nature portfolio

Peer Review File

Spatiotemporal high-resolution mapping of biological production in the Southern Ocean

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

6th Jun 23

Dear Dr Pan,

Your manuscript titled "Spatiotemporal high-resolution mapping of biological production in the Southern Ocean" has now been seen by 3 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

In particular, we require that you (1) provide comprehensive method details, which should include making your code and data available for peer review, and clearly communicate any limitations or uncertainties in your approach, (2) provide a balanced in-depth discussion of the related literature and clearly communicate the advance and new insights your analysis offers.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Jose Luis Iriarte Machuca, PhD
Editorial Board Member
Communications Earth & Environment

Clare Davis, PhD
Senior Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

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Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The manuscript “Spatiotemporal high-resolution mapping of biological production in the Southern Ocean” provides a new estimate of Southern Ocean (SO) net community production (NCP) based on a novel dissolved inorganic DIC parameterization which is applied to autonomous BGC-Argo float data (2007-2019). The DIC parametrisation is based on its relationship to temperature (T), salinity, dissolved oxygen (Do) and pressure (PR) from a large-scale ocean data depository (GLODAP) using multiple-linear regression (MLR) as well as Neural Network (NN) approaches. Results are further explored to investigate DIC drawdown (NCP), restoration (RES) and to calculate carbon removal (CS) in different areas of the SO and over time. This is important and novel information to further the understanding of the role of the Southern Ocean as a carbon sink under current and future climate conditions. The presented research has high relevance across various disciplines in Earth system science and will be an important contribution to Southern Ocean marine science.
Unfortunately, in its current form, the manuscript is tedious to read, and critical information is buried in - often wordy - text. This reviewer therefore suggests that the authors significantly revise the manuscript in order to make it more concise and to the point.
In addition - and this reviewer view may be partly affected by the current somewhat unclear manuscript language and strcuture - some key aspects/methods need to be further explored and better explained. The following questions and issues need to be addressed and clarified.
These are:
1) The NN parameterisation is based on the GLODAP data products. Where only CTD bottle data used, or was a merged (=gap-filled) GLODAP data product used? If the latter was used, how would that affect error estimates? Please clarify.
2) Most of NCP is occurring in the upper mixed layer (<200 m). This reviewer understands that full-ocean depth GLODAP data were used to train (and evaluate) the NN parameterizations. Would be using only shallow data (<200 m) allow for a better parameterization? Could Fig S1 include/be expanded to also show “zoomed in” plots for the upper 0-200 m depths? The authors need to

strengthen their arguments that they followed a plausible approach in the NN-parameterizations, their application to the BGC-Argo data and subsequent spatial extrapolation methods. There currently no clear thread through the manuscript.
3) This reviewer remains sceptical about the ODV-based extrapolation of results towards the Antarctic coast and into areas with very few BGC-Argo profiles. Could the approach and its validity be better explained?
4) Ice extent was correlated/compared to NCP in the 45deg to 60deg band. Antarctic sea ice extent (e.g., in the Indian and Pacific sectors) is generally not reaching much north of 60 deg, and thus this band might not be an appropriate area to expect influences from sea ice "dynamics". This reviewer suggests to remove this section on sea ice or provide an in-depth justification for using the NCP data from 60-45 degree for the comparison with sea ice.
5) The aforementioned text/figures on sea ice extent could be deleted and a BGC-Argo nitrogen-based NCP estimate (from floats with N-sensors) could be established and used for a direct (float-based) comparison of nitrate-drawdown NCP with the newly presented DIC-NN-NCP estimate.

Some specific comments:
Manuscript:
L 1: "net community production" is a widely used term and could be used in the title?
L 4: one example where language could be more concise: replace …."effects are changing"… with …"impacts"…
L 8: delete "ocean biological production"
L14-19: suggest to reword as currently it suggests that most of the SO (and defined as the ocean south of 30deg) is an HNLC area. Large parts are, but not most parts.
L 25" severe weather"? or rather "atmospheric"… conditions?
Line 46 -64: Suggest to combine this into a single short paragraph. Maybe also mention ice cover effects on outgassing.
Line 65 – 76: Maybe bring this paragraph up to the front to clearly articulate the aims/objectives of this research. Suggestion to add a sentence on the Sea Ice Carbon Pump (e.g. Rysgaard et al. 2007, JGR).
L 94: "coasts" are generally associated with land not with oceans, suggest to rephrase
L 104: It remains unclear how an about 20% increased heterogeneity was established/calculated.
L130: "largest and widest NCP area" ? please clarify or reword.
L134-138: As per general comments, this reviewer remains sceptical about the spatial extrapolation method (ODV-based) applied to the data, in particular in data-sparse areas with little BGC-Argo float coverage. This is particular true for Antarctic shelf areas with extensive ice cover, polynyas as well as ice-edge blooms.
L148: "to predict the future", no predictive results are presented. Suggest to reword.
Line 228: Please expand the explanation(s) why NN-constructed parameterizations can be applied to surface mixed layer BGC-Argo data, while MLR-constructed data can not. Is this just because NN is a non-linear approach? FigsS1 alone is not sufficient to justify this statement which is key to the overall findings of the manuscript. This needs better explanation and justification.
L409: Suggest to explicitly define production and restoration period in this Figure caption.
L426 - Fig 3: As per above general comment, the time-series analysis of NCP and RES with sea-ice extent using NCP and RES data from 45-60 degree band appears problematic.
L 436: Multiplied "by"
Table 2: Fix typo "parameerization"?

Supplementary Material:

L6: “NN-based DIC parameterization” rather than “DIC NN-based parameterization”?
Line 62: As per general comment/question: Can the full depth data parameterization directly applied to surface data? Is this introducing specific errors?
L67-70: shorten
Tab S1: Table caption: Suggest to replace “plunged” with “deployed”

Reviewer #2 (Remarks to the Author):

Overall Impression
The paper reconstructs upper ocean DIC in the Southern Ocean between 2004 and 2019, using BGC Argo float observations and a Neural Network approach. From the DIC estimate, they calculate the net community production (NCP) and restoration rate (RES) in this region. They then map the NCP and RES onto a regular grid to estimate the total NCP and total RES, and then estimate the temporal trend of the NCP. While the paper claims that no DIC or NCP estimates exist to date in the Southern Ocean, this is not true, as we have several global-scale estimates of both. This study is more regional and might be able to better resolve the DIC, NCP, and RES, but at the least, it should be compared to the previous estimates. Also with other aspects, the authors missed to cite important previous work. Saying that this study has a novel aspect: the authors show how the Southern Ocean NCP has changed over time, which to my knowledge had not been estimated yet (although we do have DIC estimates from which we could estimate it). The authors then extrapolate this information further to conclude how the NCP will change with climate change, which is not possible based on such a short period (2008-2017). In addition, the DIC reconstruction method is not clear from the main text. There is more technical information in the SI, but even there it is not fully detailed what the authors did. Further, the document would benefit from copy editing. It’s mostly understandable but in many cases does not flow well or uses unusual grammar. The paragraphs don’t always flow logically and often deal with multiple ideas/topics, especially in the introduction, but also throughout the document. These major issues, which I discuss in more detail below, should be addressed before publication. Once these issues are addressed, the paper will be a great addition to the scientific community.

General Comments
1) The authors claim that no DIC or NCP estimates exist in the Southern Ocean until now, however, Keppler et al. (2020, https://doi.org/10.1029/2020GB006571) and by Broullón et al. (2020, https://doi.org/10.5194/essd-12-1725-2020) have both created near-global (including the Southern Ocean) monthly climatologies of DIC. Keppler et al. (2020) even calculated the mean spring-to-fall NCP using their climatology, including in the Southern Ocean. Additionally, Lee (2001, https://10.4319/lo.2001.46.6.1287) also calculated the NCP at a global scale, using a DIC estimate. As those studies are global, and not focused on the Southern Ocean, this study likely has many novel aspects and is able to focus regionally, but their estimates should be compared to the existing ones.

2) Additionally, this study includes how the NCP has changed since 2010, which is a novel aspect compared to Keppler et al (2020) and Lee (2001) who only showed the mean NCP and not how it has changed. However, Keppler et al. (2023, https://doi.org/10.1029/2022GB007677) have recently created global mapped fields of DIC at monthly resolution from 2004 to 2020 (MOBO-DIC), which would allow estimating the NCP and its changes over that period. The NCP should be calculated with MOBO-DIC and compared to this study’s estimate.

3) BGC-Argo floats are known to have a difference compared to ship data (e.g., Gray et al., 2018, https://doi.org/10.1029/2018GL078013). I'm not convinced that the DIC estimate is robust as the neural network is trained with ship data (GLODAP) and applied to float data. This should at least be discussed, and there should be an analysis to demonstrate why we trust the DIC estimate (e.g., comparison with mapped DIC estimates).

4) The DIC reconstruction method is not clear from the main text, i.e., there is just one sentence in the methods section on it (L.237). I assumed for quite some time throughout the text, that the authors had done a gap-filling of the float data, but it later became apparent (by looking at Fig. 1) that they used T, S, DO, and Pr from floats as predictors for the NN to estimate the DIC at the location of the float only. This approach is similar to the CANYON & CANYON-B approaches by Bittig et al. (2018, https://doi.org/10.3389/fmars.2018.00328) and Sauzède et al. (2017, https://10.3389/fmars.2017.00128), which should be cited and the results compared. The authors also could have just used the CANYON-B approach, which is available open access, rather than re-inventing the wheel.

5) There are large gaps in time and space between floats. Fig. 1 clearly shows large gaps in space, but when looking at individual points in time, the gaps get even larger, especially towards the beginning of the BGC-Argo / SOCCOM program. Since the processes in the Southern Ocean are not uniform, and there are regions of hot spots beyond the fronts and sectors (see e.g., Tamsitt et al., 2017, https://doi.org/10.1038/s41467-017-00197-0, Sallée et al., 2012, https://doi.org/10.1038/ngeo1523, and Rintoul, 2018, https://doi.org/10.1038/s41586-018-0182-3), I am not convinced that the mapping method is robust. The authors do state the uncertainty of the estimates, but this should be discussed further. This becomes even more problematic when considering trends. As above, I strongly recommend adding an analysis with a gap-filled DIC product (Keppler et a., 2023) and comparing it with the estimate from this study.

6) The NCP trend analysis is only over 9 years. That is not a long enough period to make statements about the future, as this period could be strongly affected by natural variability. By convention, we need at least 30 years to be able to talk about climate change. This caveat is mentioned from L176 but should be mentioned right away and the discussion on this expanded.

7) Why was only the trend over the period 2008-2017 analyzed, when the NCP was calculated from 2004 -2019?

