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# Review of Leseurre et al. for BGS

Anonymous Referee #1

Referee comment on "Summer trends and drivers of sea surface $fCO_2$ and pH changes observed in the southern Indian Ocean over the last two decades (1998–2019)" by Coraline Leseurre et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-22-RC1, 2022

Review of

Trends and drivers of sea surface fCO2 and pH changes observed in

the Southern Indian Ocean over the last two decades (1998-2019)

by C. Leseurre et al.

Summary

The authors present a detailed analysis of multi-decadal summer trends in fCO2 and pH in the Indian sector of the Southern Ocean. They present a clear, detailed comparison with past works to discuss trends and investigate the drivers behind the changes reported within. This work is valuable to the ocean carbon community, especially given the in depth analysis for a less-observed region such as the Indian Ocean. While I found the results stimulating and well-written, the section explaining the set up of the analysis left me often

confused and could use some attention to clarify for the reader.

Overarching comments

You are only evaluating summer trends in this work, which is valuable of course, but still limited in the interpretation of your findings. Your title does not indicate that these are specifically results only for the summer season. You state clearly on Pg 3, Line 11-13 that the pH trend in the Drake Passage varies greatly in the region and by season so the fact that the results discussed and analyzed within are only inferred from summer observations is an important fact to include in the title.

While the results of this analysis are valuable and clearly discussed (Section 4), the lead-up (Section 2 specifically) left me lost as to understanding what data was being used, where the comparisons were being made, over what regions (how many?), etc. Table 1 (as I mention below) is overwhelming and needs to be split into multiple tables to truly allow the reader to follow along (scattering these throughout Section 4 so that you can see the trends right there when discussing the comparisons would be helpful)

Technical comments

Pg 1, Ln 34: Awkward, consider rewording: "…since the start of the industrialization."

Pg 2, Line 6-7: Are the decadal changes in response to climate change (as stated in the sentence) or because of climatic shifts/transitions such as the SAM index (which is mentioned later in the sentence). I do not consider these things the same thing.

Pg 2, Line 25-26: consider saying, "…the model-specific/dependent evolution of the Southern Ocean carbon sink."

Pg 2, Ln 29-30: Consider comparing to (or at least referencing) the Ocean-SODA product: Gregor, Luke; Gruber, Nicolas (2020). OceanSODA-ETHZ: A global gridded data set of the surface ocean carbonate system for seasonal to decadal studies of ocean acidification (v2021) (NCEI Accession 0220059). NOAA National Centers for Environmental Information. Dataset. https://doi.org/10.25921/m5wx-ja34. Version 2021. NOAA National Centers for Environmental Information Dataset. (https://www.ncei.noaa.gov/access/ocean-carbon-data-system/oceans/ndp_103/ndp103.html)

Pg 2, Line 42: The stated trends here (+1.0 uatm/yr and +4.0 uatm/yr)- are they from literature or from this work? It's unclear since it's the same range stated in the abstract. If it's from other work (which would be more appropriate given that it is in the Introduction) please reference which papers that provide these specific trends.

Pg 3, Ln 4: In the Brown et al. 2019 reference, the analysis is 1993-2017 but then the explanation later in that sentence says it is analysis done "using few years of data". I wouldn't call 24 years "a few".

Pg 3, Ln 5: Disconnected transition between these two paragraphs. Consider adding a sentence to guide the reader on this transition.

Figure 1: I wouldn't say this region is the "South-Western Indian Ocean" but more so the Indian Ocean sector of the Southern Ocean.

Pg 4, Line 11: You say these observations are from the OISO cruises but then in Figure 2 caption (and elsewhere) you state that the fCO2 is from SOCATv2020. I understand that you used the fCO2 values from the cruises that the SOCAT program calculated (as opposed to the measured pCO2 values on the cruise) but perhaps consistency would help to clarify this. Are the observations in Figure 2 from all observations available in SOCATv2020 in this region? Or just from the OISO cruises? Perhaps these are indeed the only obs available in SOCAT for this region but I just find it confusing when you don't mention SOCAT at all in first paragraph of Section 2.2.

