Response to Reviewer #2

We sincerely thank the reviewer for their thoughtful and constructive comments. We greatly appreciate the positive overall assessment of our manuscript and the recognition of the observational and modeling efforts involved in studying high-baroclinic-mode vortices (HBVs) in the eastern tropical North Atlantic. The reviewer's detailed feedback has helped us clarify and improve the manuscript. We have addressed all suggestions and concerns carefully, and we believe the revised version of the manuscript is substantially improved as a result. Below, we respond point-by-point to each of the reviewer's comments. Reviewer comments are reproduced in **black**, followed by our responses in **green**, and changes to the manuscript are indicated in *italics* and described where appropriate.

Anonymous Referee #2

Note: The reviewer comments are in **black**, our responses are in **green**

This manuscript presents a compelling study of high-baroclinic-mode vortices (HBVs) in the eastern tropical North Atlantic, combining shipboard and moored data with eddy-resolving model output. The authors describe how HBVs (whose subsurface cores are isolated from surface turbulent processes) transport low-oxygen water masses offshore from the eastern boundary. The study both provides evidence for the physical advection of low-O_2 water but also considers ongoing oxygen consumption via remineralization along the vortices' trajectory. These dynamics are discussed in the context of their potential implications for biogeography and biogeochemical cycling in the region. The observational challenge of capturing HBVs, given their relatively small spatial scale and intermittent frequency of generation, is well acknowledged. In that light, the dataset compiled and analyzed here is impressive and already provides a valuable contribution to the literature.

Thank you very much for this respectful word.

The use of numerical modeling to complement the observations is also appreciated, though I raise a few questions below regarding the model's ability to resolve these features.

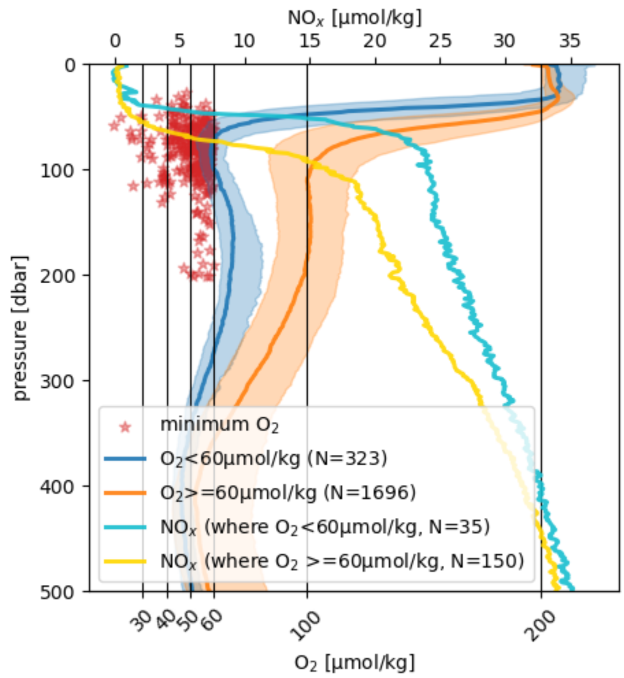
As you already mentioned, the model is used as an additional tool to complement the observations and to test the plausibility of the proposed mechanisms. We agree that the model, like any ocean circulation model, has limitations and does not fully capture the complexity of remineralization processes. To acknowledge this - since this point was raised by the other reviewers - we have added a few statements in the revised manuscript discussing potential biases (these are discussed in more detail further below in this review, e.g., the now changed L124, L330, and L770 in the manuscript).

Overall, I find the manuscript suitable for publication, pending some minor considerations centered around the following suggestions:

The authors might consider supplementing their HBV identification and tracking with additional water mass tracers such as spiciness and apparent oxygen utilization (AOU). These metrics, particularly spiciness (which is conserved along isopycnals), can provide clearer insight into the origins and evolution of the anomalies. For example, a panel showing spiciness in Figure 8 could strengthen the interpretation.

