*Response to Reviewer #1*

We sincerely thank the reviewer for their careful reading of the manuscript and their thoughtful and constructive comments. We appreciate the recognition of the scientific value of the observational dataset and the phenomenon we refer to as high-baroclinic-mode vortices (HBVs) in the near-equatorial North Atlantic. The reviewer raises important points, especially concerning the interpretation of HBV longevity based on model output and the potential limitations of the biogeochemical model representation. We value this critical perspective and have taken care to address these concerns in detail. In particular, we have clarified the limitations of the model, incorporated additional observation-based diagnostics where feasible, and revised key parts of the manuscript accordingly. 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**. Changes to the manuscript are described where appropriate and indicated in *italics*.

**Anonymous Referee #1**

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

**The authors describe a vertically and horizontally spatially constrained low dissolved oxygen (DO) event observed in the near-equatorial North Atlantic using moored and repeated ship-based observations, and investigate its origin using output from a high-resolution coupled climate model with a simplified Biogeochemical component. The manuscript provides a detailed description of the observational data and analysis methods employed. I find the observational documentation of the phenomenon the authors refer to as HBV to be of sufficient scientific value to merit publication.**

Thank you very much for this assessment.

**One major concern is that the authors argue that the long lifespan is one of the key characteristics of the observed HBVs, yet this conclusion appears to rely solely on the model output. As evidenced in the comparison between the model and observations presented in the manuscript, there may be non-negligible biases in the spatiotemporal variability of DO anomalies (see also my later comments). It should also be noted that general ocean circulation models tend to exhibit reduced dissipation of mesoscale/submesoscale structures, potentially leading to artificially prolonged features. Furthermore, the MiniBLING model employed here does not account for the diverse and complex remineralization processes that drive oxygen consumption in the mesopelagic zone.**

We thank the reviewer for raising this important concern. We would like to emphasize that our conclusions regarding the longevity of HBVs are not solely based on model results. Observational evidence, including the salinity-based analysis shown 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 (and we have also added, as you helpfully suggested, nitrate as an additional observational indicator in a new plot - see below or new figure 8).

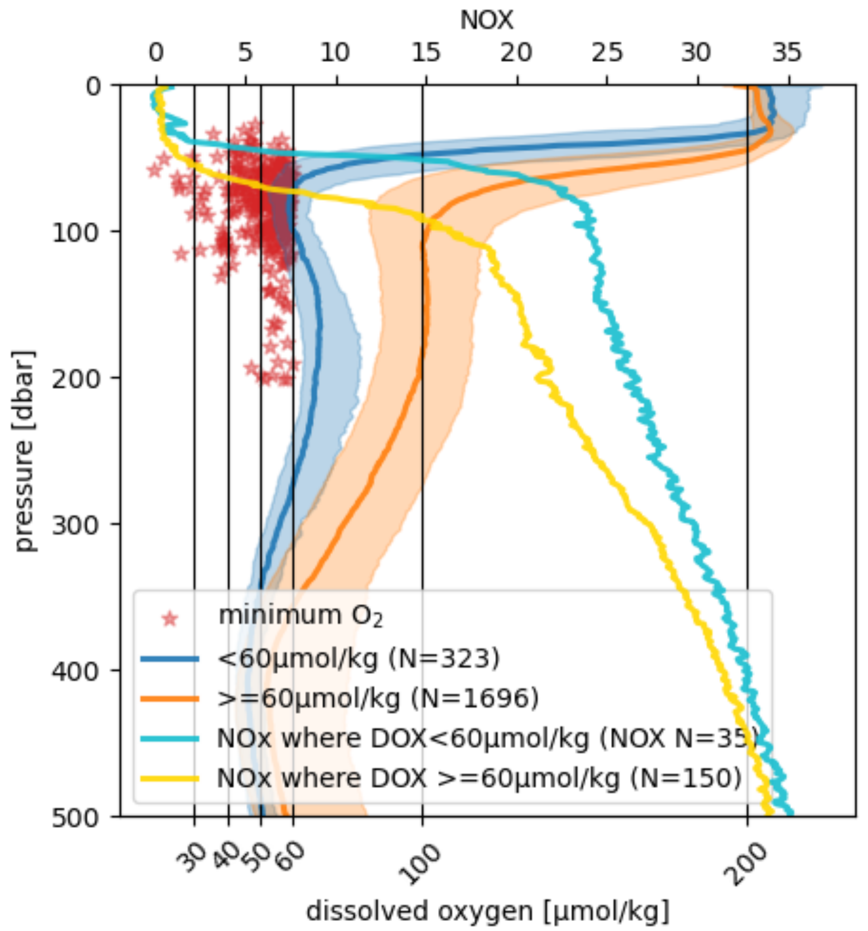
The model is used as (or meant to be) an additional tool to complement the observations and to test the plausibility of the proposed mechanisms. We agree that the model, like any ocean circulation model, has limitations and does not fully capture the complexity of remineralization processes. To acknowledge this, we have added a statement in the revised manuscript discussing potential biases:

Line 770: *The model tends to slightly underestimate PV and the 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.*

**To strengthen the argument for the longevity of HBVs based on observations, it would be beneficial to incorporate additional evidence, such as analyses using Apparent Oxygen Utilization (AOU), which carries information related to water mass age. If available, supplementary water mass diagnostics using other tracers (e.g., nitrate, phosphate) would also be valuable in corroborating the persistence of low-DO waters associated with HBVs, at least as circumstantial evidence.**

We thank the reviewer for this excellent suggestion. Following the comment, we have added 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 events. These data reveal substantially lower oxygen between 80 - 250 m, accompanied by elevated nitrate concentrations inside HBVs, consistent with ongoing biological remineralization and hence “older” water. This supports the interpretation that HBVs contain persistent, isolated water masses rather than representing only short-lived anomalies. We believe that this addition strengthens our observational evidence and nicely complements the model-based findings, as now discussed in the end of Section *4.5 Source water of high barocline vortices* as follows:

Line 711 and following: *To further support the persistence and age 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). In the mixed layer, oxygen concentrations are higher in the blue curve than in the orange curve, likely indicating increased biological productivity over the HBVs. The red stars indicate the depth 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 ongoing biological remineralization and hence “older” water. This observational evidence indicates that HBVs consist of persistent, isolated water masses rather than short-lived anomalies.*




New figure 8b 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.*

**Minor Comments:**

**Does the MIMOC dataset include dissolved oxygen? I could not find oxygen data in the source referenced by the authors (https://www.pmel.noaa.gov/mimoc/). If my understanding is correct, what dataset was used to generate Fig. 2a?**

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 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.

**Regarding Fig. 2a and 2b, it would further strengthen the manuscript if the authors could include a comparison map of the depth at which the oxygen minimum occurs.**

We thank the reviewer for this suggestion. This is indeed a useful piece of additional information. Rather than adding another panel to the already large Figure 2, we have incorporated this information into the new Figure 8b. In the figure caption, we added: "*The red stars indicate depths and dissolved oxygen concentrations of these minima*."
In the manuscript text, we also added the following at Line 721: "*The red stars indicate the depths and dissolved oxygen concentrations of the observed oxygen minima, clustering between 80 and 120 m depth*."

**Line 335: Is it valid to assume that the barotropic component is zero? In fact, the flow estimated from SLA (e.g., the gray arrows in Fig. 6a) may be significantly influenced by the barotropic component. If the barotropic flow cannot be assumed to be zero, wouldn't it be appropriate to subtract the barotropic velocity (approximated by vertically averaged velocity) from the observed velocity profiles as part of the preprocessing?**

Thank you for pointing this out. Typically, the barotropic component is negligible compared to the baroclinic components in the tropics, and SLA is clearly dominated by the baroclinic signal. However, we cannot estimate the barotropic signal since only velocity data from the upper 1000 m are available. The vertical average over the upper 1000 m is dominated by the first baroclinic mode, which has a zero crossing at around 1500 m depth. We believe that assuming a zero barotropic component is the best approach in this case.

**Line 376: *Castelao and Johns (2011)* and *Castelao et al. (2013)* are not included in the reference list. Please double-check that all cited works are properly listed in the bibliography.**

Thank you. Done.

