nature portfolio

Peer Review File

Decoupled Antarctic and Subarctic Export Productivity Under Intensified Southern Hemisphere Westerlies During the Last Interglacial

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Dr Lijuan Lu and colleagues reconstruct changes in export productivity in the Southern Ocean and Arctic Pacific covering two glacial-interglacial cycles (last 160 Ka) using 15 export productivity records from 12 marine sediment cores from the Antarctic Zone of the Sothern Ocean and comparing them with subarctic Pacific EP. Authors argue that both Antarctic and Arctic export productivity followed similar patterns during most of the record expect for the Last Interglacial (128-113 kyrs). In particular, during the LIG, Antarctic export production and atmospheric CO2 was high and coupled with a decline in Antarctic air temperatures of ca. 6ºC, while Subarctic Pacific export productivity dropped. Authors argued that this decoupling was most likely driven by an intensification of the southern hemisphere Westerlies that stimulated upwelling of Circumpolar Deep Water that enhanced export productivity and the release of CO2 to the atmosphere.
Overall, the manuscript is well-written and structured, and the figures are of high quality. Regarding the statistical treatment, some additional information is required while other analysis are beyond my expertise and therefore, I am unable to judge their validity. In regard to the scientific interest, the polar oceans play an essential role in the control of global atmospheric CO2, and model predictions point to a poleward intensification of westerlies in the coming decades. Therefore, new insights into the role of the Southern Ocean and Arctic Ocean during a period with analogies to our imminent future could be of great interest for a broad scientific audience.
Having said that, there are several important points that authors would have to deal with to prove their hypothesis and therefore before the manuscript can be considered suitable for publication in Nature Communications. Please find a list of the principal drawbacks of this paper listed below:

Biogenic silica accumulation as a proxy for export productivity:
The interpretation of biogenic silica accumulation as a direct proxy for export productivity has been questioned by multiple authors. As indicated by De la Rocha et al. (2010): “Although there is a strong link between BSi production and BSi accumulation, variability and shifts in the ratios of BSi to POC during particle production, export, and preservation complicate the extrapolation of primary production from BSi accumulation (Pondaven et al., 2000; Ragueneau et al., 2000; DeMaster 2002; Nelson et al., 2002; Ragueneau et al., 2002; Moriceau et al., 2007).”
And also in De la Rocha et al. (2010):: “…the lower glacial accumulation rates of BSi south of the APF in the Atlantic sector of the Southern Ocean can be entirely accounted for by shifts in diatom species composition and Fe availability and do not need to reflect lower glacial levels of primary production. Likewise, the higher glacial accumulation rates of BSi north of the APF may likely also not reflect higher glacial levels primary production”.
Moreover, Assmy et al. (2013) also indicated that “It follows that high sedimentary silica accumulation rates are a proxy for iron-limited diatom assemblages rather than for high surface productivity, contrary to the prevailing view”.
Therefore, in order to assess changes in export production would be important to discuss changes in diatom species composition (the main components of the Biogenic silica) rather than discussing the Biogenic Silica alone. Is diatom assemblage composition for those records available? Do diatom assemblages change? And if so, does the change in the diatom assemblage support the interpretations of the results? Although the present manuscript is not focussed on the diatom assemblage composition, some information in this regard could greatly help to support (or discard) the findings of the authors.

Temperature as main control of export productivity:

Line 176 "Assuming that temperature is the primary control on export productivity in the Antarctic Zone". The main control of algal biomass accumulation and export productivity in the modern Antarctic Zone is iron availability (Boyd et al., 2000). While temperature is most likely an important factor controlling the distribution of phytoplankton functional groups and therefore efficiency of the biological pump, the primary role of temperature in the control of export production is not clear at all. This point has to be explained in detail in the manuscript. Please clarify.

Export productivity in the modern Southern Ocean.
Lines 170-171 "Currently, the zone of maximum export productivity is located south of the Antarctic Polar Front (APF), within the Antarctic zone of the Southern Ocean". This is statement is true for part of the western Pacific sector of the Southern Ocean based on the data from the AESOPS experiment. However, it cannot be considered a circumpolar feature of the Southern Ocean. For example, POC fluxes to the deep ocean are very similar between the Antarctic Zone, Polar Frontal Zone and Subantarctic Zone in the Australian sector of the Southern Ocean: in this region annual POC fluxes are slightly higher in the SAZ than in the AZ at 2000 m and 3800 m (1.4 and 1 g C m-2 yr-1 at 2000 and 3800 m at the SAZ versus 1.2 and 0.9 g C m-2 yr-1 at 2000 and 3800 m at the AZ). See Wynn-Edwards et al. (2020) and Rigual-Hernández et al. (2015). Please also look for information in the Atlantic sector. Based on all of the above, authors should be more cautious with their statement and explain in detail the differences in EP across sectors of the Southern Ocean. Also, quantifying the difference in EP between circumpolar systems would help the reader to understand what is the current difference in export production between different zonal systems. While total mass flux is quite different across zonal systems mainly due to differences in opal export, the magnitude in POC export is small.

Correlations: e.g. lines 211-212 and Fig. 4. It is not clear in the text if the variables in Fig. 4 are normally distributed. This information is important to determine which correlation coefficient would be more appropriate. Could authors explain in the text why Pearson correlation coefficient was used and not Spearman's test? Please clarify and revised if necessary.

Methods, lines 108-110: "inverse standard deviation weighting method employs the inverse of each variable's standard deviation as weights to minimize the influence of high-variability variables on the results". This methodology is beyond my expertise and therefore I am unable to evaluate if this is an adequate approach.

Figure 5. Please explain in the text the meaning of the circle with a dot in the schematic (position of the main winds I assume).

References

Assmy, P., Smetacek, V., Montresor, M., Klaas, C., Henjes, J., Strass, V.H., Arrieta, J.M., Bathmann, U., Berg, G.M., Breitbarth, E., Cisewski, B., Friedrichs, L., Fuchs, N., Herndl, G.J., Jansen, S., Krägefsky, S., Latasa, M., Peeken, I., Röttgers, R., Scharek, R., Schüller, S.E., Steigenberger, S., Webb, A., Wolf-Gladrow, D., 2013. Thick-shelled, grazer-protected diatoms decouple ocean carbon and silicon cycles in the iron-limited Antarctic Circumpolar Current. Proceedings of the National Academy of Sciences 110, 20633-20638.
Boyd, P., Watson, A., Law, C., Abraham, E., Trull, T., Murdoch, R., Bakker, D., Bowie, A., Buesseler, K., Chang, H., 2000. Phytoplankton bloom upon mesoscale iron fertilisation of polar Southern Ocean waters. Nature 407, 695-702.
De la Rocha, C.L., Ragueneau, O., Leynaert, A., 2010. Reconsidering the meaning of biogenic silica accumulation rates in the glacial Southern Ocean, in: Smol, J.P., Stoermer, E.F. (Eds.), The Diatoms Cambridge University Press, pp. 454–461.
Rigual-Hernández, A.S., Trull, T.W., Bray, S.G., Closset, I., Armand, L.K., 2015. Seasonal dynamics in diatom and particulate export fluxes to the deep sea in the Australian sector of the southern Antarctic Zone. Journal of Marine Systems 142, 62-74.
Wynn-Edwards, C.A., Shadwick, E.H., Davies, D.M., Bray, S.G., Jansen, P., Trinh, R., Trull, T.W., 2020. Particle fluxes at the Australian Southern Ocean Time Series (SOTS) achieve organic carbon sequestration at rates close to the global median, are dominated by biogenic carbonates, and show no temporal trends over 20-years. Frontiers in Earth Science 8, 329.

Reviewer #2

(Remarks to the Author)
The manuscript by Lu et al. addresses a critical and timely question in paleoclimate: why Antarctic and subarctic Pacific EP became decoupled during the LIG despite generally co-varying with atmospheric CO_2 and temperature on glacial–interglacial timescales. The study's primary finding – that intensified, poleward-shifted SHW drove enhanced upwelling of nutrient- and CO_2-rich deep water in the Antarctic Zone during the LIG, sustaining high Antarctic EP and contributing to high atmospheric CO_2, while subarctic Pacific EP declined – is novel and significant. It challenges the traditional paradigm that Antarctic temperature was the dominant driver of Southern Ocean carbon release in warm periods. The authors provide an alternative mechanism whereby wind-driven ocean dynamics can override or modulate temperature-driven effects, which represents an important contribution to paleo-carbon cycle science and highlights the SHW as an underappreciated player in controlling past CO_2 variability.

