Reply referee 2

We would like to thank the reviewer for his/her review and constructive feedback. We appreciate the effort and time the reviewer has invested in evaluating our work. Please find our point-to-point response below in blue.

I found this to be a useful contribution to the literature on the structure of coherent anticyclonic eddies in the ocean. The authors essentially fit idealized horizontal and vertical structure functions to five coherent anticyclonic eddies. A key novelty, compared with earlier studies are the use of potential density, rather than velocity, fields in fitting the models to the observations. The authors find that heaving anomalies dominate over spiciness anomalies - which is perhaps not surprising, but nevertheless valuable to confirm. There is also an interesting finding regarding the horizontal structure of the eddies, where the exponential power is higher than Gaussian or even cubic exponential - indeed I thought that this point could have been highlighted in the conclusions. Overall, the manuscript is well written and should be published after minor revisions.

I note that the first reviewer has commented extensively on the parameter fitting/optimization process and, in particular, number of free parameters as well as the detailed methodology. I will not dwell on these issues here, on which I am not as expert, except to note that I agree with the points raised by the reviewer.

Specific points:

1. The first few sentences of the abstract need tightening. Firstly, I disagree with the statement "In situ observations and Lagrangian analyses have shown that most eddies are materially coherent", indeed the work of Abernathey and Haller (doi: 10.1175/JPO-D-17-0102.1) reaches a very different conclusion. The easiest solution, since this point is of tangental relevance to the present manuscript, is to revert to the text in the introduction about coherent vortices being long-lived and playing an important role in ocean circulation and transport. There is also the separate issue raised by Abernathey and Haller of whether "eddy" is a noun or adjective, which again I would encourage the authors to sidestep by inserting the adjective "coherent".

We thank the reviewer for this insightful remark. The concept of “coherence” is indeed complex, and there is ongoing debate within the community, particularly between proponents of Lagrangian and Eulerian approaches. While we agree that the precise proportion of “coherent” eddies remains uncertain, we respectfully disagree with the assertion from Abernathey and Haller that coherent eddies constitute only a minority. Most of Lagrangian studies use satellite-derived geostrophic fields, which are inherently two-dimensional and smoothed, to evaluate eddy coherence.. However, as illustrated in high-resolution models (e.g., Gula et al., 2022, see figure below), the ocean exhibits much greater complexity and chaos than what is captured by traditional altimetry. . The outcome of applying Lagrangian criteria to such realistic model simulations remains an open question. Furthermore, as shown by Liu et al. (2019), the number of coherent eddies identified depends strongly on the integration timescale chosen for the Lagrangian analysis..

In our view, it is perhaps too strong to claim that Lagrangian criteria applied to altimetry provide a definitive assessment of eddy coherence. We are equally critical of the application of Eulerian criteria to altimetric data. Our intent in this comment is not to question the value of Lagrangian criteria themselves, but rather to highlight the limitations of the observational tools commonly used to assess coherence.

In contrast, observational studies often suggest a higher proportion of coherent eddies. Materially coherent eddies are capable of transporting water masses over significant distances. Comparing water mass properties between eddy cores and their environment is also a valid method for assessing material coherence (Barabinot et al., 2025). Using this approach, previous studies have shown that eddies with thermohaline anomalies are not uncommon—for example, meddies (Armi et al., 1989), North Brazil Current rings (Barabinot et al., 2024), Agulhas rings (Laxenaire et al., 2019, 2020), Arabian Sea eddies (de Marez et al., 2020), and eddies in the tropical Atlantic (Aguedjou et al., 2021), among others.

In conclusion, we recognize that the wording "most of" may have been too strong, and we replaced it with "some" in the revised version. However, we maintain that coherent eddies are not necessarily a minority, as suggested by some Lagrangian studies.