8) Data availability: there is no link to the data that was created during the study (e.g., DIC, NCP, RES).

Specific Comments
L.19: There is iron limitation, which comes up later but should be mentioned here already.
L.35: Superior compared to what?
L42: First it was claimed that satellite observations are the 'superior' approach to estimating NCP, but then it was stated that shipboard measurements are better. Rephrase this section.
L. 100: Global export estimates vary widely. Mention the range here and the citations.
L.111-114: Compare also to existing carbon sink estimates in the Southern Ocean (e.g., Landschützer et al. 2016, https://doi.org/10.1002/2015GB005359).

L.133: And the AMOC?

L.139-141: Name the amount of the NCP of that study.

L.178: BGC-Argo floats are very sparse outside of the Southern Ocean so far, this should be discussed.
L.189: Is there not a standardized method to correct for the DO sensor drift? Or data that's already been adjusted for drift? Either way, there should be a citation for this adjustment.
L.209: It reads like it was the same adjustment for all floats regardless of when they measured? It seems inaccurate to not use the exact year of the float (but I don't know much about DO adjustments).
L.211: Similarly, was a constant temperature and salinity used? They vary widely in the Southern Ocean which I would guess might result in inaccuracies.
L.217: It reads like the following: 1) There should be a DO adjustment. 2) This is how we did the adjustment. 3) We then found we don't need to do the adjustment. Is this correct, i.e., no adjustment was done in the end? If so, I would rephrase and shorten this section. If I misunderstood, then it should be rephrased for clarity.
L225: Many methods do use MLRs to estimate DIC (e.g., Gruber, Watson…). I agree that NN should better capture the statistical relationships, but I would not discredit the previous MLR studies like that, i.e., rephrase this sentence.
L.229: What is meant by parameter constraints?
L. 232: Specify what an F-value is.
L.239: Above, the authors said that MLRs are not suitable to reconstruct DIC. But then they proceeded to run both a MRL and an NN. This should be rephrased.
L.242: Refer to support for this statement (e.g., a figure or numbers).

Technical Corrections
I'm only mentioning the most important technical corrections here. Copy-editing of the whole document would be good additionally.
Throughout the document: It's a personal choice but I don't think phrases with only two words need to be abbreviated (e.g., Southern Ocean (SO), carbon sink (CS). Similarly, phrases that are only used a couple of times in the document (e.g., SCM) could be written out each time.
L.32: Change "including" to "and"
L.65 (and throughout the document): I'm not sure I would call the method a 'parameterization technique'. I would call it a 'DIC reconstruction method'.
L.93: rephrase 'oceans' to 'sectors' or 'basins' (check throughout the document).
L. 94: add "upwelling caused by" before "westerly winds".
L.96: Rephrase "located in the westerlies".
L.190: Add citation for WOA18.
L235: Spell out AOU (first use).
L.243: Specify that depth (2000 m?).

Reviewer #3 (Remarks to the Author):

• Key results:
This manuscript models Net Community Production (NCP) in the Southern Ocean using new methods, that of Neural Networks, and new data, that of Argos floats. The results are based on the

new model including one decade of calculations (2008 to 2017), based on 15 years of data (2004-2019). The question is of utmost importance that of the role of the Southern Ocean in absorbing atmospheric CO2 and in particular how much of the carbon that sinks out of surface waters remains at depth. The new method can estimate how much carbon is consumed in the surface layers, how much is replenished to the surface from depth and what is the difference between these 2 processes, or carbon sink.

• Validity and Suggested improvements
The importance of the question addressed and the use of Argos floats to test previous NCP estimates makes this manuscript worth considering for publication. However, the manuscript has two aspects that need further consideration.
(1) Although the model calculations consider the whole of the Southern Ocean, the model is based on only one region of the 3 defined in the manuscript. Southern Ocean is defined as waters south of 30oS. Based on Figure S3, the data from Argos floats from 2004 and 2019 are almost limited to the region between 45oS and 60oS, and the results extrapolated to the rest of the other 2 regions, north of 45oS and south of 60oS. The absence of Argos data south of 60oS is explained (due to seasonal sea ice and shallow bathymetry), but it is unclear why no data from ARGOS floats are available between 30oS and 45oS.
Thus, the authors assume that the processes north of 35oS and south of 60oS are similar to the intermediate band over the Antarctic Circumpolar Current, and those assumptions are not explicit, nor are they evaluated. The manuscript ignores previous work based on ocean remote sensing that shows the importance of shelf processes, in particular at the sea ice zone, to overall primary productivity and implicitly to NCP. There is no mention in the manuscript or in Supplemental Materials about the assumption that data collected elsewhere is representative of the sea-ice zone region, an area with high productivity compared to its extent (e.g., see Arrigo et al. 1998 and 2008 papers).
(2) This is a methods study, as described in lines 46-52. The critical word in the title is "mapping". The methodological aspect of the study is well described, and complete. However, the manuscript lacks a biogeochemical and oceanographic evaluation of why this new method sheds new light into the Southern Ocean system, i.e., why a researcher would use this approach in comparison to other studies providing similar results (as shown in Table 2).

• Originality and significance:
NCP is more powerful than PP to estimate Southern Ocean net uptake of carbon dioxide as it includes heterotrophic respiration. Without heterotrophic respiration carbon uptake can be overestimated. In addition, by analyzing yearound data, this manuscript can estimate not only carbon absorbed during the growth season but the replenishment of carbon back to the surface in the winter. Thus, the difference between replenishment and absorption at different times of the year provide an estimate of carbon sinking.

Having said that the main Conclusion from this paper agrees with what is already known, i.e., the Southern Ocean absorbs about 40% of the anthropogenic carbon dioxide, published in Biogeosciences in 2013 (reference cited). Thus, the authors do not highlight clearly what is new in their results, what results challenge known paradigms. There is a table comparing NCP results from this study with respect to others (Table 2). This absence is noticed in the text, i.e., concluding remarks, and particularly in the Abstract.

• Data & methodology:
The authors have developed a new method to estimate NCP using Neural Network, first by determining the correlation between the variables of interest and climatological data, then applying those regressions to the Argo floats data and finally calculating NCP using an equation included in Supplemental Material. The original data is publicly available, and cited in the manuscript, however this reviewer cannot find the publication of model output and/or scripts from this study.
This reviewer is not an expert on Neural Networks, the new method applied in this study, and cannot comment in depth about this aspect of the manuscript. However, the authors include in Supplemental Materials a comparison of methods (Neural Networks and a previously used Multiple Regression approach) that provides additional assurance of the validity of the new approach.

• Use of statistics and treatment of uncertainties:
Standard deviations are included in the figures in the main text, Figure 2 (e.g, Fig. 2c and 2d). Most of the considerations on errors and/or limitations are mentioned in the Supplemental Material, (e.g., section S4). Supplemental Material does include basic methodology and basic error considerations that are essential to understand the manuscript and cited in the main text.

• Conclusions:
As mentioned previously, the Conclusions are not somewhat incomplete, there is no final evaluation of the benefit of the new model in our understanding of the Southern Ocean system or the novelty this method brings compared to others. Having a new estimate is always reassuring, however, why is this new number significant is not highlighted in the Conclusions. After reading this report my conclusion is that remote sensing gives similar results than remote sensing data (Li et al. 2021, referenced in the manuscript).

• References:
The references that relate to estimates of Southern Ocean NCP and carbon dioxide absorption are well referenced, however the authors could add some refs that will help evaluate the new method within a broader understanding of SO system, for example Arrigo's refs. By not including references with similar estimates, the manuscript is written more for the scientific community working on NCP, not so much for the broader scientific community interested in the role of the Southern Ocean in absorbing atmospheric carbon dioxide but working with other methods and sensors.

• Clarity and context:
The Abstract is poor, it is not written for a general audience nor is it attractive, there is no mention of the novelty of the results and a strong sentence making clear the main contribution from this study is missing.
The Introduction is thorough and well cited, clearly this is a methodological study.
The Conclusions need completion: as mentioned above it is not clear why this method is better than others given the results are not so different (Table 2), in particular using remote sensing data.

LIST ALL THE OTHER MAIN POINTS HERE
1. This reviewer finds the definition of NCP and RES are not coherent throughout the manuscript

(compare lines 253 to 255 with Figure S4, seems to have the correct definition).
2. "Ideal state", cited in Abstract and others, is a very subjective statement, please define.
3. Introduction: one paragraph explains clearly why DO is a problem (lines 53 to 64), the next Paragraph (lines 65 to 76) uses DO in the new method.
4. Please define acronyms in titles, figures, etc to make them self-sustaining.
5. The definition of "data" is unclear. After Table S1 and Figure S2, I think replacing data by dataset might be more clear. The authors included 154 datasets, 147 datasets, of different length, were included (Figure S2), with 27,030 vertical profiles (Figure S2) resulting in 484 NCP estimates (Figure S3).
6. Why was not mixed layer depth or stratification analyzed to explain the results? The calculation of mixed layer cis done as part of the calculations of NCP and RES. It would help to see what the data says, explaining results based on a publication that has studied only a limited region of the Southern Ocean is not expected to be applicable everywhere.

REFERENCES

Arrigo, K.R., van Dijken, G.L. and Bushinsky, S., 2008. Primary production in the Southern Ocean, 1997–2006. Journal of Geophysical Research: Oceans, 113(C8).
Arrigo, K.R., Worthen, D., Schnell, A. and Lizotte, M.P., 1998. Primary production in Southern Ocean waters. Journal of Geophysical Research: Oceans, 103(C8), pp.15587-15600.

Reviewer #1 (Remarks to the Author):

Thank you for your valuable comments. And we have tried our best to respond your comments point-by-point. We would appreciate it if you could review our responses in this file and the revised manuscript file. Our replies and modifications are shown in blue.

General comment:

The manuscript "Spatiotemporal high-resolution mapping of biological production in the Southern Ocean" provides a new estimate of Southern Ocean (SO) net community production (NCP) based on a novel dissolved inorganic DIC parameterization which is applied to autonomous BGC-Argo float data (2007-2019). The DIC parametrisation is based on its relationship to temperature (T), salinity, dissolved oxygen (Do) and pressure (PR) from a large-scale ocean data depository (GLODAP) using multiple-linear regression (MLR) as well as Neural Network (NN) approaches. Results are further explored to investigate DIC drawdown (NCP), restoration (RES) and to calculate carbon removal (CS) in different areas of the SO and over time. This is important and novel information to further the understanding of the role of the Southern Ocean as a carbon sink under current and future climate conditions. The presented research has high relevance across various disciplines in Earth system science and will be an important contribution to Southern Ocean marine science.