Overall, this is an impressive and valuable manuscript and is squarely appropriate for Nature Communications, given its significant for paleoclimate and future climate understanding. I recommend publication after revision, addressing the concerns raised below. The most critical issues to resolve are clarifications of the proposed mechanism (especially regarding changes in deep ocean circulation and the contrasting behavior of the subarctic Pacific) and some enhancements to clarity in presentation. Thus, the revision can likely be done in a straightforward manner. I look forward to seeing a revised manuscript that incorporates these suggestions.

Some key concerns:

The authors demonstrate high coherence among proxy types (opal vs. barium) and perform cross-correlations that show Antarctic EP, subarctic Pacific EP, and Antarctic temperature tracked each other closely for most of the past 160 kyr. For the critical LIG interval, the authors identify a breakdown of these correlations (the decoupling). However, I think this is not clearly shown qualitatively in figures, and I recommend quantifying the decoupling more explicitly. For example, the authors could report correlation coefficients between Antarctic and subarctic EP over specific intervals (glacial vs. interglacial vs. LIG) to statistically demonstrate that the LIG correlation is significantly lower. A running or windowed correlation analysis might also highlight when the divergence emerged.

Another methodological aspect that could use more explanation is how the subarctic Pacific export productivity record was derived or selected. The text references subarctic Pacific EP data with citations, but readers may benefit from a clearer description: e.g., is this a composite record from a particular site or region? Detailing the source and nature of the subarctic EP reference would help the audience understand its resolution and uncertainties relative to the Antarctic data. Additionally, if the subarctic record's age model was also aligned to Antarctic chronology (perhaps via atmospheric CH_4 or $\delta^{18}O$ correlations at Termination II), that should be stated for transparency.

The authors argue that in the LIG, upwelling supply outpaced biological uptake, leading to CO_2 release despite high productivity. This is an important nuance – usually high productivity draws down CO_2, but only if nutrient utilization is nearly complete. In an intense upwelling scenario, even robust productivity can be overwhelmed by the carbon influx from the deep ocean, resulting in a net CO_2 efflux. The manuscript should be commended for touching on this mechanism in the Intro and should ensure it is clearly reiterated in the Discussion when interpreting the late LIG CO_2 "paradox."

One point that would benefit from clarification is the contrast between Antarctic and subarctic Pacific regimes. The authors find that, unlike Antarctica, the subarctic North Pacific experienced a decline in productivity during the LIG cooling, remaining "tightly coupled" to Antarctic temperature changes. They attribute this to a "weakened Southern Ocean abyssal circulation" that suppressed nutrient upwelling in the subarctic Pacific, thereby limiting productivity and CO_2 release in the north. This explanation is intriguing but somewhat under-explained. The implication is that global deep-ocean circulation patterns shifted such that, even as surface wind-driven upwelling intensified around Antarctica, the supply of deep nutrients to the North Pacific was curtailed – perhaps due to reduced overturning or deep water formation changes. This raises a few questions that the authors should address:

• Mechanistic clarity: How exactly do the authors envision the chain of events linking SH westerly changes to North Pacific upwelling reduction? Is it that stronger upwelling in the Southern Ocean drew more deep water up there (and outgassed its CO_2) before it could travel to the North Pacific, effectively "robbing" the North Pacific of nutrient-rich deep water? Or was there a coincident change in deep ocean stratification or Atlantic Meridional Overturning Circulation that left the North Pacific more stratified? The phrase "weakened Southern Ocean abyssal circulation" is a bit abstract – it would help if the authors could rephrase or expand this to describe the process in more concrete terms (e.g., reduced formation of Antarctic Bottom Water during the LIG or a bottleneck in deep-water exchange between the Southern Ocean and North Pacific). Since this point is central to explaining the Antarctic vs. subarctic decoupling, a clearer discussion is warranted. Even a schematic in a figure might help illustrate the hypothesized circulation changes.

• Local vs. global factors in the subarctic: The authors might consider alternative or complementary explanations for the subarctic Pacific productivity decline. One possibility is increased upper-ocean stratification in the subarctic Pacific as global climate warmed into the LIG peak, reducing vertical nutrient mixing (warmer surface waters could strengthen the halocline, for instance). The text focuses on deep circulation, but could some of the subarctic decline simply be due to local oceanographic changes (like more stable water column or less winter mixing)? If there is literature or evidence on subarctic North Pacific stratification or nutrient changes in the LIG, referencing or discussing it would give a more balanced interpretation. The current interpretation is reasonable, but readers may wonder if the subarctic Pacific's behavior is entirely governed by Southern Ocean deep-water supply or also by its own regional climate conditions.

The climatic and carbon cycle implications of this interpretation are important and should be explicitly stated. The authors touch on it in the abstract and intro: if westerlies indeed kept CO_2 high during the LIG, this suggests a feedback wherein warming leads to stronger westerlies, which then lead to CO_2 outgassing, potentially amplifying warming further. This is a positive feedback mechanism in the Earth system. The fact that CO_2 stayed around 270–280 ppm while Antarctic temperature fell by ~6°C hints that other factors (perhaps declining Southern Hemisphere insolation or ice sheet regrowth) eventually outweighed the westerly-driven CO_2 feedback in the LIG. The authors may want to comment on why CO_2 did not continue rising indefinitely – presumably because the source of easily ventilated carbon was finite or because winds and ocean circulation reached a new equilibrium. A bit of discussion here would show the authors have considered the limits of the westerly wind effect in the natural system.

In terms of climate dynamics, I also have some comments to consider:

One aspect that could be emphasized is the parallel between the LIG scenario and the modern/future Southern Annular Mode (SAM) dynamics. The intensified, poleward-shifted westerlies the authors invoke are essentially a prolonged positive phase of the SAM (or a similar mode) under interglacial warmth. In modern observations and model simulations, a positive

SAM (strengthened poleward westerlies) leads to enhanced upwelling of deep waters around Antarctica and reduced stratification, sometimes causing outgassing of CO_2 from the Southern Ocean. The authors allude to future implications (with SHW expected to intensify under global warming), which is excellent. They could bolster this by explicitly noting that their paleo-findings are consistent with SAM-like behavior on long timescales. This not only places the result in a familiar framework for climate community but also underscores that such ocean-atmosphere coupling is already observed on shorter timescales today. If not already mentioned, integrating a brief mention of SAM or analogous wind-pattern changes would strengthen the connection to known climate modes.

Also, the idea of ocean-atmosphere feedbacks could be expanded. The authors have essentially identified a feedback loop: warmer climate (and/or changing insolation) → poleward-intensified westerlies → increased Southern Ocean upwelling → CO_2 release → further warming globally. It would be valuable to articulate this clearly. It's also worth discussing the time lag or sequence of events: Did the westerlies intensify because of the initial LIG warmth (e.g., due to a reduced equator-to-pole temperature gradient), thus later sustaining CO_2 levels even as Antarctic temperatures started to drop? If so, we are observing a hysteresis or time-lag effect where wind-driven CO_2 feedbacks prolong the warmth. Any insight the authors can provide (or speculate on, based on timing in their data) about the sequence – e.g., did Antarctic EP start rising before or just as CO_2 started rising at Termination II, and did subarctic EP falter only after the Antarctic winds revved up? – would be intriguing context.

The manuscript touches on large-scale modes indirectly by discussing "abyssal circulation" and upwelling differences. To further integrate with global-scale ocean circulation, the authors might consider the role of the meridional overturning circulation (MOC) in their scenario. A subtle point: if Southern Ocean deep upwelling was enhanced, was there any compensating change in North Atlantic Deep Water production or global MOC strength? Some studies of past warm periods (and future projections) suggest that freshening or warming can alter North Atlantic circulation, which in turn might influence the volume of deep water reaching the Southern Ocean and eventually the North Pacific. While the focus here is rightly on Southern Ocean winds, acknowledging these broader connections would show comprehensive thinking. For instance, if the authors suspect a slowdown in some component of the MOC (hence "weakened abyssal circulation"), they could tie it to known possibilities like reduced Antarctic Bottom Water formation in a warmer climate. If any PMIP Last Interglacial model outputs or previous studies (e.g., on Atlantic vs. Pacific ventilation) align with their findings, they should be referenced to give the reader confidence that the proposed dynamics are realistic.

