage (number of glider profiles per SSH bin) drops off at higher SSH values.

To directly address the question of the sufficiency of glider sampling, we have reproduced the SSH-σ relationship using a bootstrapping technique where we randomly remove 20% of the glider profiles. Using a Monte Carlo approach, we generated 500 realizations of the SSH-σ relationship where we randomly remove glider profiles. The figure below shows the standard deviation of potential density (kg m^{-3}) as a function of SSH and depth. As suggested, the variance is larger in those bins with fewer

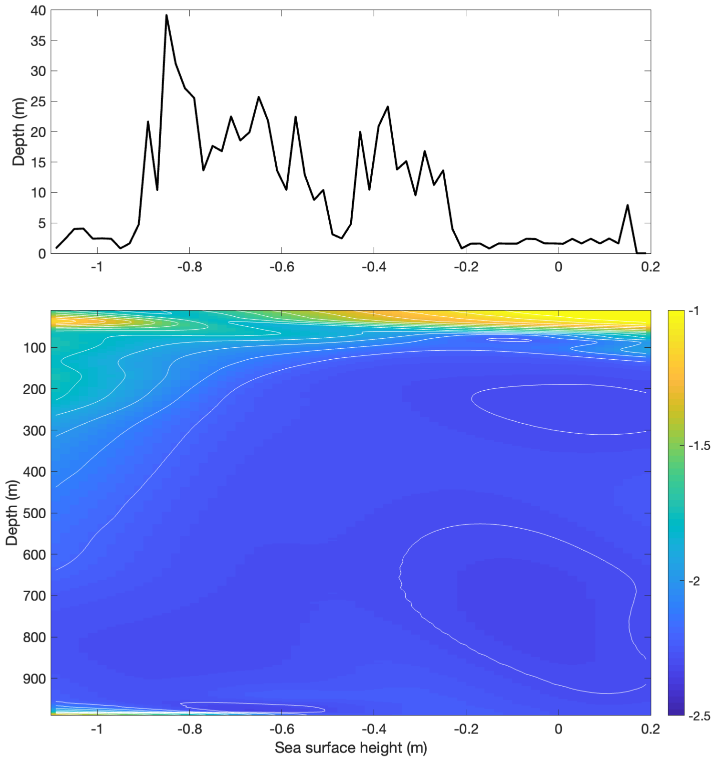

Figure 1: (**Upper panel**) Average number of profiles used for each SSH bin (0.02 m) to create 500 realizations of the SSH-σ relationship. This represents a 20% reduction in glider profiles from the full data set. The removed glider profiles are chosen at random from the full data set. (**Lower panel**) Standard deviation of the potential density from the 500 realizations; the colorscale is logarithmic such that $10^{-2} = 0.01$ kg m^{-3}.

profiles, at both low and high extreme SSH values. However, the overall change in the relationship is small; the interior density variance is $O(10^{-2}$ kg m^{-3}. Interestingly, the variance is elevated in the upper 50 m of the water column; here there is larger density variability related to surface forcing of the ocean surface mixed layer. While this uncertainty will propagate into estimates of density gradients, velocity gradients, and vertical velocity, we feel it is not sufficiently large to qualitatively change the magnitudes and statistics of the vertical velocity estimates.

This figure has been added to the supplementary material (Supplementary Figure 7).

(b) As discussed in section 2.2, a potential source of uncertainty could occur if ADT gradients are displaced or smeared out due to interpolation errors. This is particularly relevant near regions of strong SSH gradients, such as the Polar Front, which is also where we expect to see strong vertical velocities. Are there issues with applying a GEM method derived from low-resolution DUACS ADT with high-resolution SWOT SSH maps, given the differences in spatial sampling between these two data sources?

Yes, the use of the interpolated DUACS ADT data product to derive the SSH-σ relationship could be viewed as a limitation of this approach. However, it is the only feasible method to construct this relationship as the glider spends relatively little time within individual SWOT swaths; this is also true for the Argo float and ship-based profiles. Nevertheless, we believe that our approach closely parallels the construction of larger-scale Sat-GEMs for the ACC. The use of DUACS still captures a local "climatology" that is appropriate for the relationship between SSH and interior density variations that are associated with balanced, mesoscale motions; this is also consistent with application of the QG-omega method. Furthermore, as multiple profiles are collected within each SSH bin, error or uncertainty associated with using the interpolated DUACS ADT field should be unbiased, or should at least tend to toward being unbiased with a sufficiently large number of profiles.

We have not made any changes to the manuscript for this specific comment although replies to comments from the other reviewer have clarified a few related points. We hope and expect others in the community to further test and iterate on this approach.

(c) The GEM reconstruction is validated by reconstructing the potential density from SSH along the path of one of the gliders and comparing this with the true potential density (Fig 3c,d). It wasn't clear to me if the data from this glider path were used in the deriving the SSH-potential density relation that was used to reconstruct Fig 3c. If so, it would be better to withhold some of the glider data from the GEM reconstruction as an independent verification of the method. Alternatively, if there were any other independent density measurements (Argo floats, CTD profiles) collected from the region during the campaign, this would give further confidence in the method.

This is a great suggestion. After spending some time working on the inter-calibration of sensors across platforms from the FOCUS cruise, the revised manuscript does introduce independent density measurements for validation of the GEM reconstruction. We continue to determine the SSH-σ relationship using glider SG681, but we have now produced reconstructions of density from both SG681 and a second glider, SG659.

