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Comment on bg-2022-27

Anonymous Referee #1

Referee comment on "High-resolution modelling of long-term trends in the oxygen and carbon cycles of the Benguela upwelling system" by Katharina Dorothea Six and Uwe Mikolajewicz, Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-27-RC1>, 2022

Review of "High-resolution modelling of long-term trends in the oxygen and carbon cycles of the Benguela upwelling system" by Six & Mikolajewicz

Overview:

The manuscript by Six & Mikolajewicz presents a model-based study of the biogeochemistry of the Benguela upwelling system, providing a comparison between two global model setups: one based on a "traditional" model grid with polar refinement, and another in which the poles were shifted towards the region of interest, granting the area of study a finer resolution. The paper also presents a new scheme for the biogeochemical model HAMOCC, allowing a better description of the nitrogen cycle, which is very relevant in regions presenting suboxic conditions. The first part of the paper's results focuses on the comparison between the performance of the two models, spanning from a comparison of the general and regional circulation, and then diving into a comparison of the representation of the oxygen concentrations, and concluding that the higher resolution model setup has significantly better performance. Then, the authors present an O₂ budget discussing the oxygen trends in the BUS region over 1960 to 2009. Finally, they present their results on the role of the BUS as a source or sink of pCO₂

My general assessment is that the manuscript is not suitable for publication in the present state, and needs major revisions. A rejection with an encouragement to resubmit may also be suitable given the present state of the manuscript and the need of a thorough round of revisions, in my opinion. There has been clearly a large amount of work put into this

study, the analysis is probably there, but the way the work is presented in the manuscript is not clear and it's very difficult to understand what the novel findings are.

There are several problems with the manuscript in the present form: 1) the scope of the manuscript is not clear; 2) the authors seem not aware of much recent literature on the region, meaning that the paper that repropose already known results or lacks in the discussion of relevant processes; 3) large part of the model comparison provides predictable results and would better fit in a supplement; 4) the paper structure is unclear, with most of the results section reading as an introduction, model evaluation or discussion (or a mix-up of these); 4) the quality of writing is rather low and, as a result, the manuscript is difficult to follow.

Major comments:

- **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?
- **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.

- **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.

- **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.

- **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?

- **Quality of writing**
 The quality of English throughout the manuscript is often low, which makes the manuscript very difficult to read. Some sections, such as the Introduction & Methods' subsections 2.3 have poor quality of writing, while others, such as the Methods' subsections 2.1 & 2.2, are very easy to read and coherent. I have only provided a few

corrections in the detailed comments. However, the manuscript would greatly profit from an overall review of the language.

Specific comments:

General – Introduction: All the introduction needs rewriting. The English is really difficult to read and the citations are really scarce. The entire discussion is based on a very limited number of publications, many of which are also a bit dated, considered the amount of new literature on the themes. Some topics (such as ocean acidification) are barely introduced, others that may be relevant (such as stratification due to warming, changes in biological rates due to global change, previous studies of the mesoscale variability of oxygen and acidification trends, studies focusing in general on mesoscale processes and how they affect upwelling system biogeochemistry, a description of the Benguela circulation, a description of the drivers of pCO₂, a discussion of the relationship between oxygen and carbon cycle, etc) are left out. I don't think this Introduction is complete. I'm providing an (incomplete) list of papers to consider to widen the overview of previous literature touching on related topics:

Siegfried et al. 2019 <https://doi.org/10.1371/journal.pone.0210083>

Auger et al. 2021 <https://doi.org/10.1029/2019JC015272>

Stramma and Schmidtko 2021 <https://doi.org/10.1080/07055900.2021.1905601>

Brady et al. 2020 <https://doi.org/10.1038/s41467-020-15722-x>

Garcia-Reyes et al. 2015 <https://doi.org/10.3389/fmars.2015.00109>

Couespel et al. 2019 <https://doi.org/10.1029/2019GL084162>

Loescher et al. 2016 <https://bg.copernicus.org/articles/13/3585/2016/>

Thomsen et al. 2016a <https://doi.org/10.1002/2016GL070548>

Thomsen et al. 2016b <https://doi.org/10.1002/2015JC010878>

I also really suggest the authors to make a more complete literature review and really check their results against what has been done recently, in order to avoid repetitions and give more depth to the paper.

