List of all relevant changes made in the manuscript

We addressed the main concern about the lack of strong quantitative evidence for particle retention by thermohaline staircases by applying a statistical approach. By analyzing vertical changes in the Junge index across all deep stations and grouping profiles by the presence (termed « WStairs ») or absence (termed « NoStair ») of staircases, we used multiple linear regression to show that the Junge index decreases more slowly where staircases occur. This supports the idea that staircases help to retain larger particles deeper in the water column, and we have included this analysis in the discussion. A similar approach was used with the Apparent Oxygen Utilization that indicated increased in situ mineralization activity for the deep stations having multiple sharp staircases

We improve the labelling of the figures and completed their captions as requested. We incorporated information on the Richardson number and shear turbulence and added the relevant variables for the two representative stations 09 (« WStairs ») and 20 (« NoStair »).
We used these results to detail the effect of turbulence in terms of shear instabilities for different types of profiles in section "4.1 Circulation and thermohaline staircases" of the discussion. This part of the discussion has been substantially reworded.

We clarified the terminology (e.g., Junge index as proxy for particle size distribution, deep scattering layer" as micronekton detected by ADCPs) and replaced ambiguous terms throughout the text.

Point-by-point response to the reviews

Second review of EGUsphere-2024-3436
Effect of double diffusion processes in the deep ocean on the distribution and dynamics of particulate and dissolved matter: a case study in Tyrrhenian Sea by *Durrieu de Madron et al*.

First, I would like to thank the authors for their detailed responses and for clarifying several points. The manuscript has been improved in several ways. For the most important: inclusion of additional examples of the influence of staircases on the particle retention process from profiles other than the initial two, better description and explanation of some of the processes with new relevant references, improved part 4.3 with a detailed view of the diffusive fluxes over the water column at station 9 instead of just one depth level.

My main point in the first review was the lack of strong evidence of the retention process of particles by staircases. The additional examples (Figs. 9 – 11) provide close-up views of the details at three steps on profiles that were not shown before. Does the revised version convince me? Unfortunately, I would say, not really, and I will explain why below.

I do understand the comments that measurements at meter scale (the scale of the steps/interfaces) of BAC, large particle abundances, … are not easy at all, and that there is a strong natural variability at small scales, that sensors have limited response time and noise, …

Several times, I have tried to convince myself that, what I observe (with the help of your text) in the figures clearly shows that staircases alter the "normal" (without staircases) behavior of particle settling. It's clear that the profiles are different since one has a sequence of steps and layers in your data (even if it's a bit noisier for some variables), the other shows a more usual gradual vertical gradients, as you mention. But my key question is: do the data show an anomaly in particle distribution, nutrient production or AOU, when there are staircases compared to when there are no staircases? And I'm still puzzled when I try to reconcile what I see in the data with your claim that clear evidences exist for particle retention process by staircases.

For example, the discussion of the two large-scale profiles (#9 and 20) falls short of the argument of an aggregation-retention process, which is the main focus of your paper. I don't know what to conclude at the end of line 356. Yes, staircases are also visible for oxygen, nitrate, small particles concentrations, and somewhat less so for large particle abundance. You end the short section with a comment about the Junge index decreasing more in the case of staircases, but, how should I interpret that?

Conversely, I could argue that the concentration of small particles decreases more when there are no staircases (no stairs. :0.447 → 0.440, with stairs: 0.448 → 0.4435). I would expect the opposite if steps act as a "barrier" with aggregation of fine particles. Moreover, I could also argue that there is about the same abundance of large particles between the two profiles at 2000 m, that is after having crossed all the interfaces of the staircases (i.e. no excess of the large particle production when there are staircases, as I would expect with retention-aggregation). The large particle profiles are difficult to compare, because at the origin, at 600 m, the abundance is not the same. The profiles do not start with the same 'initial' conditions. Therefore, what I just wrote 2 lines above about the decrease in large particles is biased, as well as what you wrote on lines 355-356: it is difficult to interpret the decrease rates in large particles with depth since the starting points are not the same (for large particles, and thus, for the Junge index). The proximity of the anticyclone may bias the no-staircase profile as marine production in the euphotic zone strongly depends on the vorticity field (e.g. Belkin et al. 2022, 10.5194/os-18-693- 2022). This adds another degree of complexity to the staircases-nostaircase comparison.

