

We would like to thank the referee for his/her comments. Please find our point-to-point response below.

The authors provide a broad overview of the literature for 'coherent eddies', with a nice introduction on the history of the field of understanding/quantifying eddy coherence. However, when it comes to material coherence, and materially coherent eddies, they've missed quite a number of relevant references from the Lagrangian coherent structures community.

We want to clarify that it was never our intention to overlook or minimize the importance of relevant references published by the Lagrangian community (nor those from the Eulerian community). Our approach in the manuscript was to cite a limited selection of what we consider to be the most relevant and comprehensive works, from which a broader understanding of the literature can be developed. We aimed to provide a focused reference list while acknowledging that these sources represent only a part of the extensive body of work in this field. We thank the referee for providing more relevant references. We will include the suggested references in the revised manuscript.

Specifically, the discussion around how 'kinematic coherence' (KC) is reference frame dependent (and hence, not objective), is missing another key point, that eddies defined by Eulerian (frame-dependent) methods are not materially coherent. Quite a number of studies have shown that ocean eddies, identified from the Eulerian perspective (for e.g., Okubo-Weiss parameter, SSH anomaly), leak material across their identified boundaries relatively quickly (Andrade-Canto et al. 2020, Beron-Vera et al. 2013, Haller et al. 2013, Liu et al. 2019, Serra et al. 2017, Denes et al. 2022). This is a major drawback of using Eulerian approaches for quantifying material coherence, and mass transport by eddies..

Our intention is not to criticise the material frame-indifferent coherent (MC) vortex theory or the concept of "kinematic coherence" in eddies. We fully agree with the mathematical and physical formulation underlying the Lagrangian framework. The primary objective of our study is to explore the impact of material coherence on the thermohaline structure of eddies, which is a direct result of the "coherent" mass transport by oceanic eddies. We will, however, include additional details on the limitations of the Eulerian approach. It is also important to highlight that, while the Lagrangian approach is theoretically robust, it has been misapplied in some published studies. This is particularly the case when ocean observations are used that do not correspond to the required velocity fields, such as when geostrophic surface velocities derived from satellite altimetry maps are used incorrectly in the context of this theory. We can expand also on this point in the revised version of the manuscript.

I also found the definition of material coherence in the manuscript to be rather qualitative, that a 'vortex ceases to be coherent when it loses its trapped water mass'. How does one determine the point at which a vortex loses its trapped water mass? Recent Lagrangian analyses have considered the coherence of ocean eddies, specifically Abernathey et al. (2018), and Wang et al.(2015), found that only small 'coherent inner cores' of ocean eddies existed over long time periods, while Denes et al. (2022) found ocean eddies may consists of coherent inner cores, and quasi/semi-coherent outer rings (and question the notion that ocean eddies have precise boundaries). This is briefly touched on by the authors on lines 70-71, but I think more is needed here.

Indeed, Lagrangian coherent eddies are theoretical fluid structures that maintain their identity over time, characterized by a consistent material coherence in the flow. They are identified based on their ability to transport mass without "significant mixing" with surrounding fluid, preserving their internal material properties. This coherence is, as the reviewer states, best determined using Lagrangian methods that track the trajectories of fluid particles, ensuring that the eddies' boundaries remain impermeable and that the particles within them move cohesively over a given time period.

The impermeability of eddy boundaries in the Lagrangian sense is mathematically defined by the concept that the boundary is a material surface that does not allow fluid exchange with the surrounding flow. This is quantified using the concept of Lagrangian Coherent Structures (LCS), which are identified using methods such as finite-time Lyapunov exponents (FTLE) or geodesic theory. These methods ensure that particles within the coherent eddy remain within the boundary over time, and particles outside do not cross into it.

A mathematically rigorous way to define impermeability is through the geodesic theory of coherent vortices, which involves identifying elliptic LCS. An elliptic LCS is a closed curve in the flow that acts as a transport barrier, meaning no particles can cross it over the time period considered. The key references for the definition of such "Impermeability" are:

Haller, G., & Beron-Vera, F. J. (2013). "Coherent Lagrangian vortices: The black holes of turbulence." *Journal of Fluid Mechanics*, 731, R1-R16. This paper introduces the idea of "Lagrangian Coherent Vortices" (LCVs) as impermeable structures in a turbulent flow, using the concept of geodesic theory to define and identify them.