We thank the reviewer for this valuable suggestion. We analyzed nitrate as an additional tracer (as this was also suggested by another reviewer) and included a new plot (new Figure 8b) showing observed oxygen concentrations, the depth of the oxygen minimum, and the corresponding nitrate profiles from CTD casts taken inside and outside of low-oxygen eddies. These data reveal substantially lower oxygen concentrations between 80 - 250 m, accompanied by elevated nitrate levels inside HBVs, consistent with ongoing biological remineralization and thus "older" water. This supports the interpretation that HBVs represent persistent, isolated water masses rather than short-lived anomalies. We believe that this addition strengthens our observational evidence and nicely complements the model-based findings, as now discussed at the end of Section 4.5 (*Source water of high-barocline vortices*). In addition, we emphasize that our conclusions regarding the longevity of HBVs are not solely based on model results. Observational evidence, including the salinity-based analysis in Fig. 8, provides independent support for our interpretation, with salinity acting as a conservative tracer that confirms the coastal origin of the eddies and their offshore persistence. The figure 8b and a new paragraph are included as follows:

Line 711 and following: *"To further support the persistence and longevity of HBVs, we analyzed CTD observations of oxygen and nitrate inside and outside of low-oxygen events. Fig. 8b shows the median oxygen profiles for CTD casts with a minimum in the upper 200 m of the water column below 60 µmol/kg (blue curve) and those above 60 µmol/kg (orange curves). Mixed layer oxygen concentrations for both cases indicate increased near-surface biological productivity of HBVs compared to outside of HBVs. The red stars indicate the depths of the observed oxygen minima clustering between 80 to 120m depth. Corresponding nitrate profiles are shown in turquoise (<60 µmol/kg oxygen) and yellow (>60 µmol/kg oxygen). The results reveal substantially lower oxygen concentrations between 80 - 250 m inside HBVs, accompanied by elevated nitrate levels, consistent with enhanced accumulated biological remineralization due to enhanced productivity and/or "older" water. This observational evidence indicates that HBVs consist of persistent, isolated water masses rather than short-lived anomalies."*

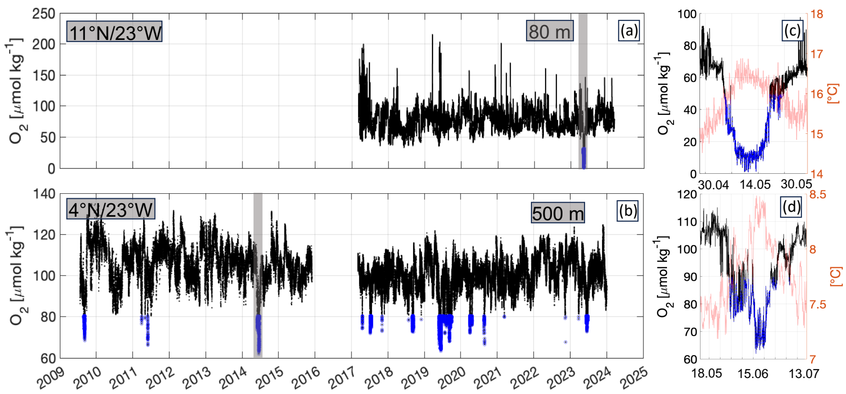


New caption: ***Figure 8b:*** *The blue curve shows the median of all oxygen CTD profiles with a minimum below 60 µmol/kg in the upper 200 m. The red stars indicate depths and dissolved oxygen concentrations of these minima. Orange curves represent profiles with a minimum above 60 µmol/kg. Shaded areas indicate the standard deviation. The turquoise line depicts the mean nitrate profile for the profiles with oxygen minima below 60 µmol/kg, and the yellow line shows the mean nitrate profile for the profiles with minima above 60 µmol/kg.*

I have questions around the decision to define HBV events using an arbitrary 10th percentile threshold (e.g., in Figures 3 and 4). This approach may flag low-oxygen "anomalies" even in relatively quiescent regions with little true HBV activity. In Figure 3a, for instance, I find only the event between 2023–2024 particularly convincing. It may be worth considering alternative detection criteria, such as thresholds based on standard deviations or interquartile ranges, which could provide a more statistically grounded definition of outliers.

Thank you for pointing that out. First, we would like to clarify that the majority of low-oxygen eddies are not detected solely using a percentile threshold. We define a low-DO extreme event in the CTD data as any profile with a minimum DO below 60 µmol kg^{-1} in the upper 200 m (e.g., the data shown in Figure 4). This threshold was chosen because values below 60 µmol kg^{-1} in the upper 200m of the water column are generally absent in the large-scale oxygen distribution of the open Atlantic (see Figure 2a), indicating that such low-oxygen events are associated with isolated transport from the coast or coherent, long-lived eddies combined with biogeochemical oxygen depletion. In the CTD dataset, 74 out of 976 profiles meet this criterion, roughly corresponding to the 10th percentile of all observations.