Figure 2 in this reply shows, from top to bottom, SSH interpolated to the position of each glider (SG681 in the left-hand panels; SG659 in the right-hand panels), potential density measured by the gliders, reconstructed potential density from the SSH-σ relationship, and the difference between measured and reconstructed density. In both cases, the density differences peak around 0.3 kg m^{-3} although these magnitudes are limited to regions of strong lateral density gradients, as discussed in the manuscript. For most of the time series the density differences are quite small $O(0.1)$ kg m^{-3}; the difference is also dominated by large-scale gradients: the reconstruction tends to be smaller than observed density south of the Polar Front and greater than the observed density north of the Polar Front. This large-scale pattern most likely occurs because of smoothing of the sharp density gradient across the Polar Front. The smoothing can be adjusted by individual users. Critically, uncertainty in the reconstruction of glider SG659, based on a relationship derived only from glider SG681, has the same magnitude of as the comparison between SG681 observed and reconstructed densities. The density difference in SG659 peaks at a slightly higher value around day 10, but that is also related to the fact that SG659 records a stronger along-track density gradient. As the reviewer suggests, this provides a more robust validation of the density reconstruction.

We considered updating Figure 3 in the main text to include the separate glider, SG659, but the value of using SG681 is that the reconstructed potential density can be directly compared to the observed potential density in Figure 2. Therefore, we have added a new supplementary figure (Supplementary Figure 4) that includes reconstructions of both gliders so that readers can compare the two reconstructions in detail.

(2) Vertical velocity reconstruction.

The use of the QG omega equation to reconstruct vertical velocities from subsurface velocity and buoyancy (density) fields has been demonstrated in several previous studies (which are appropriately cited here). The method has some well-known limitations, in particular the small-Rossby number limit of QG theory and the method used for the inversion of the omega equation (Equations 1, 2). The method is well-described in this study, and validated using the numerical model. However, I have some (possibly naive) questions that might help further clarify the methodology:

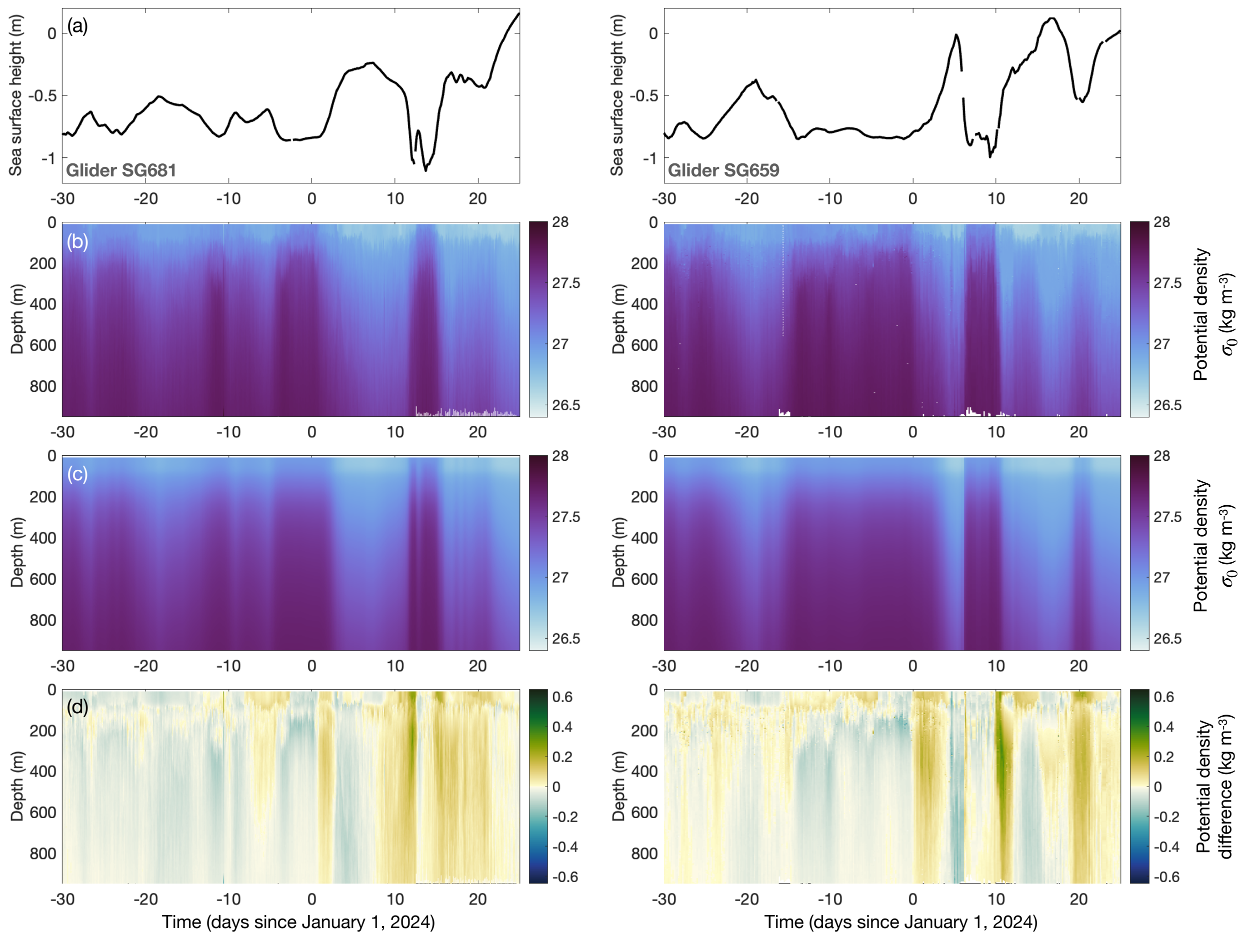

Figure 2: **Density reconstruction for two ocean gliders, SG681 (left panels) and SG659 (right panels).** (a) Sea surface height (m) interpolated to the position of each glider using the DUACS ADT daily product. (b) Measured potential density (σ_0, kg m^{-3}) as a function of depth and along the glider trajectory. (c) Reconstructed potential density (kg m^{-3}) as a function of depth and position along the glider trajectory using the SSH values in panel (a) and the SSH-σ_0 relationship in Figure 2b (kg m^{-3}). (d) Difference between measured and reconstructed potential density (difference between panel (b) and (c)).

We provide direct replies to the questions below, but we also wanted to add some additional thoughts on the applicability of the QG-omega equation and its use in this study.

Use of the QG Omega equation:

First, it should be noted that despite the large SSH gradients in this region, inferred vorticity values that lead to Ro ≈ 1 are still quite rare. Strong vorticity values in the data set are typically around Ro ≈ 0.5, which of course breaks the small-Ro assumption of quasi-geostrophic dynamics, but only marginally. Furthermore, studies have shown that QG dynamics are often robust outside of the strict Ro $\ll 1$ regime.

At fronts with steeply sloping isopycnals, we acknowledge that the vertical velocity field w is more accurately captured by the semi-geostrophic (SG) form of the omega equation, often referred to as the Sawyer–Eliassen (SE) equation, than by its quasi-geostrophic (QG) counterpart (Hakim & Keyser 2001, Thomas *et al.* 2008). Hakim & Keyser (2001) derive both SG and QG solutions of the 2D (x, z) SE equation (see also Thomas *et* *al.* 2008 and Siegelman *et al.* 2020), which leads to the scaling,

$$\frac{w_{SG}}{w_{QG}} \sim \left(1 + \mathrm{Ro} - \frac{1}{\mathrm{Ri}}\right)^{-1/2}, \tag{1}$$

where Ro is the Rosbby number ($\mathrm{Ro} \equiv \zeta/f$) and Ri the Richarson number ($\mathrm{Ri} \equiv f^2N^2/|\nabla b|^2$). Siegelman *et al.* (2020), using high resolution *in situ* observations, found that the factor w_{SG}/w_{QG} is typically smaller than 2. There are, of course, a wide range of Ro and Ri combinations in this data set, but Ro peaks around 0.5 whereas small values of Ri are typically between 1 and 10.

(a) If I am following the method correctly, the "forcing" on the right-hand side of the QG omega equation involves gradients in the subsurface buoyancy field and the subsurface horizontal velocity field (Equation 2). In turn, the subsurface velocity field is derived from the buoyancy field (and the surface SWOT velocities) using the thermal wind equation (5). Finally, the buoyancy field itself is derived from the SWOT SSH field using the GEM method. Is there a danger of "double dipping" here, since the subsurface buoyancy fields and velocity fields appearing in Equation (2) are not in fact independent? Might errors in the SWOT data amplify as successive gradients are applied? Is this why the Gaussian filter is applied after each spatial derivative?

We thought critically about the use of the density field (and the thermal wind relationship) to arrive at a three-dimensional map of the interior velocity field, and it is certainly true that the reconstruction of the interior velocity field is incomplete. In particular, the reconstruction misses contributions from ageostrophic re-stratifying motions that occur exactly where the diagnosed w is elevated. In fact, these motions can lead to an asymmetry across fronts even if the background flow is assumed to be in balance (Bai & Callies, in review). The justification for our approach is that our analysis focuses on spatial scales where the thermal wind relationship should make the dominant contribution to variations in the velocity field. For this study, the most straightforward way to validate this approach is the use of a high resolution numerical model. We provide a preliminary analysis of the model output in this study, which we feel supports the results from the observational analysis sufficiently to share these vertical velocity estimates with the community. However, members of our project team are also conducting a follow-on study where the sensitivity to processing choices are explored more robustly and with a higher resolution numerical model. This regional, higher resolution numerical simulation was not available during the preparation of this manuscript.

The need to estimate multiple derivatives of the SSH/buoyancy field led to noisy vertical velocity distributions when not smoothing at each step. This is consistent with other SWOT studies that have attempted to estimate vorticity from SWOT observations (e.g. Skinner *et al.* 2026). We are aware of a growing number of approaches for isolating the balanced component of the SSH field (Zhang *et al.* 2025). Admittedly, some of these are more sophisticated than our Gaussian filtering, and could certainly be adopted in future studies that use the QG-omega approach. At the same time, the SWOT Velocity working group has been engaged in filtering discussion where a linear Gaussian approach has been advocated. We feel that it is important to be clear about the filtering approach we have applied, and that it is beyond the scope of the present study to revise our processing in this way.

