## Reply referee 1

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.

### 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?*

We thank the reviewer for this relevant comment. We agree that, in principle, the dynamics of cyclonic and anticyclonic eddies should not be fundamentally different, except for the sign of their anomalies. Consequently, there should be no systematic difference in their horizontal or vertical structures. The reason cyclonic eddies are not analyzed in detail in our study is solely due to the lack of suitable in situ data. Specifically, only a few cyclonic eddies were identified in our cruise dataset, and those that were observed were either sampled with insufficient resolution or only partially sampled (e.g., cross-sections did not capture the full vertical structure). This limitation is therefore a matter of data quality rather than an underlying physical difference. We have clarified this point and included a dedicated paragraph in the revised version of the manuscript (see Section 5.4 in the Discussion).

*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 R1 and R2 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 x1 and x2 be estimated directly as the location of the maximum surface density anomaly rather than by fitting? The fitted values of Rand 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?*

We thank the reviewer for this constructive remark. We agree our study raises several important questions, many of which merit dedicated investigation in future work, iincluding our own ongoing research. Please find below additional details and hypotheses in response to the reviewer's queries:

- *Application to Argo Data:* We have recently conducted a Master's project to test whether our formulas can be applied to characterize eddies using Argo float profiles. The results are promising (including for the estimation of B and D), although they are not yet published. his demonstrates that Argo data can indeed be used to extend our analysis of eddy vertical structure.
- Physical Interpretation of B and D: Our working hypothesis is that the parameters B and D are linked to baroclinic vertical modes, but this remains to be confirmed with further analysis.
- Comparison of $R_1$ and $R_2$ from Satellite and In Situ Data: The question regarding R1 and R2, the question raised can be addressed by comparing satellites observations with in situ measurements which is feasible and could form the basis of a future study. In our present work, assuming $R_1$ and $R_2$ to be constant was sufficient to model the horizontal structure of eddies at various depth levels. However, this assumption may not hold in all cases, suggesting that satellite data alone may not always be adequate.
- Interpretation of $x_1$ and $x_2$: As shown in Table A1, x1 is approximately equal to x2, representing the average eddy center location on cross-sections. At the surface, it thus makes sense to estimate x1 and x2 at the location of maximum surface density anomaly. If the eddy is drifting, x1 and x2 would be expected to vary with depth.
- Variability of R and H: Understanding the spatial variability of R and H, as well as their temporal evolution throughout the eddy lifecycle, is indeed crucial. The main objective of our article was to propose and validate formulas to characterize eddy structure at specific moments ("snapshots"). However, a comprehensive analysis of their space–time variability will require further dedicated research.

We have clarified this point and included a dedicated paragraph in the revised version of the manuscript between lines 516 and 520.

*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 x1 or x2? 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.*

We thank the reviewer for this helpful comment. The first step of our optimization process is the decomposition of the observed anomaly into spiciness and heaving modes, as described in Section 3.4. Subsequently, the optimization is performed first for the vertical structure and then for the horizontal structure. In practice, however, the order is not critical because the variables are decoupled. We chose to optimize the vertical structure first to highlight the novelty of our approach, which lies in the formulation of the vertical structure function.

The vertical structure function is indeed optimized at the eddy center, defined as the location where the orthogonal velocity to the ship transect is zero. If the eddy center varies with depth, we use its average location. We have clarified this point in the revised manuscript (see line 374). In our dataset, the sampled eddies remain nearly vertical, with minimal variation in their center positions. As a result, we chose to keep $x_1$ and $x_2$ constant; however, it is possible to introduce a depth dependence, $x_1(z)$ and $x_2(z)$, to account for potential drift. Our formulas are intended as a flexible basis for eddy modeling and can be adapted to specific cases as needed.

Regarding the consistency of the vertical structure, , we confirm that it remains valid at different radial distances from the eddy center, as demonstrated in Figure 13. In this figure, we reconstructed the two-dimensional vertical structure of the sampled eddies using our model and compared it with in situ data. The agreement remains good at various radial distances, with a notable increase in error only beyond $R_1$.