Fine particles: should decrease more with staircases (consumed by aggregation) Large particles: should decrease less with staircases (produced by aggregation) Thus Jungle index (fine/large) should decrease more with stairs, but the starting point in terms of fine and large particle abundance should be the same at shallow depth (above the first step of the staircase structure) for the comparison to be meaningful. Finally, we are left with the possible impact of staircases with the three close-up examples of steps (Figs. 9 – 11). The discussion of these figures ends up on l. 486 – 487, with the statement that the observed variations provide 'strong evidence' that staircases have a significant influence on particle abundance and size distribution. I agree that these figures show that staircases alter the distribution of the finest particles seen by the transmissiometer, the oxygen and the nitrate concentrations. This is easily seen when I compare the interface gradient with the upper and lower mixed layer from off the interface in these examples. For large particles (UVP measurements by particle size), there is not much evidence, point-to-point measurements show a large noise, noise that is as large as what the authors are trying to estimate. And therefore, I do not agree with the statement: l. 481, 'an increase is evident for the largest particles' (largest class seen by the UVP). Then follows a useful paragraph on the retention-aggregation process with references.

My conclusion is that the close-up views on these three interfaces do not show any clear evidence of retention-aggregation process, the main topic of the paper according to the abstract. They only show that mixed layers homogenize the distribution of fine particles (and nitrate, O2), and that in between, there are more or less large interfaces connecting the two mixed layers, as one would expect if one creates two successive mixed layers from an initially smooth constant gradient of properties. Unfortunately, this has nothing to do with proving the existence of a retention-aggregation process in this case of small density gradients across the interfaces.

To get such a proof, you have to show that staircases remove more fine particles and produce more large particles than you would observe in a region without staircases. This is very hard to do as the real ocean is not a controlled lab experiment where the initial conditions are the same. Given the large variability in your examples of interfaces, I would suggest doing a statistical study among all interfaces of the staircases observed in your entire data set (not just a pick-up of very few examples). For example, you could statistically study what happens within mixed layers on the one hand, and across interfaces on the other hand (sign, intensity of gradients, variability inside the mixed layer and inside interfaces), try to relate things to some parameters (dependence on the amplitude of the interface density gradient, …) and try to contrast these statistics with what is observed for the set of profiles without staircases and for the same depth ranges. You might find some undeniable statistical evidence of a retention-aggregation process. You could also look at the whole staircase structure of each profile, and try to relate whole depth variations in the abundance of fine and large particles, to the number of interfaces in the profiles, the cumulated thickness of interfaces, the intensity of the sum of the density gradients, … compare that with the 'typical' variations observed when there is no staircase and see if something clear emerges…. From my point of view and given the oceanic natural and instrumental variability, you cannot avoid such a statistical study to support unquestionably your conclusions about the aggregation-retention process, the main subject of your study.

Response to Reviewer 1 – General Comment

We thank the reviewer for his careful reading and for raising a number of important points regarding the interpretation of particle dynamics in relation to the presence of thermohaline staircases. We fully acknowledge the difficulty of comparing profiles that originate under different initial conditions, and we have taken steps to better quantify the influence of staircases using normalized approaches and statistical testing. By examining the slope of a relevant indicator of the abundance and size of particles over a shared depth range, rather than comparing absolute values, we minimize the effect of these confounding factors.

Therefore, to better understand the link between staircases and particle retention or aggregation, we performed a quantitative comparison of the vertical evolution of the Junge index for all deep stations, which have profiles characterised by multiple significant staircases in some cases and not in others. We selected this index primarily because it captures the relative abundance of fine versus large particles and is particularly well suited to detecting subtle shifts in particle size distributions between 80 and 400 μm. Additionally, unlike other parameters, the Junge index exhibited a relative linear trend between 700 and 1900 meters, making it more appropriate for regression analysis. Our analysis based on a multiple linear regression with interaction model, testing the difference in slope between the two groups, shows that :

The Jung index decreases significantly more slowly with depth in profiles containing staircases than in profiles without staircases (slope of -0.0007 m^{-1} vs -0.0008 m^{-1}; $p < 0.01$).

- This result was obtained by pooling data from multiple stations into 2 groups identified as "With Staircases (Wstairs)" and "No Staircase (NoStair)" and fitting independent linear regressions, which reduces the impact of noisy local variations and initial concentration differences.
- The slower decrease in the Junge index suggests that in the presence of staircases, the relative contribution of larger particles to the suspended particle pool is maintained deeper in the water column.