Reviewer #3

(Remarks to the Author)
This is a very interesting study that produced a stacked record of Antarctic Southern Ocean sediment proxy records of export productivity and compared this productivity stack with Antarctic temperature, atmospheric CO2, and Subarctic North Pacific export productivity variations over the last 160 kyr, with a specific interest in the Last Interglacial period. The study examines temporal variations in the relationships between these climatic parameters through comparison of time series and through direct correlation analysis and highlights that the positive linear correlation between atmospheric CO2, Antarctic temperature, and Antarctic and Subarctic productivity seem to break down during the late Last Interglacial. Based on this analysis, the authors conclude that the Last Interglacial period may have been associated with poleward migration of the Southern Hemisphere westerlies, which enhanced nutrient upwelling, productivity, and deep carbon release in the Antarctic Southern Ocean. In contrast, the authors suggest that the North Pacific experienced reduced abyssal upwelling, which caused a reduction in subarctic productivity and a decoupling between subarctic productivity and atmospheric CO2.

This work tackles the dynamic mechanism behind global carbon cycling in warmer-than-modern climate conditions, which is an interesting problem on its own and very relevant for accurate predictions of future climate behavior associated with anthropogenic warming. I appreciate the analytical approach of this study and commend the authors for the very good data accessibility provided in their Materials and Methods and supplemental tables. Although this is not the first study to support poleward migration of the westerlies during warmer-than-modern paleo-intervals, this is the first one to my knowledge that directly connects this mechanism to disparate changes in Antarctic and subantarctic export productivity across the Last Interglacial. However, I do have a few suggestions, including additional analysis of the data, that I believe would strengthen the impact of this work. Therefore, I recommend that the authors be given the opportunity to address the following comments before finalizing a publication decision for Nature Communications.

General Comments:

(1) Broadly, the comparison between Antarctic export productivity records (synthesized into a stack of 15 different proxy time series from 12 different sediment cores) with the subarctic North Pacific export productivity records (indicated by two barium-based proxies from a single sediment core at Site 882) seems unbalanced. How well does export productivity at Site 882 reflect the entire subarctic North Pacific? The Antarctic records exhibit some degree of spatial variability (for example, ODP 1094 looks very different from U1537), would be expect the same in the North Pacific? Even if there are not additional North Pacific productivity records covering this interval (which I am not sure about), this manuscript would benefit from a discussion of how representative Site 882 is of the region based on more recent intervals, where additional productivity data is more likely to be available.

(2) The argument that the dynamics observed during the Last Interglacial period represent a unique climate regime would be more compelling if the study also included analysis of how the Antarctic temperature, atmospheric CO2, Antarctic export productivity, and subarctic export productivity records compared across the Holocene/current interglacial period. The Antarctic productivity stack that this study has generated is well suited for this analysis because it covers the last 160 kyr

completely. It seems the Site 882 productivity record does not span the Holocene, so perhaps another North Pacific productivity record could be used here for the comparison?

(3) I appreciate the inclusion of the correlation plots in Figure 4 as a way to examine the data beyond the time series comparisons illustrated in Figures 2 and 3, with respect to differences between records during MIS6, T2 and the LIG. Panel 4d is especially helpful for illustrating the decoupling between Antarctic temperature and atmospheric CO2 during the LIG and Panels 4e and 4f very clearly suggest that atmospheric CO2 concentrations seem to be much more driven by Antarctic Zone productivity than by North Pacific productivity.

However, the circled LIG data in panels 4a and 4b look to me more like a regime shift than a complete decoupling between Antarctic temperature and Antarctic productivity or between Site 882 productivity and Antarctic productivity (e.g., the data still look to be linearly correlated, though perhaps with a different slope and intercept). What correlation coefficients do you get when you examine just the LIG data, rather than the full 150-113 ka record? Are the LIG-labelled data points that fall closest to the 150-127 ka trendline the ones closest in age to T2, or are they from the middle of the LIG? It would also be helpful to see how data from MIS 3, 4, and 2 would plot on these graphs (at least in the supplement if it makes the main figure too crowded), to see how representative the MIS 6 and T2 relationships are of the last glacial cycle as well.

Specific Comments:

Line 34: Suggest to rephrase to "…abrupt decline in the Antarctic air temperature stack (ATS)…" so unfamiliar readers will understand where the S is coming from in the acronym.
Lines 37-40: I think the conclusion of this sentence would be easier to follow if an additional step in the logic were explained (for example, please specify here why a tight coupling between subarctic Pacific export productivity and Antarctic temperature indicates suppressed abyssal upwelling).
Line 91: Suggest to clarify here, rather than waiting until the methods section, that the 15 export productivity records are from 12 sediment cores (especially as there are only 12 orange dots in the map figure).
Lines 139- 141: Suggest to include the correlation coefficients for the full record here between the Antarctic proxy stack, the ATS and the subarctic records for consistency in comparison across time periods.
Lines 164-168: Suggest to rephrase to "in contrast, iron fertilization does not appear to have been impactful in the Antarctic Zone, which exhibits lower export productivity during glacial periods" to clarify this point.
Lines 176-178: It is my understanding that Ref 32 does not assert that temperature is the primary control on export productivity in the Antarctic zone, rather they suggest that low glacial productivity in this region may be caused by reduced wind-driven upwelling associated with a northward migration of the Southern Ocean fronts. Consequently, I suggest not using this reference to justify the first part of this sentence.
Lines 247-249: Suggest to continue to specify that "temperatures" refers to "Antarctic temperatures" here, to avoid confusion associated with asynchronous millennial-scale warming between Greenland and Antarctic temperature records (e.g., Blunier et al., 1998).
Line 560 (Figure 3 caption): More detail is needed here regarding the upwelling index denoted (e.g., suggest to specify that it's the subarctic upwelling index based on U1343 and Site 1012). Since the paper typically indicates what geochemical proxies are used to infer each parameter for productivity, ACC strength, etc…, I suggest to include the proxy basis of the upwelling index here as well (or perhaps where this is discussed in the main text).

Figure 5: I found this figure very helpful for visualizing the different suggested circulation scenarios, but I can't find a reference to this figure anywhere in the main text.

Lines 599-601: Please explain here why you included these two cores into the productivity stack, although they do not span the LIG range targeted by this study.
Line 678: I assume records were normalized by dividing each by its mean. Could you please specify the exact approach used so unfamiliar readers can better follow along?
Lines 686-691: After looking through Figures S7 and S8, I wonder if this inverse dispersion weighting is artificially removing legitimate high frequency signal from the stacked record (leading to an over-smoothing). There are many Antarctic sediment cores in which sedimentation rates vary largely between productive climates and low-productivity climates (ODP 1094 is a good example), such that the ability of each record to capture millennial-scale changes varies temporally. There are also clear difference between sediment cores in their ability to capture high frequency variations in export productivity (for example, TN057-14PC4 captures much more resolution than PS75-072-4 during MIS3), does that mean TN057-14PC4 receives a lower weighting than PS75-072-4? Should it? Would the stacked export productivity records look very different if you weighted each record by sedimentation rate across a given interval, rather than inverse dispersion?

Also please note that, in general, Marine Isotope Stage labels are used throughout the figures, but do not appear to be mentioned or explained in the main text.

References Mentioned:
Blunier, T., Chappellaz, J., Schwander, J. et al. Asynchrony of Antarctic and Greenland climate change during the last glacial period. Nature 394, 739–743 (1998). https://doi.org/10.1038/29447

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
I have reviewed the authors' responses and the revisions in the manuscript and believe that the authors have made a great effort to respond to all of the reviewers' comments. Also, I find both the changes to the text and the new figures to be very appropriate. There is only one point that requires a small, but relevant, clarification. Fragilariopsis kerguelensis is not generally considered an indicator of high productivity (as indicated in the response to the reviewers). Other diatom species tend to dominate in highly productive environments such as coastal and shelf systems of the Antarctic continent. This does not mean that changes in the abundance of this species cannot be related to an increase in productivity. According to some studies (e.g. Nair et al., 2020 and other references in the text) increases in the flux of F. kerguelensis (the main vector of silica to the ocean floor) appear to be associated with greater nutrient input. Therefore, my only suggestion would be to clarify this point better in the text. The authors could indicate something like "changes in the abundance of F. kerguelensis can be taken as an indicator of productivity changes in the pelagic Southern Ocean" or something along those lines. Please also specifically mention in the text if authors are referring to absolute abundance and/or relative abundance of F. kerguelensis in the manuscript.
I have no further suggestions from my side and would like to congratulate the authors on this very interesting work.