We have also attempted to fit the observed horizontal structure with the function proposed by Zhang et al. (2013):

$$\phi(r) = (1 - (r/R_0)^2)\, exp(- (r/R_0)^2) \quad (1)$$

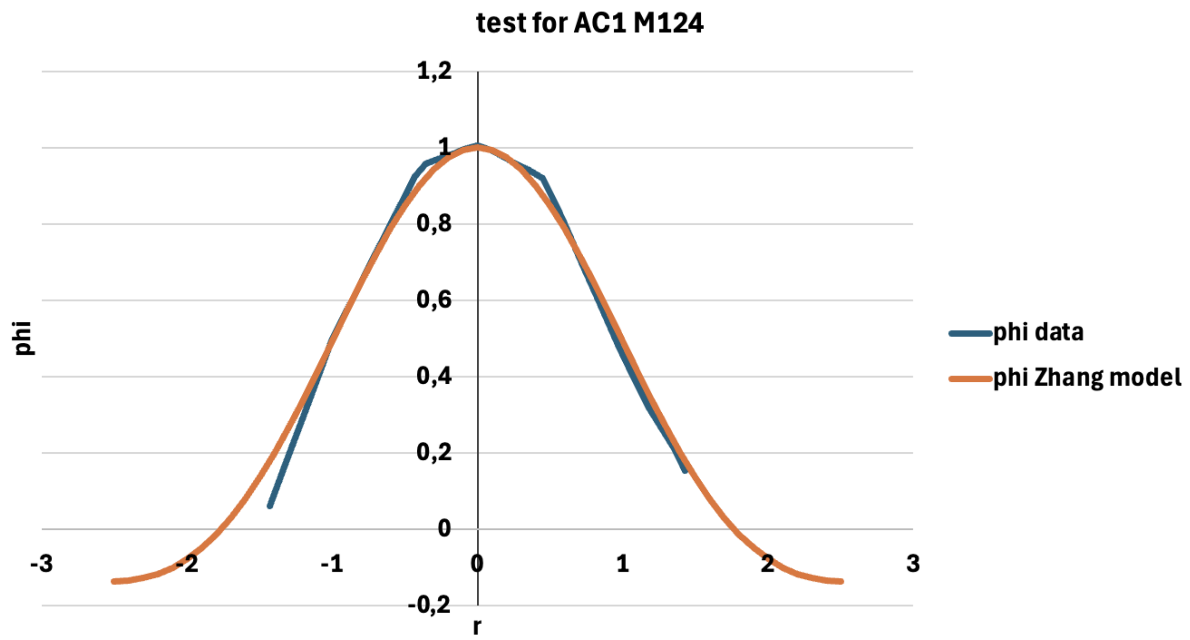
For comparison, our model uses:

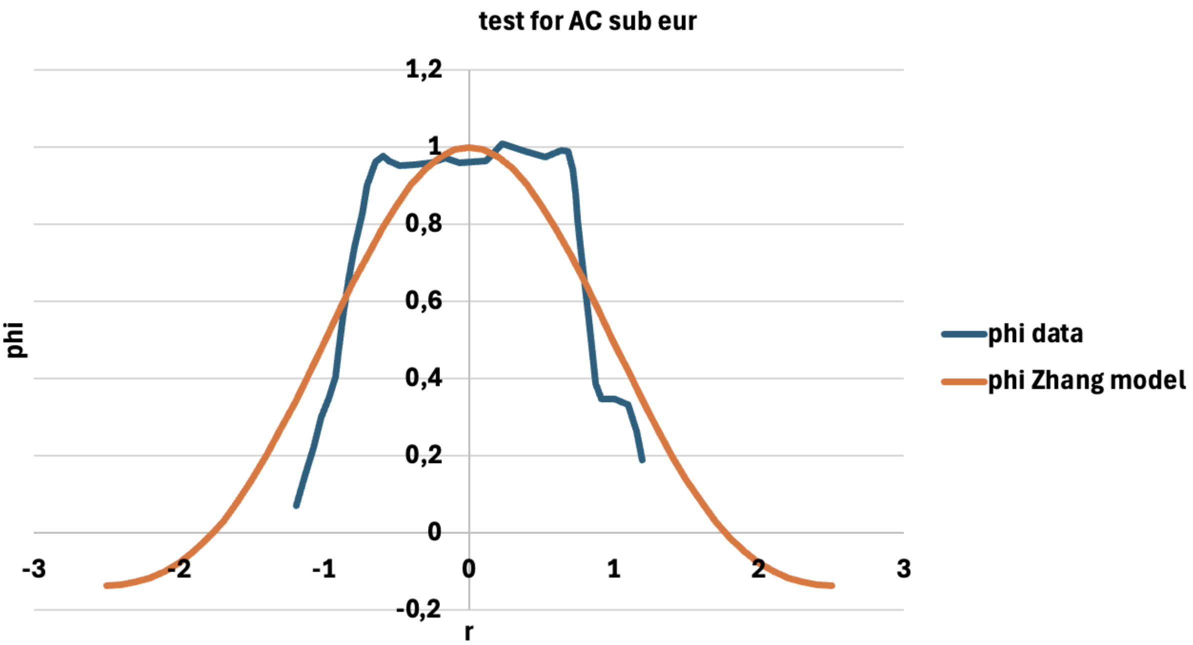
$\phi(r) = exp(- (r/R_1)^\alpha)$ (2) (corresponding to Eq. 27 in our manuscript)