Unfortunately, in its current form, the manuscript is tedious to read, and critical information is buried in - often wordy - text. This reviewer therefore suggests that the authors significantly revise the manuscript in order to make it more concise and to the point.

In addition - and this reviewer view may be partly affected by the current somewhat unclear manuscript language and strcuture - some key aspects/methods need to be further explored and better explained. The following questions and issues need to be addressed and clarified.

#Response

To increase the readability of the manuscript, we have added flowcharts about this study as well as revised some of the content in the original manuscript (sentences in blue).

#Comment 1.1

The NN parameterisation is based on the GLODAP data products. Where only CTD bottle data used, or was a merged (=gap-filled) GLODAP data product used? If the latter was used, how would that affect error estimates? Please clarify.

#Response 1.1

We used GLODAP bottle data for constructing parameterization. The original manuscript did not contain this information, so we added it (Line 261). Error considered in this study are shown in Supplementary Information **“S4: Uncertainties of NCP, Restoration and CS”.**

#Comment 1.2

Most of NCP is occurring in the upper mixed layer (<200 m). This reviewer understands that full-ocean depth GLODAP data were used to train (and evaluate) the NN parameterizations. Would be using only shallow data (<200 m) allow for a better parameterization? Could Fig S1 include/be expanded to also show “zoomed in” plots for the upper 0-200 m depths? The authors need to strengthen their arguments that they followed a plausible approach in the NN-parameterizations, their application to the BGC-Argo data and subsequent spatial extrapolation methods. There currently no clear thread through the manuscript.

#Response 1.2

[Parameterization in the upper mixed layer]

We used data from 0m to bottom for the construction of parameterization. Figure S1 (a)-(c) have been redrawn to better show the relationship between predicted and observed values in the 0-200m. The black dots represent the data from 0-200 m, and the gray dots represent the data at depths greater than 200 m. Please check them.

In the revised manuscript, we strengthened our approach as follow:

[NN-parameterization]: We emphasized the innovation of our MLR-NN hybrid parameterization method. See section “DIC Parameterization methods” for detail (Line 245-271).

[BGC-Argo application]: We added a comparison between the predicted DIC derived from the BGC-Argo and the GLODAP observations (Fig. S8).

[Spatial extrapolation method]: We added some introduction about the error derived from our spatial extrapolation method (Supplementary information Line 106).

#Comment **1.3**

This reviewer remains sceptical about the ODV-based extrapolation of results towards the Antarctic coast and into areas with very few BGC-Argo profiles. Could the approach and its validity be better explained?

#Response 1.3

We used all currently available BGC-Argo data for the estimation. But there are still some regions that are not covered by the data (Fig. S3). For this, we used the extrapolation method to expand the data for these uncovered regions. In order to evaluate the appropriateness of the extrapolation method, we estimated the error deriving from the extrapolation process (Supplementary Information **"S4: Uncertainties of NCP, Restoration and CS"), which** in the regions with no BGC-Argo passing through as 7%, resulting in 3% of an area-weighted gridding error over the SO. Few data along the Antarctic coast (Fig. S3) have the potential to introduce greater error, and we have added a statement about this part in the original manuscript (Supplementary Information Line 105-106).

#Comment 1.4

Ice extent was correlated/compared to NCP in the 45deg to 60deg band. Antarctic sea ice extent (e.g., in the Indian and Pacific sectors) is generally not reaching much north of 60 deg, and thus this band might not be an appropriate area to expect influences from sea ice "dynamics". This reviewer suggests to remove this section on sea ice or provide an in-depth justification for using the NCP data from 60-45 degree for the comparison with sea ice.

#Response 1.4

We agree to what you said. However, previous study suggested that iron released from the melting sea ice may have broader impacts on the biological production in the Southern Ocean [Alderkamp et al., 2012], and temporal variability of ice extent may affect the NCP in the Southern Ocean widely. Therefore, we used NCP data from 60°S to 45°S for the analysis of temporal variation and estimated the correlation between its temporal variation and Antarctic ice extent, although the ice extent did not reach this latitude range. We found a significant positive correlation between the time-series of NCP and the time series of ice extent in the Southern Ocean, suggesting the sea ice melting with iron have reached 60°S to 45°S. Thus, we added the related statement to the revised manuscript (Line 175-186).

#Comment 1.5

The aforementioned text/figures on sea ice extent could be deleted and a BGC-Argo nitrogen-based NCP estimate (from floats with N-sensors) could be established and used for a direct (float-based) comparison of nitrate-drawdown NCP with the newly presented DIC-NN-NCP estimate.

#Response 1.5

The number of Argo float equipped with N sensors is currently very limited and not sufficient to cover our target region. In addition, if using nitrate for NCP estimation, stoichiometry ratio needs to be used to change nitrate to carbon, which is unexpected at the beginning (Line 61-64). Therefore, we did not use the N changes measured by the sensors to compare with our results.

Some specific comments:

Manuscript:

L 1: “net community production” is a widely used term and could be used in the title?

In this study, we estimated “NCP” as well as “Restoration” and “carbon sink”, not only “NCP”. So we think “biological production” is more appropriate for the title.

L 4: one example where language could be more concise: replace ….“effects are changing”… with …“impacts”…

We followed your comment and rewrote it (Line 8).

L 8: delete “ocean biological production”

We deleted it according to your comment.

L14-19: suggest to reword as currently it suggests that most of the SO (and defined as the ocean south of 30deg) is an HNLC area. Large parts are, but not most parts.

We followed your comment and rewrote it (Line 23).

L 25” severe weather”? or rather “atmospheric”… conditions?

We followed your comment and rewrote it (Line 32).

Line 46 -64: Suggest to combine this into a single short paragraph. Maybe also mention ice cover effects on outgassing.

We combined these two paragraphs according to your comment. In addition, we added the content about the ice cover effects on outgassing in line 65-67.

Line 65 – 76: Maybe bring this paragraph up to the front to clearly articulate the aims/objectives of this research. Suggestion to add a sentence on the Sea Ice Carbon Pump (e.g. Rysgaard et al. 2007, JGR).

This paper follows the order of presenting the problems in previous studies first, then the methodology and innovations of this work. We do not think it is necessary to change this order. However, considering your comment, we modified the “Abstract” to clarify more the aims of our study. In the other hand, as for Sea Ice Carbon Pump, we added a sentence about it in line 21-23.

L 94: “coasts” are generally associated with land not with oceans, suggest to rephrase

We followed your comment and rewrote it (Line 108).

L 104: It remains unclear how an about 20% increased heterogeneity was established/calculated.

We are sorry about that this content in the original manuscript was wrong. What we want to claim is that the tNCP estimated in our study is generally ~20% larger than the average value of the previous studies in Table 2. Therefore, we rewrote these sentences along with the footnote of Table 2.

L130: “largest and widest NCP area” ? please clarify or reword.

We followed your comment and rewrote it (Line 143-144).

L134-138: As per general comments, this reviewer remains sceptical about the spatial extrapolation method (ODV-based) applied to the data, in particular in data-sparse areas with little BGC-Argo float coverage. This is particular true for Antarctic shelf areas with extensive ice cover, polynyas as well as ice-edge blooms.

Please check “Response 1.3” for the answer.

L148: “to predict the future”, no predictive results are presented. Suggest to reword.

We deleted it according to your comment.

Line 228: Please expand the explanation(s) why NN-constructed parameterizations can be applied to surface mixed layer BGC-Argo data, while MLR-constructed data can not. Is this just because NN is a non-linear approach? FigsS1 alone is not sufficient to justify this statement which is key to the overall findings of the manuscript. This needs better explanation and justification.

We think there was a misunderstanding due to inadequate explanation. We are using both MLR and NN methods (MLR-NN hybrid method) to parameterize the DIC for the entire Southern Ocean. We have rewritten the text and added a reference figure to make this parameterization method clearer (Fig. S1, Line 245-271).

L409: Suggest to explicitly define production and restoration period in this Figure caption.

We followed your comment and rewrote it (Line 471).

L426 - Fig 3: As per above general comment, the time-series analysis of NCP and RES with sea-ice extent using NCP and RES data from 45-60 degree band appears problematic.

Please check “Response 1.4” for the answer.

L 436: Multiplied “by”

We followed your comment and rewrote it (Table 1).

Table 2: Fix typo “parameerization”?

We followed your comment and rewrote it.

Supplementary Material:

L6: “NN-based DIC parameterization” rather than “DIC NN-based parameterization”?

We followed your comment and rewrote it (Line 6).

Line 62: As per general comment/question: Can the full depth data parameterization directly applied to surface data? Is this introducing specific errors?

Please check “Response 1.2” for the answer.

Tab S1: Table caption: Suggest to replace “plunged” with “deployed”

We followed your comment and rewrote it.

Reviewer #2 (Remarks to the Author):

Thank you for your valuable comments. And we have tried our best to respond your comments point-by-point. We would appreciate it if you could review our responses in this file and the revised manuscript file. Our replies and modifications are shown in blue.

General comment:

The paper reconstructs upper ocean DIC in the Southern Ocean between 2004 and 2019, using BGC Argo float observations and a Neural Network approach. From the DIC estimate, they calculate the net community production (NCP) and restoration rate (RES) in this region. They then map the NCP and RES onto a regular grid to estimate the total NCP and total RES, and then estimate the temporal trend of the NCP. While the paper claims that no DIC or NCP estimates exist to date in the Southern Ocean, this is not true, as we have several global-scale estimates of both. This study is more regional and might be able to better resolve the DIC, NCP, and RES, but at the least, it should be compared to the previous estimates. Also with other aspects, the authors missed to cite important previous work. Saying that this study has a novel aspect: the authors show how the Southern Ocean NCP has changed over time, which to my knowledge had not been estimated yet (although we do have DIC estimates from which we could estimate it). The authors then extrapolate this information further to conclude how the NCP will change with climate change, which is not possible based on such a short period (2008-2017). In addition, the DIC reconstruction method is not clear from the main text. There is more technical information in the SI, but even there it is not fully detailed what the authors did. Further, the document would benefit from copy editing. It’s mostly understandable but in many cases does not flow well or uses unusual grammar. The paragraphs don’t always flow logically and often deal with multiple ideas/topics, especially in the introduction, but also throughout the document. These major issues, which I discuss in more detail below, should be addressed before publication. Once these issues are addressed, the paper will be a great addition to the scientific community.