Haller, G. (2015). "Lagrangian Coherent Structures." *Annual Review of Fluid Mechanics*, 47(1), 137-162. This review further elaborates on the mathematical foundations of impermeable boundaries in coherent structures, providing a broad overview of the techniques used to identify such structures.

However, real ocean mesoscale eddies are less well-defined coherent structures compared to their theoretical Lagrangian representations. The dynamic nature of the ocean—characterized by fast rotation, stratification, continuous interaction with the atmosphere in the upper layers, and interactions with the ocean floor and steep topography in the bottom layers—introduces various instabilities. These instabilities, further influenced by neighboring eddies (since ocean eddies are rarely isolated), can lead to partial permeability of eddy boundaries or even complete disruption and dissipation of the eddy. In particular, these processes are inherently related to the 3D nature of oceanic vortices. The 3D coherence of oceanic vortices is therefore not fully grasped by MC theory applied to surface velocities only.

The reviewer correctly points out that observed ocean eddies generally consist of a coherent inner core (which is not as small as might be assumed; see Barabinot et al., 2024, and the relatively stable core evolution observed in satellite-derived Absolute Dynamic Topography maps: Laxenaire et al., 2019; 2020) surrounded by an outer region that actively exchanges fluids with the surrounding environment. We will incorporate these references, along with those suggested by the reviewer, and integrate the reviewer's valuable insights into the revised manuscript.

A particular concept raised in 'material coherence' theory is that the coherence occurs over a finite window of time. As described in the manuscript, ocean eddies tend to have lifetimes of months up to years, however, with entrainment and flushing of water, an ocean eddy is not materially coherent over its entire lifetime. To assess the material coherence of an ocean eddy from a single snapshot in time seems rather suspect to me. As the authors note, it is 'impossible to perform temporal studies with in situ data', however material coherence has a time-evolution component that I think is quite important when assessing mesoscale ocean eddies.

I suggest the authors consider an alternate motivation for the paper. I can see the techniques described and analysis developed as nice ways to 'Reconstruct 3D ocean eddies from 2D in situ transects'.

We thank the reviewer for his suggestion. However, we do not agree with his arguments. Indeed, it is accurate to note that our analysis does not explicitly demonstrate a temporal correspondence between the MC theory and the presence of anomalies in the core of eddies. However, the presence of thermohaline and potential vorticity anomalies is indicative of the existence of water that does not exhibit the same thermohaline properties as the surrounding water.

In this study we want to emphasize how the Lagrangian coherence definition can be coupled to the uniqueness of ocean water masses. In fact, the latter represent distinct "fingerprints" within the ocean, characterized by specific combinations of temperature and salinity that are not randomly distributed but rather result from precise regional conditions. Each water mass originates in a specific region where unique air-sea interactions imprint it with a characteristic temperature-salinity (T-S) signature. Once formed, these water masses are remarkably consistent in their properties as they are advected within the ocean, below the mixing layer, allowing them to be identified and tracked over great distances.

As postulated by the MC theory, the material boundaries maintain the thermohaline characteristics of the transported water, from the eddy region of generation. Consequently, the trapped water exhibits disparities from the surrounding water as a consequence of the eddy motion in the ocean. Anomalies on isopycnals represent a primary indicator of heterogeneous water masses within eddy cores, enabling the computation of heat and salt content in these structures through the use of in situ data (Aguedjou et al., 2021; Yang et al., 2015; Dong et al., 2017; Dong et al., 2014; Chen et al., 2012). For instance, Armi et al. (1989) show that meddy remains essentially coherent for 2 years before collapsing very rapidly by thermohaline intrusions. This is one of the few instances where we have 4 radials in a single eddy, over several years.

In the revised manuscript, we will more clearly elucidate this evident relationship, which justifies the main objective of the study as succinctly summarized by our proposed title, "Assessing the Material Coherence of Mesoscale Eddies using In Situ Data."

Particular comments:

1. Methodology I particularly liked the descriptive methodology of the paper, the authors explain and derive their methodology nicely. However, at times I was a little lost. I think the manuscript would benefit from an additional paragraph or two which may provide an overview of the methodology. For example, section 3 describes four identification methods, but no introductory sentence/paragraph which describes why these identification methods are used let alone useful. What alternative methods exist, and why are these four chosen?