The function (1) fits well the data when $\alpha = 2$, as shown in panels (a) and (d) in Figure 8. However, our data do not extend beyond $r/R_1 > 1.5$, so we cannot observe the change of sign of $\phi(r)$ as reported by Zhang et al. (2013; see their figure 2).

Moreover, function (1) does not fit our data well for $\alpha = 4\ or\ 6$. We provide two illustrative examples below, where the blue curve represents the data and the orange curve is the best fit using the Zhang et al. (2013) model. The steepness of funtion (1) is insufficient to represent the sharp boundary of the anticyclones sampled during EUREC4A-OA, making (2) a more appropriate choice.

Based on these results, and to avoid further lengthening an already extensive manuscript, we have decided not to include the Zhang et al. (2013) function in our analysis.




We have added a sentence in line 192-195 to clarify this point.

*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−x1 is negative, the eddy anomaly will increase exponentially from its center, which is unphysical. Perhaps, the absolute value should be taken for x−x1?*

We thank the reviewer for this careful observation. The absolute value is now correctly included in equations (53) and (54) in the revised version of the manuscript.

*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.*

We thank the reviewer for his/her comment. This suggestion was initially raised by a previous reviewer who found it difficult to distinguish between functions and values due to the abundance of notations. We agree that the change has improved the clarity of the article, especially given the complexity of the notation used.

*3. Line 119: I think η should be the vertical displacement of isopycnal with respect to the mean state, not the state of rest.*

We thank the reviewer for this remark. We agree with the suggestion and have modified the expression accordingly in the revised manuscript (see line 120).

*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.*

We thank the reviewer for his/her suggestion. The wording has been modified in the revised manuscript accordingly (see line 134).

*5. Lines 141 and 170: Missing spaces between ˜·and texts.*

We thank the reviewer for bringing this to our attention. The typographic errors have been corrected in the revised manuscript (see lines 142 and 171).

*6. Equation (21): What's the difference between T(r,θ,σ0) and T(σ0)? It feels that equation (21) is just zero equals zero.*

We thank the reviewer for this remark. $\widetilde{T}(r, \theta, \sigma_0)$ represents the in situ temperature field, while $\overline{\widetilde{T}}(\sigma_0)$ denotes the mean state. In the presence of an eddy with a spiciness mode anomaly, the temperature is not constant along isopycnal surfaces. For the same density, multiple (T,S) pairs can coexist. Therefore, equation (21) does not simply express "zero equals zero.".

*7. Line 185: I do not think it is accurate to say “no exact analytical expression for ϕ(r) exists”. Zhang et al. (2013) has proposed an analytical expression for the radial structure of eddies.*

We thank the reviewer for bringing this to our attention. The reviewer is indeed correct,. and we have modified the sentence accordingly in the revised version (see line 186).

*8. Line 208: “radius” →“radial distance”.*

We thank the reviewer for bringing this to our attention. The wording has been corrected in the revised manuscript (see line 219).

*9. Line 223: I suggest drop “assuming” for the same reason mentioned before.*

We thank the reviewer for this suggestion. We agree and have removed the word “assuming” in the revised manuscript (see line 234).

*10. Line 248: H1 is a characteristic depth scale.*

We thank the reviewer for bringing this to our attention. We agree with the suggestion, and the wording has been modified accordingly in the revised manuscript (see line 259).

*11. Table 1: A “vertical” is not capitalized.*

We thank the reviewer for bringing this to our attention. We agree, and the expression has been corrected in Table 1 of the revised manuscript.

*12. Line 268: How exactly is the eddy center be determined from velocity analysis?*

We thank the reviewer for his/her relevant comment. The sentence in question has been deleted from the revised manuscript, as the method for determining the eddy center is already described in Section 3.1.4 (lines 320-322). Specifically, we used the methodology described in Nencioli et al (2008) to estimate the position of the eddy center in the (x,y) plane.