#Comment 2.1

The authors claim that no DIC or NCP estimates exist in the Southern Ocean until now, however, Keppler et al. (2020, https://doi.org/10.1029/2020GB006571) and by Broullón et al. (2020, https://doi.org/10.5194/essd-12-1725-2020) have both created near-global (including the Southern Ocean) monthly climatologies of DIC. Keppler et al. (2020) even calculated the mean spring-to-fall NCP using their climatology, including in the Southern Ocean. Additionally, Lee (2001, https://10.4319/lo.2001.46.6.1287) also calculated the NCP at a global scale, using a DIC estimate. As those studies are global, and not focused on the Southern Ocean, this study likely has many novel aspects and is able to focus regionally, but their estimates should be compared to the existing ones.

#Response 2.1

Our explanation in the original manuscript was insufficient, misleading you to understand the point we were trying to claim. In our manuscript, we stated the ideal conditions for NCP estimation and pointed out that existing studies are insufficient to meet these conditions. We have also added the previous studies you mentioned to our citation list, as we did not cite them in our original manuscript. The shortcomings of these previous studies and the innovations of our study are described below, and we have added them to the revised manuscript.

Keppler et al. (2020) estimated the DIC of the global ocean with a SOM-FFN neural network and used this predicted data to estimate the NCP; SOM is an unsupervised neural network model and its prediction process is a black box, so determining its validity is difficult (Telszewski et al. 2009) In addition, when using FFN, Keppler et al. (2020) used a number of basic hydrographic data along with nutrient data as input parameters. They suggest that there is covariance among many of these input parameters, which may lead to overfitting of the model. Furthermore, the SOM-FFN method does not allow the model to be expressed in equations, so its estimates cannot be easily reproduced or followed up.

The NCP estimation of Lee (2001) was accomplished by predicting surface DIC using surface seawater temperature and nitrate as parameters. Due to the sparse observational data used, a low-resolution grid was used. In addition, the Lee (2001) estimates are based on a mixed layer model, which makes several assumptions that differ significantly from real ocean processes, such as advection and diffusion being constant, introducing significant uncertainty in the accuracy of these estimates.

The innovations of this paper are (1) to construct an MLR-NN hybrid parameterization of the DIC to avoid the NN black box, thus making the equations explicit and increasing their accuracy; the MLR is a linear model and was used to select input parameters; as an NN model, we used one A simple back-propagating NN model with a hidden layer and three neurons was used. Furthermore, (2) without using idealized assumptions such as stoichiometric ratios, the above MLR-NN hybrid parameterization is an innovative method to estimate the distribution and temporal variability of NCP, RES, and CS in the Southern Ocean by applying the above MLR-NN hybrid parameterization to Argo observations. The method is highly accurate and applicable to the widely deployed BGC-Argo. The manuscript has been revised to more clearly show these innovations (Line 194-199, Fig. S1).

#Comment 2.2

Additionally, this study includes how the NCP has changed since 2010, which is a novel aspect compared to Keppler et al (2020) and Lee (2001) who only showed the mean NCP and not how it has changed.

However, Keppler et al. (2023, https://doi.org/10.1029/2022GB007677) have recently created global mapped fields of DIC at monthly resolution from 2004 to 2020 (MOBO-DIC), which would allow estimating the NCP and its changes over that period. The NCP should be calculated with MOBO-DIC and compared to this study's estimate.

#Response 2.2

In Keppler et al. (2023), which based on neural network model and Argo floats (2015-2020), the trend of NCP south of 35˚S of about -0.1% year^{-1} was estimated by assumption that the average DIC was 2100 mmol year^{-1} in the mixed layer and the DIC was totally consumed by NCP. However, this neural network model is black box because they are difficult to show as equations, and it is hard to reproduce their estimate. Moreover, the selection of the input parameters in this previous study is subjective and there are covariances between parameters, which brings uncertainties to these estimates. As we have described in Response 2.1, SOM-FFN neural network is a black box and there is covariance between the input parameters. This make it is difficult to compare our result with SOM-FFN based study. The innovation of this paper is to construct an MLR-NN hybrid parameterization for DIC and to estimate the distribution and temporal variability of NCP, RES and CS over the Southern Ocean based on Argo observations, and without using idealized stoichiometry ratio. The manuscript has been revised to show these innovations more clearly (Line 194-199, the footnote of Fig. 3, Fig. S1).

#Comment 2.3

BGC-Argo floats are known to have a difference compared to ship data (e.g., Gray et al., 2018, https://doi.org/10.1029/2018GL078013). I'm not convinced that the DIC estimate is robust as the neural network is trained with ship data (GLODAP) and applied to float data. This should at least be discussed, and there should be an analysis to demonstrate why we trust the DIC estimate (e.g., comparison with mapped DIC estimates).

#Response 2.3

In Gray et al., 2018, they used seawater temperature, salinity, DO, nitrate, and pH data from Argo to estimate air-sea CO_2 flux. They corrected DO data by the atmospheric O_2, and corrected nitrate and pH by the bottle measurements.

In our study we used seawater temperature, salinity, DO and pressure for predicting DIC. Currently BGC Argo's seawater temperature, salinity, and pressure sensors are considered to have sufficient accuracy, at least to meet the error accuracy required by our parameterization. As for dissolved oxygen, we considered that Argo's dissolved oxygen sensor might be biased with time, we compared and corrected these data with WOA's dissolved oxygen climatology data before using them.

In addition, to demonstrate the credibility of our prediction of DIC, we add a figure for comparing the BGC Argo-derived DIC_{pre} and GLODAP DIC observations around Antarctica in the Supplementary Information (Fig. S8). Around Antarctica, the predicted DIC derived from BGC-Argo data were in great agree with the GLODAP DIC observations within the RMSE of our parameterization.

#Comment 2.4

The DIC reconstruction method is not clear from the main text, i.e., there is just one sentence in the methods section on it (L.237). I assumed for quite some time throughout the text, that the authors had done a gap-filling of the float data, but it later became apparent (by looking at Fig. 1) that they used T, S, DO, and Pr from floats as predictors for the NN to estimate the DIC at the location of the float only. This approach is similar to the CANYON & CANYON-B approaches by Bittig et al. (2018, https://doi.org/10.3389/fmars.2018.00328) and Sauzède et al. (2017, https://10.3389/fmars.2017.00128), which should be cited and the results compared. The authors also could have just used the CANYON-B approach, which is available open access, rather than re-inventing the wheel.

#Response 2.4

Our study predicts DIC using discrete data from Argo and then expands the discrete data to the entire Southern Ocean by weighted averaging interpolation.

Our approach differs from that of the CANYON & CANYON-B approaches in the following points:

1. The previous studies selected the basic hydrographic parameters as well as the location parameters as inputs in the construction of the neural network model. We think that there is a covariance between hydrographic parameters and location parameters, and location parameters are not suitable for analyzing temporal variations, so we only use hydrographic parameters for the construction of neural network model.
2. The neural network model cannot directly represent the relationship between each input parameter and the output value, which is why we cannot select the input parameters scientifically. In contrast, MLR model can be used for the selection of input parameters. That is why we used a hybrid of MLR model and neural network model for the parameterization construction in this study. We have added a text to explain the content of this article because it is insufficient (Line 245-271).

#Comment 2.5

There are large gaps in time and space between floats. Fig. 1 clearly shows large gaps in space, but when looking at individual points in time, the gaps get even larger, especially towards the beginning of the BGC-Argo / SOCCOM program. Since the processes in the Southern Ocean are not uniform, and there are regions of hot spots beyond the fronts and sectors (see e.g., Tamsitt et al., 2017, https://doi.org/10.1038/s41467-017-00197-0, Sallée et al., 2012, https://doi.org/10.1038/ngeo1523, and Rintoul, 2018, https://doi.org/10.1038/s41586-018-0182-3), I am not convinced that the mapping method is robust. The authors do state the uncertainty of the estimates, but this should be discussed further. This becomes even more problematic when considering trends. As above, I strongly recommend adding an analysis with a gap-filled DIC product (Keppler et a., 2023) and comparing it with the estimate from this study.

#Response 2.5

Fig. 1 shows the distribution of all Argo data used to estimate the climatological NCP. These discrete Argo data do have some spatial bias, but we must used these data to estimate NCP in the entire Southern Ocean at this stage by using a weighted average interpolation. In order to evaluate the appropriateness of the interpolation method, we estimated the error deriving from the extrapolation process (Supplementary Information "S4: Uncertainties of NCP, Restoration and CS"), which in the regions with no BGC-Argo passing through as 7%, resulting in 3% of an area-weighted gridding error over the SO. Few data along the Antarctic coast (Fig. S3) have the potential to introduce greater error, and we have added a statement about this part in the original manuscript (Supplementary Information Line 105-106).

Furthermore, in the trend estimates, the Argo data within a single year become more dispersed, especially on the southern and northern side of the Southern Ocean. To make the trend estimates more convincing, we only used data between 45°S and 60°S, where more data are available, for the analysis.
According to your recommendation, we compared the NCP estimation in Keppler et al. (2023) and our study. The method of Keppler et al. (2023) has the problems described in Responses 2.1 and 2.2. However, we added a comparison between Keppler et al. (2023) and our study as a footnote of Fig.3.

#Comment 2.6

The NCP trend analysis is only over 9 years. That is not a long enough period to make statements about the future, as this period could be strongly affected by natural variability. By convention, we need at least 30 years to be able to talk about climate change. This caveat is mentioned from L176 but should be mentioned right away and the discussion on this expanded.

#Response 2.6

As you pointed out, we recognize that a mere 9 years may not be sufficient to reflect the long-term trend of NCP. However, due to the insufficient amount of data, we can only use 9 years of data for an estimate of potential trends at this time. The important point is that trend estimation is indeed possible using our method, and as the amount of data improves in the future, more accurate trends can be obtained by our method. To make this caveat clearer, we have revised Line 190-193 of the manuscript.

#Comment 2.7

Why was only the trend over the period 2008-2017 analyzed, when the NCP was calculated from 2004 - 2019?

#Response 2.7

Using the Argo data before 2008 are difficult to estimate the NCP due to the sparseness of data to cover the entire Southern Ocean. We used the data after 2008 which covered the entire Southern Ocean, and estimated the trend of NCP in the Southern Ocean (Line 162-164, Fig. S4).

#Comment 2.8

Data availability: there is no link to the data that was created during the study (e.g., DIC, NCP, RES).

#Response 2.8

We have added a description of the data used in this study.

Specific Comments

L.19: There is iron limitation, which comes up later but should be mentioned here already.

We followed your comment and added the content about iron limitation (Line 25).

L.35: Superior compared to what?

We rewrote this sentence to avoid misunderstandings (Line 41-42).

L42: First it was claimed that satellite observations are the 'superior' approach to estimating NCP, but then it was stated that shipboard measurements are better. Rephrase this section.

We deleted this sentence to avoid misunderstandings.