Thank you for your valuable suggestion. We will incorporate an additional paragraph in the revised version as proposed. In Section 3, we described four methods that have been widely applied to in situ data analysis. Specifically, the use of T/S anomalies, gradients, and potential vorticity (PV) has been extensively employed to develop diagnostics for eddies sampled during in situ experiments (Aguedjou et al., 2021; Paillet et al., 2002; Paillet et al., 1999; Bosse et al., 2019; Carton et al., 2002). These methods have proven effective in enhancing our understanding of the dynamic properties of oceanic eddies.

2. 'Acronymisation' I felt there were a few too many acronyms, but in cases some things could have been 'acronymised'. For example, at one point the authors use 'AC' and 'C', to describe (from what I understand) anticyclonic and cyclonic eddies (line 268, or 403, but not described anywhere in text). Especially when, in Table 3, 'C' is used to describe 'Complete' eddies. In contrast, to be in line with 'Kinematic coherence' and 'Material coherence' (KC and MC), I think the authors can use 'TC' for 'Thermohaline coherence', which would help with the flow of the discussion in section 5.2, where they describe 'thermohalinely coherent cores', for example.

We thank the reviewer for his/her insightful remarks. We will clarify the points in the revised version of the article according to your suggestions.

3. Line 32, the term 'eddy boundary' needs to be placed in context. Eddy boundaries are method dependent, and some literature argues that eddies do not have a sharp boundary. I think this sentence could be qualified further.

We thank the reviewer for his/her remark. We agree that the definition of eddy boundaries is method dependent. We will provide a more detailed explanation of this in the revised manuscript.

4. Line 64-66 on MC theory Some material coherence theory is derived from 'minimal mixing' criteria, specifically, that a boundary of a materially coherent structure is undistorted/unfilamented over a finite-time window, such that diffusive mixing across the boundary is minimised. This sentence can be qualified further.

We thank the reviewer for his/her insightful remark. We agree that the sentence on material coherence theory could benefit from further clarification. In the revised manuscript, we will provide a more detailed explanation of how material coherence theory is derived from the 'minimal mixing' criteria, specifically focusing on how the undistorted and unfilamented

boundaries of a materially coherent structure minimize diffusive mixing over a finite-time window.

5. Line 67-68 on number of MC eddies vs. KC eddies. I wasn't sure what the purpose of this sentence is. As the authors suggest, material coherence provides a rather rigorous definition for ocean eddies, and one should expect that more structures will appear from a less restrictive approach than a more restrictive approach. Is this considered a downside of MC approaches, or just a statement of fact?

We thank the reviewer for his/her thoughtful remark. We will revise the tone of this sentence to ensure clarity. The sentence is intended as a statement of fact, highlighting that the more rigorous definition of ocean eddies provided by material coherence may result in fewer identified structures compared to less restrictive approaches. This distinction is significant, as it directly impacts the assessment of heat and salt transport by mesoscale eddies.

6. Describing the trapped water as 'heterogeneous' (e.g. line 85, 306) I found this a little confusing, as the trapped water mass is typically homogeneous within the eddy core, but distinct from the surrounding water mass, as it is 'characteristic of the region of formation of that eddy'. I would think of this water mass as 'distinct' .

We thank the reviewer for his/her suggestion. We will change the word "heterogeneous" by "distinct".

7. Line 91 – providing a first answer to the question of the 3D material coherence of eddies I'm not entirely sure what the question actually is. That 3D material coherence of eddies can be assessed using water masses and the PV approach? Certainly, 3D material coherence of eddies has been considered in the past, see Froyland et al. (2012).

We thank the reviewer for his/her comment. We will rephrase the question in the manuscript to ensure clarity. The intent is to explore how 3D material coherence of eddies can be assessed using water masses and the PV approach. While we acknowledge that 3D material coherence has been addressed in previous studies, such as Froyland et al. (2012), our focus is on providing a new perspective through these specific methodologies.

8. Definition of the center of an eddy The authors make contrasting statements '... is reached at the eddy center (region where the velocity tends to zero)' on line 100, and 'The center of the eddy is defined as the point where the mean tangential velocity is maximum' , and '... cross the exact eddy center, defined as the location of the zero velocity' on lines 344-345. To me, these three statements are inconsistent. Additionally, in the first instance the eddy center is a region, in the second it is either a point or a curve, and in the third it a point (possibly a region?).

