

Editor Comments, Author Responses and Manuscript Changes

We would like to thank the reviewer for his/her careful reading and constructive comments on the manuscript. These comments significantly improved the manuscript. We have responded to the comments made by the reviewer in this document. We hope that the reviewer and the editor agree with the changes we have made to make the manuscript be suitable for publication.

In the following, black indicates the comments by the editor; blue is our response.

Comments:

The response to my comments of the manuscript are sufficient in my view. Only minor corrections remain. These are below:

Minor comments:

1. L78: It's confusing to me why El Nino / La Nina are included in the introduction when there is no mention of it throughout the rest of the study.
We agree with your comment and that paragraph was removed from the text.
2. L132: The temperature, salinity, and oxygen sensors on the gliders seemed to not have any post factory calibration or validation. I understand that in the response to my first review, the interest is rather in variability than magnitude. Still, the magnitude of the data values may be incorrect as a result of sensor drift over time, which appears not to have been checked. This should be explained more carefully.
This is incorrect. All the sensors on the gliders are returned to the manufacturer on an annual basis. This includes CTD, DO and Ecopuck. Analysis of all the calibration data from the Ecopuck indicate no major changes over time span of a more than a decade (Thomson et al., 2026). Similar results were observed for the CTD and DO sensors. Note that the CTD is the same model as that on ARGO floats which are not calibrated after deployment.
3. L134: Horizontal currents using the DAC in regions influenced by boundary currents may be representative of many different dynamics, such as combined effects of surface flow and undercurrents, coastal upwelling, eddy-driven transport, and interactions with variable bathymetry. Given the heavy leaning of Rossby dynamics put on the glider DAC, I feel that there needs to be a better explanation about which of these dynamics the DAC is representing.
Thank you for your comment and concern. In our understanding, the identification of sub-mesoscale structures relies on the coherence between DAC patterns, hydrographic anomalies, looping trajectories, and complementary observations rather than on DAC measurements alone.
4. L152: Is it reasonable to use wind data 25 km inland? Why not use a satellite or reanalysis product instead?
This is a misunderstanding the wind record was collected 25 km offshore off the coast on Rottneest Island. We have identified the location on Figure 1c.
5. When using Ro numbers to indicate ageostrophic motions, be careful to use the absolute Rossby number, $|Ro|$, when referring to cyclonic rotation as you do now, as these are in fact negative $Ro < 0$. For instance, L381 “cyclonic eddies had $Ro > 1$ ” is not true. It is rather $Ro < -1$ or $|Ro| > 1$.
Done.

6. L392: how is it determined that the anti-cyclonic eddies were in cyclostrophic balance despite low Ro numbers?

Although these eddies did not present $|Ro| > 1$, the observed internal signatures of both upwelling and downwelling suggest that the flow was not purely geostrophic. This indicated that centrifugal acceleration may contribute to the momentum balance, potentially leading to gradient-wind or partial cyclostrophic balance (e.g., Figs. 6, 7).

7. The DWST paragraph is speculative to me. Do DWST have temperature and salinity properties that can be cross-referenced against your observations of deeper water upwelling? Can you provide more evidence to support your argument of upwelling DWST? We have removed the reference to deep water upwelling DWST.

8. L476: Does the 33-hour low-pass filter reduce HF radar velocity variability such that Ro numbers are reduced significantly?

Thank you for your comment. Although the 33-hour low-pass filter may reduce velocity variability, our sensitivity analysis indicates that its impact on the estimated Rossby numbers is limited. We repeated the calculations shown in Fig. 13 using unfiltered HFR velocities and obtained similar results (see Fig. 1 below). We therefore attribute the relatively low Ro values mainly to the coarse spatial resolution of the HFR dataset.