For the PIRATA mooring data shown in Figure 3, we cannot query the minimum in the upper 200 m, only at the depth of optode measurements, so we initially applied the lowest 10th percentile as a simple criterion. (In other analyses of mooring data throughout the manuscript, the presence of an HBV is further corroborated by velocity data). The PIRATA mooring data shown in Figure 3 were primarily just intended to illustrate variability and low-oxygen events as an overview of a long time series. Nevertheless, we acknowledge that alternative, statistically grounded thresholds should be applied, such as those based on standard deviations or interquartile ranges (IQR). Following this suggestion, we have now applied the IQR method to the PIRATA time series, which confirms and supports the identification of low-oxygen events while providing a more robust statistical definition of outliers. We changed figure 3 to:



And changed the text in the manuscript accordingly to:

L464: “*A low-DO extreme event is defined based on the interquartile range (IQR) of the respective time series, with events identified as values below the lower quartile minus 1.5 × IQR.*”

The number of detected events in the time series changed little, except at 11° N and 80 m depth, where one particularly strong event was observed. So, we adapted L469: “*At 11°N, about one event per year occurs at these depths, and at 80 m only one strong event was detected within seven years.*”

While the focus on anticyclonic eddies (ACEs) is understandable given their higher detection frequency in observations, the manuscript would benefit from a (slightly) more symmetric treatment of cyclonic eddies (CEs). Figures 9 and 10 do a good job of characterizing ACE dynamics and evolution via model output; a similar analysis of a representative CE from the model could be similarly instructive. For instance, this could be used to tie in the discussion around CE instability and decay mechanisms (e.g., interaction with high-PV water, lines 884–893). Including this could both reinforce the contrast between eddy types while also showing their similarities, at least from model output.

We thank the reviewer for this valuable suggestion. We agree that a similar analysis of cyclonic eddies (CEs) could be informative and would provide additional context regarding CE dynamics, instability, and decay mechanisms. However, performing a comparable in-depth analysis for CEs would effectively constitute a separate study beyond the scope of the current manuscript. In addition, our focus in the manuscript is on the observed high-barocline anticyclonic vortices (HBVs/ACEs), which are much more frequently detected in the dataset. Moreover, we do not consider the model to fully reproduce the statistics and properties of the eddies; the model is primarily used here as a complementary tool to support interpretation of the observational data. A detailed analysis of CEs would therefore rely solely on model output. We therefore chose to maintain the emphasis on ACEs, while noting in the discussion that CEs may exhibit complementary dynamics.

I also have additional minor comments, labeled with specific line numbers:

[L. 45-46]: The authors could cite the recent study from Deutsch et al. (2020) (https://doi.org/10.1038/s41586-020-2721-y). That study convincingly shows that temperature and O_2 shape the biogeography of marine organisms.

Thank you for pointing that out. The study is indeed very relevant, and we have now mentioned it there.

[L. 117]: The authors could introduce the acronym ‘SCV’ here after the first mention of submesoscale coherent vortices.

We have changed several instances of “SCV” to “HBV” in the text. “Subsurface Coherent Vortices” is now only mentioned once in the introduction, which is why we decided not to abbreviate it.

[L. 124]: Did the authors mean to write "mesoscale-permitting"?

We thank the reviewer for pointing this out, that was misleading. The model we use is eddy-rich, and at low latitudes it can resolve submesoscale features, though not fully. To clarify, we have revised the sentence to read:

Line 124: *However, ocean models are often submesoscale "permitting" only, in the sense that the model has sufficient resolution to begin representing submesoscale processes but does not fully resolve them, particularly with increasing distance from the equator.*

[L. 212]: This makes it seem like authors are only showing WOA oxygen during the model validation, but the authors frequently cite MIMOC data in the text. If MIMOC includes oxygen, please mention that here.

We thank the reviewer for pointing this out. You are correct - the MIMOC dataset publicly available does not provide dissolved oxygen. This was an oversight in the initial version of the manuscript. All oxygen figures with regard to spatial patterns, including Fig. 2a, were now generated using the World Ocean Atlas 2023 (WOA23). We have corrected the data source accordingly and removed all references to MIMOC in the revised manuscript.

[L. 219]: CM2.6 only has a resolution of 0.1 degree, is that high enough to resolve HBVs? It may be helpful to briefly discuss the model resolution (both horizontal and vertical) in the context of HBV scales, especially if the model is close to the margin of resolving such structures.

We thank the reviewer for raising this important point. The GFDL CM2.6 model has a nominal ocean resolution of 0.1°, which corresponds to roughly 10 km in our study region near 10° N. At these low latitudes, CM2.6 is mesoscale eddy-resolving and submesoscale-permitting, resolving only the larger submesoscale features (Hallberg et al., 2013). As shown in our Figure 1, the local first baroclinic Rossby radius of deformation (60–150 km in the area of interest) is resolved with approximately more than six grid cells in the model. However, the resolution is close to the lower limit for explicitly representing HBV-scale vortices, which typically have observed radii of 20–45 km (average ≈ 34 km in our observations). Nevertheless, the model is capable of reproducing coherent, high-baroclinic anticyclones with reasonably realistic horizontal scales, consistent with both our observations and previous studies (e.g., see Fig. 6 or Zhang et al., 2021; Frenger et al., 2018).