We have revised the discussion (section 4.2) to incorporate these statistical analyses.

We also applied this statistical approach to the AOU for both groups of stations. The results indicate a slower decrease in AOU with depth for stations with multiple staircase steps, which could reflect a more active microbial cycle and a greater contribution of organic matter degradation to the oxygen balance. We have incorporated this statistical analysis in the discussion (section 4.3).

Other comments:
l. 261–269: water masses acronyms have already been defined on page 3. No need to repeat.
As you rightly pointed out, the full names of the water masses were redundant at this stage of the manuscript. We have therefore removed them and retained only the acronyms, which had already been introduced earlier – except for TIW, which we now define upon first mention.

Fig. 2: add TIW on the subplots
We have added the TIW position to the temperature and salinity subplots, as for the other water masses. The TIW corresponds to the small subsurface water bubble at stations (13, 14, 20, and 19) in the anticyclonic eddy between 11 and 12° E.

l.276: 'The antcicylonic eddy…': first time it is mentioned, change for 'An anticlyclonic eddy…'
Done

l. 277–280: You can also mention the signature of the cyclonic eddy to the west of the anitcylone on the property distribution.
We have modified the sentence accordingly to also mention the cyclonic eddy located west of the anticyclone and its influence on the vertical structure of the water column. The revised sentence now reads:
'An anticyclonic eddy around 12°E and the boundary current jet, especially over the eastern part of the section, modify the intermediate water depth (EIW) by a few hundred metres without significantly affecting the deep water depth (TDW). To the west, the signature of a cyclonic eddy is also visible and contributes to the vertical modulation of isopycnals. These mesoscale structures affect the distribution of oxygen and nitrate, which increase less rapidly with depth beneath them.'

l. 288: 'The cyclonic eddy…', to be more accurate, I 'd write "The southern edge of the cyclonic eddy" given Fig. 3a. The center is sensibly further North.
We agree that the center of the cyclonic eddy is indeed likely located further north, as suggested by Fig. 3a. We have therefore modified the sentence as recommended in the revised manuscript to refer more accurately to "the southern edge of the cyclonic eddy."

Fig. 3a: add station numbers
We understand the interest in identifying the station numbers directly on the figure. However, after careful consideration, we chose not to add them in order to preserve the clarity and readability of the figure, which is already quite dense. For consistency and to avoid redundancy, we prefer to refer readers to Figure 1, where the station numbers on the map are clearly indicated. In addition, station numbers are included in the new Figure 3d representing the Richardson number, as requested by the third reviewer.

Fig. 4. To me, not essential. Could be removed if you need room for other figures.
We prefer to keep this figure as the estimation of instantaneous vertical velocities is rarely done and in this case, it is useful to consider their effect on particle sedimentation.

l. 317: To better locate immediately the turbid tongue, add: (sta. #19–20).
Done

l. 424 and following: In your data, staircases largely disappear under the anticyclone, not just on the periphery (although the section does not cross the core). There is no such removal of staircases in the vicinity of the cyclone to the west. Another possibility for eroding staircases, with a contrasting behavior between cyclones and anticyclones, comes from the focusing of near inertial internal waves and associated enhanced velocity shear and mixing in anticyclones (e.g. Fer et al., 2018: The dissipation of kinetic energy in the Lofoten Basin Eddy, J. Phys. Ocean. 48, pp. 1299–1316. Doi:10.1175/JPO-D-17-0244.1).
We have modified the text accordingly to reflect this more accurately. We also thank the reviewer for highlighting the potential role of near-inertial internal wave focusing and enhanced shear in anticyclones, as discussed in Fer et al. (2018). We have integrated this reference and the suggested mechanism into the revised version of the manuscript, as it provides a valuable hypothesis to explain the observed erosion of staircases under the anticyclone and the contrasting behavior near the cyclone. We added this sentence:
"This erosion is not observed near the neighbouring cyclonic structure to the west, possibly due to the focusing of near-inertial internal waves and enhanced mixing within anticyclones, as described by Fer et al. (2018) in the Lofoten Basin Eddy.'