L. 100: Global export estimates vary widely. Mention the range here and the citations.

We have compared our result with the credible estimation of global ocean organic carbon export by DeVries & Weber, 2017, which is based on both remote sensing and oceanographic tracer observations. We rewrote this sentence (Line 114).

L.111-114: Compare also to existing carbon sink estimates in the Southern Ocean (e.g., Landschützer et al. 2016, https://doi.org/10.1002/2015GB005359).

We followed your comment and added the comparison of carbon sink between our study and Landschützer et al. 2016 (Line 124-126).

L.133: And the AMOC?

We followed your comment and added the content about AMOC (Line 146).

L.139-141: Name the amount of the NCP of that study.

The amount of the NCP of the previous study is shown in Fig. S6.

L.178: BGC-Argo floats are very sparse outside of the Southern Ocean so far, this should be discussed.

We followed your comment and discussed it (Line 196-197).

L.189: Is there not a standardized method to correct for the DO sensor drift? Or data that's already been adjusted for drift? Either way, there should be a citation for this adjustment.

The DO correction is important for BGC Argo-based studies but is often ignored. We presented our correction method in the revised manuscript (Line 208-222).

L.209: It reads like it was the same adjustment for all floats regardless of when they measured? It seems inaccurate to not use the exact year of the float (but I don't know much about DO adjustments).

Argo's DO correction uses data from the deeper ocean below it along with the mixed layer. Because the data below the mixed layer to 2000m have a residence time of several decades or more, there is no need to take into account the timing of Argo measurements for Argo's DO correction. However, the explanation was insufficient, so that we rewrote the text (Line 211-212).

L.211: Similarly, was a constant temperature and salinity used? They vary widely in the Southern Ocean which I would guess might result in inaccuracies.

L.217: It reads like the following: 1) There should be a DO adjustment. 2) This is how we did the adjustment. 3) We then found we don't need to do the adjustment. Is this correct, i.e., no adjustment was done in the end? If so, I would rephrase and shorten this section. If I misunderstood, then it should be rephrased for clarity.

Response for L.211&L.217: This paragraph is intended to illustrate the effect of the reduction in atmospheric oxygen concentration due to anthropogenic CO_2 release on our DO correction. The results demonstrate that the effect of anthropogenic CO_2 is negligible. However, the explanation was insufficient, so that we rewrote the text (Line 223-240).

L225: Many methods do use MLRs to estimate DIC (e.g., Gruber, Watson…). I agree that NN should better capture the statistical relationships, but I would not discredit the previous MLR studies like that, i.e., rephrase this sentence.

We agree that MLR is a useful method in specific situation. We used the MLR-NN hybrid method in this study. However, the explanation about in this point was insufficient, so that we rewrote the text (Line 245-271). Please check it.

L.229: What is meant by parameter constraints?

When we used MLR to construct DIC parameterization in the SO, several constraints need to be imposed, since the errors in some waters such as surface water were large (Pan et al., 2020&2022). In this study, we used the MLR-NN hybrid method. The constrains considered in the MLR parameterization of DIC in this study were shown in Table S2. Contrastingly, when we used NN to construct DIC parameterization, it can be applied to the whole SO including the surface mixed layer without any constrain (Line 252).

L. 232: Specify what an F-value is.

We followed your comment and added an introduction about F-value (Line 256).

L.239: Above, the authors said that MLRs are not suitable to reconstruct DIC. But then they proceeded to run both a MRL and an NN. This should be rephrased.

We used the MLR-NN hybrid method in this study. However, the explanation about in this point was insufficient, so that we rewrote the text (Line 245-271). Please check it.

L.242: Refer to support for this statement (e.g., a figure or numbers).

The DIC parameterization we constructed using the NN method has almost the same accuracy as the MLR-based parameterization, and the NN parameterization includes data in the mixing layer. Therefore, we said that “NN model can effectively reconstruct the DIC over the SO, including the surface mixed layer” (Fig. S2, Table S2).

Technical Corrections

I’m only mentioning the most important technical corrections here. Copy-editing of the whole document would be good additionally.

Throughout the document: It’s a personal choice but I don’t think phrases with only two words need to be abbreviated (e.g., Southern Ocean (SO), carbon sink (CS). Similarly, phrases that are only used a couple of times in the document (e.g., SCM) could be written out each time.

SO, CS are general abbreviations used in many papers. And SCM was used five times in our manuscript. So, we think it is necessary to use these abbreviations.

L.32: Change “including” to “and”

We followed your comment and rewrote it (Line 38).

L.65 (and throughout the document): I'm not sure I would call the method a 'parameterization technique'. I would call it a 'DIC reconstruction method'.

According to your general comments, we refer our method as "MLR-NN hybrid parameterization" in the revised manuscript. And the term "parameterization technique" only appear at line 72 to refer to a general parameterization method.

L.93: rephrase 'oceans' to 'sectors' or 'basins'.

We followed your comment and rewrote it (Line 107).

L. 94: add "upwelling caused by" before "westerly winds".

We followed your comment and rewrote it (Line 108).

L.96: Rephrase "located in the westerlies".

We followed your comment and rewrote it (Line 110).

L.190: Add citation for WOA18.

We followed your comment and added the citation of WOA18 (Line 209).

L235: Spell out AOU (first use).

We followed your comment and rewrote it (Line 259).

L.243: Specify that depth (2000 m?).

We followed your comment and added the specific depth (Line 267).

Reviewer #3 (Remarks to the Author):

Thank you for your valuable comments. And we have tried our best to respond your comments point-by-point. We would appreciate it if you could review our responses in this file and the revised manuscript file. Our replies and modifications are shown in blue.

General comment:

This manuscript models Net Community Production (NCP) in the Southern Ocean using new methods, that of Neural Networks, and new data, that of Argos floats. The results are based on the new model including one decade of calculations (2008 to 2017), based on 15 years of data (2004-2019). The question is of utmost importance that of the role of the Southern Ocean in absorbing atmospheric CO2 and in particular how much of the carbon that sinks out of surface waters remains at depth. The new method can estimate how much carbon is consumed in the surface layers, how much is replenished to the surface from depth and what is the difference between these 2 processes, or carbon sink.

• Validity and Suggested improvements

The importance of the question addressed and the use of Argos floats to test previous NCP estimates makes this manuscript worth considering for publication. However, the manuscript has two aspects that need further consideration.

#Comment 3.1

Although the model calculations consider the whole of the Southern Ocean, the model is based on only one region of the 3 defined in the manuscript. Southern Ocean is defined as waters south of 30oS. Based on Figure S3, the data from Argos floats from 2004 and 2019 are almost limited to the region between 45oS and 60oS, and the results extrapolated to the rest of the other 2 regions, north of 45oS and south of 60oS. The absence of Argos data south of 60oS is explained (due to seasonal sea ice and shallow bathymetry), but it is unclear why no data from ARGOS floats are available between 30oS and 45oS. Thus, the authors assume that the processes north of 35oS and south of 60oS are similar to the intermediate band over the Antarctic Circumpolar Current, and those assumptions are not explicit, nor are they evaluated. The manuscript ignores previous work based on ocean remote sensing that shows the importance of shelf processes, in particular at the sea ice zone, to overall primary productivity and implicitly to NCP. There is no mention in the manuscript or in Supplemental Materials about the assumption that data collected elsewhere is representative of the sea-ice zone region, an area with high productivity compared to its extent (e.g., see Arrigo et al. 1998 and 2008 papers).

#Response 3.1

In estimating the distribution of climatology of the NCP, we define three regions through the front, where enough Argo data exist in all three regions (Fig. S2).

In estimating the temporal variation of the NCP, we separate all the Argo data by year, which is what you mentioned in Fig. S3. Since the annual Argo data are mainly concentrated between 45°S and 60°S, the main latitude of the circumpolar current. Therefore, we only consider the data between 45°S and 60°S when analyzing the temporal variation (cf. Fig. S1).

We do not assume that the northern side of the Southern Ocean has the same processes as the central part. Relatively few available Argo data exist on the northern side of the Southern Ocean, and we evaluated the uncertainty associated with this data sparsity (Supplementary Information **"S4: Uncertainties of NCP, Restoration and CS"),** which in the regions with no BGC-Argo passing through as 7%, resulting in 3% of an area-weighted gridding error over the SO..

We compared our distribution of NCP with the primary production map in Arrigo et al. 2008 and find a same pattern. The previous studies did find primary production high in some Antarctic coastal waters, but these coastal estimates are relatively narrow in scope and do not have a significant impact on our overall Southern Ocean estimates.

#Comment 3.2

This is a methods study, as described in lines 46-52. The critical word in the title is "mapping". The methodological aspect of the study is well described, and complete. However, the manuscript lacks a biogeochemical and oceanographic evaluation of why this new method sheds new light into the Southern Ocean system, i.e., why a researcher would use this approach in comparison to other studies providing similar results (as shown in Table 2).

#Response 3.2

Firstly, thank you for your comment. As mentioned in our manuscript the main existing NCP estimation methods are based on the in-situ observation and the remote sensing satellite. The in-situ observation method (e.g. Quay et al., 2009) is mainly limited by the fact that only local regions can be estimated. The remote sensing satellite method (e.g. Arteaga et al. 2018) is limited by the fact that only surface observations can be made. Moreover, the existing methods usually require the use of idealized stoichiometry ratios. Our method overcomes the disadvantages of the above methods and maintains their advantages as much as possible. According to your comment, we showed a comparison of our results with other studies including the NCP and its trend in Table 2 and Fig. 3. However, as mentioned previously, these previous study have problems such as the neural network is a black box and there is covariance between the input parameters (Line 79-89, 194-199, 245-271, Table 2, Fig. 3).

• Originality and significance:

#Comment 3.3

NCP is more powerful than PP to estimate Southern Ocean net uptake of carbon dioxide as it includes heterotrophic respiration. Without heterotrophic respiration carbon uptake can be overestimated. In addition, by analyzing yearound data, this manuscript can estimate not only carbon absorbed during the growth season but the replenishment of carbon back to the surface in the winter. Thus, the difference between replenishment and absorption at different times of the year provide an estimate of carbon sinking.

Having said that the main Conclusion from this paper agrees with what is already known, i.e., the Southern Ocean absorbs about 40% of the anthropogenic carbon dioxide, published in Biogeosciences in 2013 (reference cited). Thus, the authors do not highlight clearly what is new in their results, what results challenge known paradigms. There is a table comparing NCP results from this study with respect to others (Table 2). This absence is noticed in the text, i.e., concluding remarks, and particularly in the Abstract.