We thank the reviewer for his/her detailed remark. We understand the concern regarding the apparent inconsistencies in the definitions of the eddy center. To clarify, on a 2D vertical section, the eddy center is identified as the region where the in situ velocity orthogonal to the ship tends to zero, indicating that this center lies within the plane of the section. In 3D, however, the eddy center is determined at each depth level on a 2D horizontal plane using the Nencioli routine on in situ data (Nencioli et al., 2008). This routine involves constructing a grid with a specified resolution around the ship's track, and for each grid point, the mean

radial and tangential velocities are calculated, assuming that point is the eddy center. The grid point with the minimum radial velocity or maximum tangential velocity is then considered the eddy center at that depth level. We will revise the manuscript to ensure these definitions are consistently and clearly presented. For further details, we encourage the reviewer to refer to Nencioli et al. (2008).

9. Line 244 'These values are small compared to the first-order terms' I would disagree, in the horizontal case they are of the same order of magnitude. They are smaller, but not negligibly small. Is this not important?

We thank the reviewer for his/her thoughtful remark. We agree that while the values are smaller, they are not negligibly small, particularly in the horizontal case where they are of the same order of magnitude. To address this, we also tested a second-order centered scheme ($\partial_x a = (a(x + dx) - a(x - dx))/(2dx)$) which was not detailed in the article. This approach resulted in less than a 5% change in the outcomes. We will clarify this point in the revised manuscript to acknowledge the importance of these terms.

10. Lines 300-301 and lines 441-442 – Motivation The motivation for the analysis described in both these sections would be great to see in the introduction. In both cases, I did not know this would form part of the analysis until I reached these sentences.

We thank the reviewer for his/her remark. The revised manuscript will be modified accordingly.

11. Line 361 – 'To reduce this uncertainty, ...' By computing volumes for a subset of eddies, you aren't exactly reducing uncertainty. Rather, you are only showing results for eddies where the uncertainty isn't too large. I would rephrase this.

We thank the reviewer for his/her remark. This sentence will be rephrased accordingly.

12. Layout of Figures 4 and 5 Use of figure labelling would be really useful for both of these figures, using a), b) etc. Also, the change in orientation for the bottom panels of Figure 4 I found rather jarring, is there a reason to have these laid out vertically rather than horizontally like the top panel of Figure 4 and all of Figure 5? Lastly, I suggest a second diverging colorbar for the salinity anomaly (different to the thermal anomaly colorbar).

We thank the reviewer for his/her valuable remark. We will revise Figures 4 and 5 to improve clarity. In the revised version of the manuscript, we will include figure labeling (a, b, etc.) to make the figures easier to reference. Additionally, we will adjust the orientation of the bottom panels in Figure 4 to match the horizontal layout of the top panel and Figure 5 for consistency. We will also introduce a separate diverging colorbar for the salinity anomaly to distinguish it from the thermal anomaly colorbar.

13. Lines 458-459 – 'Lagrangian studies suggest that the ability of eddies to trap a water mass is a consequence of closed trajectories'. I'm not certain what you mean by this. What is a closed trajectory in this instance? A citation would be very useful.

We thank the reviewer for his/her remark. To clarify, by "closed trajectories," we are referring to the circular or looping paths that fluid particles follow within an eddy, which allow the eddy

to trap water masses effectively. We will add a citation to relevant Lagrangian studies in the revised version of the manuscript to support this statement and provide further context.

14. Figure 7a. Plotted velocity vectors - I find these velocity vectors distract from the plots, especially in panel g), where I feel the figure is too busy to see what is really going on. b. Panel title (a) – ‘fev’, and panel title (i) – missing date (same on Figure 8)

We appreciate the reviewer’s insightful comments. In response, we will adjust Figure 7 by either reducing the density of the velocity vectors or providing an alternative visualization to reduce clutter, particularly in panel g). Additionally, we will correct the panel title in Figure 7a from ‘fev’ to the appropriate label and ensure the date is added to panel i) in both Figure 7 and Figure 8 in the revised manuscript.