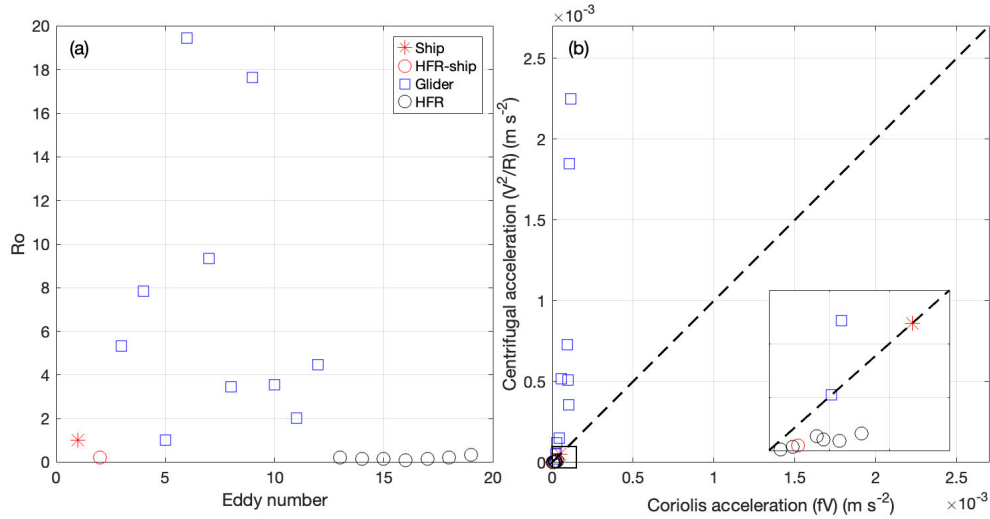


Figure 1 - (a) Rossby number ($Ro=V/(fR)$) estimated for the selected events using different observing platforms: ship (red asterisk), **unfiltered** HFR–ship (red circle) matching with ship transect (Event 1), glider (blue square), and **unfiltered** HFR (black circle). (b) Centrifugal acceleration (V^2/R) versus Coriolis acceleration (fV). The dashed line in (b) indicates the 1:1 relationship (cyclostrophic balance).

9. A few more typos were spotted. E.g. L429. Check carefully. The manuscript was revised and all typos were corrected.

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Comments:

The manuscript “Three-dimensional structure of sub-mesoscale eddies on the continental shelf” by Bitencourt et al. presents a multi-platform observational analysis of submesoscale eddies (<10 km) on the continental shelf off southwestern Australia, using satellite observations, HF radar, ship measurements, autonomous gliders, and moorings.

I found the topic original and potentially important, as relatively few studies have addressed the characterization of submesoscale eddies in shallow continental shelf environments. The observational dataset assembled by the authors is impressive and offers significant potential for advancing our understanding of shelf submesoscale dynamics.

Overall, I believe the manuscript deserves publication in Ocean Science. However, the present version does not yet fully exploit the richness of the dataset nor effectively convey the significance of the results. I therefore recommend major revisions before publication.

General Comments:

The manuscript would benefit from:

1. clearer and more rigorous definitions of the submesoscale regime and eddy properties;
Done.
2. a more transparent methodological description, particularly regarding vorticity estimation;
We thank the reviewer for this suggestion. We revised the methodology section to provide a clearer and more transparent description of the different approaches used to estimate relative vorticity and Rossby number across the observational platforms. In particular, we now explicitly distinguish between direct vorticity calculations from the two-dimensional HFR velocity fields and the order-of-magnitude shear-based proxies derived from glider and shipborne ADCP observations, while also clarifying the associated assumptions and limitations.
3. improved consistency in terminology and notation throughout the manuscript (Table 2 and Fig. 5-12)
Done.

Specific comments:

4. Title: I would suggest refining the title by including the study region, e.g. “... on the western Australian shelf”, since this is fundamentally a regional study and no attempt is made to generalize the findings globally.
Thank you for your comment. However, we believe this title is appropriate as it can also describe sub-mesoscale eddies (and peddies) behaviour and anatomy in other similar regions. For such reason, we decided to keep the title as is.

5. L6: “O~days” → “O(days)”.

Done.

6. L8: “peddies”: is this a newly introduced term for small submesoscale eddies? I am not familiar with it and suggest defining it clearly upon first use.

Yes. This is a definition created by Pattiaratchi and Mihanović (2014) for describing sub-mesoscale eddies along the shelf with diameters < 10 km and lifespans of 1-2 days, occurring in water depths <200 m and occupying majority of the water column. We have defined it when first used (lines 11-12 in Abstract).

7. L6 / L31 / L92: The spatial scale used to define “submesoscale” should be homogenized throughout the manuscript (10 km vs 20 km). More generally, the manuscript should acknowledge that there is no universal spatial threshold for the submesoscale regime, since it depends strongly on the local Rossby deformation radius (ie latitude and stratification).