We have now added a sentence to the discussion that highlights the Zhang *et al.* (2025) study, which was not accepted at the time of our manuscript's submission, and the ongoing discussion of SWOT filtering.

(b) Vertical velocity could also be calculated directly from the horizontal velocity field at each depth using the 3D incompressibility condition. How does this reconstruction compare with the omega equation?

Thank you for this suggestion. We have now included a supplementary figure that includes distributions of vorticity, strain, and horizontal divergence for the depth-along swath section shown in Figure 4. The divergence field does show some similarities to the diagnosed vertical velocity field. The divergence (normalized

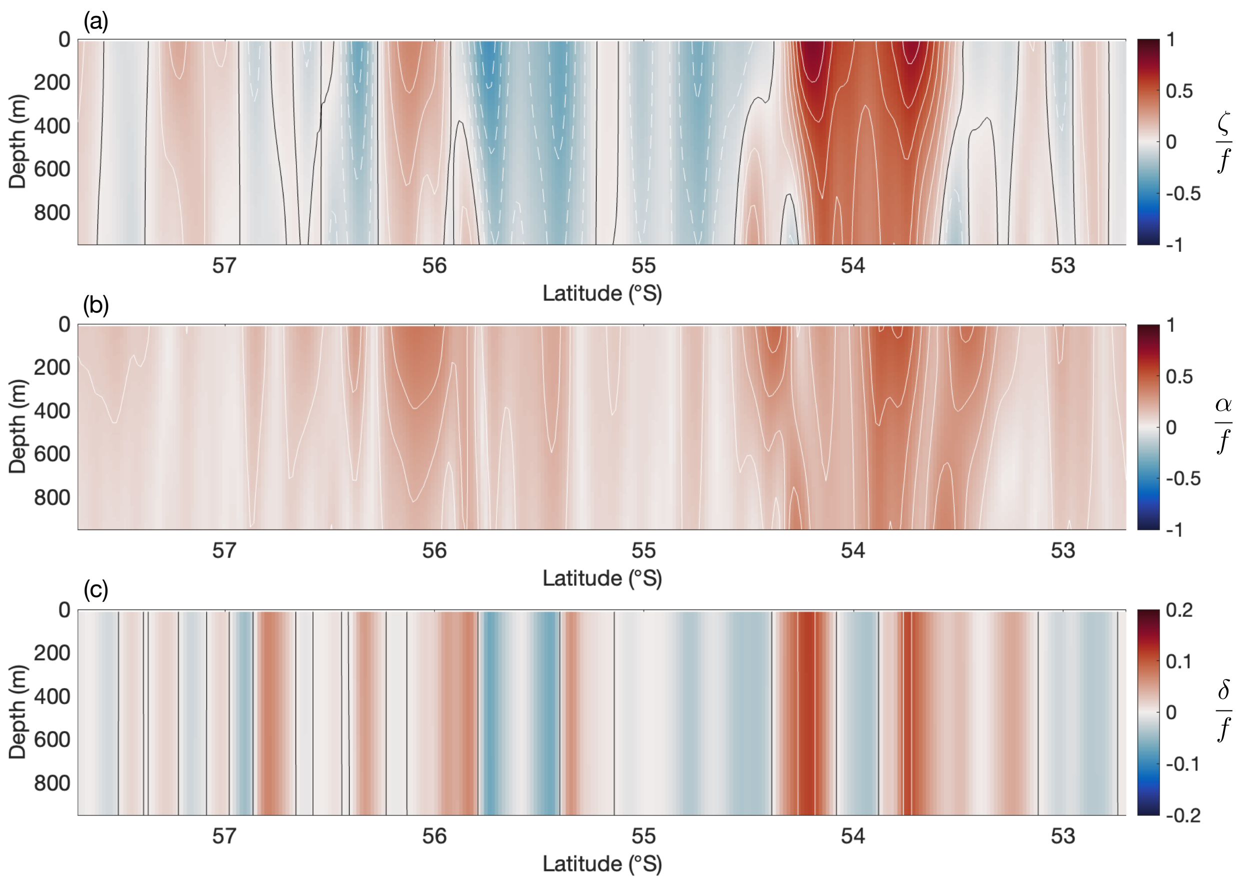

Figure 3: **Vorticity, strain, divergence.** Distributions of normalized (a) vorticity (ζ/f), (b) strain (α/f), and (c) divergence (δ/f) for the along-swath section shown in Figure 4e-h. There are no vertical variations in the divergence field because the velocities are determined from the thermal wind relationship.

by the negative Coriolis frequency) takes the opposite sign of w, consistent with downwelling where $\delta/f > 0$ (convergent regions) and upwelling where $\delta/f < 0$ (divergent regions). However, the divergence field does not have any vertical structure because the interior horizontal velocities are diagnosed using thermal wind. Determining the magnitude of w from the horizontal divergence requires an assumption about a reference value for w. Taking $w = 0$ at the surface would imply an exceedingly (and continually increasing) w at depth. Therefore, applying divergence from the inferred velocity field is not an appropriate means for estimating w.

We have added a new Supplementary Figure (Supplementary Figure 5) and have added a brief discussion of the vertically uniform divergence field.

(c) Why was a Dirichlet boundary condition applied at $z = -2000$ m, rather than directly applying a Neumann condition at $z = -1000$ m, if this is the desired result? Why are periodic boundary conditions applied in both the across-swath and along-swath directions? Is the curvature of the domain significant enough that the periodic box is not exactly rectangular?