Section 4.2, l. 451–455: This is the first time you mention the particle size spectrum and its slope (?), while no such spectrum is shown in the study. It's a bit confusing. I guess you are simply referring to the Junge index that was discussed before, which is a very rough proxy of what a real spectrum would be. I don't think the wording 'spectrum' has to be used in this study. The whole discussion can be done using the simpler Junge index that you show and discuss.
We agree. We added in the materials and methods section that the Junge index was a proxy for the particle size spectrum, and in section 4.2 we changed the reference to the spectrum to the Junge index. We also changed particle size spectrum by particle size distribution throughout the text.

l. 454: 'deep scattering layer', why scattering? Meaning in this context?
The origin of the deep scattering measured from ADCP hull data is explained in section 3.1.3 Turbidity, large particle abundance and Junge index. It essentially corresponds to the presence of live micronekton (small mesopelagic fish, crustaceans and cephalopods). In this part of the discussion on the characteristics of staircase stations compared to 'non-staircase' stations, we want to emphasise that the transition zone below the EIW between 400 and 800 m, where most staircases occur, is also a zone of dominant biological (grazing, predation) and biogeochemical (remineralisation, respiration) activity, which is likely to influence the abundance and nature of particulate material.

l. 468–469: 'macroscale' and 'microscale' are unfortunate in this context. Usually, in physical oceanography, vertical large-scale = O (>100 m), finescale (1 m – 10 m), microscale (1cm – 10 cm).
We have modified this section as follows:
'*The overall diminution in suspended particle abundance, concurrent with an augmentation of the coarse particle fraction, is evident not only at large vertical scales throughout the water column beneath the Eastern Intermediate Waters (EIWs), but also at finer scales spanning several decametres within the thermohaline staircases.'*

l. 474: the largest thickness among the three examples reaches ~150 m.
Done

Fig. 9–11: indicate what is the red line (mean, median, …)
We have now added a description of the data representation in the 3 figures captions to clarify the distinction between individual measurements and averaged profiles. Specifically, we now indicate that *'grey dots represent in situ measurements, while the red line corresponds to the averaged profile.'*

l. 578–579: homogenize writing of unit exponents
Done

Fig. 12: Thanks for this useful figure. Is the divergence in nitrate fluxes between two interfaces compensated for by nitrate production? If yes, does the number make sense in terms of nitrate production by processes?
Thank you for this question. We have evaluated if the observed divergence in nitrate fluxes between two interfaces could be accounted for by in situ nitrate production via organic matter remineralization. Based on our estimates, the divergence in nitrate fluxes between two successive interfaces ranges from 5 to 10 µmol NO_3^- m^{-2} d^{-1} over a layer thickness of 70 to 250 m.
To assess whether this is consistent with known remineralization processes, we considered published values of nitrate production in the mesopelagic zone (500-2000 m). Reported rates of nitrification and nitrate accumulation due to mineralization vary between:

- 1 to 30 µmol NO_3^- m^{-3} d^{-1} in the open ocean (Yool et al., 2007; Clark et al., 2020),
- 5 to 20 µmol NO_3^- m^{-3} d^{-1} in the Mediterranean mesopelagic layer (Christaki et al., 2021; Santinelli et al., 2015).

The nitrate production integrated over the water layer thickness is: Production = layer thickness × rates.

- For a layer thickness of 250 m (case for the mixed layer between the interfaces at ~1000 and 1250 m), the NO_3^-production = 250 m × 5 to 20 µmol m^{-2} d^{-1} = 1250 to 5000 µmol m^{-2} d^{-1}.
- For a layer thickness of 70 m (case for the mixed layer between the interfaces at ~890 and 960 m), the NO_3^-production = 70 m × 5 to 20 µmol m^{-2} d^{-1} = 350 to 1400 µmol m^{-2} d^{-1}.

These values are about two orders of magnitude higher than the observed divergence of 5–10 µmol m^{-2} d^{-1}, indicating that local nitrate production is more than sufficient to compensate for the flux divergence observed across the stair interfaces.

