Authors response to comments by Erik van Sebille to the manuscript "Physical processes in the upwelling regions of the tropical Atlantic" by Brandt et al. (pbrandt@geomar.de).
We would like to thank Erik van Sebille for the detailed and helpful comments to improve the manuscript. Below, we use black text for Erik van Sebille's comments and green text for our response.

RC2: 'Comment on egusphere-2022-1354', Erik van Sebille, 28 Feb 2023

Full disclosure: I am writing this review as Topic Editor for this manuscript. It has proven very difficult to find a second independent and knowledgeable reviewer for this manuscript, as most experts are already involved in the manuscript as authors. After discussions with two of the Chief Editors, I decided to write this second review as Topic Editor; focussing mostly on the effectiveness of the manuscript as a review article - Erik van Sebille

The topic of the manuscript (physical processes in the upwelling regions of the tropical Atlantic) surely warrants an in-depth review, and Ocean Science is an appropriate journal for it. The authors are from a wide and diverse range of institutes and expertises. This is all very strong and supports the trust in the review as unbiased and complete.

However, I have a few suggestions that I think will make the manuscript stronger and more impactful as a review article:

1. I encourage the authors to think about a schematic or figure that summarizes the key processes that are discussed in the paper. Such a figure might be hugely impactful, as it illustrates what the review is about

Difficult to find such a schematic. However, we improve and extended existing Figures to make the point of the review clearer.

2. The meridional extent of the region is never defined. While the zonal extent is discussed (line 54 and further), the meridional extend stays vague. Is it the 20S to 10N of figure 1?

We now removed the term “inner tropical Atlantic” and refer to the tropical Atlantic between 10°N and 20°S. The different upwelling systems are now marked in Fig.1.

3. Some of the modeling results (e.g. Figs 7, 8, 9) miss a clear reference. It is unclear now whether these figures have been created specifically for this manuscript, or come from another paper. In the first case, there should be much more discussion of the model setup etc (and it might be doubtful whether new/unpublished results fit in a review article like this); in the second case the captions need a clear reference.

We now state clearly the sources of the model output shown in Figs. 7, 8. and 9.

4. Section 5 misses figures. Whereas the other sections all have figures, this doesn't. Is that intentional? More generally, the style of the different sections varies quite a bit. I realize that each co-author was probably responsible for leading one section, but I would then still recommend carefully going through the text (and figures) to harmonize the style. That will help readers, and thus increase impact.

We included a new figure showing the interannual SST anomalies during the upwelling season of the three upwelling systems discussed (new Fig. 12). We also added the mean seasonal cycles of SST and Chlorophyll in the three upwelling systems in Fig. 1 and tried to harmonize the text as much as possible.

5. The manuscript tends to be fairly descriptive (answering what is happening) and relatively low on (physical) explanation. For example, line 601 states that "The zonal velocity field instead is dominated by the equatorial basin resonance of the 2nd and 4th baroclinic modes resulting in an EUC that vertically migrate largely independent of the thermocline (Brandt et al. 2016)" This sentence does not provide much information on _why_ it are the 2nd and 4th modes that are important here. That information may be in the reference, but the purpose of a review article is also to provide an accessible overview of the state-of-the-art; in this case (because of the title) also in terms of physical processes.

We are now more detailed regarding the explanation of the resonant equatorial basin modes of the 2nd and 4th baroclinic modes or the role of mixing in the TAUS for SST seasonality. Throughout the manuscript, we modified the text to better explain the different physical processes at work.

6. I somewhat missed a discussion of the similarities and differences with upwelling regions of the tropical Pacific and Indian Ocean. While the uniqueness of the basin is described well in the introduction; the extend to which the physical processes differ or agree in the other two basins is not very well discussed. I would expect that to be another purpose of such a review article?

In the final chapter, we added some open questions and future prospects regarding upwelling studies also relevant for the Pacific and Indian Ocean. We think that this will make the review article more attractive to a larger community.

Minor comments:

- line 14: 'inner' is a strange word here. 'Offshore'?

“inner” is removed throughout. We now refer to “the tropical Atlantic (10°N-20°S)”

- line 21: misses 'the' before 'northern boundary'

Changed

- line 22: remove comma after both

Changed

- line 22: nonlinearity in what?