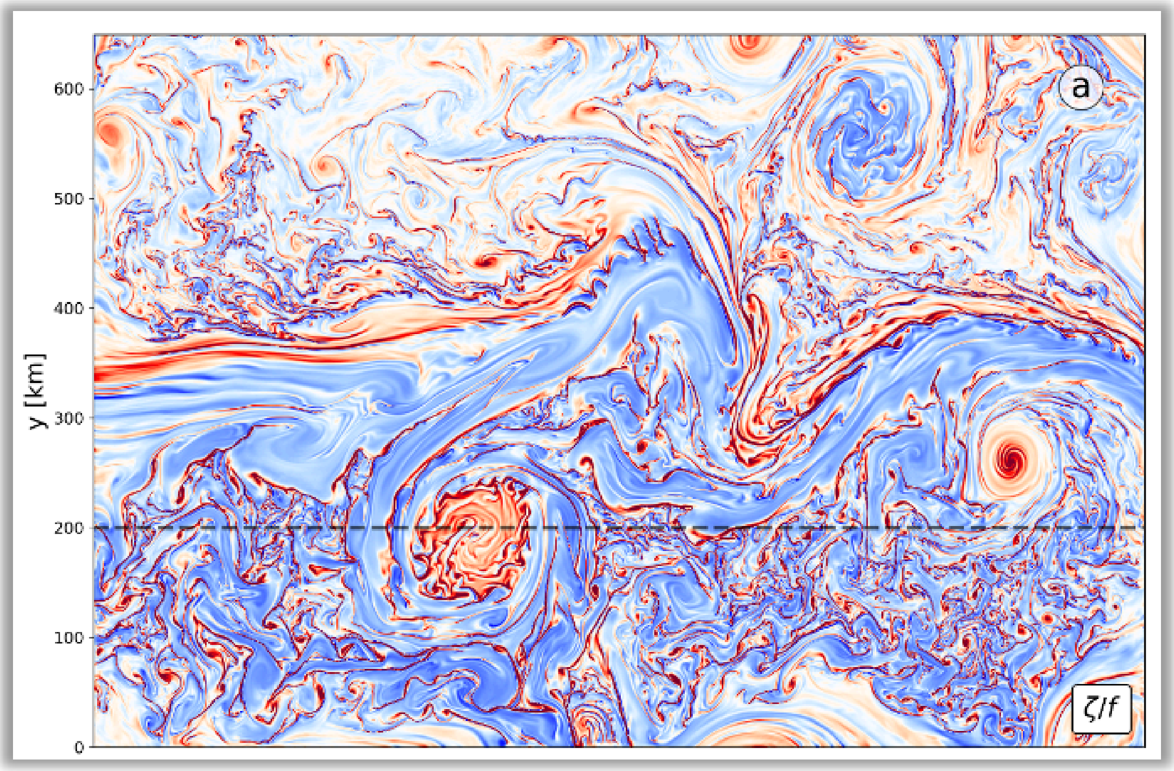

Figure 1. Surface relative vorticity normalized by the Coriolis parameter in the Gulf stream in a very high resolution numerical model simulation Gula et al. (2022) (dx=500 m)

Secondly, I don't understand the statement "laboratory experiments indicate that eddies locally modify stratification in accordance with thermal wind balance, regardless of whether they trap a water mass". It is not obvious that stratification needs to altered at all in a heaving mode, but merely that the isopycnals and raised or lowered. And at low Rossby number, away from boundaries, any flow should be close to thermal wind balance, but I don't believe it follows that this causes eddies to modify stratification, only that lateral density gradients and vertical shear will co-vary in accordance with thermal wind balance.

Our intention was to convey that an eddy locally deflects isopycnal surfaces, thereby altering the vertical gradient of buoyancy (and also the horizontal gradient of buoyancy). We have clarified this sentence in the revised manuscript.

2. I understand that it is convenient to use potential density referenced to the surface, but is there a good reason not to have used neutral density for this study, given that the amount of data involved does not seem particularly prohibitive?

We thank the reviewer for his/her relevant comment. As noted, we used the potential density for convenience. Since the eddies considered in this study are confined to the upper ocean layer, the difference between neutral density and potential density is negligible.

3. Line 84: I think you need to add that the stratification is assumed to be stable for invertibility.

We thank the reviewer for his/her relevant suggestion. A clarifying sentence has been added in the revised manuscript (see line 83).

4. Figure 1: I suggest reducing the magnitude of the sea surface displacement in the upper panel.

We thank the reviewer for this suggestion. The magnitude of the sea surface displacement in the upper panel has been reduced in the revised manuscript.

5. I understand the rationale for invoking the quasigeostrophic approximation for analytical tractability, but you should comment on the extent to which the quasigeostrophic assumptions are (not) satisfied in the eddies under consideration, and whether this is a significant limitation, in your view, or otherwise (and why).

We thank the reviewer for his/her suggestion. Additional comments have been included in the revised manuscript (see lines 211-216).

6. I must confess I spent some time to derive equation (38) and wonder if you can provide some pointers to help the reader? Also, is there a good reason that the zero is moved to the left hand side of (37) compared with the original equation (34)?

We thank the reviewer for his/her remark. To assist the reader, we have added an additional step in the derivation of equation (38) in the revised manuscript (see line 241). In addition, we have corrected equation (37).

7. Line 215: I understand what you mean by the "nonlinear" term, but technically this remains a linear equation. It is nonlinear only in the vertical coordinate.

We thank the reviewer for bringing this to our attention. We have proposed an alternative formulation in the revised manuscript (see line 233).

8. Diffusion arguments" - what specifically do you mean by diffusion? Is this physical, based on an oceanographic process? I have no idea from what is written, and this must be explained and justified.

We thank the reviewer for his/her relevant comment. In experimental studies, the diffusion of momentum governs the self-similar profile of the density anomaly in eddies, which explains why eddies often exhibit a Gaussian shape. Therefore, we sought a formula consistent with this observation. To clarify this point, we have modified the subsection title to “Arguments based on experimental studies” in the revised manuscript.

References

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