Reference (please note that references are just a suggestion, there is no need to include more references if authors do not considered relevant)
Nair, A., Ghadi, P., Mohan, R., Manoj, M. C., Crosta, X., Shukla, S. K., and Thamban, M.: Glacial-interglacial flux and size variability of Fragilariopsis kerguelensis and Thalassiosira lentiginosa from the Indian sector of the Southern Ocean, Deep Sea Research Part II: Topical Studies in Oceanography, 178, 104746, https://doi.org/10.1016/j.dsr2.2020.104746, 2020.

Reviewer #2

(Remarks to the Author)
Thanks authors for hard working on revising the manuscript. The authors’ revisions address all concerns. I recommend acceptance in Nature Communications.

Reviewer #3

(Remarks to the Author)
I am satisfied with the changes made in the revised manuscript and applaud the authors for their thorough incorporation of reviewer feedback and discussion. I believe these changes have made the manuscript stronger and, as such, I recommend this manuscript for publication.

Point-by-point response to reviewers' comments

Reviewer #1 (Comments to Author (shown to authors):

Dr Lijuan Lu and colleagues reconstruct changes in export productivity in the Southern Ocean and Arctic Pacific covering two glacial-interglacial cycles (last 160 Ka) using 15 export productivity records from 12 marine sediment cores from the Antarctic Zone of the Sothern Ocean and comparing them with subarctic Pacific EP. Authors argue that both Antarctic and Arctic export productivity followed similar patterns during most of the record expect for the Last Interglacial (128-113 kyrs). In particular, during the LIG, Antarctic export production and atmospheric CO_2 was high and coupled with a decline in Antarctic air temperatures of ca. 6ºC, while Subarctic Pacific export productivity dropped. Authors argued that this decoupling was most likely driven by an intensification of the southern hemisphere Westerlies that stimulated upwelling of Circumpolar Deep Water that enhanced export productivity and the release of CO_2 to the atmosphere.

Overall, the manuscript is well-written and structured, and the figures are of high quality. Regarding the statistical treatment, some additional information is required while other analysis are beyond my expertise and therefore, I am unable to judge their validity. In regard to the scientific interest, the polar oceans play an essential role in the control of global atmospheric CO_2, and model predictions point to a poleward intensification of westerlies in the coming decades. Therefore, new insights into the role of the Southern Ocean and Arctic Ocean during a period with analogies to our imminent future could be of great interest for a broad scientific audience.

Having said that, there are several important points that authors would have to deal with to prove their hypothesis and therefore before the manuscript can be considered suitable for publication in Nature Communications. Please find a list of the principal drawbacks of this paper listed below:

Response:

Thank you very much for your thorough and constructive review. We appreciate your positive comments on the writing and structure of our manuscript, as well as your recognition of the broader scientific interest of our study in the context of future climate change. In response to your comments, we have provided additional details on our statistical methods, including the use of Spearman's or Pearson's correlation based on data distribution, and a more thorough explanation of the inverse standard deviation weighting approach. We have also revised key sections of the manuscript and provided stronger evidence supporting our hypothesis. For example, we have added a discussion of changes in diatom species composition that further support biogenic silica accumulation as a reliable proxy for export productivity. A detailed point-by-point response is provided below.

Main concern:

1. Biogenic silica accumulation as a proxy for export productivity: The interpretation of biogenic silica accumulation as a direct proxy for export productivity has been questioned by multiple authors. As indicated by De la Rocha et al. (2010): "Although there is a strong link between BSi production and BSi accumulation, variability and shifts in the ratios of BSi to POC during particle production, export, and preservation complicate the extrapolation of primary production from BSi accumulation (Pondaven et al., 2000; Ragueneau et al., 2000; DeMaster 2002; Nelson et al., 2002; Ragueneau et al., 2002; Moriceau et al., 2007)." And also in De la Rocha et al. (2010): "…the lower glacial accumulation rates of BSi south of the APF in the Atlantic sector of the Southern Ocean can be entirely accounted for by shifts in diatom species composition and Fe availability and do not need to reflect lower glacial levels of primary production. Likewise, the higher glacial accumulation rates of BSi north of the APF may likely also not reflect higher glacial levels primary production". Moreover, Assmy et al. (2013) also indicated that "It follows that high sedimentary silica accumulation rates are a proxy for iron-limited diatom assemblages rather than for high surface productivity, contrary to the prevailing view". Therefore, in order to assess changes in export production would be important to discuss changes in diatom species composition (the main components of the Biogenic silica) rather than discussing the Biogenic Silica alone. Is diatom assemblage composition for those records available? Do diatom assemblages change? And if so, does the change in the diatom assemblage support the interpretations of the results? Although the present manuscript is not focused on the diatom assemblage composition, some information in this regard could greatly help to support (or discard) the findings of the authors.

Response:

The reviewer raised a crucial issue regarding potential limitations of biogenic silica (BSi) accumulation as a proxy for export productivity, noting it can be significantly modulated by diatom species composition, iron availability, and preservation dynamics. We agree completely with the reviewer that BSi accumulation is not a direct, unambiguous proxy and thank them for highlighting this critical issue. To address this, we have taken a multi-pronged approach to ensure the robustness of our export productivity reconstructions:

1) Explicit acknowledgement of limitations: We have revised the main text (lines 106-110) to explicitly state the potential complications with the BSi proxy, citing key literature:

"The content and fluxes of biogenic opal have long been used as proxies for past export productivity, particularly in the diatom-rich Antarctic Zone of the Southern Ocean (Anderson et al., 2009). However, biogenic opal accumulation can be influenced by various factors such as diatom species composition and iron availability (e.g., De la Rocha et al., 2010; Assmy et al., 2013), in addition to particle aggregation, differential dissolution, and post-depositional preservation effects (Pondaven et al., 2000; Ragueneau et al., 2000; DeMaster, 2002; Nelson et al., 2002; Ragueneau et al., 2002; Moriceau et al., 2007)."

2) Multi-proxy approach: To mitigate the biases of any single proxy, we developed independent stacks for biogenic opal (BSi content and flux) and biogenic barium ((flux, concentration and elemental ratios (Ba/Ti, Ba/Al, and Ba/Fe)). A combined stack integrating both proxies provides a more comprehensive and robust qualitative framework. The strong agreement (Spearman's correlations of $\rho = 0.94$- 0.98) between these independent stacks via different methods (inverse weighting & principal component analysis (PCA)) strongly indicates they capture the true underlying export productivity signals, not proxy-specific artifacts. All stacks exhibit synchronous glacial-interglacial patterns, broadly consistent with previously published biogenic opal and barium records from the Antarctic Zone of the Southern Ocean, further supporting their reliability. Please see Fig.S2-S8 and lines 110-130 in Main Text.

3) Validation with diatom assemblages data: As suggested, we incorporated independent paleoecological data. The abundance of *Fragilariopsis kerguelensis*, a dominant species in the permanent open-ocean zone of our study region and a key indicator of high productivity, closely tracks our reconstructed stacks (Fig. r1). This provides direct biological validation that our stacks reliably capture past changes in BSi-based export productivity. Please see lines 130-134 and Fig. S9 in Main Text.