#Response 3.3

Our main conclusion is not that the Southern Ocean absorbs 40% of the anthropogenic CO_2. It is a spatiotemporal estimate of the interannual NCP and the restoration of the Southern Ocean. The innovation of this paper is to estimate the distribution and temporal variability of NCP, RES and CS over the Southern Ocean, where controls the ocean carbon cycle and the Earth climate system. Our results have overcome the problems in the previous studies. We have revised the manuscript (Line 79-89, 194-199, 245-271) to show these innovations more clearly.

In addition, according to your comment, we showed a comparison of our results with other studies including the NCP and its trend in Table 2 and Fig. 3. However, as mentioned previously, these previous study have problems such as the neural network is a black box and there is covariance between the input parameters (Line 79-89, 194-199, 245-271, Table 2, Fig. 3).

• Data & methodology:

#Comment 3.4

The authors have developed a new method to estimate NCP using Neural Network, first by determining the correlation between the variables of interest and climatological data, then applying those regressions to the Argo floats data and finally calculating NCP using an equation included in Supplemental Material.

The original data is publicly available, and cited in the manuscript, however this reviewer cannot find the publication of model output and/or scripts from this study.

This reviewer is not an expert on Neural Networks, the new method applied in this study, and cannot comment in depth about this aspect of the manuscript. However, the authors include in Supplemental Materials a comparison of methods (Neural Networks and a previously used Multiple Regression approach) that provides additional assurance of the validity of the new approach.

#Response 3.4

We used a simple back propagation NN model rather than any complex NN model in this study in order to provide the equation of our parameterization. Our NN model includes one hidden layer with three neurons. Since it is difficult for us to judge the validity of each input parameter, we select the input parameters in advance by a MLR model. The indicators considered are the F-values to evaluate the significance of the parameters and the VIF to evaluate the co-correlation between the parameters. We add a table (Table S3) to present these evaluation results.

• Use of statistics and treatment of uncertainties:

#Comment 3.5

Standard deviations are included in the figures in the main text, Figure 2 (e.g, Fig. 2c and 2d). Most of the considerations on errors and/or limitations are mentioned in the Supplemental Material, (e.g., section S4). Supplemental Material does include basic methodology and basic error considerations that are essential to understand the manuscript and cited in the main text.

#Response 3.5

Thank you for your comment.

• Conclusions:

#Comment 3.6

As mentioned previously, the Conclusions are not somewhat incomplete, there is no final evaluation of the benefit of the new model in our understanding of the Southern Ocean system or the novelty this method brings compared to others. Having a new estimate is always reassuring, however, why is this new number significant is not highlighted in the Conclusions. After reading this report my conclusion is that remote sensing gives similar results than remote sensing data (Li et al. 2021, referenced in the manuscript).

#Response 3.6

Regarding the inadequate emphasis on innovation in the paper, we have revised it in the manuscript, referring to *Response 3.3*.

• References:

#Comment 3.7

The references that relate to estimates of Southern Ocean NCP and carbon dioxide absorption are well referenced, however the authors could add some refs that will help evaluate the new method within a broader understanding of SO system, for example Arrigo's refs. By not including references with similar estimates, the manuscript is written more for the scientific community working on NCP, not so much for the broader scientific community interested in the role of the Southern Ocean in absorbing atmospheric carbon dioxide but working with other methods and sensors.

#Response 3.7

As you said, we add the required reference (Arrigo et al., 2008). (Line 42)

Arrigo et al., 2008 was based on the satellite observation. The remote sensing satellite method is limited by the fact that only surface observations can be made. Moreover, the existing methods usually require the use of idealized stoichiometry ratios. Our method overcomes the disadvantages of the above methods and can be applied to the Southern Ocean.

However, according to your comment, we showed a comparison of our results with other studies including the NCP and its trend in Table 2 and Fig. 3. However, as mentioned previously, these previous studies have problems such as the neural network is a black box and there is covariance between the input parameters (Line 79-89, 194-199, 245-271, Table 2, Fig. 3).

• Clarity and context:

#Comment 3.8

The Abstract is poor, it is not written for a general audience nor is it attractive, there is no mention of the novelty of the results and a strong sentence making clear the main contribution from this study is missing.

The Introduction is thorough and well cited, clearly this is a methodological study.

The Conclusions need completion: as mentioned above it is not clear why this method is better than others given the results are not so different (Table 2), in particular using remote sensing data.

#Response 3.8

We have added a background introduction in the “Abstract” and emphasized more about the innovations of our MLR-NN hybrid method in the “conclusions” (Line 194-199) to make them more friendly for a general audience.

LIST ALL THE OTHER MAIN POINTS HERE

1. This reviewer finds the definition of NCP and RES are not coherent throughout the manuscript (compare lines 253 to 255 with Figure S4, seems to have the correct definition).

We followed your comment and added the definition of NCP and RES in line 281-282.

2. "Ideal state", cited in Abstract and others, is a very subjective statement, please define.

The “ideal state” here refers to the conditions required to achieve the accurate spatiotemporal NCP. This was already stated in the original manuscript (Line 53-59).

3. Introduction: one paragraph explains clearly why DO is a problem (lines 53 to 64), the next Paragraph (lines 65 to 76) uses DO in the new method.

We first pointed out the problem of using DO with DO/carbon ratio to estimate the NCP since DO/carbon ratio is not a constant in the real ocean. Next, we stated that the parameterization of DIC in this study can avoid this influence (Line 59-71). Please reconfirm this point.

4. Please define acronyms in titles, figures, etc to make them self-sustaining.

We followed your comment and checked the acronyms again (especially changed “Restoration” to “RES”).

5. The definition of "data" is unclear. After Table S1 and Figure S2, I think replacing data by dataset might be more clear. The authors included 154 datasets, 147 datasets, of different length, were included (Figure S2), with 27,030 vertical profiles (Figure S2) resulting in 484 NCP estimates (Figure S3).

We followed your comment and changed the “data” in Table S1 into “dataset”, because Table S1 shows all the BGC-Argo data used in this study, and we defined it as a dataset.

6. Why was not mixed layer depth or stratification analyzed to explain the results? The calculation of mixed layer cis done as part of the calculations of NCP and RES.

We used critical depth as integral depth for the estimation NCP and RES water column inventory (Fig. S5). According to your comment, we added a map of the critical depth in the SO (Fig. S9, Line 285-288).

It would help to see what the data says, explaining results based on a publication that has studied only a limited region of the Southern Ocean is not expected to be applicable everywhere.

We have compared our NCP estimation with other satellite or model derived estimation over the SO (Table 2). We also chose a previous study which focus on the Indian sector for a basin-scale comparison (Line 151-158).

REFERENCES

Arrigo, K.R., van Dijken, G.L. and Bushinsky, S., 2008. Primary production in the Southern Ocean, 1997–2006. Journal of Geophysical Research: Oceans, 113(C8).

Arrigo, K.R., Worthen, D., Schnell, A. and Lizotte, M.P., 1998. Primary production in Southern Ocean waters. Journal of Geophysical Research: Oceans, 103(C8), pp.15587-15600.

20th Sep 23

Dear Dr Pan,

Your manuscript titled "Spatiotemporal high-resolution mapping of biological production in the Southern Ocean" has now been seen by two of the original reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

*****Please take care to match our formatting and policy requirements. We will check revised manuscript and return manuscripts that do not comply. Such requests will lead to delays. *****

Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Jose Luis Iriarte Machuca, PhD
Editorial Board Member
Communications Earth & Environment

Clare Davis, PhD
Senior Editor
Communications Earth & Environment

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REVIEWERS' COMMENTS:

Reviewer #3 (Remarks to the Author):

COMMSENV-3459-1
Revised COMMSENV-23-0427
NATURE Communications, Earth & Environment
Reviewer 3

The Authors responded clearly to comments from Reviewer 3 and completed requested information. I thank them for the careful consideration of all comments. There are a few issues that the authors might want to consider before the manuscript is ready for publication.

In response to the revised text, and requests from the Authors, please consider:

1. Abstract was given a longer introduction to justify the study, however it still misses what the authors consider the new aspects of this study, their main contribution, as developed in the Results and Discussion section of the manuscript.

Based on authors' response to comments 3.3 and Table 2, the authors might want to consider highlighting what is new in the Abstract (added in color to the submitted Abstract):

Abstract: The Southern Ocean (SO) is considered to play an important role in the global biogeochemical cycles of carbon and nutrients. During the Last Glacial Maximum (LGM, ~25,000 years ago), an increase of biological production in the SO may have reduced atmospheric CO_2 concentration and contributed to global cooling, and vice versa. It is still unclear how biological production in the SO impact climate change owing to the remarkable gap between the current and the ideal state of research. In order to fill in the gap and clarify the contribution of SO to the Earth climate system, we proposed a new concept of combining Neural Network (NN) based parameterization of dissolved inorganic carbon (DIC) with Biogeochemical Argo floats, highlighting spatiotemporal distribution of net community production (NCP) in the entire SO based on the DIC change. Based on annual estimates with a 19-year time series of DIC Consumed within 0-200 m depth, DIC Restoration to the mixed layer and Carbon Sink, the NN model indicated total NCP of 4.1 ± 0.3 Pg-C year-1 (Pg = 1015 g) over the SO, which accounts for 40% of global annual oceanic carbon export and is 20% higher than the average of previous estimates. Furthermore, the NCP in the SO was decreasing 0.8% year-1 since the 2010s, indicating that it may be contributing as positive feedback to the increase in atmospheric CO_2.

2. With respect to the Response to Comment 3.1, this reviewer understands there are no ARGOS data in the coastal sea ice region, and the new NN model cannot be applied there. The authors have included a declaimer in Figure S4.2, and it is accepted by the reviewer. However, the comment that "their [Arrigo's et al.] coastal estimates are relatively narrow in scope and do not have a significant impact on our overall Southern Ocean estimate" is somewhat misleading. First, the Arrigo study also calculates a PP of 4.4 Tg C yr-1, somewhat higher than this study, and the area covered is smaller, south of 50S, not 30S. In that sense remote sensing provides a higher PP than this study. Second, the Sea Ice Zone, mostly close to the coast, contributes 9.8% of the PP (I agree with the author's that the coastal zone per se is only 1.8% of total PP, rather small). To what extent is the SIZ zone included in this study? Third, the authors might want to consider that the Southern Ocean definition used in this manuscript is unusually large. SO is officially defined as waters south of 60oSouth, not 30oS. 60oS as the northern limit of the Southern Ocean is the official definition of the governments (i.e. https://www.cia.gov/the-world-factbook/oceans/southern-ocean/#geography; the British Antarctic Survey, etc.) and the Antarctic Treaty. The region between 30oS and 60oS is usually considered the sub-Antarctic Zone.

3. I have doublechecked the response to comment 3.7 and I agree the issue is well documented in the manuscript.

4. I confirm the introduction to DO, the limitations of the previous methods and how it was resolved for this NN method is well explained.