As our aim is to use the model primarily as a supporting tool to complement the observations and to provide additional insight into the origin and persistence of the vortices, we consider CM2.6 suitable for this purpose. However, we agree that this limitation should be explicitly mentioned. To address this, we have added the following clarification in the revised manuscript:

Lines 393 and the following: "*With a nominal ocean resolution of 0.1°, CM2.6 is mesoscale eddy-resolving and submesoscale-permitting at low latitudes, capturing only the larger submesoscale vortices. The local Rossby radius of deformation (60-150 km; Fig. 1) in the area is resolved, but smaller eddies are near the lower limit of resolvable scales. However, the model has been shown to simulate low-oxygen mesoscale eddies at latitudes poleward of about 12°*

(Frenger et al., 2018) and provides a useful framework in this study to complement the observational analysis."

[L. 235]: What do the authors mean by "five daily model outputs"? I'm assuming they mean to say the output resolution is every 5-days. Please clarify.

We meant that we used model output averaged over five-day intervals. We hope this is clearer now in the manuscript.

New Line 247: *Here, we used model output averaged over five-day intervals for the last 20 years of the simulation.*

[L. 260]: Could the upper OMZ in observations be caused by HBV advection? If so, doesn't that say that the model is not accurately capturing their influence?

The upper OMZ primarily arises from biological oxygen consumption in the upper water column, modulated by physical transport processes, including HBVs. For example, further north, Schütte et al. (2016b) estimated a reduction of 7-16 µmol kg^{-1} in the depth range of the shallow OMZ due to eddies. HBVs are therefore not the sole cause, but can locally enhance the intensity and position of oxygen minima. We agree that the model may underestimate the effects of HBVs on the upper OMZ, as the eddies in the model tend to be somewhat weaker and do not produce as strong low-oxygen anomalies as observed (e.g., Fig. 6). We have added a sentence in the revised manuscript (Lines 770 and following) to clarify that the model likely underestimates the impact of HBVs on the observed DO distribution.

Line 770: *The model tends to slightly underestimate PV and associated O_2 anomalies, indicating somewhat weaker eddy coherence compared to observations. At the same time, due to reduced dissipation in the circulation model, the lifespan of the eddies is slightly prolonged. Additionally, the MiniBLING model does not fully account for remineralization processes in the mesopelagic zone, which likely leads to an underestimation of oxygen consumption. Taken together, this implies that HBVs in the model appear with weaker anomalies but with an artificially prolonged lifespan, which we consider in our interpretation of the results.*

[L. 261]: Just a suggestion, but since the authors mention depths deeper than 500m, then panels in Figure 2-f could be extended to at least 700m (the deepest depth mention during the validation).

This is correct. We mention 700 m in the text as the lower boundary of the deep OMZ. However, for the manuscript and the focus of this study, the upper 200-300 m are most relevant. We aimed to focus on this region. Extending the axis further would reduce the visibility of key details in the figures. We therefore prefer to keep it as is. This choice ensures that key structures and variability in the upper OMZ (where the HBVs are) are clearly visible.

[Section 3.1.1 - 3.1.1 & 3.2]: While very useful to include, these sections could be moved to a Supplementary material. The methods section is quite long as currently presented, and these sections broadly introduce standard oceanographic methodologies (e.g., methods introduced in physical oceanography textbooks). However, if manuscript length is not a

concern, feel free to keep them in since they are very useful to frequently reference during discussion of results (Section 4).

We thank the reviewer for the suggestion. We carefully considered moving Sections 3.1.1–3.2 to the Supplementary Material (we also discussed that during the initial manuscript preparation). However, after internal discussion among the authors, we decided to keep these sections in the main text. We have no length restrictions, and we believe that including the detailed methodological descriptions is beneficial. While these sections are indeed extensive, many standard procedures (e.g., modal decomposition and fitting) are not always sufficiently or clearly described in the literature. Providing a step-by-step explanation within the manuscript helps ensure reproducibility and clarity. We therefore prefer to retain them in the main text.

[L. 392]: "The horizontal eddy center at each model time step". The authors don't mean the computation time-step here, but the output frequency of the model (5 days?). It could be helpful to clarify this.