References

Yool, A., Martin, A. P., Fernández, C., & Clark, D. R. (2007). The significance of nitrification for oceanic new production. Biogeosciences, 4(4), 447–479. https://doi.org/10.5194/bg-4-447-2007

Clark, D. R., Mayor, D. J., Saunders, R. A., et al. (2020). The seasonal cycle of nitrification in the Northeast Atlantic. Global Biogeochemical Cycles, 34(11), e2020GB006564. https://doi.org/10.1029/2020GB006564

Santinelli, C., Ribera d'Alcalà, M., & Manca, B. B. (2015). The Mediterranean Sea dark inorganic carbon production. Geophysical Research Letters, 42(12), 4752 – 4760. https://doi.org/10.1002/2015GL064853

Christaki, U., Van Wambeke, F., Lefèvre, D., et al. (2021). Microbial food web functioning in the open Mediterranean Sea: A synthesis of two decades of experimental investigations. Deep-Sea Research Part II, 188–189, 104,939. https://doi.org/10.1016/j.dsr2.2021.10493

Second review of EGUsphere-2024-3436
Effect of double diffusion processes in the deep ocean on the distribution and dynamics of particulate and dissolved matter: a case study in Tyrrhenian Sea by *Durrieu de Madron et al*.

Suggestions for revision or reasons for rejection

I have carefully examined the revised manuscript by Durrieu de Madron et al., along with their comprehensive responses to my comments and those of other reviewers. I am pleased to observe a significant enhancement in both the readability of the text and figures, as well as in the scientific content, which is now presented in a more structured and coherent way. In particular, the Introduction and Materials and Methods sections have been significantly improved, they are now comprehensive, complete, and easily understandable. I also greatly appreciate the graphical abstract, which effectively conveys the essence of the article in a single image. Results show now improved figures and comprehensive information. The Discussion section has significantly improved, particularly in how it highlights scientific considerations supported by the results and presents coherent hypotheses and related references to explain the section that was previously regarded as weak. Additionally, the decision to include three more stations near the initially selected ones further enriches the considerations expressed. The conclusions are significant and provide an important element to the understanding of the salt fingers phenomenon, as this study represents the first instance (to my knowledge) where research on Tyrrhenian staircases transitions from abiotic to biotic components. The significance of double diffusion processes is growing in tandem with increased knowledge on the subject, and this study marks a significant step forward in our understanding of how these peculiar dynamics also influence biotic components.

We thank Reviewer 2 for the time and effort dedicated to the review of the revised manuscript.

Here some minor suggestion:

Line 46: If salinity is expressed in g kg^{-1} m^{-1}, you are referencing Absolute Salinity, as recommended by the scientific community. However, since “classical” salinity (PSU) and Absolute Salinity exhibit slight differences in their values (there is a small shift), it would be prudent to use a consistent expression. Given that your study employs salinity (PSU), it is important to avoid potential misunderstandings by clearly specifying when either salinity or Absolute Salinity is cited.

We agree. Although practical and absolute salinities differ by about 0.5% or less, a vertical absolute salinity gradient of 0.5 g/kg/m is almost equivalent to a gradient of 0.5 PSU/m. We have simply changed the unit and expressed the gradient in PSU/m for consistency throughout the text.

Line 184: in the final version there is a blue paragraph here, check it out!

Done

Line 143: Fuda et al. (2002) hypothesized that water denser than TDW found in the deep Tyrrhenian (2000– 3500 m) might form locally occasionally, but this hypothesis was not supported by further observations. Relevant references can be found in various recent studies, including Durante et al. (2021), which you have already cited. I therefore recommend the removal of this section.

As recommended, we have removed the corresponding sentence and the reference to Fuda et al. (2002).

Line 334: do you mean "potential temperature, salinity etc..."?
We have revised the sentence for improved clarity and readability. It now reads: "*In this section we describe the vertical distribution of physical (potential temperature, salinity, potential density anomaly), dissolved and particulate parameters (dissolved oxygen, nitrate, apparent oxygen utilization, beam attenuation coefficient, large particle abundance, and Junge index) at station 20, which shows virtually no significant staircase (Fig. 6), and station 09 which shows marked staircases between 700 and 1700 m (Fig. 7).*"

Second review of EGUsphere-2024-3436
Effect of double diffusion processes in the deep ocean on the distribution and dynamics of particulate and dissolved matter: a case study in Tyrrhenian Sea by Durrieu de Madron et al.

Overview
The manuscript by Durrieu de Madron et al. presents CTD, ADCP and optical profile data along a section crossing the Tyrrhenian Sea and investigates salt finger staircase effects on particulate and dissolved matter distribution. The authors have significantly improved the readability and presentation of their results, but there are still a few corrections and clarifications to be made, and some points that could be improved. Therefore, I recommend a minor revision based on the comments below.
We thank Reviewer 3 for their constructive feedback and positive evaluation of our revised manuscript. We have addressed the remaining comments and suggestions with care in this new version.

