Response to reviewers & editor

Editor

After thorough review of the manuscript and consideration of all reviewer comments, I recommend publication of this dataset subject to minor revisions as suggested by the reviewers. The information presented constitutes a valuable contribution to the field that merits publication, even though some concerns have been raised about the calibration procedures. The dataset provides interesting and potentially useful information that fills an existing knowledge gap.
I suggest including, around section 2.3, some sentences to acknowledge and document in the article the concerns of reviewer #2:
"I still have concerns about the calibration approach as no statistical test or method was used for that. As exposed by the authors, they just adjusted the STD gas value to the result it should give in comparison to the Ragged Point, Barbados (RPB) station. They have insufficient discrete samples to compare with the underway measurements, which also don't match on the AROC, the site with greater variability. The scatter plot helps to see the tendency of an agreement between measurements (discrete and continuous) in the oceanic part; however, it needs to take into account that it is just 5 points for 14,000 km, which is insignificant for a region with high variability."
Even though it is not possible to solve the issue due to lack of data, it is worth mentioning this potential weakness.
Once the authors have addressed the minor revisions outlined in the reviewer reports, the dataset will be accepted for publication in ESSD

Dear Editor, thank you very much for support and guidance throughout the review process. We appreciate your comments, and constructive suggestions, which have been very helpful in improving the manuscript.
We have addressed all the minor revisions suggested by the reviewers, as detailed below. In particular, we have added several sentences to Section 2.3 to transparently acknowledge the concerns raised by Reviewer 2 regarding the calibration approach and the limited number of discrete samples. Our aim was to clearly communicate the limitations of the dataset and ensure users are appropriately informed when interpreting the data. We added: As no simultaneous dataset can be used to cross-quality check the data, the agreement tendency between fCO_2 estimated from 5 samples and the continuous fCO_2 measurements is important, and is used here to validate the data. However, the number of samples, particularly in the open ocean, is very limited relative to the distance covered, which limits the statistical robustness of the validation of both the dataset and more importantly of the calibration approach. Users should therefore interpret the data, especially in coastal and river-influenced regions, with appropriate caution.

Reviewer 1

I'm happy to see that the authors put in significant effort to evaluate all reviewer's suggestions, not only mine. I'm feel overall satisfied with the changes and the responses. I honestly think that the revised version is improved compared to the original one. Data product is clearer now. That was my main concern considering the journal we are dealing with. Figures are also improved. I would like to see this article accepted after minor edits and technical corrections are done.
In the last lecture of the manuscript, I only did the following annotations:

This citation could be added to lines 57 and/or 80 and/or 86 and/or 90
https://bg.copernicus.org/articles/7/1587/2010/
Other useful cites: https://link.springer.com/article/10.1007/s00267-015-0630-x
Thank you pointing us towards these two interesting papers. The first reference has been included line 55 and the second one line 81.

Sentence ending in line 95 "sparse (CITATION?)." I miss a citation here.
Thank you we included a citation toward the global carbon budget 2024, that shows the unexplained and unresolved difference between the observed and model CO2 sink (one of the possible explanations being the lack of surface ocean CO2 data).

Table 1. Buenos Aires arrival date is missing. From August (08) to November (11) there are 3-months, not 4 as you say later in the conclusions.
Thank you for the valid remark, we didn't include the data of arrival to Buenos Aires because the dataset stops before. Furthermore, it doesn't stop on November 11th but at the limit of the Uruguayan EEZ on November 25th, and the end-date is now modified.

Line 648 (tracked version): The temporal resolution of "fCO2 measurements every minute" is mentioned here for the first time. This should be introduced earlier in the methods or results section, not in the conclusions.
Thank you for pointing it out, it is now included in the methods, section 2.5: "Following the recommendations of Pierrot et al. (2009) and of SOCAT, the dataset provides for each location and time step the measured data: molar fraction of CO2 in the equilibrator (xCO2eq), sea surface salinity (SSS), temperatures (SST and Teq), and pressure (Patm), the calculated variables (pCO2sw, fCO2sw), averaged over one minute."

Line 649 (tracked version): The dataset spans a four-month period but is not continuous. The conclusions should explicitly state the total number of days and hours with valid observations to clarify the temporal coverage.
We agree, the dataset spans over 3.5 months and we collected 45 days and 8 hours of valid fCO_2 coverage. In the text, we modified the sentence by: this dataset of 65,000 measurements spanning over almost 4 month (from August to the end of November 2021, for a total of 45 days and 8 hours of valid fCO_2 data) shows large fCO_2 variability.

Line 650 (tracked version): The inclusion of the standard deviation here is not informative, as it is inflated by the high values from the Amazon River plume.
Thank you, we agree and removed the standard deviation.

Line 656 (tracked version): I would replace "heart of the plume" with "core of the plume" (?) to avoid physiological metaphors.
We agree, and replaced heart of the plume by core of the plume.