Indeed. We thank the reviewer for pointing out this ambiguity and added: Line 426: "*The horizontal eddy center was determined for each 5-day model output and…*"

[L. 421]: You could present this additional time-series in the supplementary material.

We can do that. We have created a supplementary document where all the mooring time series are provided.

[L: 483]: Can the authors speculate on what is driving the events not linked to subsurface eddies (#5, #6, #8-10)?

Events #5 and #6 are most likely associated with subsurface eddies, based on their spatial and temporal characteristics. However, due to the lack of velocity data for these cases, we cannot conclusively demonstrate the eddy structure, which is why we refrained from making a definitive claim. For events #8–10, the interpretation is less clear. While one could speculate that an HBV core may have missed the mooring, and we are possibly only observing the southern edge of an HBV. In general, we chose to take a conservative approach, avoiding strong interpretations where the available data do not allow for robust confirmation.

[L. 523]: Just a suggestion, but the authors could use sea-surface height anomaly products (e.g. Satellite or re-analysis), mapped to the location of the mooring, to determine if there was a pronounced surface signature of these events. That could help determine if the feature is driven by surface-intensified ACEs (if strongly positive) or subsurface-intensified ACEs (if no or weak signature). The authors choose to do this for CTD profiles around L. 585, so why not extend this here? If their arguments from L. 585-588 hold, then mention this for the mooring data as well.

We thank the reviewer for this suggestion. We indeed examined satellite products extensively at the mooring locations. While very weak signals could occasionally be associated with the events, they were neither unambiguous nor as pronounced as those observed further north near the Cape Verde region. Tracking these features via satellite was therefore not feasible. This is actually a key point of our study: the smaller, southern HBVs cannot be reliably detected from satellite data.

Regarding the text around L. 523, we acknowledge that it was somewhat unclear. There, we intended to convey that the mooring data do not capture the complete vertical structure of the eddies up to the surface. The lack of a surface signature is confirmed by the satellite data. We added a sentence in the manuscript after that paragraph to make this point clearer:

L589: "*Notably, none of these vortices exhibited a clear surface signature in satellite data that could be unambiguously associated with the subsurface features*."

[L. 696]: In this section, the references for specific Figure 6 panels are incorrect. Please update them.

Thank you. This is done.

[L. 748]: This should be referencing Figure 9, not Figure 8.

Thank you. This is done.

[L. 862]: Why do the authors report the model having Ro of roughly 0.4 when the time-series in Figure 10 clearly shows lower values near 0.1?

This is correct: the value of ~0.4 from the model referred to a specific eddy snapshot shown in Figures 6g-l. The time series analysis presented in Figure 10 provides a more representative estimate of the model Rossby numbers, which are generally lower, around 0.1-0.2. Accordingly, we have updated the sentence in the manuscript to reflect this.

Line 946: "*Rossby numbers were below 1, with values of approximately 0.3-0.7 estimated from shipboard observations (one eddy crossing is shown in Figure 6; others not shown) and around 0.1-0.4 in the GFDL CM2.6 model simulation (exemplarily shown in Figure 6 and Figure 10e).*"

Typos (note there were several more, but I forgot to write their locations, so a second read-through is warranted):

Yes, we did that and corrected several typos.

[L: 170]: "…additionally a DO sensor…" (an —> a)

Done

[L: 424]: There a few typos in this sentence.

Changed in: Line 434: *As expected, both the DO variability and amplitude of DO anomalies are generally greater at shallower depths (e.g., 80 m), due to more intense near-surface dynamics and elevated background DO concentrations.*

[L. 914]: Africa.

Done

<u>References:</u>

Frenger, I., Bianchi, D., Stührenberg, C., Oschlies, A., Dunne, J., Deutsch, C., Galbraith, E., and Schütte, F.: Biogeochemical Role of Subsurface Coherent Eddies in the Ocean: Tracer Cannonballs, Hypoxic Storms, and Microbial Stewpots?, Glob. Biogeochem. Cycle, 32, 226-249, doi:10.1002/2017GB005743, 2018.

Hallberg, R. (2013). Using a resolution function to regulate parameterizations of oceanic mesoscale eddy effects. Ocean Modelling, 72, 92–103. https://doi.org/10.1016/j.ocemod.2013.08.007

Schütte, F., Karstensen, J., Krahmann, G., Hauss, H., Fiedler, B., Brandt, P., Visbeck, M., and Körtzinger, A.: Characterization of "dead-zone" eddies in the eastern tropical North Atlantic, Biogeosciences, 13, 5865-5881, 10.5194/bg-13-5865-2016, 2016b