Review of Insights Into Mesoscale Eddy Dynamics: A Three-Dimensional Perspective on Potential Density Anomalies

1 Summary

This manuscript investigates the 3D structure of mesoscale eddies based on in situ observations and theories. The authors decompose the eddy density anomaly into the spiciness mode and heaving mode, finding the heaving mode to be dominant. They further evaluate the vertical and horizontal structures of eddy density field in light of the quasigeostrophic and diffusion theories, concluding that the diffusion theory effectively captures the observed 3D eddy structure. Overall, I find the analysis and discussion to be thorough and insightful. The approach of decomposition into spiciness and heaving modes is novel, and the proposed diffusion theory offers valuable perspectives on the vertical structure of eddy density anomalies. However, several aspects of the analysis and interpretation require further clarification to strengthen the overall argument. I recommend a major revision addressing the following comments.

2 Major Comments

1. This manuscript focuses primarily on anticyclonic eddies and leaves the 3D structure of cyclonic eddies mysterious. I do not think that the dynamics underlying cyclonic and anticyclonic eddies are fundamentally different. In fact, previous studies (including those cited in the manuscript) have identified similar horizontal or vertical structures for mesoscale eddies of both polarities (Flierl, 1987; Chelton et al., 2011; Zhang et al., 2013). In the QG framework, there should be no difference in the horizontal and vertical structures between cyclonic and anticyclonic eddies, except for the sign of their anomalies. While the dataset used here only includes anticyclonic eddies, I suggest adding a discussion of whether and how the theories can be applied to cyclonic eddies. Would any adjustment be needed to extend the theoretical framework to account for cyclonic eddies?

2. The theoretical formulations for the eddy 3D structure (eq. (53)-(56)) contain 14 unknown parameters, which were estimated through fitting. Some parameters, such as B and D in equations (55) and (56), do not have a clear physical meaning. When the observation is sparse, such as Argo observations, it is unlikely to conduct the fit to estimate those parameters. Can any of the parameters be estimated directly from limited observations? For example, if R_1 and R_2 in equations (53) and (54) represent the radii of eddies, could they be estimated from satellite altimetry using an approach similar to that in Chelton et al. (2011)? Could x_1 and x_2 be estimated directly as the location of the maximum surface density anomaly rather than by fitting? The fitted values of R and H are different at different regions. Does this difference reflect physical characteristics of the local environment, such as the Rossby deformation radius and e-folding depth of stratification?

3. It is unclear how the parameters in equations (53)-(56) are estimated through optimization. The manuscript briefly mentions that the optimization is conducted for the vertical structure first and then for the horizontal structure, but it remains unclear how the horizontal and vertical structures are obtained from the observational data. For instance, the vertical structure function is said to be optimized at the eddy center. Is the eddy center at x_1 or x_2? What if the location of eddy center changes with depth when an eddy tilts vertically? Furthermore, is the vertical structure, when normalized by its maximum, consistent at different radial distances from the eddy center? In addition, Zhang et al. (2013) proposed a universal horizontal structure function for mesoscale eddies (their equation (2)), which includes a sign reversal of eddy pressure anomalies, beyond approximately 1.4 times the eddy radius (their figure 2). However, this feature does not appear to be present in figure 8 of the manuscript. I suggest comparing the observed horizontal structure to the function proposed by Zhang et al. (2013) to provide a better justification for the proposed framework.

3 Minor Comments

1. Equation (53) and (54): Is the α enforced to be an integer? It seems that if α is odd and $x - x_1$ is negative, the eddy anomaly will increase exponentially from its center, which is unphysical. Perhaps, the absolute value should be taken for $x - x_1$?
2. I am not sure whether the tilde above all variables is necessary. It seems that it does not have a special meaning and is in fact dropped in later sections. To make the derivations clearer, I suggest dropping tilde completely.
3. Line 119: I think η should be the vertical displacement of isopycnal with respect to the mean state, not the state of rest.
4. Line 134: I suggest dropping "we assume". You can estimate the typical density variation corresponding to the isopycnal height change, not subjectively assuming it.
5. Lines 141 and 170: Missing spaces between $\tilde{\cdot}$ and texts.
6. Equation (21): What's the difference between $T(r, \theta, \sigma_0)$ and $\tilde{\bar{T}}(\sigma_0)$? It feels that equation (21) is just zero equals zero.
7. Line 185: I do not think it is accurate to say "no exact analytical expression for $\phi(r)$ exists". Zhang et al. (2013) has proposed an analytical expression for the radial structure of eddies.
8. Line 208: "radius" $\rightarrow$ "radial distance".
9. Line 223: I suggest drop "assuming" for the same reason mentioned before.
10. Line 248: H_1 is a characteristic **depth** scale.
11. Table 1: A "vertical" is not capitalized.
12. Line 268: How exactly is the eddy center be determined from velocity analysis?
13. Table 2: The filter scale for x is different for different observations. What's the justification for the filter scale? Is it based on the local deformation radius?
14. Line 319 and others: "cutoff period" $\rightarrow$ "cutoff scale" or "filter scale".
15. Line 367: ψ_0 should be η_{z0}.
16. Line 373: "ψ and ξ " $\rightarrow$ "η_z and $\delta_z^2\sigma$"? I see no ψ and ξ in equations (53)-(56).
17. Line 374: There is an extra period.
18. Line 374: "locations where the amplitude of ψ and ξ are maximal" $\rightarrow$ "depth where the amplitude of η_z and $\delta_z^2\sigma$ are maximal"?
19. Line 389: Why is the water in an anticylconic eddy be colder than its surroundings?
20. Figure 8: Which depth are the ϕ and χ from? The depth where η_z is the maximum? Is $\phi(x)$ or $\chi(x)$ different at other depth?
21. Line 441: There is an extra "function".
22. Figures 9 and 10: Which location are the vertical profiles from? The eddy center? Is $\eta_z(z)$ or $\delta_z^2\sigma(z)$ different at different x?
23. Line 465: The theory here has more tuning parameters than the models of Flierl (1987) and Zhang et al. (2013). The comparison between them seems to be unfair.
24. Line 508: "reconstruct" $\rightarrow$ "reconstructed".

25. Figure 14: Labels a, b, and c are repeated in the second row, and there is no alphabetic label in the third row. Please correct them.
26. Line 549: “diffusivity” → “diffusion”.
27. Line 553: There should be more details about how the vertical extent is controlled by the stratification and what the vertical symmetry means.

References

Chelton, D. B., M. G. Schlax, and R. M. Samelson, 2011: Global observations of nonlinear mesoscale eddies. *Prog. Oceanogr.*, **91 (2)**, 167–216.

Flierl, G., 1987: Isolated eddy models in geophysics. *Annu. Rev. Fluid Mech.*, **19 (1)**, 493–530.

Zhang, Z., Y. Zhang, W. Wang, and R. Huang, 2013: Universal structure of mesoscale eddies in the ocean. *Geophys. Res. Lett.*, **40**, 3677–3681.