Figure 2: This is a very confusing figure. First, I don't feel that the black dots marking the cruise lines/fCO2 observations are needed. I do not understand the point of the yellow boxes (labeled in caption as "yellow squares" even though they are not squares and it is unclear their purpose of being labeled on the map. Could you just say that the data within +/- latitude bounds were averaged monthly for trend analysis? The red "squares" (again,

not squares and very difficult to identify the boundaries) are used the identify the 6 areas used for trend analysis. But in the abstract you say you do analysis over "three domains". Why such jagged and somewhat random region definition? Why not just N/S of the frontal zones and then define an area around the islands based on the chl bloom or simple lat/lon range? Overall, as a reader, just from figures 1 and 2 im confused and overwhelmed. Is it 3 regions or 6? Is it based on the fronts or not (many red and yellow-defined regions in Fig 2 cross the frontal boundary). Even a statement about why it's so important to have these strange boundary regions would at least allow the reader to understand the need for something as confusing as this.

Pg 5: After reading through your discussion of the observation methods and related uncertainties I think it would be very valuable to have a table (or add it on to another table) where you include these uncertainties due to measurement along with the trends you are seeing. The uncertainty calculation you explain on Pg 8, Ln 32 is outside of the uncertainty in obtaining the measurements themselves so this would be a worthwhile comparison to allow for in a table.

Pg 6, Ln 19-22: Here is where you explain the "three datasets" you will evaluate trends for. This needs to be more prominent, but also it is confusing because it is more than just 3 datasets as it is shown in Table 1. I understand the 3 to be 1 using underway fCO2, 1 using underway At and Ct, and the last using mixed layer values at stations. Perhaps acronyms or abbreviations for these would be helpful to define/assign because Table 1 is absolutely overwhelming in its current format. Which leads me to…

Table 1: There is too much text here in one table and it is too small. Perhaps putting the stations in a separate table? Or at least the bottomr 3 that are looking at the island bloom regions. Also, why 6 regions when everywhere else in the manuscript you say you are looking at "3 domains"? How do we now suddenly have 6? Why are we breaking it into "north HNLC" and "south HNLC"? no justification was provided for that. Also, in your Table 1 caption it repeats "the HNLC part of the north POOZ and the HNLC part of the south POOZ" which just adds to the confusion. Why present trends with different start/end years and call them comparable? Why not just truncate all time-series to a common trend? Do you consider if the significant trends are also significant with +/- one year of observations? I.e are they dependent on the specific start/end year or are the trends persistent for the long-term (20-ish) year time period overall?

Pg 6, Line 32: Please include your chosen definition of the mixed layer here.

Pg 6: How does the standard deviation and or *n* (number of observations) in underway compare to the values at the stations (given the monthly means you calculate here). Such a comparison would help to provide proof that the box is represented by the station.

Pg 7, Ln 14: How often was At not available? Also, did you compare this to the LIAR algorithm of Carter et al? Carter et al., Limnol. Oceanogr.: Methods 16, 2018, 119–131

Figure 5: I like the display of information here (and color designation for the HNLC/bloom stations) but perhaps include a small map of where these stations are located as a subplot to this figure. It would help with the interpretation immensely.

Pg 15, Line 21: Does the idea that the summer observed trend is close to the trend in the atmosphere imply that "there is no significant change in the CO2 uptake in the summer"? That seems like a confusing way to state it. It means that the ocean is tracking the atmosphere for these decades for summertime ocean carbon uptake. But to say "no significant change"? Change from what? Change caused by biological uptake in summer months as compared to annual trend? In the following sentences, when comparing to the stronger trend reported by Metzl 2009, that is a fair comparison to make but the word choice in that first sentence is inappropriate.

Pg 22, Ln 21-30: The paragraph discussing GOBMs, while valuable and definitely important, seems out of place here. Could you connect it to the work in the introduction to provide some motivation for this work instead of here in the conclusions?