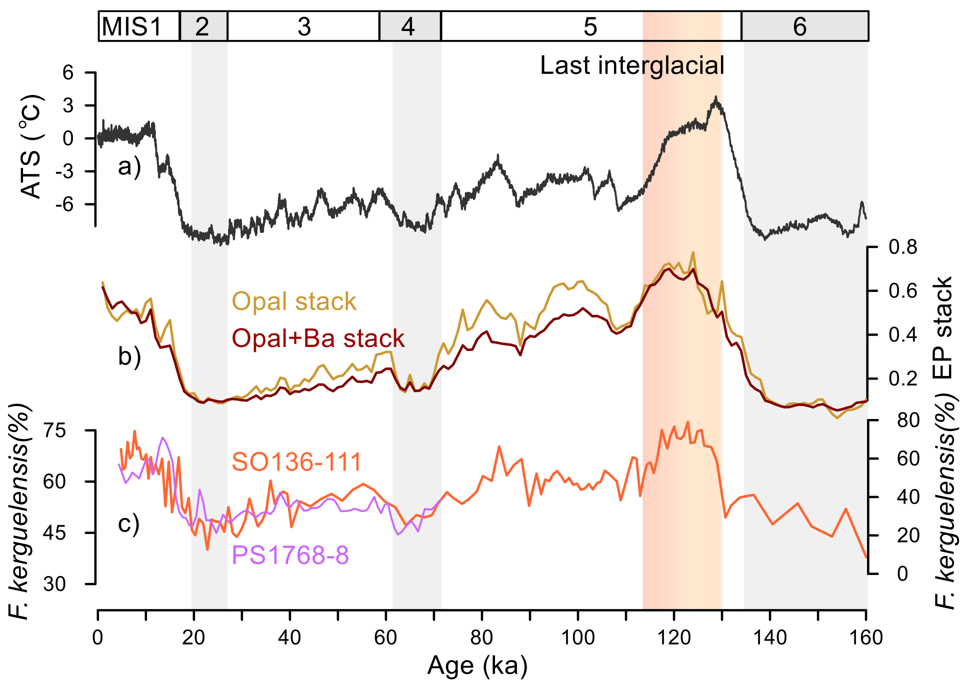

Fig. r1 (new Fig. S9 of the revised manuscript) | Comparisons of biogenic opal and (opal+Ba) stack with the *Fragilariopsis kerguelensis* abundance records (Crosta et al., 2004; Abelmann et al., 2006), a dominant diatom species in the permanent open ocean zone.

> 2. Temperature as main control of export productivity:
>
> Line 176 "Assuming that temperature is the primary control on export productivity in the Antarctic Zone". The main control of algal biomass accumulation and export productivity in the modern Antarctic Zone is iron availability (Boyd et al., 2000). While temperature is most likely an important factor controlling the distribution of phytoplankton functional groups and therefore efficiency of the biological pump, the primary role of temperature in the control of export production is not clear at all. This point has to be explained in detail in the manuscript. Please clarify.

Response:

This is an excellent point that allowed us to clarify our hypothesis. We agree that on modern, seasonal timescales, iron limitation is paramount (Boyd et al., 2000). Our study focuses on glacial-interglacial timescales, where the mechanisms we explore involve temperature's indirect role in modulating the physical supply of nutrients (including iron) via its control on buoyancy and upwelling (e.g., Watson et al., 2015). We have revised the text to clarify this distinction: the long-term correlation suggests temperature influences upwelling intensity and nutrient supply, but the MIS 5 decoupling shows that wind forcing can override this temperature-driven mechanism. The persistence of high productivity during Antarctic cooling is precisely the evidence that another driver (Southern Hemisphere Westerlies) played an important role during this interval:

"Previous studies suggested that Antarctic air temperature plays an important role by affecting Southern Ocean surface buoyancy forcing over glacial-interglacial cycles (e.g., Watson et al., 2015), which in turn influences upwelling intensity and nutrient supply. Hence, one would expect export productivity and Antarctic air temperature to exhibit a close relationship across the entire glacial-interglacial cycles. We then tested this hypothesis using linear regression across the entire record, which yields a moderate fit (R^2=0.76, n=149). Excluding MIS 5, however, results in a much stronger fit (R^2=0.90, n=92; Fig. 3a), revealing that MIS 5 is an interval during which the relationship deviates from the long-term mean relationship. More importantly, during the late Last Interglacial, Antarctic export productivity remained elevated despite an abrupt 6 °C drop in Antarctic air temperature (Figs. 4b and 4d), suggesting that Antarctic air temperature cannot fully account for the variability in Antarctic export productivity."

Please see lines 202-214 in Main Text.

3. Export productivity in the modern Southern Ocean.

Lines 170-171 "Currently, the zone of maximum export productivity is located south of the Antarctic Polar Front (APF), within the Antarctic zone of the Southern Ocean". This is statement is true for part of the western Pacific sector of the Southern Ocean based on the data from the AESOPS experiment. However, it cannot be considered a circumpolar feature of the Southern Ocean. For example, POC fluxes to the deep ocean are very similar between the Antarctic Zone, Polar Frontal Zone and Subantarctic Zone in the Australian sector of the Southern Ocean: in this region annual POC fluxes are slightly higher in the SAZ than in the AZ at 2000 m and 3800 m (1.4 and 1 g C m-2 yr-1 at 2000 and 3800 m at the SAZ versus 1.2 and 0.9 g C m-2 yr-1 at 2000 and 3800 m at the AZ). See Wynn-Edwards et al. (2020) and Rigual-Hernández et al. (2015). Please also look for information in the Atlantic sector. Based on all of the above, authors should be more cautious with their statement and explain in detail the differences in EP across sectors of the Southern Ocean. Also, quantifying the difference in EP between circumpolar systems would help the reader to understand what is the current difference in export production between different zonal systems. While total mass flux is quite different across zonal systems mainly due to differences in opal export, the magnitude in POC export is small.

Response:

The reviewer noted that modern export productivity is not uniformly high south of the Polar Front and exhibits significant regional variability, suggesting caution against over-generalization. We thank the reviewer for this important correction. Below, we provide additional context and the revisions made in the manuscript:

1) Regional differences context: Observations and model-based estimates confirm that export productivity varies across sectors. Results from the Antarctic Environment and Southern Ocean Process Study (AESOPS; 1996-1998) in the Pacific sector showed that the highest annual fluxes of particulate organic carbon (POC; ~200 mmol m^{-2} yr^{-1}) and biogenic silica (~1400 mmol m^{-2} yr^{-1}) to the seafloor occurred in the southern Antarctic Circumpolar Current (61.5°S-65.5°S; Nelson et al., 2002). In contrast, in the Australian sector, long-term sediment trap data indicate that POC fluxes are comparable or even slightly higher in the Subantarctic Zone than in the Antarctic Zone (1.4 and 1.0 g C m^{-2} yr^{-1} vs 1.2 and 0.9 g C m^{-2} yr^{-1}; Rigual-Hernández et al., 2015; Wynn-Edwards et al., 2020). Additionally, model-based estimates suggest that a large fraction of POC export is concentrated in a circumpolar belt aligned with the ACC and in coastal upwelling zones off Peru, Chile, and Namibia (Schlitzer, 2002). While we agree with the reviewer that a quantitative comparison of export productivity across circumpolar systems would be highly informative, this remains difficult due to the uneven spatial and temporal coverage of observations and methodological differences between sediment traps, satellite algorithms, and models.

2) Revisions to manuscript text: While we provide detailed context on regional export productivity (AESOPS observations, other sectors, and model estimates), our study focuses on the Antarctic Zone of the Southern Ocean. Accordingly, we revised the sentence to avoid overgeneralization: the phrase "the zone of maximum export productivity is located south of the Polar Front" has been changed to "**the zone of high diatom productivity is largely located south of the Polar Front**," to more accurately reflect the regional variability in export production patterns. Please see lines 194-199 in Main Text.

> 4. Correlations: e.g. lines 211-212 and Fig. 4. It is not clear in the text if the variables in Fig. 4 are normally distributed. This information is important to determine which correlation coefficient would be more appropriate. Could authors explain in the text why Pearson correlation coefficient was used and not Spearman's test? Please clarify and revised if necessary.

Response:

We thank the reviewer for this precise statistical guidance. We have re-examined the data distributions and adjusted our analysis accordingly:

Southern Ocean stacks: Data are not normally distributed. We now use Spearman's correlation (ρ) to assess agreement between stacks and PC1, and between stacks and individual site records (see Figs. S3, S4, and S6 and the related captions, lines 579-580 in Main Text).

Subarctic Pacific stack: Data are approximately normally distributed. We retain Pearson's correlation for these comparisons (see Figs. S10 and S12 and the related captions; lines 632-636 in Main Text).

Figure 3: We clarify that this figure presents linear regression analyses to better illustrate relationships between variables, which is distinct from correlation coefficients.