We would also like to clarify that this eddy center is different from the eddy center identified along a ship cross-section, where it is defined as the location where the orthogonal velocity to the ship transect is zero. To clarify this point, we have added a sentence in lines 383–384 of the revised manuscript.

*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?*

We thank the reviewer for his/her question. The choice of thresholds (or cutoff scale) is somewhat subjective and depends on the spatial scales under investigation. Our primary aim was to minimize the influence of submesoscale effects. Therefore, we selected a cutoff value slightly larger than the mean resolution of the raw data.

*14. Line 319 and others: “cutoff period” →“cutoff scale” or “filter scale”.*

We thank the reviewer for bringing this to our attention. This is indeed the “cutoff scale”, and the expression has been corrected in the revised manuscript.

*15. Line 367: $\psi 0$ should be $\eta z0$.*

We thank the reviewer for bringing this to our attention. This notation was carried over from a previous version of the manuscript and is no longer relevant in the current. The expression has been corrected in the revised version (see line 380).

*16. Line 373: “$\psi$ and $\xi$” →“$\eta z$ and $\delta 2\ z\ \sigma$”? I see no $\psi$ and $\xi$ in equations (53)-(56).*

We thank the reviewer for bringing this to our attention. This notation originated from a previous version of the manuscript and is not relevant in the current context). The expression has been corrected in the revised version (see line 386).

*17. Line 374: There is an extra period.*

We thank the reviewer for bringing this to our attention. The typographic error has been corrected in the revised version (see line 387).

*18. Line 374: “locations where the amplitude of $\psi$ and $\xi$ are maximal” →“depth where the amplitude of $\eta z$ and $\delta 2\ z\ \sigma$ are maximal”?*

We thank the reviewer for bringing this to our attention. This notation originated from a previous version of the manuscript and is not relevant for the current version. The expression has been corrected in the revised version (see line 388).

*19. Line 389: Why is the water in an anticylconic eddy be colder than its surroundings?*

We thank the reviewer for his/her relevant question. The statement that anticyclonic eddies always transport warmer water than their surroundings is not universally valid. Both positive and negative temperature anomalies can occur, as demonstrated, for example, in the study of Aguedjou et al (2021).

*20. Figure 8: Which depth are the $\phi$ and $\chi$ from? The depth where $\eta z$ is the maximum? Is $\phi(x)$ or $\chi(x)$ different at other depth?*

We thank the reviewer for his/her relevant remark. In Figure 8, $\Phi$ and X are fitted at the depth where $\eta z$ is maximum.

Regarding the reviewer's second question, it was indeed important to demonstrate that the optimization works at all depths. This is the purpose of Figure 13, which shows the error between the in situ density field and the reconstructed field. The error remains small within the eddy cores, indicating that $\Phi$ and X are nearly constant with depth.

*21. Line 441: There is an extra "function".*

We thank the reviewer for bringing this to our attention. The extraneous occurrence of "function" has been deleted in the revised manuscript (see line 455).

*22. Figures 9 and 10: Which location are the vertical profiles from? The eddy center? Is ηz (z) or δ2 z σ(z) different at different x?*

We thank the reviewer for his/her remark. Yes, the vertical profiles are from the eddy center. Here, the eddy center is the appearing eddy center refers to its position on the two-dimensional cross-section, defined as the location where the orthogonal velocity to the ship transect is zero.

As previously mentioned, it was indeed important to demonstrate whether the optimization was effective at different x-locations.. This is addressed in Figure 13, which shows the error between the in situ density field and the reconstructed field. We found that, $\eta_z(z)$ or $\delta_z^{\ 2}(z)$ remain valid when $r$ is less than the radius of maximum velocity.

*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.*

We thank the reviewer for his/her remark. We fully agree and discuss this point in the Discussion section (see lines 512 and 517).

*24. Line 508: "reconstruct" →"reconstructed".*

We thank the reviewer for bringing this to our attention. The error has been corrected in the revised manuscript.

*225. 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.*

We thank the reviewer for his/her remark. The assignment of one column to each letter, with each letter corresponding to a specific eddy, was an intentional choice. We agree that the third row was missing a letter, and we have corrected the figure accordingly in the revised version.

*26. Line 549: "diffusivity" →"diffusion".*

We thank the reviewer for bringing this to our attention. The error has been corrected in the revised manuscript.

*27. Line 553: There should be more details about how the vertical extent is controlled by the stratification and what the vertical symmetry means.*

We thank the reviewer for his/her comment. Additional details have been included in the conclusion of the revised manuscript (see lines 571 and 574).

*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.*

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