5. I appreciate the addition of Figure S9 and the relationship between NCP and Critical Depth and also the addition of Table S2. Please add the significance of the relationship of NCP and Critical Depth, $R^2 = 0.3$ in Figure S9.

6. Figure 2, panel a): Region 1 goes from PF to the coast, not 90oS, i.e South Pole in the middle of the continent.

7. Line 268: a typo, please replace "agree" with "agreement".

Reviewer #4 (Remarks to the Author):

Review of: Spatiotemporal high-resolution mapping of biological production in the Southern Ocean

The manuscript "Spatiotemporal high-resolution mapping of biological production in the Southern Ocean" uses a multiple linear regression (MLR) and neural network (NN) approach to estimate DIC and net community production in the Southern Ocean. The MLR-NN hybrid parameterization algorithm was trained using shipboard measurements of temperature, salinity, pressure, dissolved oxygen and DIC (GLODAP) from 2000 to 2020. The obtained parameterization was then applied to BGC-Argo observations (2007-
2019). Results are then used to describe spatial and interannual variability in net community production (NCP), restoration and carbon removal in the Southern Ocean. This manuscript is a re-submission after major revisions were requested by three anonymous reviewers.

The new methodology addresses the limitations of obtaining in situ DIC measurements and thus represents an important aid in advancing the understanding of the carbon cycle in the Southern Ocean. The results are an important contribution to Southern Ocean and climate science in general. However, the manuscript is difficult to read, the logic appears disorganized, and there are sentences with inadequate grammar or syntaxes. The introduction is particularly affected by the lack of flow, and the main goal and novel results of the study are missing from the abstract. These issues were brought up by previous reviewers, but I find they have not been properly addressed in the revised version. In some instances, text that appears to have been added/modified during the first round of revisions is the source of confusion, unclarity or appears to be unnecessary. Major revisions are still needed in order to bring the manuscript into publication-ready shape. The manuscript may benefit from reorganization and copy editing. Below I include some specific suggestions I hope help improving the clarity of the text.

Specific comments:

1. Abstract: The abstract starts mentioning the Last Glacial Maximum, which I consider an unnecessary detail given that the paper does not address those long timescales. Also, I'm not sure what is meant with the use of "vice versa" here. I also suggest highlighting the novel spatio-temporal variability results in the abstract. As it is now, the result brought forward in the abstract just confirms that the Southern Ocean accounts for about 40% of global annual oceanic carbon export, as previously shown by other studies. I would not put emphasis in the NCP trend either, as the period studied is not sufficient to elucidate between significant trends and interannual variability. Furthermore, the trend obtained is only weakly significant.
2. Lines 29 – 32 appear out of place. The idea is repeated in the next paragraph.
3. SCM acronym unnecessary. If it's only mentioned a couple of times (5), it's easy to forget what it is and hard to find where it is defined.
4. Line 43 satellite-based estimation of what?
5. Line 56 solar activity? Like solar flares?
6. I would suggest shortening the introduction by summarizing the ideas of paragraph 3 and 4 only in

a few sentences. The fact that we would like DIC measured directly at high spatial and temporal resolution globally and sustained over long timescales doesn't need to be explained with such detail (as in paragraph 4). Instead, focus on the novel aspect of the methodology and results.
7. Line 76. "To achieve these…" to achieve what? The estimates? The input parameters?
8. Is there a reason to have the results and discussion section before the methods section? In order to avoid redundancy in the text, it's better to explain what was done before presenting the results.
9. I suggest being more direct and concise in the description of data and methods. For example, instead of saying: "[lines 97-101] To obtain large amounts of T, S, DO, and Pr, which can be used to reconstruct the spatiotemporal distribution of DIC over the SO, we turned our attention to the BGC-Argo data. By applying our NN-based parameterization of DIC to the T, S, DO, and Pr measured in 27,039 cycles of 154 BGC-Argos from 2004 to 2019, we obtained the spatiotemporal distribution of DIC over the SO (see Method section "Data used in this study" for details)."; you can say: "We used T, S, DO and Pr measurements from 27,039 BGC-Argo profiles (154 floats) to map the spatiotemporal distribution of DIC in the Southern Ocean by applying the parameters found using the NN algorithm." Also, these details correspond to methods section.
10. Lines 111-120. I found these sentences particularly hard to read. a) By describing methods first you will be able to remove explanation of how NCP was estimated and what tNCP is. I find the extra units are unnecessary. It also took me sometime to realize that 9.1 PgCyear{-1} is the global ocean carbon export, the way it's written it appears that it is SO carbon export in a previous study, which contradicts the next few sentences and Table 2. Some discussion is missing here to put the results in context. How were each (or at least a few) of the other estimates obtained. E.g., if the difference is due to integrating over the critical depth, make it clear other studies only considered surface values.
11. Lines 143-144. The entire ocean has NCP. Do you mean is the largest area with high NCP? If so, how do you define high NCP? Also, it is not clear from Fig1 that the Atlantic has the largest area with [high] NCP.
12. Fig. 2.c, d. Gray line indicates CS, from what I understand. Line plots should be used to indicated continuity in the date, which is not the case. Use just the markers or add a thinner bar inside the NCP/RES bar.
13. One of the main results from this study is new gridded NCP, RES and CS maps for the SO. However, the maps are hidden in the supplement (Fig S6). I suggest combining the gridded maps with Fig1. Black dots with Argo locations are barely noticeable and missing from subplot S6c. Also, I find the extrapolated results dubious. For instance, a hot spot of NCP/RES is found south of West Antarctica, where it is clear that results came from a single float off-the coast. The same is noticeable in many hotspots found near the northern boundaries of the domain.

Reviewer 3

Thank you for your valuable comments. And we have tried our best to respond your comments point-by-point. We would appreciate it if you could review our responses in this file and the revised manuscript file. Our replies and modifications are shown in blue.

The Authors responded clearly to comments from Reviewer 3 and completed requested information. I thank them for the careful consideration of all comments. There are a few issues that the authors might want to consider before the manuscript is ready for publication.

In response to the revised text, and requests from the Authors, please consider:

#Comment 3.1

1. Abstract was given a longer introduction to justify the study, however it still misses what the authors consider the new aspects of this study, their main contribution, as developed in the Results and Discussion section of the manuscript.

Based on authors' response to comments 3.3 and Table 2, the authors might want to consider highlighting what is new in the Abstract (added in color to the submitted Abstract):

Abstract: The Southern Ocean (SO) is considered to play an important role in the global biogeochemical cycles of carbon and nutrients. During the Last Glacial Maximum (LGM, ~25,000 years ago), an increase of biological production in the SO may have reduced atmospheric CO2 concentration and contributed to global cooling, and vice versa. It is still unclear how biological production in the SO impact climate change owing to the remarkable gap between the current and the ideal state of research. In order to fill in the gap and clarify the contribution of SO to the Earth climate system, we proposed a new concept of combining Neural Network (NN) based parameterization of dissolved inorganic carbon (DIC) with Biogeochemical Argo floats, highlighting spatiotemporal distribution of net community production (NCP) in the entire SO based on the DIC change. Based on annual estimates with a 19-year time series of DIC Consumed within 0-200 m depth, DIC Restoration to the mixed layer and Carbon Sink, the NN model indicated total NCP of 4.1 ± 0.3 Pg-C year-1 (Pg = 1015 g) over the SO, which accounts for 40% of global annual oceanic carbon export and is 20% higher than the average of previous estimates. Furthermore, the NCP in the SO was decreasing 0.8% year-1 since the 2010s, indicating that it may be contributing as positive feedback to the increase in atmospheric CO2.

#Response 3.1

We revised the Abstract based on your advice.

#Comment 3.2

2. With respect to the Response to Comment 3.1, this reviewer understands there are no ARGOS data in the coastal sea ice region, and the new NN model cannot be applied there. The authors have included a declaimer in Figure S4.2, and it is accepted by the reviewer. However, the comment that "their [Arrigo's et al.] coastal estimates are relatively narrow in scope and do not have a significant impact on our overall Southern Ocean estimate" is somewhat misleading. First, the Arrigo study also calculates a PP of 4.4 Tg C yr-1, somewhat higher than this study, and the area covered is smaller, south of 50S, not 30S. In that sense remote sensing provides a higher PP than this study. Second, the Sea Ice Zone, mostly close to the coast, contributes 9.8% of the PP (I agree with the author's that the coastal zone per se is only 1.8% of total PP, rather small). To what extent is the SIZ zone included in this study? Third, the authors might want to consider that the Southern Ocean definition used in this manuscript is unusually large. SO is officially defined as waters south of 60oSouth, not 30oS. 60oS as the northern limit of the Southern Ocean is the official definition of the governments (i.e. https://www.cia.gov/the-world-factbook/oceans/southern-ocean/#geography; the British Antarctic Survey, etc.) and the Antarctic Treaty. The region between 30oS and 60oS is usually considered the sub-Antarctic Zone.

#Response 3.2

Here, we answered your questions point by point.

Response to the first question

The PP = 4.4Pg-C yr^{-1} that you pointed out is the gross primary production (GPP) value of Arrigo et al. (1998). This value is calculated based on chlorophyll values obtained by Coastal Zone Color Scanner (CZCS) and is very uncertain because it may overestimate chlorophyll in high latitude waters such as the Southern Ocean. Therefore, Arrigo et al. (2008) used chlorophyll data from the Sea-viewing Wide Field-of-view Sensor (SeaWiFS) to improve the GPP, resulting in an estimated GPP of 1.9 Pg-C y^{r-1} for the Southern Ocean, which is less than half of the GPP of Arrigo et al. (2008). It is also less than half of our NCP = 4.1 Pg-C yr^{-1}, even though the GPP must be higher than the NCP. That is because, as we point out in the text, the estimates of Arrigo et al. (1998, 2008) rely solely on satellite data and the stoichiometric ratio of chlorophyll to carbon (Lines 45-49).

However, it was problematic that we did not include Arrigo et al. (2008) in Table 2. Thus, we have added the values of Arrigo et al. (2008) to Table 2.

Response to the second question

We do not explicitly define the extent of the SIZ, you can recognize the SIZ as the regions near the Antarctic coast where there is no Argo data considering the fact that Argo floats cannot work in sea ice zone. For the method of extrapolation in the sea ice coverage area including the SIZ and the estimation of the error, refer to the manuscript. (Lines 293-299 in manuscript and section “S4: Uncertainties of NCP, RES and CS” in Supplementary information)

Response to the third question

As you said, the international definition of the Southern Ocean is south of 60˚S latitude. However, there are also many recent studies that refer to the Antarctic zone (south of 60˚S) and the sub-Antarctic zone (60˚S to 30˚S) in a unified way as the Southern Ocean (e.g. Sarmiento et al. 2004; Tuck et al. 2003; Bourgeois et al. 2002). We have also noted our definition of the Southern Ocean at the very beginning of the article (Line 21).