5. Methods, lines 108-110: "inverse standard deviation weighting method employs the inverse of each variable's standard deviation as weights to minimize the influence of high-variability variables on the results". This methodology is beyond my expertise and therefore I am unable to evaluate if this is an adequate approach.

Response:

We have significantly expanded the description of this method in the Methods section (see lines 526-545 and 576-580 in Main Text) to enhance clarity:

1) Principle: The inverse standard deviation weighting approach is based on a well-established principle in quantitative data analysis, where variables are weighted inversely proportional to their variability (standard deviation). Such weighting approaches ensure more stable records contribute more strongly to the composite stack, thereby minimizing the influence of noisy records.

2) Data preparation: All records were resampled to 1 kyr resolution. Missing values were filled via linear interpolation. Each record was then normalized using Min-Max normalization, defined as $X_{norm} = (X_{sample} - X_{min})/(X_{max} - X_{min})$. This transformation rescales the data to the range [0, 1], allowing for direct comparison across datasets with different scales and units while reducing scale-related bias in weight calculations.

3) Weight calculation & stack generation: The standard deviation of each normalized record was calculated, and initial weights were set as $w_i = 1/\sigma_i$. The final stack is the weighted average of all records, with weights normalized to sum to 1.

4) Validation of the method: The results from this method show exceptionally strong agreement ($\rho > 0.94$) with the results from PCA, confirming the robustness of the approach and that it captures the dominant common signal.

To improve clarity for readers who are less familiar with this approach, we have added a more detailed explanation in the revised manuscript (see lines 526-545 and 576-580 in Main Text).

6. Figure 5. Please explain in the text the meaning of the circle with a dot in the schematic (position of the main winds I assume).

Response:

We have updated the caption for Figure 5 to explicitly state: "Southern Hemisphere westerlies (a circle with a dot in the center)".

Reviewer #2 (Comments to Author (shown to authors):

The manuscript by Lu et al. addresses a critical and timely question in paleoclimate: why Antarctic and subarctic Pacific EP became decoupled during the LIG despite generally co-varying with atmospheric CO_2 and temperature on glacial-interglacial timescales. The study's primary finding – that intensified, poleward-shifted SHW drove enhanced upwelling of nutrient- and CO_2-rich deep water in the Antarctic Zone during the LIG, sustaining high Antarctic EP and contributing to high atmospheric CO_2, while subarctic Pacific EP declined – is novel and significant. It challenges the traditional paradigm that Antarctic temperature was the dominant driver of Southern Ocean carbon release in warm periods. The authors provide an alternative mechanism whereby wind-driven ocean dynamics can override or modulate temperature-driven effects, which represents an important contribution to paleo-carbon cycle science and highlights the SHW as an underappreciated player in controlling past CO_2 variability.

Overall, this is an impressive and valuable manuscript and is squarely appropriate for Nature Communications, given its significant for paleoclimate and future climate understanding. I recommend publication after revision, addressing the concerns raised below. The most critical issues to resolve are clarifications of the proposed mechanism (especially regarding changes in deep ocean circulation and the contrasting behavior of the subarctic Pacific) and some enhancements to clarity in presentation. Thus, the revision can likely be done in a straightforward manner. I look forward to seeing a revised manuscript that incorporates these suggestions.

Response:

We sincerely thank the reviewer for their encouraging comments and for highlighting the importance of our study for understanding past and future climate dynamics. These suggestions have greatly strengthened the mechanistic narrative of our paper. We have revised the manuscript accordingly, adding additional analyses to illustrate the regional decoupling of Antarctic and subarctic Pacific productivity during MIS 5, and integrating these findings with global meridional overturning circulation to clarify the underlying mechanism. We have also clarified how Southern Annular Mode (SAM)-driven Southern Ocean upwelling sustained CO_2 levels despite Antarctic cooling during the late Last Interglacial, providing a mechanistic explanation for the "CO_2 paradox." Additionally, we have revised the manuscript's structure, improved figure presentation, and carefully edited the text to enhance readability and precision. Below, we provide our responses to the reviewer's comments in detail.

Some key concerns:

1. The authors demonstrate high coherence among proxy types (opal vs. barium) and perform cross-correlations that show Antarctic EP, subarctic Pacific EP, and Antarctic temperature tracked each other closely for most of the past 160 kyr. For the critical LIG interval, the authors identify a breakdown of these correlations (the decoupling). However, I think this is not clearly shown qualitatively in figures, and I recommend quantifying the decoupling more explicitly. For example, the authors could report correlation coefficients between Antarctic and subarctic EP over specific intervals (glacial vs. interglacial vs. LIG) to statistically demonstrate that the LIG correlation is significantly lower. A running or windowed correlation analysis might also highlight when the divergence emerged.

Response:

To better quantify the decoupling between Antarctic and subarctic Pacific export productivity (EP), we have conducted a comprehensive statistical analysis. We conducted linear regressions for key variable pairs over the entire 160-kyr record and then excluding MIS 5:

1) Antarctic vs. Subarctic EP: The fit is moderate for the full record ($R^2 = 0.58$, n=149) but becomes very strong when MIS 5 is excluded ($R^2 = 0.90$, n=92) (Fig. r2b). This quantitatively demonstrates that MIS 5 is a clear outlier from the long-term relationship.

2) Other pairs: We found similar decoupling for Antarctic EP vs. Antarctic temperature (R^2: 0.76 full record vs. 0.90 excl. MIS 5), Subarctic EP vs. CO_2 (R^2: 0.66 full record vs. 0.88 excl. MIS 5), and Antarctic temperature vs. CO_2 (R^2: 0.82 full record vs. 0.92 excl. MIS 5) (Figs. r2a, r2d, and r2f). Conversely, the fit of Antarctic EP vs. CO_2 remains strong throughout (R^2=0.92) (Fig. r2e).

3) Timing: The point of maximum divergence occurs during the late LIG (~115-113 ka), coinciding with the pronounced Antarctic cooling. These new analyses are presented in Fig.3 and discussed throughout the results and discussion (lines 169-175, 207-210, 365-374, 395-402 in the Main Text).

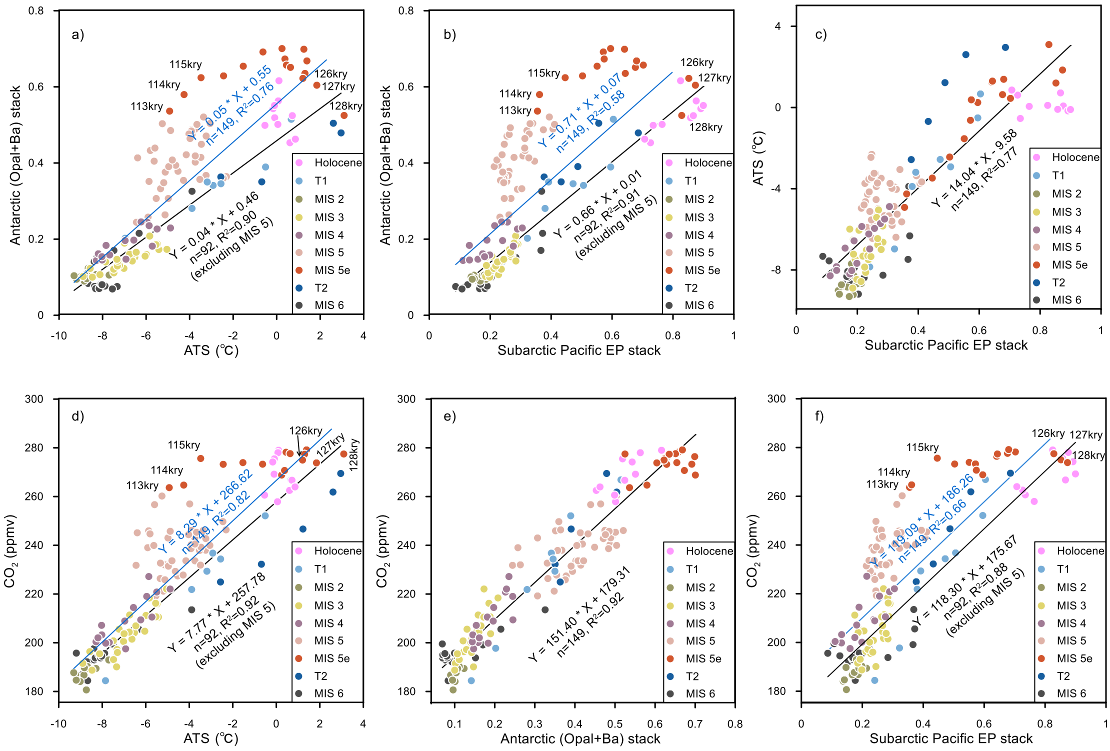

Fig. r2 (new Fig. 3 of the revised manuscript) | a) and **b)** illustrate cross plot of the Antarctic (biogenic opal+ biogenic barium) stack versus Antarctic temperature stack (ATS) and the subarctic Pacific export productivity stack, respectively; **c)** Cross plot of ATS versus subarctic Pacific export productivity stack; **d), e),** and **f)** are cross plots of atmospheric CO_2 concentrations versus ATS, Antarctic (biogenic opal+ biogenic barium) stack and subarctic Pacific export productivity stack.