Reviewer 2

Dear,
I would like to acknowledge the authors for the responses to the suggestions and concerns that were raised and the good job they did to improve the manuscript after the major reviews. The manuscript is better written and clear, the figures are more comprehensive and contribute more to the understanding of the dataset now.
Due to the changes, I agree with the authors that the dataset should be submitted as whole, and not only the Amazonas River-Ocean Continuum (AROC) part as suggested previously.
I still have concerns about the calibration approach as no statistical test or method was used for that. As exposed by the authors they just adjusted the STD gas value to the result it should give in comparison to the Ragged Point, Barbados (RPB) station. They have insufficient discrete samples to compare with the underway measurements, which also don't match on the AROC, the site with greater variability. The scatter plot helps to see the tendency of an agreement between measurements (discrete and continuous) in the oceanic part, however it needs to take in account that is just 5 points for 14.000 km, which is insignificant for a region with high variability.
Thank you for raising this important point. We fully understand the reviewer's concerns and agree that the limited number of discrete samples, particularly over such a long and variable transect, restricts the robustness of any statistical validation of our calibration approach. Given the constraints of the dataset, we chose what we considered the most appropriate method, but we recognize its limitations.
To address this, we have revised Section 2.3 (Validation) to explicitly acknowledge these limitations and emphasize the need for caution when interpreting the data—especially in dynamic or river-influenced regions. Our aim is to be fully transparent with the users about the calibration choices and the associated uncertainties. We added:
As no simultaneous dataset can be used to cross-quality check the data, the agreement tendency between fCO_2 estimated from 6 samples and the continuous fCO_2 measurements is important, and is used here to validate the data. However, the number of samples, particularly in the open ocean, is very limited relative to the distance covered, which limits the statistical robustness of the validation of both the dataset and more importantly of the calibration approach. Users should therefore interpret the data, especially in coastal and river-influenced regions, with appropriate caution.

I also have a few minor comments on some sentences in the manuscript that I included below.

Regarding terminology, the authors use the word "significant" in the manuscript but they don't have any metric to support it. Therefore, I suggest they change it through the whole document, for example in:
210-215: In "Tara crossed highly variable regions during its voyage, supporting our confidence that the uncertainty on the dataset due to this span value is not significantly impacting the results".
Thank you for the comment. We have replaced the word "significant" throughout the manuscript with more appropriate alternatives, except in the introduction where the phrase "significant carbon sink" is supported by previous studies. In particular, around lines 210–215, we also took the opportunity to highlight the limitations and uncertainties of the calibration approach, in line with the concerns raised in previous comments.
Tara crossed highly variable regions during its voyage, supporting our confidence that the uncertainty on the dataset associated with this span value has limited influence on the overall results, but should nevertheless be take into account when analyzing the dataset.

410-415: Nevertheless, we observe significant variability of fCO2 even if the salinity does not change anymore.
In this example, we removed the word significant

250-255: In "As no simultaneous dataset can be used to cross-quality check the data, the good agreement between fCO2 estimated from the samples and the continuous fCO2 measurements is quite important, and the mean differences are in the range of uncertainties related to inferring fCO2 from the DIC and TA measurements."
This is not possible to say as in the manuscript there are only 5 samples to compare (figure 4). My suggestion is to delete or rephrase - suggestion "As no simultaneous dataset can be used to cross-quality check the data, the suggested agreement/agreement tendency between fCO2 estimated from 5 samples and the continuous fCO2 measurements is important, and is used here to validate the findings as the mean differences are in the range of uncertainties related to inferring fCO2 from the DIC and TA measurements."
We agree, thank you for the nice reformulation, we just modified 5 by 6, it is now included in the manuscript.

405-410: In "The zone of lowest SSC is between the isobaths 10 m and 20 m, associated to a diatom bloom in 1983 (Curtin and Legeckis, 1986; DeMaster et al., 1986). It is also the region where we observe the transition from a source to a sink of CO2 (Fig. 9)."
I don't understand the relevance of mentioning a bloom from 40 years ago. As a reader, it gave the impression that it is a recurrent event that justifies the authors finding of the transition of behaviour (source to sink).
Now I know that the authors want to compare their results with the first literatures and descriptions in the region, but for this sentence my suggestions are to see if there is an

updated literature about the suspended sediment characteristics of these zones, or some literature that supports a recurrent bloom in this area. If none is available, simply rephrase. Suggestion: “The zone of lowest SSC found by Curtin and Legeckis, (1986); DeMaster et al., (1986) is between the isobaths 10 m and 20 m, which it is also the region where we observe the transition from a source to a sink of CO2 (Fig. 9).”
Thank you for this perspective. We understand what the reviewer means, and indeed is was not in our intention to use a singular event to justify a permanent behavior. We modified the manuscript according to the sentence suggested.

460-465: In “Despite sampling most of the American coastline along the South Atlantic Ocean, it represents only a small fraction of the world’s coastlines”
This sentence plays against the relevance of the manuscript. If this is just a small fraction of the global coastlines why is important to study it then? I suggest deleting it.
We wanted to highlight the need to increase the data coverage along the coast, but we do not want to deserve our manuscript, so we deleted the sentence as suggested.