Thank you for your comment. We revised the text and consistently improved the terminology of sub-mesoscale throughout the manuscript. For instance, we included the following in L43-50:

“These features occur in the transition between mesoscale dynamics and small-scale turbulence (Thomas et al., 2008; Kubryakov et al., 2022). **Sub-mesoscale processes are typically characterised by horizontal scales < 20 km, although their spatial extent is not universal and depends strongly on the local Rossby deformation radius and stratification. Their temporal scales generally range from hours to a few days.** Their strong ageostrophic nature can lead to Rossby numbers $\sim O(1)$ or larger and induce high vertical velocities in their core (10-100 m day⁻¹; Lévy et al., 2012; McWilliams, 2016; Thomas et al., 2008; Zatsepin et al., 2019).”

8. Figure 1: Please specify the contour definition in panel (b). I also found the labels difficult to read. (same comment for other figures like 8).

Contour lines in Fig. 1b refers to “bathymetry” in metres. This information was already included in figure caption as:

“Figure 1. (a) Remotely sensed chlorophyll-a on 28 March 2016, (b) long-term (2010-2018) averaged alongshore velocities from altimetry and HFR observations in Wadjemup Continental Shelf; and (c) HFR domain (dashed line), glider (coloured lines) and ship (red line) tracking, and mooring locations (coloured squares). **Bathymetric contours (in metres) are shown in (b,c).** Black dots at the shoreline in (a,b,c) represent HFR shore stations.”

9. L59: “remote forcing” → please clarify what processes are being referred to.

“Remote forcing” refers to the influence of large-scale oceanic and atmospheric processes originating outside the WCS, including the propagation of coastally trapped waves, variability associated with the Leeuwin Current system, and large-scale wind forcing that modulates shelf circulation in the WCS area. We have clarified this in the revised manuscript (L84-88) as:

“Complex circulation patterns across multiple scales characterise the WCS area, such as an anomalous poleward eastern boundary current, micro-tidal regime, wind-driven coastal circulation, sub- to meso-scale eddies, complex bathymetric features with a canyon reaching

as deep as 4000m, and circulation responses to remote forcing associated with the Leeuwin Current system and coastally trapped waves.” (L88-89).

10. L86–88: “peddies” are introduced only in L88. I suggest reversing the sentence order.
Done. Now defined in the Abstract (see #6 above).
11. L120: There is no mention of typical glider DAC values or of how the missions were designed. Since shelf deployments can be challenging, a short discussion of navigation constraints and operational issues would be useful for a broader audience.
The UWA glider operations have completed more than 450 successful missions around Australia with the majority on the continental shelf in depth < 200 m. The local marine environment is diurnal micro-tidal with tidal currents < 0.05 ms^{-1} . On the shelf the maximum wind driven currents are ~ 0.20 ms^{-1} . The largest currents are generated by the Leeuwin Current with currents > 0.50 ms^{-1} . These values were reflected in the DAC speeds recorded in over 100 glider missions in the study area over a period of 18 years. Yes, shelf deployments can be challenging but does differ from location to location therefore a generalisation is not possible and depends on the local knowledge of the major currents and strategies to avoid them – but are not always successful. We refer the reviewer to Pattiaratchi et al. (2017) for a summary. We believe that ‘discussion of navigation constraints and operational issues’ is outside the scope of this paper dealing with sub-meso scale eddies. However we have included a statement on the DAC values in the local system (L205-206).
12. L156: This statement seems applicable to submesoscale eddies in general, not specifically to “peddies”.
This information is now included in the text in L228-230 as: “To characterize the vertical structure and dynamical properties of sub-mesoscale eddies and peddies, we estimated relative vorticity and the Rossby number using velocity measurements from different observational platforms.”
13. L189: Numbering eddies (#1–#16) would greatly improve clarity and make Table 2 and all figures easier to follow. It would also help to indicate explicitly which observational platforms were used to characterize each eddy.
Done.
14. L236: It would be informative to provide the magnitude of glider DACs during the looping events.
We included DAC information (maximum value during the loops) in the following sentence to L339-341:
“The peddies had a high Rossby number with $|\text{Ro}| > 10$ and reached maximum DAC of 0.52 m s^{-1} and 0.51 m s^{-1} during the loops on 17 and 18 August, respectively (Fig. 5b).”
15. Figure 6: I find it difficult to believe that the eddy detection algorithm identifies nearly perfect circular eddies with similar radii across snapshots. The detected structures do not appear consistent with the velocity field, which looks significantly elongated in the north–south direction. Is this a result of the winds?
This is an artifact and limitation of the eddy detection algorithm, as is the case in other algorithms as well. The velocity field often exhibits elongated patterns, likely influenced by

background circulation variability and wind-induced shear. As our main goal when using the eddy detection algorithm was to estimate eddy position and size, we do not expect this limitation to significantly affect the overall interpretation of our results.