Unfortunately the `xinvert` software package that is used to solve for w does not currently permit mixed boundary conditions. Therefore, we apply this approach to arrive at a close approximation to the Neumann boundary conditions. We have made that point clearer in the Methods section (line numbers 180-182).

While eventually it would be nice to have a solver where the appropriate boundary conditions can be applied, comparison with the numerical model gives us confidence that this makes a small contribution to the uncertainty in the vertical velocity estimates.

Periodic boundary conditions were applied to both the along-swath and across-swath directions for simplictiy. It is true that we could choose to only tile in the across-swath direction and then zoom in on a latitudinal range of interest, assuming that only the boundary regions are affected during the inversion. Another approach to tiling and using periodic boundary conditions is to interpolate either over the nadir gap or between swaths. This is the approach used by Carli *et al.* (2025) and others for the surface QG inversion for vertical velocity. However, because of the temporal separation between swaths, we decided not to use this method.

(d) Velocity distributions: The lack of a persistent skewness in the vertical velocity is curious. Was the same thing observed in the numerical model? Even if the vertical velocity distribution is symmetric, there could still be a net vertical transport if there are correlations between the vertical velocity field and tracers such as heat. It is pointed out in the Discussion section that this would involve separate measurements of (e.g.) temperature or salinity, which are not available everywhere. However, it should be possible to do this analysis along the glider paths, which may give some indication if there is a correlation between vertical velocity and temperature/salinity. Likewise, this calculation should be possible in the numerical model.

Many studies have shown that relative vorticity is characterized by a strong positive skewness ($\sim 1.5-2$). This is explained by the presence of axisymmetric small-scale cyclones with large vorticity values (up to $4f$) whereas anticyclonic vorticity is limited to -f because of inertial instability. On the other hand, divergence driven by frontogenesis is characterized by a much smaller skewness ($\sim 0.1-0.4$), since there is no radial strain around axisymmetric eddies, and therefore no frontogenesis and associated divergence are generated. The numerical simulation used in this study also reveals a clear positive skewness in vorticity (cyclonic dominance, skew = 0.4), while the horizontal divergence remains only weakly skewed (skew = -0.15). The vertical velocity distribution is similar to divergence distribution. Both high-resolution numerical simulations and analyses of observational data have reported these different skewnesses; these studies include Rocha *et al.* (2016), Barkan *et al.* (2019), and Torres *et al.* (2024). We added a couple of additional sentence to the discussion (starting at Line 241) that addresses this expected difference in skewness between relative vorticity and divergence.

We appreciate the comment about estimating heat and other tracer fluxes in this study; this would be similar to the calculation that was performed in the *Nature Geoscience* publication led by Lia Siegelman in 2020. We are extremely interested in the ability to use the QG-omega equation to estimate tracer fluxes, which is the focus of on-going work. However, this is not simple to do along the glider track because, as mentioned above, it is rare that the glider and SWOT are sampling at the same spatial and temporal location. We feel that there is sufficient information in the manuscript between describing the vertical velocity inversion method and the statistical properties of w that an additional nuanced discussion and analysis of tracer fluxes would be beyond the scope of this manuscript. We hope to follow this paper with new results on tracer fluxes soon (especially making use of surface ocean color and *in situ* backscatter observations)!

Minor points:

L47 and elsewhere: Not sure what the convention is, but it seems strange to me to use a superscript for in-line citations.

We have modified these in the manuscript by adding the author names.

L169: Again, this is a convention issue, but I don't usually see punctuation (:) before an equation.

We have changed the colon to a comma.

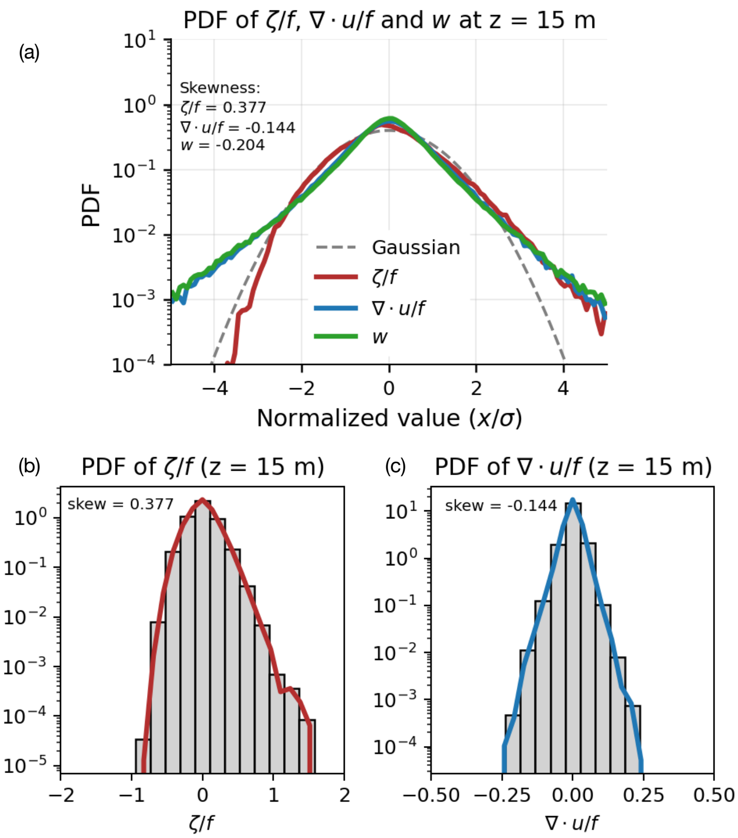

Figure 4: **Skewness of model output.** (a) PDF of vorticity (red curve), horizontal divergence (blue curve), and vertical velocity (green curve) from the 3-km COMPAS model output. The vorticity and divergence curves are expanded in panels (b) and (c), respectively, to highlight the difference skewness characteristics.