15. Figure 9 colorbar and panel titles I would change the colorbar to something that contrasts better with the isopycnal, EPV, and salinity curves (being black and blue), otherwise it is very hard to appreciate these curves. I also found the panel titles to be rather confusing and could do with an update.

We thank the reviewer for his/her valuable feedback. We will revise the colorbar in Figure 9 to provide better contrast with the isopycnal, EPV, and salinity curves, enhancing their visibility. Additionally, we will update the panel titles to improve clarity and reduce any potential confusion. These changes will be reflected in the revised version of the manuscript.

16. Line 519-520 – ‘The reconstructed volume associated with the thermal anomaly is the most convex of all shapes’. I’m not sure what you mean by this, nor am I sure if I agree with the statement. In fact, some of the shapes plotted in Figure 10 aren’t convex (panel (c) for example).

We thank the reviewer for his/her remark. We agree that the term “convex” is inappropriate here. The appropriate word is “connected”. This sentence will be modified in the revised version of the manuscript.

17. Figure 10 – panel titles If you could add titles to each of the panels (not just in the figure caption), that would really help guide the reader. Also, given the circularity constraint, I think each of these plots should have equal x-y aspects, figure (c) looks out of shape due to the non-equal aspect ratio.

We thank the reviewer for his/her remark. We will modify accordingly the figures in the revised version of the manuscript.

18. Lines 525 – 533 regarding Figures 11 and 12 The authors say ‘This figure refers to the ellipses (E1) mentioned earlier’ and later ‘(ellipses E2)’, but the only mention I could find to E1 or E2 were in the caption of figure 9. Where do these numbers come from, and how were they computed? Secondly, what is the purpose of an elliptical reconstruction? That was not clear to me.

We thank the reviewer for pointing out this issue. The references to (E1) and (E2) were intended to refer to the ellipses described in Appendix B. We apologize for the confusion and

will correct these mentions in the revised manuscript to clearly associate them with the appropriate sections.

Regarding the purpose of the elliptical reconstruction, as outlined in Chen et al. (2019), ellipses often provide the best fit for the shape of eddies observed in altimetric maps. Furthermore, an eddy exhibiting barotropic instability with a second mode, as discussed by Marez et al. (2020), typically assumes an elliptical form. This makes an elliptical reconstruction a suitable approach for accurately characterizing the eddy structure in our study. We will clarify this rationale in the revised manuscript.

19. Line 569-570 – ‘This paper presents an evaluation of the thermohaline coherence of mesoscale eddies based on in situ data collected during several cruises...’ While I agree with this statement, the title of the manuscript is on assessing ‘material coherence’. While I don’t think the manuscript has addressed the material coherence of ocean eddies, this statement in the conclusion is at odds with the title and abstract of the manuscript.

We thank the reviewer for this insightful comment. We acknowledge the inconsistency between the manuscript’s title and the focus on thermohaline coherence in the conclusion. To resolve this, we will revise the title and abstract to more accurately reflect the focus on thermohaline coherence. Alternatively, we will adjust the discussion to better address aspects of material coherence, ensuring alignment with the title. The revised manuscript will present a consistent narrative that accurately represents the study’s objectives and findings.

Typos/grammar/missing info:

Line 58 – there is an erroneous space between ‘velocity fields’ and the comma.

Line 128 “boundary current systems off Labrador” - Do you mean Labrador sea?

Line 196 – ‘Localization’ should be ‘Localisation’.

Lines 105, 192, 438 – ‘confront’ could be ‘compared’.

Line 195-197 – Could you provide a citation to the data described?

Line 233, Fig 4 caption, Fig 5 caption – you use ‘gyre’, do you mean eddy?

Line 280 – ‘the EPV id the ocean at rest’, what do you mean by ‘rest’?

Equation 7 – f_0 is defined, but f is not defined.

Line 321 – what are \overline{b} and b' ?

Line 327 – ‘adimensionalizing’ should be ‘nondimensionalising’.

Line 364 – ‘analytically’, I wasn’t sure what you meant by this.

Line 366 – ‘described’ should be ‘described’.

Line 564 – ‘this’ should be ‘thus’.

Line 582 – ‘This result corroborates the findings of previous studies...’ citations are needed for this statement.

We sincerely thank the reviewer for his/her thorough review and valuable comments. We will carefully address all the minor suggestions, ensuring that the revised manuscript is fully corrected and improved accordingly.

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