16. L373: How and where are eddy lifetimes estimated?

Eddy lifespan was estimated using the eddy detection and tracking algorithm (Nencioli et al., 2010) and applying the method described in Bitencourt et al. (2025): “Lifespan is defined at the first and last time when a sub-mesoscale eddy is detected within the HFR coverage.”

17. L388: Cyclostrophic balance is introduced rather abruptly here, before Sect. 4.3. This concept should be introduced earlier and more progressively.

We revised the manuscript to introduce this concept more progressively earlier in the discussion, providing additional context before discussing its relevance to the observed sub-mesoscale structures (see lines L493-496):

“High Rossby numbers associated with submesoscale features indicate that processes other than geostrophic balance become important. In these conditions, centrifugal acceleration may play a significant role, potentially leading to gradient-wind or cyclostrophic balance, especially within compact and intense vortices (e.g., McWilliams, 2016; Thomas et al., 2008).”

18. L435: Is this dense water formation process seasonal, as observed in other dense shelf water cascading regions?

Thank you for your comment. Yes, previous studies have shown that dense shelf water transport/cascading off Western Australia is strongly seasonal, occurring primarily during austral autumn and winter. Although higher density water is present in spring and summer, strong winds due to sea breezes results in a well-mixed water column (Pattiaratchi et al., 2011). This seasonality is associated with enhanced evaporation during summer followed by surface cooling and heat loss during autumn/winter, which increase nearshore water density and promote cross-shelf density gradients driving offshore dense water transport (e.g., Mahjabin et al., 2019, 2020; Mahjabin and Pattiaratchi, 2018). We clarified this in the revised manuscript as (L545-552):

“Previous studies showed that dense shelf water transport/cascading off Western Australia is predominantly seasonal, occurring mainly during austral autumn and winter when evaporation-driven salinity increases and surface cooling enhance nearshore water density gradients (Mahjabin et al., 2019, 2020). The colder and saltier upwelled waters observed in August 2010 and May-June 2015 (Figs. 6, 10, 11, 12) are consistent with hydrographic characteristics previously associated with DWST off Western Australia (Mahjabin and Pattiaratchi, 2018; Mahjabin et al., 2019, 2020), suggesting that DWST may contribute to the observed deeper upwelling signatures.”

19. L464: It is not surprising that the 4 km HF-radar product cannot resolve eddies of 1-5km radius. Thank you for your comment.

20. L497: This statement does not appear to arise directly from the results presented in the manuscript.

Although we mentioned this in results and discussion, we agree that is not one of the main remarks of this research and decided to remove from conclusions.

Major methodological concern:

21. The discussion in L165–176 is currently confusing, particularly regarding the derivation of vorticity from glider measurements.

We agree that the original text could give the impression that the full eddy relative vorticity was directly derived from along-track glider measurements. This was not our intention. Due to the limited spatial sampling of the glider observations, we could not reconstruct the full two-dimensional eddy velocity field or estimate vorticity in cylindrical coordinates (e.g., $\zeta \sim 2V_m/R_m$ for solid-body rotation; Bosse et al., 2016). Instead, we used along-track horizontal shear as a proxy to obtain an order-of-magnitude estimate of Rossby number and assess whether the observed structures were consistent with sub-mesoscale dynamics ($|R_o| \sim O(1)$). We follow the methodology used by Rossi et al., (2013). We revised the text extensively to clarify this limitation and distinguish between direct vorticity estimates from HFR data and approximate shear-based, one dimensional, estimates from glider and shipborne observations.

22. As written, it seems that the authors estimate vorticity directly from glider DAC measurements along-track. However, eddy vorticity fundamentally depends on both the eddy radius (R_m) and the maximum azimuthal velocity (V_m). In cylindrical coordinates, the vertical vorticity is: $1/r \, d/dr(rv)$ which reduces approximately to: $2V_m/R_m$ for a circular eddy in solid-body rotation (see, for example, Bosse et al. 2016).

Please, refer response to question #21.

23. DAC measurements can indeed help identify the eddy center, after which eddy radius and maximum swirling velocity can be estimated. However, the present manuscript instead appears to calculate vorticity directly along the glider track, which is difficult to interpret physically.

Please, refer to question #21.