1 General comments
a) Add Richardson number Ri to Fig. 3, and buoyancy frequency N2, vertical shear S2, Ri depth profiles in Figs. 6 and 7; expand the discussion on turbulent mixing in terms of shear instability to support your claims on lines 414–443.
We have added to Figure 3 the distribution of Richardson, Ri, values along the section. We have also added to Figures 6 (station 20) and 7 (station 09) the profiles of buoyancy N (in cph), Ri, and a comparison between S2 and 4×N2 (to delineate the region where Ri <0.25). The turbulent zones are concentrated in the mixing layers that sandwich the density steps.
We used these results to detail the effect of turbulence in terms of shear instabilities for different types of profiles in section "4.1 Circulation and thermohaline staircases" of the discussion. This part of the discussion has been substantially reworded.

2 Specific comments

Line 167: It is hereafter referred to in the
We have revised the sentence to read: '*The UVP provides quantitative data on the abundance (in number of particles per litre, # L^{-1}) of large particles, estimated from raw images (Picheral et al., 2010), for different size classes between 80 µm and 2000 µm (equivalent circular diameter). This material is hereafter referred to in the text as large particulate matter (LPM).*'

Line 269: visible below Fig. 2: I would have expected distance to increase with increasing station number, so this is a bit confusing to me. Maybe at least mark east and west? Also, the panels are not aligned. Fig. 2 caption: Upper x axes show…

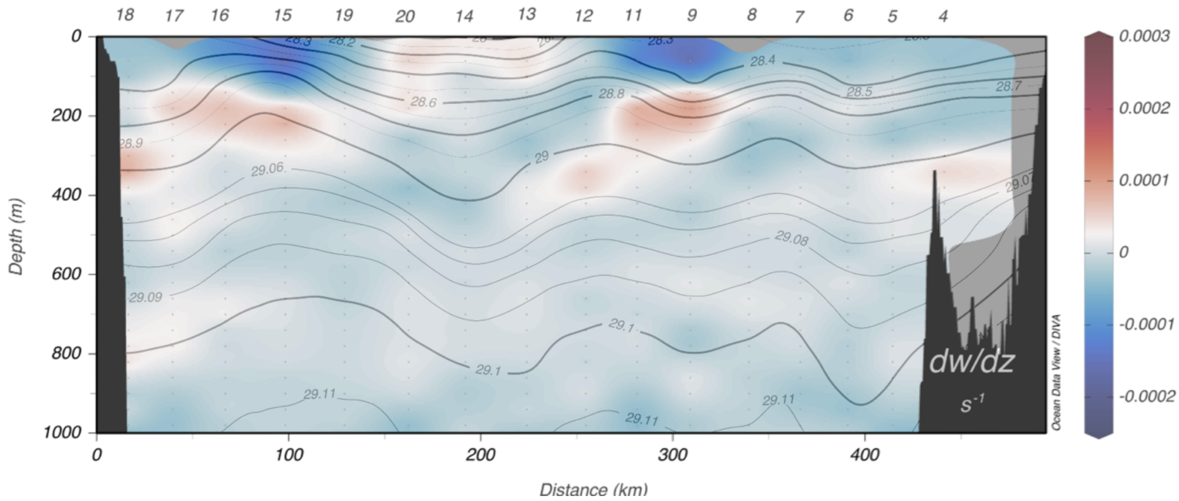
The initial sampling plan of the cruise was disrupted by the COVID-19 crisis, which led the amateur to urgently recall all vessels of the French oceanographic fleet to port. Having just recovered a profiling float and completed station 14, we were able to negotiate with the captain to continue the section to its western end (Sardinia) until the following morning, even if this meant leaving part of the transect unsampled. We then managed to persuade the captain to delay the return trip by a day to complete the section with the two missing stations (19 and 20). We have added information about station numbers to the caption of the figures showing the section.

Line 286: Dynamic topography and observed horizontal velocity reveal
Done

Line 295: Figure 3: (a) Absolute dynamic topography and surface geostrophic currents derived from daily Copernicus Marine Environment Monitoring Service Gridded L4 data (interpolated to 3.75 arcmin), averaged over the period of the cruise (14–16 March 2020). (b)
Done

Line 301: between 21
Corrected.