It’s the nonlinearity of the momentum equations, i.e., the inertia of the Guinea Current. We removed the term nonlinearity in the abstract and changed to “… role of the Guinea Current, its separation from the coast and the shape of the coastline …”

- Figure 1: I don't understand the units of mg m-3. Until what depth is this then? Would it not make more sense as mg m-2?

This is the unit provided by Copernicus-GlobColour. In the description at https://www.copernicus.eu/en/access-data/copernicus-services-catalogue/satellite-ocean-colour it is stated: Mass chlorophyll-a per unit of volume of near-surface water.

- line 82: the capitalization rules between tAUS and EAUS are not clear.

We changed tAUS to TAUS throughout.

- line 145: is the ecosystem indeed nitrate-limited? Would be good to provide a reference for that implicit assumption

The tropical Atlantic is mainly nitrate limited, see e.g., Moore et al. (2004), which now is included in the reference list. For that reason, model studies mostly focus on the nitrate budget and nitrate fluxes (see, e.g., Radenac et al. (2020)), and, when describing their results, we use nitrate as well. In general, we discuss nutrient supply in upwelling regions and changed therefore from nitrate supply to nutrient supply.

- Figure 3: why not use the same time period for all four datasets. Most of the period overlaps, but the start and end years are not consistent. It would be stronger to harmonize that

Now, all data used in Figs. 3 and 4 are averaged for the common period 1998-2020.

- line 194: is this the mean in time? Or the mean in space?

Changed to “The annual mean tropical cells in the central tropical Atlantic”

- line 203: 'helps to define' is vague wording here.

Changed to “is the strongest cooling term of the mixed layer heat budget during the onset of the ACT and sets”

- line 210: 'seasonally varying'

Changed

- line 231-234: so what is the conclusion then?

We included additional panels to Fig. 5 to show the seasonal variability of the EUC and changed the text as follows:

Radenac et al. (2020) indeed showed a different behaviour of the EUC during boreal spring and autumn, where a shallow EUC during boreal spring might prevent upward mixing of nitrate compared the deep phase of the EUC during boreal autumn, when nitrate more easily reach into the shear zone above the EUC core (Fig. 5). However, also the equatorial role being at maximum strength during boreal autumn (Heukamp et al., 2022) might contribute to the nitrate supply to the mixed layer by upwelling slightly south of the equator.

- line 268: why use the fourth baroclinic mode? What is so special about the fourth one?

We expanded the explanation of the resonant basin mode:
This behaviour can be associated with the resonance of the equatorial basin at the annual period. The period of a resonant equatorial basin mode is given by the total travel time of an equatorial Kelvin wave and its reflected equatorial Rossby wave. For the width of the equatorial Atlantic basin, the resonance period of the 4th baroclinic mode is close to the annual cycle. This basin mode is associated with maximum eastward velocity in the near-surface layer in boreal spring and maximum westward flow in boreal autumn (Brandt et al., 2016).

- line 309: which capes are meant with 'the capes'?

We clarified: Cape Three Points and Cape Palmas

- line 329-330: the 25C threshold for upwelling seems fairly arbitrary. The paper that it is based on in almost 50 years old. Has no newer research been done on this?

The threshold for upwelling is indeed arbitrary but is often used to define upwelling in the tropical Atlantic, see, e.g., Caniaux et al. (2011). However, we think that a reference for such an arbitrary threshold might not be appropriate and we removed the reference to Bakun (1978).

- line 487: what type of model was used by Zeng et al (2021)? A bit more description might help readers gauge the applicability of these conclusions

We now write: By applying a regional general circulation model forced solely by barotropic tides at the open boundaries, …

- line 513: this first sentence of section 5 is very vague. Which part of the upwelling? Variability of what?What is meant with 'It is the area and season most impacted [...]'?

We removed this sentence and started with a new figure showing the interannual SST anomalies during the upwelling season of the three upwelling systems discussed (new Fig. 12).

- line 519: 'boreal summer' is a biased and confusing term. While not simply write the names of the months?

We changed that to: It is most pronounced in the equatorial cold tongue east of 23°W and peaks during June-July (Keenlyside and Latif, 2007).

- line 543: what is meant with 'first' here?

We removed “first”. It is not necessary.

- line 575: also provide a reference for the event in 2021?

We included a new Figure (Fig. 12) showing that event.

References

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