**Line 385 ("The optimal eddy center allows…"): The meaning of this sentence, particularly the latter part, is unclear. Please revise for clarity.**

Yes, the sentence was not easy to understand. We changed it to: (Line 385) *Identifying the optimal eddy center allows us to analyze the circular (azimuthal) velocity around it. From this, we determine the eddy radius as the distance from the center where this velocity reaches its maximum - effectively separating the inner core of the eddy from its outer ring.*

**Line 407: The method for estimating the propagation speed is not clearly described. Please revise this section to clarify how the speed was calculated.**

We changed it to: (Line 412) *To estimate the propagation speed (c), we tracked the eddy core positions at each time step, defined by the streamfunction minimum. The speed was calculated as the horizontal distance between two successive eddy centers divided by the time interval between them*.

**Figure 3c,d: What does the x-axis represent? Please add labels or clarify in the caption.**

It is the time. We included in the caption: *(date on the x-axis)*

**Line 585: In the discussion of discrepancies between ship-based and satellite-derived observations, spatial resolution is indeed important, but temporal resolution is also critical. Note that the raw satellite data used for gridding does not have daily temporal resolution. In addition, what is the reason for omitting near-surface velocities in Fig. 6d and 6e? If such data are available, do satellite-derived velocities correspond better to near-surface velocities from ship-based observations, or to vertically averaged velocities?**

Thank you very much for this valid and important comment. We agree that, in addition to spatial resolution, the temporal resolution of gridded satellite altimetry also plays a crucial role. The limited temporal resolution (multi-day interpolation) can lead to small-scale, short-lived, or fast-moving mesoscale structures such as HBVs being missed or inadequately resolved. We have now addressed this point more explicitly in the revised manuscript:

Line 594 *One reason might be that that the resolution of gridded SLA from conventional altimetry (in time and space) is not sufficient to resolve such small-scale features.*

Regarding the velocity data shown in Fig. 6d and 6e: these are based on ADCP measurements from hull-mounted instruments. Due to blanking distance and flow distortions around the ship's hull, reliable velocity estimates are typically only available from depths of about 18 m and deeper. Therefore, we did not include near-surface currents in the figure, as they could not be reliably measured in our dataset.

However, a direct comparison to satellite-derived geostrophic velocities is meaningful, since the ADCP measurements at ~18 m still lies within the mixed layer and thus provide a reasonable representation of near-surface currents. We expect the main discrepancies between ship-based and altimeter-derived velocities to arise not from the ADCP sampling depth but from the coarse temporal and spatial resolution of gridded SLA products, which do not adequately resolve small-scale velocity structures such as HBVs - one of the points we want to highlight with this paragraph. Ageostrophic velocities are also a potential source of discrepancies, but likely of secondary importance in the context and resolution of Fig. 6.

**Line 626: Does this refer to Equation 9? Please clarify.**

That was indeed unfortunate, and the wrong equation was referenced in this case. We have now restructured the entire paragraph slightly and corrected the reference to point to the appropriate equation. The revised version starts at line 366.

*For the observed SCVs, we could not derive propagation speeds in a similar way. Instead, we followed an approach by Nof (1981) and Rubino et al. (2009), who formulated the westward translation of isolated high baroclinic eddies on a plane, which is given as a function of the $n$-th baroclinic Rossby radius of deformation and the Rossby number:*

$$C_n = -\frac{1}{3}\beta R_{d,n}^2 (1 - Ro)^{-1} \qquad (9)$$

*with $\beta$ being the meridional derivative of the Coriolis parameter.*

**Line 626 ("The solid black line represents…"): In Fig. 6f, I can only identify the line representing HBV speed. Could you clarify what this sentence is referring to?**

This is correct. We rephrased the sentence and now it is clear:

Line 652: *The solid, dashed and dotted black lines represent Ro = 0.5, while the shaded area indicates the range 0.3 < Ro < 0.7.*

**Line 603: Since Fig. 6 only displays modes 6 and 10, it is not possible to assess the behavior across modes 4 to 10. Please revise this sentence or clarify with additional figures if necessary.**

Thank you very much. That was unclear. We have changed the sentence as follows starting in line 632:

*These higher baroclinic modes n > 4 (exemplarily shown here for mode 6 and 10 for 9°N, 23°W in Fig. 7a and 7c for vertical displacement and pressure/horizontal velocity, respectively) have zero crossings in the upper few hundred meters, that are of similar vertical length scale compared to the vertical extent of low-DO HBVs (near-surface to 250 m, see section 4.2.1).*

**Line 692 (regarding Fig. 4a and 4b): There appears to be a stark difference in the zonal distribution of the strength of low-DO extremes (i.e., the lower end of the dots) between the observations and the model. The observations show the most pronounced low DO events offshore (around 24–21°W), whereas the model indicates such events occur closer to the coast. This inconsistency could point to a potentially significant model bias—possibly arising from the model misrepresentation of HBV origins, trajectories, or associated biogeochemical processes along the path toward 23W. Even though the mean fields (e.g., Fig. 2 and the medians in Fig. 4c) appear consistent, this does not guarantee that the model correctly reproduces the variability targeted in this study (e.g., the dot distribution in Fig. 4c, d). While observational data may be limited and subject to sampling bias—potentially explaining some of the discrepancies—this possibility should be explicitly considered. I encourage the authors to justify the model's suitability for this analysis, for example, by performing a model–observation comparison using pseudo-observations from the model. Alternatively, as noted earlier, the conclusions should not rely solely on the model results and should incorporate more observation-based evidence.**

We thank the reviewer for the careful examination of Fig. 4 a, b. We note that these panels compare individual shipboard profiles with model climatology over 20 years. Because the observations represent snapshots of particular events while the model averages over a longer period, differences in the zonal distribution of low-DO extremes are expected. This does not indicate a systematic model bias, but rather arises from the different sampling approaches.

To clarify this point, we have added text in the manuscript:

Line 748: *It should be noted that Fig. 4a,b compare individual shipboard observations with the 20-year model climatology. Observations represent snapshots of specific events, whereas the model averages over a longer temporal period. Consequently, apparent differences in the* *zonal distribution of low-DO extremes are expected and do not necessarily indicate a systematic model bias.*

We would like to point out that we have already performed such pseudo-observation comparisons with the model, which largely support our conclusions, while acknowledging that eddies and extreme events cannot occur synchronously in the model and observations. This is illustrated in Figures 6 and 9. Finally, we emphasize that our conclusions are not solely based on the model. Observational data, including the salinity-based analysis in Fig. 8, provide independent support for our findings. In addition, following the reviewer's suggestion, we have now included nitrate as an additional observational indicator, providing further evidence to corroborate the model-based results (see page 1 and 2 of this review).

**Lines 606, 776, 947: Please define "SCV" upon first usage.**

This was an error and should have read HBV. The mentioned instances have been corrected.

**Line 825 ("Rossby numbers were below 1"): Is there a corresponding figure showing the Rossby number distribution? If so, please reference it.**

Thank you for this remark. An exemplarily Rossby number from the model simulation are shown in Fig. 8e. For the shipboard observations, no figure is included in the manuscript, but the Rossby numbers were calculated (values of about 0.3 - 0.7) and are therefore reported in the text. To clarify this, we have revised the text to explicitly state that the observational Rossby numbers are not shown in a figure, whereas the model values are illustrated in Fig. 8e.

Line 825: *Rossby numbers were below 1 (about 0.3 - 0.7 found from shipboard observations, not shown; around 0.4 found in the GFDL CM2.6 model simulation, exemplarily shown in Fig. 8e).*

**Line 859: Please define "SACW" and "NACW" when first mentioned.**

Done

References:

Nof, D. (1981). On the β-Induced Movement of Isolated Baroclinic Eddies. *Journal of Physical Oceanography, 11*(12), 1662-1672. https://doi.org/10.1175/1520-0485(1981)011<1662:OTIMOI>2.0.CO;2

Rubino, A., Dotsenko, S., & Brandt, P. (2009). Nonstationary Westward Translation of Nonlinear Frontal Warm-Core Eddies. *Journal of Physical Oceanography, 39*(6), 1486-1494. https://doi.org/10.1175/2008JPO4089.1