L239: remove extra parenthesis

We have made this change.

L387: Please provide reference or further information for the statement "the velocity field has strong agreement with..."

We have modified these sentences so that it is now clear that the agreement refers to the study cited in the previous sentence (see line 394).

Following analysis detailed in [41] [Tranchant *et al.* 2025], we use the SWOT SSH field to generate a surface velocity field using cyclostrophic balance based on a 9-point stencil. This study showed that the estimated velocity field has good agreement with de-tided velocities measured using surface drifters in the same region.

Equation 6: What are the typical Ro and Ri values here?

There are a range of values (and combinations of Ro and Ri), but peak values of Ro are around 0.5 and minmium values of Ri are typically 1-5.

L404: remove period at start of line.

We have made this change.

L427: Please provide reference or further information regarding the ”separate study”. Is this planned future work?

This sentence referred to planned model development and future work. As we do not anticipate that this study will be published in the next few months, we have removed this sentence.

References

Skinner, J., J. Callies, A. Lawrence & X. Zhang, 2026. Isolating balanced ocean dynamics in SWOT data. *in review*.

Zhang, X. & J. Callies, 2025. Assessing submesoscale sea surface height signals from the SWOT mission. *J. Geophys. Res.*, **130**, e2025JC022879.

The three-dimensional structure of fine-scale, vertical velocities inferred from SWOT and glider observations

Reply to Reviewer 2
February 2026

This article estimates vertical velocities with a new approach: exploit the gravest empirical mode method (GEM) to establish a SSH-σ_0 relationship using glider data, which is then used to produce profiles within the high resolution SWOT swaths. Additionally, a 3D interior velocity field is constructed from surface velocities and thermal wind, which indicate that large Rossby numbers are present well below the mixed layer. The availability of 3D velocity and density fields then allow the use of the QG-omega equation to infer vertical velocities. The method is validated with a numerical model. High vertical velocities are found at the periphery of an eddy with high vertical coherence down to 1000 m.

The method seems sound and superior to SQG. It has high potential to be applied to further high resolution satellite (SWOT) data, given the availability of concurrent in situ below surface T,S data. One caveat here seems to have been that the SSH-σ_0 relationship could not be established here from the SWOT data directly due to insufficient glider data beneath the swaths. Another caveat seems to be that QG-omega assumes low Ro, however large vertical velocities are found and high Rossby numbers, which appears to be a contradiction? Also, it should be stressed more, what is the significant new result here apart from the new method, since these large vertical velocities at the peripheries of eddies have been found before (and also its vertical coherence down to large depths?).

We thank the reviewer for their time in reviewing this manuscript, your positive assessment of the manuscript, and the constructive comments regarding clarification of the novelty of the method. Please find replies to each of the points in the review; our replies are presented in blue and changes made to the text are presented in red.

Abstract:

Specify what is novel in the method exactly.

We have now clarified that this is, to our knowledge, the first attempt to reconstruct a high-resolution, three-dimensional vertical velocity field using the QG-omega method. This approach has been attempted previously based on sparse observational data, which has typically relied on 2D approximations of the QG-omega equation, e.g. Siegelman *et al.* (2020), or significantly smoothed interior density distributions, Ruiz *et al.* (2009). Additionally, the ability to reconstruct vertical velocities from multiple SWOT swaths allows an “observational” analysis of vertical velocity statistics that have only been available from numerical models.

We appreciate that a significant fraction of the manuscript is devoted to describing QG-omega new method, which we feel is important so that future studies can leverage this approach. However, we also feel that the study provides deeper insight into vertical velocity distributions in an energetic region of the Southern Ocean as compared to previous studies. We have revised the abstract to emphasize these fine spatial scales:

“Here, we diagnose vertical velocities by exploiting the high resolution sea surface height observations collected by the Surface Water and Ocean Topography (SWOT) altimeter along with interior hydrographic measurements from ocean gliders. A reconstructed density field is combined with SWOT-derived surface currents to estimate ocean vertical velocities through the quasi-geostrophic (QG) omega equation at spatial scales roughly an order of magnitude finer than previous studies.”

Set the number of 150 m/day in perspective. What do you compare this number to? Is this number a novel result?

We have indicated that while vertical velocities of this magnitude have been suggested in previous studies, our study is able to show that velocities of this magnitude are statistically robust; they are frequent occurrences in these energetic regions of the Southern Ocean. Due to the length constraints of the abstract, we have not made a change here, but have clarified this point in the main text.

An additional sentence has been added to the Conclusion section that states: "This study also indicates that these vertical velocities are statistically robust and are expected to occur frequently in energetic regions of the Southern Ocean."

Introduction:

The terms mesoscale and submesoscale are mentioned multiple times in the introduction. Do you define these terms here with respect to scale or dynamical regime (Rossby number)? This seems relevant since QG-omega assumes low Rossby numbers.

