We thank Reviewer #1 for the constructive and detailed review.

In view of the recommendation to restructure the entire paper, we reply only to the major comments. In the following, original comments of reviewer #1 are repeated in italics.

Scope of the manuscript: *The title focuses on oxygen and carbon, while the methods focus in great part on a new scheme for nitrogen cycling and two model setups, there are 2 results sections: one focusing on a model comparison mixed with O2 trends and another focusing on pCO2, the introduction mentions mostly just oxygen and acidification. The cycling of carbon is not even mentioned in the introduction. What is the scope of the manuscript?*

Changes in oxygen (deoxygenation) and the carbon cycle (acidification) are emerging problems for coastal areas, such as the Benguela upwelling system (BUS). Mean state and changes result from a complex interplay of physical and biogeochemical processes on very different time scale (meso-scale variability, seasonality, and long-term variability introduced by upwelling of "old" water). Various regional modelling studies put a focus on the mean state and interannual variability of oxygen, but even the mean state of the carbon cycle in the BUS was, to our knowledge, never addressed. The source/sink characteristic of the BUS is an open question.

Our study fills that gap by simulating long-term trends in oxygen and carbon with a model set-up that includes the temporal and spatial variability from meso to decadal scales. The model also includes feedback loops between biological production and OMZ variations due to a comprehensive nitrogen cycle. Nitrogen loss processes also affect alkalinity and could enhance ocean acidification. We show that a poor representation of the OMZ, as found in coarse resolution models, results is a very different answer of the regional anthropogenic carbon uptake.

Literature: *The authors miss on many important recent manuscripts that addressed the circulation and oxygen variability in the Benguela upwelling system using similar methods, and many statements read a bit dated. This is very evident in the Introduction, but also in the Results, as many of the findings presented as novel are not such. It also emerges from the fact that the authors recurrently cite the same few papers across the manuscript. The manuscript would really benefit from a better and more up-to-date literature review and a comparison with recent work. I am providing below a list of references that were missed by the authors, although the list is by no mean exhaustive.*

We are grieved that we did not study more carefully the very recent literature. We will include discussions on the latest findings and update the references in the manuscript.

Paper structure: *The paper's Introduction is too short and lacks in focus on the topics spanned by the analysis, section 3 "Model results compared to observations" mixes model evaluation with results regarding oxygen trends, and section 4 "Surface pCO2 and decadal trends of the carbon inventory in the BUS" suddenly shifts topic to pCO2. Model evaluation and Results are mixed up, which makes it very difficult to understand what the novel findings are presented by this study. Citations are better suited to the Introduction or Discussion (the latter is currently missing, which leads to this unclear structure). Due to this mix-up, I strongly suggest reorganizing the sections, separating literature review, model evaluation, and the study of the dynamics regulating oxygen distribution and trends. I would almost suggest to split this paper in two dedicated manuscripts: one on O2 and one on pCO2, which would help with both length and clarity. Or else, the connection between the two topics must be strongly clarified in the introduction. The manuscript is currently too long and not well organized in my opinion, the key findings get lost in the text.*

We will follow the suggestion to reorganize the manuscript. As mentioned by the reviewer, the whole model setup and implementation of the extended nitrogen cycle was time-demanding. We agree that large part of theses descriptions could be transferred to the supplement to improve readability.

A comparison of two models at different resolution: *I find many of the results of the model comparison between GR15 and higBUS rather predictable, as it is now expected that a model with lower resolution will not be able, for example, to resolve the upwelling pattern or intensity as well as a model with higher resolution. Many of the plots lack a comparison with the observations. I would suggest the authors to summarize the discussion of the differences between the two models only focusing on the really innovative results, merge the plots and/or move some to a model evaluation section or even a supplement.*

The coarse resolution model is a typical ocean configuration, which is applied in Earth system models for predicting future climate variation. We also expected most of the results. However, we want to contribute to the ongoing discussion on increasing the complexity of the biogeochemical cycles (e.g. by including an extended nitrogen cycle) in ESMs. Our study shows that, first and foremost, an improved representation of physical variability (i.e. mesoscale resolution) is needed to capture the observed trends in biogeochemistry.

What is the advantage of resolving the Pacific at such low resolution with the higBUS grid setup? *Shifted poles grids have been used in several studies before, especially for upwelling systems, mostly using cropped grids that covered only the Atlantic or Pacific basin, depending on the upwelling system of focus. What is the advantage of resolving the entire ocean and especially the Pacific at such low resolution of only 2 to 3 degrees (with all the consequences for atmospheric forcing, representation of currents and biogeochemical cycles) when the focus is the OMZ and the biogeochemistry the BUS?*

All model setups that are restricted to an ocean area/basin need physical and biogeochemical conditions at the boundaries of the model domain. In case of transient simulations, these boundary conditions should ideally include transient signals and their temporal and spatial variabilities. As shown by Espinoza-Morriberon et al. (2021), trends in oxygen within a model domain are sensitive to the variability prescribed at the domain boundaries.

The "beauty" of the global setting applied here is that all tracer distributions and their variabilities within the ocean are the result of a consistent interplay of the physical and biogeochemical processes represented by the model. Potential model deficiencies become visible and are not covered up by artificial boundary conditions. The long spin-up procedure guarantees a quasi-stationary initial condition for the transient simulation. It is also likely that our approach requires less computing time than a two-way nested model configuration.