Line 305: upper 300 m. Also, what do you mean by vertical shear, $\partial w/\partial z$? This is potentially interesting since it represents divergence. Is the sign inside the eddy consistent with what you 1 expect from isopycnal slope? Fig. 5: mark (a), (b) etc
We did indeed mean $\partial w/\partial z$. Its distribution looks consistent with the expected structure of vertical motions in an anticyclonic eddy. We provided here a close-up view of this variable between the surface and a depth of 1000 m, as well as the density field. In this figure, the change in sign of $\partial w/\partial z$ observed around 100-200 m for station pairs 15-16 (west side) and 11-9 (east side) corresponds respectively to the western and eastern edges of the anticyclonic eddy, and borders the descent of the isopycnals towards the centre of the eddy at stations 20, 14 and 13. At the center of the eddy, in the first 200 meters, the vertical divergence ($\partial w/\partial z > 0$) indicates that downward motion intensifies with depth, and is directly associated with horizontal convergence ($\partial u/\partial x + \partial v/\partial y < 0$) as expected. Deeper down, between 200 and 500 m, the situation is reversed: vertical convergence ($\partial w/\partial z < 0$) indicates that downward motion is diminishing, accompanied by the horizontal divergence ($\partial u/\partial x + \partial v/\partial y > 0$) required to compensate for mass balance. On the flanks of the eddy, the change in sign of $\partial w/\partial z$ – positive between 500 and 200 m, then negative between 200 m and the surface – indicates that upward flow is intensifying as it rises from 500 m to around 200 m, before attenuating towards the surface.



We have verified that the sub-plots in each figure have been labelled correctly.

Line 327: tongues?
We changed the wording to 'tongue-like feature'.

Line 359, 367: delete 'and' Fig. 7 caption: 'Major density steps are delineated by horizontal grey lines. The horizontal dashed lines indicate the base of the main density interfaces.' – these two sentences are redundant
In order to maintain the clarity of figures containing numerous subplots, we have removed the density step boundaries.

Line 373: position
Done

Line 393: (a) Depth
Done

Line 394: basin measured by Fig. 8 is inconsistent between (a) and (b). Firstly, (a) is not marked. I presume the dots in (b) are steps identified using the procedure referenced in Methods; dots are lacking from (a). (a) shows isopycnals, (b) does not. What is 'average layer depth' in y axis label in (b), is it just depth (the float was profiling, not in isopycnal following mode, correct?)? Please present consistent information between the panels and clearly explain what is illustrated in the caption.
Yes, the float was profiling. We have redrawn the figures so that they are comparable (both include now the points indicating the stages identified using the procedure mentioned in the Methods section, as well as the isopycnals). The legend has been completed.

Line 404: wind forcing
Done

Line 427: of concurrent direct
We have revised the sentence to remove the term 'direct' as suggested, to improve clarity. The revised sentence now reads: 'While this co-occurrence does not establish causality, and in the absence of turbulence measurements, the potential disruptive effect of this eddy on staircase formation warrants consideration'.

Line 429: delete ' This observation is consistent with the emerging understanding of the dynamic interplay between mesoscale features and fine-scale thermohaline structures in the ocean interior.' – redundant

Done

Line 434: highlighted the persistence of

Done

Line 467: in suspended

Done

Line 468: coarse particle fraction

Done

Line 468–469: 'microscale' in the ocean is O (mm)-few cm! And 'macroscale' is not established nomenclature for a specific vertical scale. Please replace 'macroscale' and 'microscale' by something like 'scales between/of the order of …' to be specific.

We have revised the sentence to replace the terms 'macroscale' and 'microscale' with more precise descriptions of the vertical scales involved, as suggested. The sentence now reads: '*The overall diminution in suspended particle abundance, concurrent with an augmentation of the coarse particle fraction, is evident not only at large vertical scales throughout the water column beneath the Eastern Intermediate Waters (EIWs), but also at finer scales spanning several decametres within the thermohaline staircases.*'

Line 506: delete 'in the literature'

Done

Line 515: interfaces

Done

Line 542: sedimentation patterns, promoting

Done

Line 546: organic matter?

Done

Line 572: delete 'in'

Done

Line 577: assessed at the depth of each step 2

Done