#Comment 3.3

3. I have doublechecked the response to comment 3.7 and I agree the issue is well documented in the manuscript.

#Response 3.3

Thank you for your comment.

#Comment 3.4

4. I confirm the introduction to DO, the limitations of the previous methods and how it was resolved for this NN method is well explained.

#Response 3.4

Thank you for your comment.

#Comment 3.5

5. I appreciate the addition of Figure S9 and the relationship between NCP and Critical Depth and also the addition of Table S2. Please add the significance of the relationship of NCP and Critical Depth, R^2 = 0.3 in Figure S9.

#Response 3.5

We added the p-value of the relationship between NCP and Critical Depth in Fig. S9.

#Comment 3.6

6. Figure 2, panel a): Region 1 goes from PF to the coast, not 90oS, i.e South Pole in the middle of the continent.

#Response 3.6

We revised this figure in the manuscript (Fig. 2a).

#Comment 3.7

7. Line 268: a typo, please replace "agree" with "agreement".

#Response 3.7

We revised this content in the manuscript (Line 263).

Reviewer 4:

Thank you for your valuable comments. And we have tried our best to respond your comments point-by-point. We would appreciate it if you could review our responses in this file and the revised manuscript file. Our replies and modifications are shown in blue.

Review of: Spatiotemporal high-resolution mapping of biological production in the Southern Ocean

The manuscript "Spatiotemporal high-resolution mapping of biological production in the Southern Ocean" uses a multiple linear regression (MLR) and neural network (NN) approach to estimate DIC and net community production in the Southern Ocean. The MLR-NN hybrid parameterization algorithm was trained using shipboard measurements of temperature, salinity, pressure, dissolved oxygen and DIC (GLODAP) from 2000 to 2020. The obtained parameterization was then applied to BGC-Argo observations (2007-2019). Results are then used to describe spatial and interannual variability in net community production (NCP), restoration and carbon removal in the Southern Ocean. This manuscript is a re-submission after major revisions were requested by three anonymous reviewers.

The new methodology addresses the limitations of obtaining in situ DIC measurements and thus represents an important aid in advancing the understanding of the carbon cycle in the Southern Ocean. The results are an important contribution to Southern Ocean and climate science in general. However, the manuscript is difficult to read, the logic appears disorganized, and there are sentences with inadequate grammar or syntaxes. The introduction is particularly affected by the lack of flow, and the main goal and novel results of the study are missing from the abstract. These issues were brought up by previous reviewers, but I find they have not been properly addressed in the revised version. In some instances, text that appears to have been added/modified during the first round of revisions is the source of confusion, unclarity or appears to be unnecessary. Major revisions are still needed in order to bring the manuscript into publication-ready shape. The manuscript may benefit from reorganization and copy editing. Below I include some specific suggestions I hope help improving the clarity of the text.

Specific comments:

#Comment 4.1

1. Abstract: The abstract starts mentioning the Last Glacial Maximum, which I consider an unnecessary detail given that the paper does not address those long timescales. Also, I'm not sure what is meant with the use of "vice versa" here. I also suggest highlighting the novel spatio-temporal variability results in the abstract. As it is now, the result brought forward in the abstract just confirms that the Southern Ocean accounts for about 40% of global annual oceanic carbon export, as previously shown by other studies. I

would not put emphasis in the NCP trend either, as the period studied is not sufficient to elucidate between significant trends and interannual variability. Furthermore, the trend obtained is only weakly significant.

#Response 4.1

We would like to attempt to show the importance of NCP of the SO to climate through the relationship between biological production and climate during the LGM period. Considering your comments and the above point, we revised the Abstract on this content. We also revised the Abstract to make it emphasizes more about the innovations of this study.

#Comment 4.2

2. Lines 29 – 32 appear out of place. The idea is repeated in the next paragraph.

#Response 4.2

As you say, we have repositioned this part of the article to make it more readable (Lines 38-65).

#Comment 4.3

3. SCM acronym unnecessary. If it's only mentioned a couple of times (5), it's easy to forget what it is and hard to find where it is defined.

#Response 4.3

As you say, we will not use the SCM acronym in this paper (Lines 48&53).

#Comment 4.4

4. Line 43 satellite-based estimation of what?

#Response 4.4

It is "satellite-based estimation of the NCP". We rewrote this content in the manuscript (Line 47).

#Comment 4.5

5. Line 56 solar activity? Like solar flares?

#Response 4.5

What we are trying to convey here is that uptake and remineralization of carbon have a cycle of about one year is due to the revolution of the Earth. We rewrote this content in the manuscript (Line 41).

#Comment 4.6

6. I would suggest shortening the introduction by summarizing the ideas of paragraph 3 and 4 only in a few sentences. The fact that we would like DIC measured directly at high spatial and temporal resolution globally and sustained over long timescales doesn't need to be explained with such detail (as in paragraph 4). Instead, focus on the novel aspect of the methodology and results.

#Response 4.6

Here our showing the inadequacy of the current research methodology and the necessity of importing the methodology of this study, these two paragraphs have been deliberately included. However, your comment makes sense. Thus, we have shortened some of them to make them more readable (Lines 38-65).

#Comment 4.7

7. Line 76. "To achieve these…" to achieve what? The estimates? The input parameters?

#Response 4.7

We would like to convey that "to overcome the shortcomings of the neural network model in the previous studies". We rewrote this content in the manuscript (Line 73).

#Comment 4.8

8. Is there a reason to have the results and discussion section before the methods section? In order to avoid redundancy in the text, it's better to explain what was done before presenting the results.

#Response 4.8

We placed the methods section at the end of the original manuscript according to the journal's formatting requirements. We have added remarks in boldface type throughout the text to help readers better understand what methods are used as they read through the text (e.g., line 77, "see Method section **DIC Parameterization methods"**).

#Comment 4.9

9. I suggest being more direct and concise in the description of data and methods. For example, instead of saying: “[lines 97-101] To obtain large amounts of T, S, DO, and Pr, which can be used to reconstruct the spatiotemporal distribution of DIC over the SO, we turned our attention to the BGC-Argo data. By applying our NN-based parameterization of DIC to the T, S, DO, and Pr measured in 27,039 cycles of 154 BGC-Argos from 2004 to 2019, we obtained the spatiotemporal distribution of DIC over the SO (see Method section “Data used in this study” for details).”; you can say: “We used T, S, DO and Pr measurements from 27,039 BGC-Argo profiles (154 floats) to map the spatiotemporal distribution of DIC in the Southern Ocean by applying the parameters found using the NN algorithm.” Also, these details correspond to methods section.

#Response 4.9

Your comment makes sense. Thus, we revised this section of the manuscript to make it more concise (Lines 91-94).

#Comment 4.10

10. Lines 111-120. I found these sentences particularly hard to read. a) By describing methods first you will be able to remove explanation of how NCP was estimated and what tNCP is. I find the extra units are unnecessary. It also took me sometime to realize that 9.1 PgCyear{-1} is the global ocean carbon export, the way it’s written it appears that it is SO carbon export in a previous study, which contradicts the next few sentences and Table 2. Some discussion is missing here to put the results in context. How were each (or at least a few) of the other estimates obtained. E.g., if the difference is due to integrating over the critical depth, make it clear other studies only considered surface values.

#Response 4.10

We followed the journal's formatting requirements by placing the methods section at the end of the original manuscript.

We used two kinds of units to show our results. Pg-C is usually used in previous studies. In Fig. 2, Tmol-C is used for ease of understanding since Tmol-C has a larger scale. However, there were some points that were difficult to understand, so we have added notes on these two estimation units in the manuscript (Line 106).

To be precise the SO NCP estimated in this study is 45% of the global NCP (which was approximated as 40% in the previous manuscript), which is higher than the value in the previous studies and is consistent with the subsequent results of this study being 20% higher than the previous studies. We revised this content in the manuscript (Line 107).

As for the reason why our result is higher than those of the previous studies, we noted it in Lines 112-114.

#Comment 4.11

11. Lines 143-144. The entire ocean has NCP. Do you mean is the largest area with high NCP? If so, how do you define high NCP? Also, it is not clear from Fig1 that the Atlantic has the largest area with [high] NCP.

#Response 4.11

Here we discuss the magnitude of the total NCP (tNCP) in different sectors in the SO. Since the Atlantic Ocean has the largest NCP per unit area, we can see in Fig. 1 and Fig. S6 that a significant portion of the Atlantic Ocean has a higher NCP (larger than 4mol-C m^{-2} $year^{-1}$) (Line 137).

#Comment 4.12

12. Fig. 2.c, d. Gray line indicates CS, from what I understand. Line plots should be used to indicated continuity in the date, which is not the case. Use just the markers or add a thinner bar inside the NCP/RES bar.

#Response 4.12

According to your comment, we revised this figure in the manuscript. (Fig. 2c,d)

#Comment 4.13

13. One of the main results from this study is new gridded NCP, RES and CS maps for the SO. However, the maps are hidden in the supplement (Fig S6). I suggest combining the gridded maps with Fig1. Black dots with Argo locations are barely noticeable and missing from subplot S6c. Also, I find the extrapolated results dubious. For instance, a hot spot of NCP/RES is found south of West Antarctica, where it is clear that results came from a single float off-the coast. The same is noticeable in many hotspots found near the northern boundaries of the domain.

#Response 4.13

Considering also that the extrapolated distributions in Fig. S6 may be affected by the lack of Argo in some regions, we only discuss in the main text the NCP distributions in the seas where Argo data points are present (Fig. 1), and the total NCP in the Southern Ocean computed from the extrapolations. We thus consider that the extrapolated distribution in Fig. S6 is only a reference for the reader.

As for the rationality of calculating the total Southern Ocean NCP by extrapolation, we already mentioned the followings in the manuscript: We used all currently available BGC-Argo data for the estimation. But there are still some regions that are not covered by the data (Fig. S3). For this, we used the extrapolation method to expand the data for these uncovered regions. In order to evaluate the appropriateness of the extrapolation method, we estimated the error deriving from the extrapolation process (Supplementary Information **"S4: Uncertainties of NCP, Restoration and CS")**, which in the regions with no BGC-Argo passing through as 7%, resulting in 3% of an area-weighted gridding error over the SO. Few data along the Antarctic coast (Fig. S3) have the potential to introduce greater error (Supplementary Information Line 105-106).

These points should be taken into consideration.