The original manuscript included the sentence: "Vertical velocities become elevated at smaller scales where the Rossby number approaches unity and ageostrophic motions become more prominent, with magnitudes occasionally reaching many tens to hundreds of m day^{-1} ..." We have further emphasized in the Introduction that our discussion of mesoscale and submesoscale motions are framed in terms of dynamics (magnitude of the Rossby number). However, it is important to keep in mind that our processing of the altimetry data is scale-dependent. In other words, we choose a filtering scale, rather than assessing changes in Ro.

line 79: what do you mean by "local" at this point?

Thank you for this comment. The use of "local" here was to distinguish our derived SSH-σ relationship from the circumpolar SatGEM efforts as in Meijer *et al.* (2011) and Wyatt *et al.* (2025). We have made this distinction clearer in the revised manuscript.

Fig 1: bathymetry contour levels are barely visible. Add map of greater area to show where the location is.

We have updated the figure with a darker and thicker bathymetric contours in panel (a). We also added a small inset plot that shows the location of the FOCUS study region south of Australia and New Zealand.

Use same color scale for Fig. 2c and 3c.

We purposefully chose different color scales for these two panels. Figure 2c shows the density measured by the glider, whereas Figure 3c shows density anomalies/differences. We selected a divergent color scale for Figure 3, rather than the unidirectional scale in Figure 2. We have not updated the figures for this reason. Note that in response to Reviewer 1, we now provide a reconstruction of two separate gliders (SG659 and SG681); the full reconstructed potential density and the density difference (divergent colorscale) are provided as the new Supplementary Figure 4.

Fig.4: Magnitude of Rossby number is hard to decipher. It seems well above 0.1 in the frontal regions and this is precisely where the vertical velocity estimated from the omega equation requiring Ro $\ll 1$ is large?

Thank you for this comment. In an earlier version of the manuscript we included a panel that showed the magnitude of Ro. We have added a figure to the Supplementary Material that shows the magnitude of Ro. Despite the large SSH gradients in this region, inferred vorticity values that lead to Ro ≈ 1 are still quite rare. Strong vorticity values in the data set are typically around Ro ≈ 0.5, which of course breaks the small-Ro assumption of quasi-geostrophic dynamics, but only marginally. Furthermore, studies have shown