Additional concerns:

24. L169: The choice of using only the zonal component appears somewhat arbitrary (probably because ship transects are primarily east–west). Estimating vorticity from only one velocity component (e.g. (dv/dx) or (du/dy)) will systematically lead to an underestimation by approximately a factor of two.

We agree that estimating relative vorticity from a single velocity component likely underestimates the magnitude of the full vorticity field. We clarified in the revised manuscript that the choice of the zonal shear component for the shipborne ADCP observations was dictated by the predominantly east–west orientation of the transect. We followed the approach by Rossi et al. (2013) who also used shipborne ADCP observations. We also added text acknowledging that these single-component estimates should be interpreted as qualitative, order-of-magnitude proxies for Rossby number rather than complete representations of the two-dimensional vorticity field.

25. L170: The statement that one velocity component is insufficient is not entirely correct. Assuming rotational symmetry around an eddy center, one component may already constrain the circulation structure well enough to characterize the eddy vorticity.

We agree that, under the assumption of approximate rotational symmetry, a single velocity component can still provide useful information about the circulation structure and associated vorticity. We therefore revised the text to remove the statement implying that one velocity component is inherently insufficient. Instead, we now clarify that the along-track shear measurements provide a useful qualitative estimate of circulation strength and Rossby number, while acknowledging the limitations associated with partial spatial sampling.

26. L173: If the objective is simply to determine whether the Rossby number is $O(0.1)$, $O(1)$, or $O(10)$, this should be stated explicitly at the beginning of the section, together with a more precise discussion of methodological limitations regarding Ro calculations.

We revised the beginning of the section to explicitly state that the objective of the Rossby number calculations was to obtain order-of-magnitude estimates (i.e., to assess whether Ro was $O(0.1)$, $O(1)$, or larger) rather than to reconstruct the full two-dimensional vorticity field. We also clarified the methodological limitations associated with the different observational platforms, particularly for the glider and shipborne datasets where only partial velocity information was available.

27. L176: I do not understand why the glider trajectory is fitted with an ellipse unless the glider is strongly advected and behaves quasi-Lagrangian, similarly to a drifter. If this is indeed the case, it should be clearly stated and justified. Vorticity calculation would then become quite different than what has been presented.

In the case shown in Fig. 5, the glider trajectory exhibited a looping pattern whilst entrained in the the eddy, suggesting temporary trapping and advection within the eddy core. The ellipse fitting was therefore used only as an approximate method to estimate the horizontal dimensions of the structure, rather than to derive vorticity dynamically from the trajectory itself. We agree that the glider cannot be considered a purely Lagrangian platform, and we revised the text to clarify the assumptions and limitations associated with this approach. Here is the updated text as follows (L262-266):

“For the glider observations, two methods were applied on a case-by-case basis: **(1) fitting an ellipse to looping glider trajectories in cases where the platform appeared temporarily trapped and advected within the eddy core, providing an approximate estimate of the horizontal scale of the structure**, and (2) defining the horizontal extent of the surface temperature anomaly.”

28. Finally, very little is said regarding the dynamical characterization using moored ADCP observations, although these measurements could potentially provide a more robust estimate of eddy kinematics and vorticity structure.

The moored ADCP observations were mainly used to characterize the temporal evolution and vertical structure of the observed features. However, because the moorings provide measurements at fixed locations without sufficient horizontal spatial coverage, they do not allow direct estimation of horizontal velocity gradients or two-dimensional vorticity fields comparable to those derived from the HFR observations. We have clarified this limitation in the revised manuscript and better described the role of the moored ADCP data in the dynamical interpretation of the observed structures. We included the following lines in L242-244:

“The moored ADCP observations were mainly used to investigate the temporal variability and vertical structure of the observed features, rather than to estimate horizontal vorticity, since the fixed-point measurements do not resolve horizontal velocity gradients.”

29. The methodology should therefore be revised and clarified substantially. This issue may also explain the large discrepancies in vorticity estimates between observational platforms.

Following the reviewer’s suggestions, we substantially revised and reorganised the methodology section to clarify the different approaches used for each observational platform and the associated limitations. In particular, we now explicitly distinguish between direct relative vorticity estimates derived from the two-dimensional HFR velocity fields and the

order-of-magnitude Rossby number proxies obtained from along-track glider and shipborne ADCP velocity measurements. We have also clarified that differences in spatial sampling and methodological assumptions between the observational platforms likely contribute to discrepancies in the estimated vorticity magnitudes.