Line 18: Please, do not start with "There is..."

Line 19-20: Please, add "some", as in: "host some of the most productive ecosystems"

Line 25: Why only hypothesized?

Lines 36-28: Please, connect these two sentences, it's very hard to read.

Lines 41-43: There are a few global models at less than a quarter-degree resolution and definitely more than one at 25 km resolution, what global models are you referring to in this sentence? The ones studied by S  f  rian et al. (2020)? Please, specify.

Lines 49-50: Sentence is ungrammatical and lacks a comma

Line 61: In which upwelling systems is anammox important? Is BUS one of them? If yes, specify as this is relevant for the manuscript

General – Section 3 “Model results compared to observations”: This section contains too many parts that should be either in the Introduction or in the Discussion as a comparison with previous literature. This section should be split into a model evaluation (e.g. subsections 3.1-3.3) and in a dedicated section presenting the results of this study regarding oxygen, and not on what previous studies say. There is too much literature review in this section, which often evidences that the presented results are already known. The analysis of oxygen variability and trends, which should be the (first) topic of the manuscript according to the paper’s title is lost at the end of Section 3, which doesn’t even mention oxygen in its title.

Lines 255-257: A paragraph cannot be two sentences only (this applies throughout the entire paper). Additionally, the sentence describing what a spinoff is reads bad and seems to repeat in the following sentence, please, rephrase it.

Lines 373-377: This is expected from the choice of grids, I don't think this is really surprising or novel.

Table 1: GR15 seems to perform better than higBUS at reproducing mass flow through most passages. Could you please comment on this further in the text and discuss whether it impacts your results or not?

Figure 5: The two subplots look like if they have the same grid resolution, why? Was the model output regridded?

Lines 385-393: The oxygen budget and the drivers of oxygen variability were never explained in the introduction. Biological uptake via remineralization is not the only process determining the formation of the oxygen minimum on the Benguela shelf (e.g., Monteiro et al. 2006 <https://doi.org/10.1029/2006GL026234>, Siegfried et al. 2019 <https://doi.org/10.1371/journal.pone.0210083>). Why is the impact of physics (including the large scale flow) not discussed here? Also, line 393 "This is a first indication that different biogeochemical processes are at play in GR15 and higBUS." Why should the bgc processes be different if the model is the same? This is a very confusing sentence and should be rephrased providing a better explanation.

Lines 399 – 400 : "This is a first indication that different biogeochemical processes are at play in GR15 and higBUS." - This statement is too generic and doesn't hit the mark of highlighting what was found.

Lines 471-482: This should be in the Introduction or in a Model evaluation, this is not results.

General – Section 4 "Surface pCO₂ and decadal trends of the carbon inventory in the BUS": after such a long Section 3 containing many subsections, Section 4 is surprisingly short. On the one side, this is an advantage as the topic of this section is clear (again, section 3 could be split in 2 parts, one focusing only on oxygen). On the other side, also this section is too focused on a comparison with previous work. Please, use this section to

focus on your own results. The Discussion is suitable to compare your results with existing literature. The paper really misses a Discussion section.

Lines 534-539: This is not results

General – Conclusions: The conclusions will need rewriting once the manuscript will have a better structure and a more clear focus. There are too many topics touched upon in the conclusions, reflecting the fact that the paper doesn't have a very clear focus in my opinion. Again, the fact that model resolution matters is not surprising per se. The conclusions (and the abstract) should really highlight your novel findings.

Data availability: the model output should be uploaded on a repository.