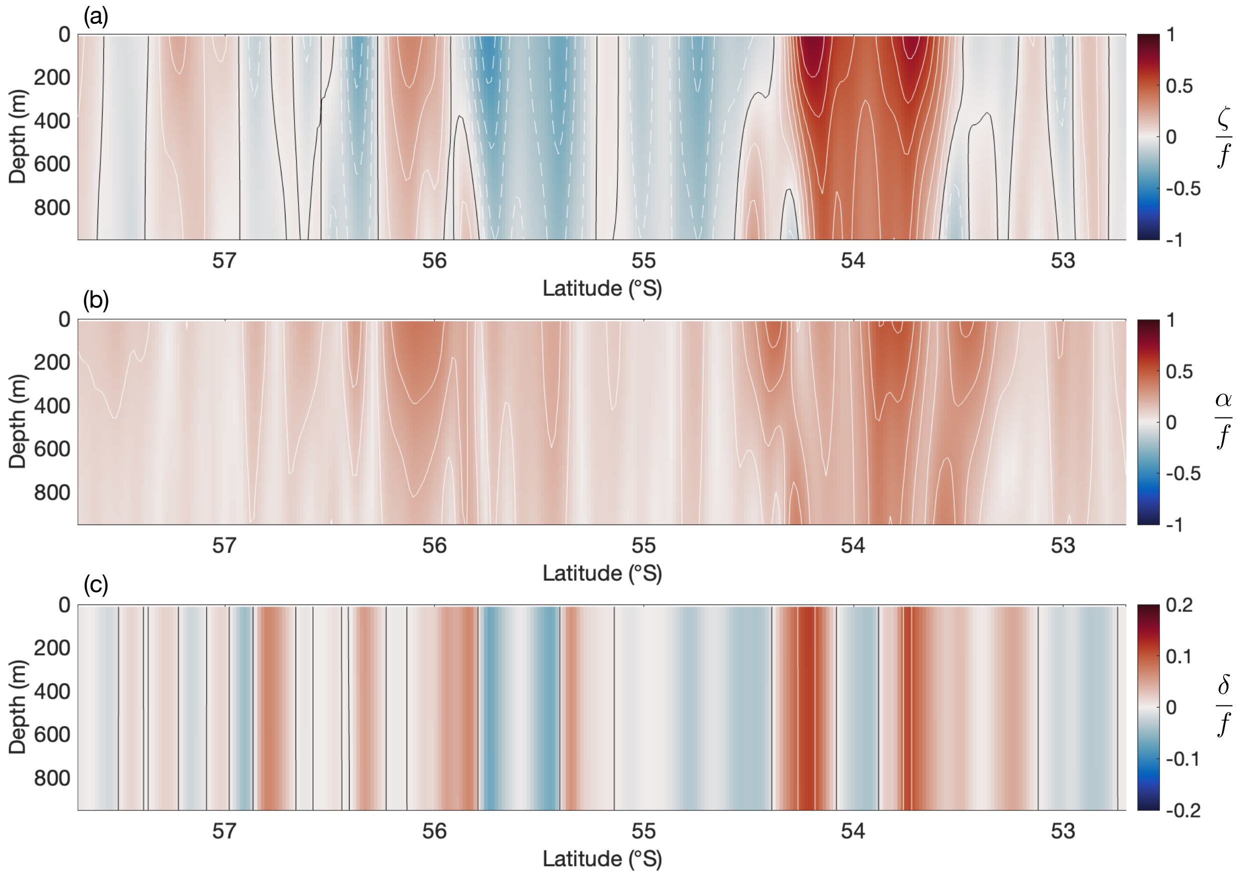

Figure 1: **Vorticity, strain, divergence.** Distributions of normalized (a) vorticity (ζ/f), (b) strain (α/f), and (c) divergence (δ/f) for the along-swath section shown in Figure 4e-h. There are no vertical variations in the divergence field because the velocities are determined from the thermal wind relationship.

that QG dynamics are often robust outside of the strict Ro $\ll$ 1 regime.

Please see the new Supplementary Figure 5, which shows distributions of vorticity, strain, and horizontal divergence for the depth-along swath section shown in Figure 4.

Fig. 7: What are the Rossby numbers in the model?

The Rossby number has an RMS value of 0.20 at 15 m depth in our 3-km model (see Figure 2 below). This indicates that the resolved flow is predominantly associated with a moderate Rossby number regime (Ro<1), consistent with QG dynamics at the mesoscale. However, the distribution also exhibits a pronounced tail extending beyond Ro > 1, indicating the presence of localized ageostrophic dynamics. This is consistent with the 3-km horizontal resolution of the model, which only permits the resolution of the upper range of submesoscale dynamics. We are conducting a follow-on study using higher-resolution simulations to better test the sensitivity of this approach in high Rossby regime.

Discussion:

What is the difference in vertical velocity magnitude from SWOT versus the traditional SSH DUACS, stress the benefits/novelty of SWOT in terms of the velocities.

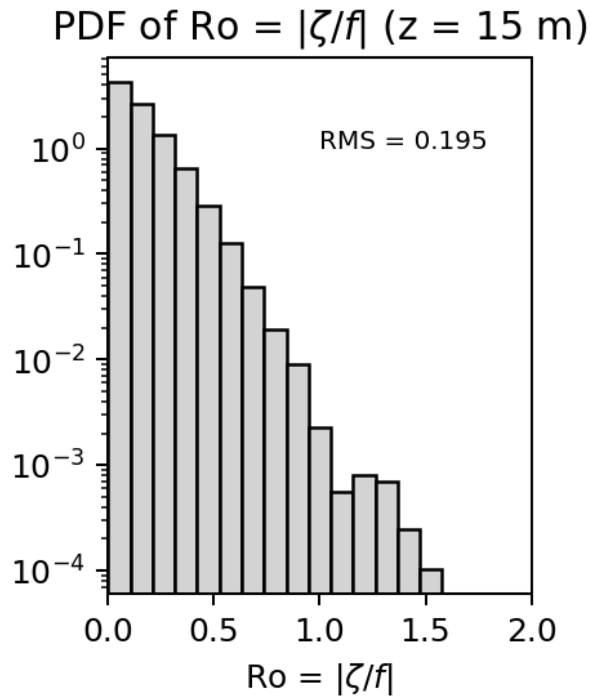

Figure 2: Probability distribution function of the magnitude of the Rossby number ($|\zeta|/f$ from the 3-km COMPAS model.

This is a good suggestion – we did not directly compare to the magnitude of the vertical velocities between a SWOT-based and DUACS-based QG-omega calculations. However, the QG-omega equation has been used by other studies, e.g. Ruiz *et al.* (2019) using traditional gridded altimetry. Even in regions with strong mesocale features, this gives rise to vertical velocities that are typically only a few 10s of m day^{-1}. Furthermore, in Carli *et al.* (2025), w reconstructed using eSQG from SWOT are typically about a factor of 2 larger than those computed from gridded products SSH products.

line 258: so these previous studies did not provide the 3D structure, or not with that resolution? Stress here what distinguishes this study from the others.

Please see the comment above in the Abstract section. Yes, the novelty is in the ability to construct full three-dimensional maps of vertical velocity and also to allow a first look at statistical properties of vertical velocity over a period of one month. On-going future work is exploring whether this can be extended to larger spatial and temporal scales and to begin to probe seasonal to inter-annual variability in these quantities. However, we feel that this is beyond the scope of the present study.

The three-dimensional structure of fine-scale, vertical velocities in the Southern Ocean inferred from space

Reply to Reviewer 1
April 2026

I thank the authors for their thoughtful and detailed responses to my comments in their manuscript. I am more than satisfied that they have addressed all of my questions and am happy to recommend this manuscript for publication. Well done Andy and colleagues on an exciting and impactful study!

S.Keating, UNSW Sydney

We thank the reviewer again for their time and constructive comments, which improved the manuscript.

Reply to Reviewer 2
April 2026

Thank you for addressing all comments. I just have one minor request: you mentioned studies that show that "QG dynamics are often robust outside of the Ro $\ll$ 1 regime". Please include these citations in your manuscript.

We searched our manuscript and did not find this statement. We believe this statement was removed during an earlier iteration of the manuscript. Therefore we have not made additional changes to the text.