Comments to authors on “Observations of tracer ventilation in the Cape Basin, Agulhas Current Retroflection” by Renske Koets, Sebastiaan Swart, Kathleen Donohue, and Marcel du Plessis, submitted to Ocean Science with a EGUsphere preprint discussion (Manuscript ID: egusphere-2025-3112).
Date of review: 08/08/2025
Reviewer: Referee#X

General comments

This manuscript presents a valuable high-resolution glider dataset collected in the Cape Basin, upstream of the Agulhas Retroflection, and combines it with satellite observations and derived dynamical diagnostics to investigate meso- to submesoscale processes driving ventilation and particulate organic carbon (POC) export. The study provides clear observational evidence linking low apparent oxygen utilization (AOU) at intermediate depths to recent ventilation events, and relates these signals to frontal structures, enhanced strain fields (FSLE), and shear-driven instabilities. The integration of glider-based physical and biogeochemical measurements, optical backscatter-derived particle metrics, and altimetry-derived strain diagnostics is a notable strength, offering a multi-scale view of transport and mixing.

The paper is generally well structured, the figures are informative, and the topic is highly relevant to understanding smaller-scale drivers of ventilation in energetic boundary current systems. I find the work to be of interest to the EGUsphere audience and potentially to the broader oceanographic community, though minor clarifications and some methodological details require further attention before publication.

Specific comments
(# points to line, section, or figure number)

#80
“The remaining thermal lag in the final dataset was found negligible, as the absolute difference between the mean of all climbs and dives in conservative temperature and absolute salinity at the thermocline was 0.04 °C and 0.015 g kg^{-1}, respectively.”
This difference is interpreted solely as the effect of thermal lag, but the glider is unlikely to sample exactly the same water masses during consecutive climbs and dives, and the observed differences could also reflect spatial variability, especially in such a frontal zone. I suggest that this point be acknowledged in the text, and that the authors provide an estimate of the typical horizontal distance between the end of a dive and the start of the subsequent climb, to put these differences into context. This mention could be mentioned in some clarification in the section 2.3.

#127
it should be specified, at least here when defining the terms, that the shear refers here to a “geostrophic shear”, to avoid confusion with any finer or smaller-scale shear that can be employed in the literature in mixing studies.

#128, Eq(3)

The term bx should be detailed, with the dx that is applied (I guess 1.5 km, given in section 2.3 ?).

#143
Please define the terms (theta, rho, S etc…).

#166
To help the reader be more familiar with FSLE diagnostics, please define the units of delta0 and deltaf (degrees ?), then please provide the correspondence (in meters) to help the reader understand the spatial scales.

#Section 2.5
The authors could anticipate the discussion about the difference between bx and FSLE by recalling in the paragraph the spatial scales “sampled” by the glider. It could help the reader understand better the experimental design limitations, and anticipate the discussion about this later.

#194
Low AOU discussed in the paragraph could be pre-pointed on the Figure 2 using the same kind of markers as in the Fig. 2d.

#Figure 2, Figure 4, and Figure 5
The authors could plot some reference isopycnal in bold (e.g., 27 kg.m-3), to better orient the reader during the description between Figures 2,4,5. The description could be defined backward from the Figure 6 that identifies the isopycnal of interest (e.g. the “barrier-27”, or the 26.5 too regarding POC, or the 26.25 for spiciness/AOU/POC).

224, FSLE
If the FSLE is scale-dependent, maybe you could better justify the choice of deltaf and tau ? Would smaller-scale choices (e.g. tau of 3 days …) shift the FSLE sensitivity toward faster, smaller-scale deformation — i.e., closer to what the glider sees in terms of sharp fronts and subduction ? Or maybe just adding noise to the estimates ?
This could be anticipated in the methods and then recalled there.

#301 and #311
“Sharp SST gradients”: Please report some value in the text to support the statement .

#351 “In some instances, the glider may cross into a different water mass, making it difficult to precisely locate the source of the ventilated waters. It is possible that these waters have been recently ventilated through surface processes in a neighboring region and are subsequently advected into the observed area.” …
This part is the occasion to discuss more the localized ventilation at approximately 80 km along the glider’s track, mentioned at the line #201, that was not much discussed and could be highlighted there.

Shear driven vs front induced, in section 3.4 and 4.1
In Sections 3.4 and 4.1, I found it difficult to understand the criteria used to distinguish the processes at 630 km and 660 km, given that both locations are associated with low Ri and

high FSLE. Is the distinction based on the weaker POC signal at depth for the front-induced case (Fig. 8b), or more simply due to the geometry of the glider path (e.g., more cross-front sampling at 630 km vs. more along-flow sampling at 660 km) ? I suggest clarifying this distinction in Section 3.4, so that the discussion in Section 4.1 is more clearly aligned with the synthesis presented in Fig. 8. In addition, a zoomed-in view of Fig. 7 might help support the description. For example, by adding a subplot showing the 35–37° S range on top and 37–38° S below, or by using a variable latitudinal grid to expand 37–38° S, or by including a supplementary figure.

Fate of the ventilation
The manuscript describes episodes of low AOU and enhanced POC at densities around 27 kg m^{-3} (~400 m depth), but it is not clear what their fate is further downstream across the basin. Could the authors elaborate on whether these water masses interact with other water mass types, and how (or if) they eventually connect to a branch of the AMOC? Some discussion of the potential spatial influence of these ventilation/export events would be valuable. For example, is there a region where FSLE signals are systematically more intense, indicating a persistent hotspot of this mechanism? Is the process observed here specific to the Cape Basin, or does it occur more widely in the surrounding South Atlantic? Finally, the manuscript could benefit from a short statement on the likely fate of the cumulative POC and oxygen anomalies generated by these events.

Tipos corrections

#142, Section 2.4.4
Please use Spiciness instead of Spice.

#341
Tipo (double parenthesis for the citation to be corrected).

References section:
Some DOIs have inconsistent formatting.