> 2. Another methodological aspect that could use more explanation is how the subarctic Pacific export productivity record was derived or selected. The text references subarctic Pacific EP data with citations, but readers may benefit from a clearer description: e.g., is this a composite record from a particular site or region? Detailing the source and nature of the subarctic EP reference would help the audience understand its resolution and uncertainties relative to the Antarctic data. Additionally, if the subarctic record's age model was also aligned to Antarctic chronology (perhaps via atmospheric CH_4 or $\delta^{18}O$ correlations at Termination II), that should be stated for transparency.

Response:

In the revised manuscript, we have expanded the Methods section (lines 587-638) and Table S2 to provide full details of subarctic Pacific export productivity:

1) Data sources: The subarctic Pacific export productivity signal we use is derived from a regional composite data set based on five independent export productivity reconstructions (biogenic barium concentrations and $^{231}Pa/^{230}Th$) from ten sediment cores spanning the subarctic Pacific (Table r1). These cores include ODP 882, MR98-05-3PC, JPC17, ODP887, and a group of six closely spaced cores from the Juan de Fuca Ridge (JdFR) area: AT26-19-05PC, AT26-19-09PC/06MC, AT26-19-12PC-TC/10MC, AT26-19-35PC, AT26-19-38PC, and AT26-19-39BB. Spatially, these cores are distributed between 44.96°N and 54.22°N, and from 164.98°E to 49.39°W, spanning a range of hydrographic settings across the subarctic Pacific, including both the western and eastern subarctic sectors and marginal seas such as the Bering Sea, with water depths ranging from 2209 to 5507 m. Given the geographic proximity of the JdFR cores, which exhibit similar productivity signals and trends, we adopted an existing regional productivity stack derived from these six sites, which provides a continuous record over the past 160 kyr and serves as a representative index for this sector. This approach ensures better temporal continuity and reduces redundancy in overlapping signals.

2) Methodology: We applied the same two stacking methods (inverse weighting & PCA) used for the Antarctic, ensuring consistency for interhemispheric comparison.

3) Age model: For some sites, detailed age-model data are not publicly available, limiting the possibility of further improving the chronology. Therefore, we used the original published age models largely based on radiocarbon dating, magnetostratigraphy, biostratigraphy, astronomical tuning, or $\delta^{18}O$ tie points, to maintain consistency and avoid introducing additional uncertainties in the stack construction (Table r1).

4) Representativeness: The strong agreement ($R = 0.97$) between the two independent stacking methods and high correlations ($R > 0.7$) with previously published records from the region (Fig. S10-S13) confirm the stack's robustness and regional representativeness.

5) High-resolution record: We incorporated the high-resolution, astronomically tuned Ba/Al record from ODP Site 882 as a complementary dataset to better resolve millennial-scale variability (Jaccard et al., 2005).

These details have now been clarified in the revised manuscript (see Figs. S10-S13, Table S2, lines 136-157 and 587-638).

Table r1 (new Table S2 in the revised manuscript). Details for sediment cores for reconstructing the subarctic Pacific export productivity

Number	Sites	Depth (m)	Latitude	Longitude	Proxy	Age scale (ka)	Chronology	Reference
1	ODP882	3244	50.33	167.58	Biogenic Ba	149.7-8.6	Magnetostratigraphy; tuning of GRAPE density (precession band) to 65°N summer insolation; inter-site alignment of benthic $\delta^{18}O$ (Sites 882 & 883) within $CaCO_3$-bearing intervals, corroborated by magnetic susceptibility; fine-tuning of BioBa record (Site 882) to Vostok δD	Jaccard et al., 2005
2	ODP887	3647	54.22	-148.27	Biogenic Ba	161-1	AMS ^{14}C; survey core-887B stratigraphic and geochemical correlation; $\delta^{18}O$ correlation to SPECMAP	McDonald et al.,1999
3	MR98-05-3PC	5507	50.00	164.98	Biogenic Ba	142-3.5	AMS^{14}C; Normalized remanence intensity vs paleointensity curve; Opal content correlation to nearby core RAMA44PC	Fukuma and Harada, 2001; Shigemitsu et al., 2007
4	JPC17	2209	53.93	178.90	Biogenic Ba	118.8-0.6	AMS ^{14}C; Planktonic and benthic $\delta^{18}O$	Brunelle et al., 2007
5	Juan de Fuca Ridge (JdFR) stack	2794	44.96	-130.65	$^{231}Pa/^{230}Th$	160-0	AMS ^{14}C; benthic $\delta^{18}O$; stratigraphically tuned density cycles	Costa et al., 2016, 2018

Note that the Juan de Fuca Ridge (JdFR) $^{231}Pa/^{230}Th$ stack is a regional average of Sites AT26-19-05PC, 09PC/06MC, 12PC-TC/10MC, 35PC, 38PC, and 39BB, as the glacial-interglacial trends in these six cores are sufficiently similar to be combined into a single stack (Costa et al., 2018).

3. The authors argue that in the LIG, upwelling supply outpaced biological uptake, leading to CO_2 release despite high productivity. This is an important nuance – usually high productivity draws down CO_2, but only if nutrient utilization is nearly complete. In an intense upwelling scenario, even robust productivity can be overwhelmed by the carbon influx from the deep ocean, resulting in a net CO_2 efflux. The manuscript should be commended for touching on this mechanism in the Intro and should ensure it is clearly reiterated in the Discussion when interpreting the late LIG CO_2 "paradox."

Response:

We have expanded the discussion (lines 376-393) to clarify this critical mechanism. The paradox is resolved by considering the balance between physical supply and biological consumption:

"*Notably, during warm periods, high export productivity in the Antarctic zone of the Southern Ocean always coincided with elevated atmospheric CO_2, even during the late Last Interglacial, when Antarctic temperatures dropped by more than 6 °C (Figs. 2a, 2b, and 2d). This apparent paradox of elevated export productivity coinciding with increased atmospheric CO_2 can be explained by an imbalance between upwelling supply and biological nutrient utilization, as supported by low $\delta^{15}N_{db}$ records indicating high surface nitrate supply during interglacial periods. Vigorous upwelling may have continuously transported carbon- and nutrient-rich deep waters to the surface, exceeding the capacity of nutrients and CO_2 assimilation through photosynthesis. As a result, nutrient consumption was incomplete, constraining net biological CO_2 fixation and allowing a portion of the upwelled carbon to escape to the atmosphere, a phenomenon analogous to that observed in contemporary high-nutrient, low-chlorophyll (HNLC) regions of the Southern Ocean. Thus, an HNLC-style imbalance between supply and utilization likely persisted during the Last Interglacial, maintaining elevated atmospheric CO_2 even under high export productivity. This interpretation is further supported by the co-evolution of export productivity with increased nitrate supply in the Antarctic zone of Southern Ocean and rising CO_2 (Figs. 4d and 4f).*

4. One point that would benefit from clarification is the contrast between Antarctic and subarctic Pacific regimes. The authors find that, unlike Antarctica, the subarctic North Pacific experienced a decline in productivity during the LIG cooling, remaining “tightly coupled” to Antarctic temperature changes. They attribute this to a “weakened Southern Ocean abyssal circulation” that suppressed nutrient upwelling in the subarctic Pacific, thereby limiting productivity and CO_2 release in the north. This explanation is intriguing but somewhat under-explained. The implication is that global deep-ocean circulation patterns shifted such that, even as surface wind-driven upwelling intensified around Antarctica, the supply of deep nutrients to the North Pacific was curtailed – perhaps due to reduced overturning or deep water formation changes. This raises a few questions that the authors should address:

• Mechanistic clarity: How exactly do the authors envision the chain of events linking SH westerly changes to North Pacific upwelling reduction? Is it that stronger upwelling in the Southern Ocean drew more deep water up there (and outgassed its CO_2) before it could travel to the North Pacific, effectively “robbing” the North Pacific of nutrient-rich deep water? Or was there a coincident change in deep ocean stratification or Atlantic Meridional Overturning Circulation that left the North Pacific more stratified? The phrase “weakened Southern Ocean abyssal circulation” is a bit abstract – it would help if the authors could rephrase or expand this to describe the process in more concrete terms (e.g., reduced formation of Antarctic Bottom Water during the LIG or a bottleneck in deep-water exchange between the Southern Ocean and North Pacific). Since this point is central to explaining the Antarctic vs. subarctic decoupling, a clearer discussion is warranted. Even a schematic in a figure might help illustrate the hypothesized circulation changes.

• Local vs. global factors in the subarctic: The authors might consider alternative or complementary explanations for the subarctic Pacific productivity decline. One possibility is increased upper-ocean stratification in the subarctic Pacific as global climate warmed into the LIG peak, reducing vertical nutrient mixing (warmer surface waters could strengthen the halocline, for instance). The text focuses on deep circulation, but could some of the subarctic decline simply be due to local oceanographic changes (like more stable water column or less winter mixing)? If there is literature or evidence on subarctic North Pacific stratification or nutrient changes in the LIG, referencing or discussing it would give a more balanced interpretation. The current interpretation is reasonable, but readers may wonder if the subarctic Pacific’s behavior is entirely governed by Southern Ocean deep-water supply or also by its own regional climate conditions.

Response:

We greatly appreciate the reviewer's insightful comments on the contrasting productivity responses between the Antarctic and subarctic Pacific during the Last Interglacial, which have helped us clarify the mechanistic links between Southern Hemisphere westerlies and nutrient supply in the subarctic Pacific, as well as consider the potential influence of local factors:

1) Mechanistic linking Southern Hemisphere westerlies to subarctic Pacific nutrient supply. Modern ocean circulation indicates that no new deep water is formed in the subarctic Pacific; nutrients there are largely supplied by lateral advection of nutrient- and CO_2-rich abyssal waters, which are composed of Antarctic Bottom Water (AABW) and recirculated North Atlantic Deep Water (NADW) incorporated into Lower Circumpolar Deep Water (LCDW), forming Pacific Deep Water (Talley, 2013). During the late Last Interglacial, intensified Southern Hemisphere westerlies and a southward shift of ACC enhanced CDW upwelling, increasing Southern Ocean productivity and CO_2 outgassing. This occurred alongside reduced AABW export and restricted LCDW transport to the subarctic Pacific, which corresponds to weaker deep-ocean ventilation that limited nutrient supply, reducing surface productivity and CO_2 exchange. A schematic illustrating the hypothesized circulation changes was added in Fig. r3. Please see Fig.5 and lines 294-337 and 395-412 in Main Text.

2) Consideration of local factors on subarctic Pacific processes: Local processes such as enhanced upper-ocean stratification due to alterations in sea ice extent may have further influenced nutrient upwelling and productivity. Our compilation of five export productivity records from subarctic Pacific sites covers diverse hydrographic settings (44.96° N- 54.22° N, 164.98° E -49.39° W; Table r1). This dataset includes pelagic sites, such as ODP Site 882, which were largely free of sea ice even during glacial periods, indicating that sea-ice cover was not a major limitation. Moreover, the co-variation between subarctic Pacific productivity (Ba/Al at ODP Site 882) and Antarctic temperature across interglacial-glacial and millennial timescales indicates that Southern Hemisphere climate was likely the dominant influence on subarctic Pacific productivity, given that the subarctic Pacific is unlikely to have been the primary driver of the climatic shifts documented in Antarctic ice cores. Please see lines 279-292 in Main Text for more details.

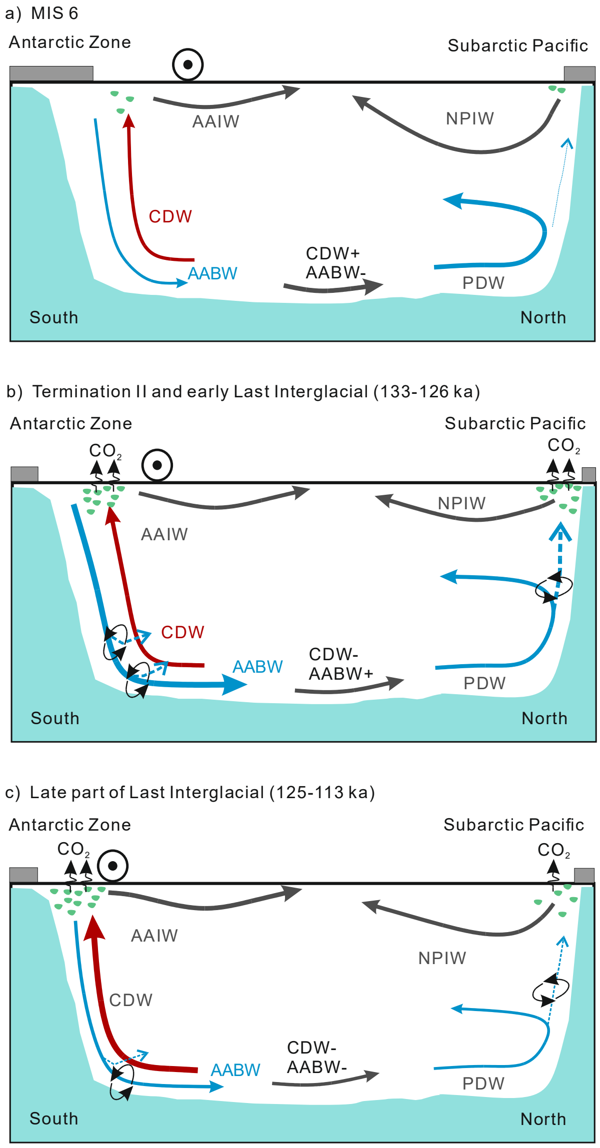

Fig. r3 (new Fig. 5 of the revised manuscript) | Schematic of the polar export productivity and the associated ocean circulation during MIS 6 (a), Termination II and early Last Interglacial (b), and late Part of the Last Interglacial (c). AAIW= Antarctic Intermediate Water, CDW= Circumpolar Deep Water, AABW= Antarctic Bottom Water, NADW= North Atlantic Deep Water, NPIW= North Pacific Intermediate Water, PDW= Pacific Deep Water. Export productivity (green dots). Southern Hemisphere westerlies (a circle with a dot).

5. The climatic and carbon cycle implications of this interpretation are important and should be explicitly stated. The authors touch on it in the abstract and intro: if westerlies indeed kept CO_2 high during the LIG, this suggests a feedback wherein warming leads to stronger westerlies, which then lead to CO_2 outgassing, potentially amplifying warming further. This is a positive feedback mechanism in the Earth system. The fact that CO_2 stayed around 270-280 ppm while Antarctic temperature fell by ~6°C hints that other factors (perhaps declining Southern Hemisphere insolation or ice sheet regrowth) eventually outweighed the westerly-driven CO_2 feedback in the LIG. The authors may want to comment on why CO_2 did not continue rising indefinitely – presumably because the source of easily ventilated carbon was finite or because winds and ocean circulation reached a new equilibrium. A bit of discussion here would show the authors have considered the limits of the westerly wind effect in the natural system.

Response:

We agree with the reviewer that the potential feedback involving warming, Southern Hemisphere westerlies, and atmospheric CO_2 during the Last Interglacial is crucial for understanding Earth system dynamics. In response, we have revised the Discussion to clarify this positive feedback and its limits:

1) Climate-carbon feedback mechanism: We have now emphasized a positive feedback loop: initial Antarctic warming via the bipolar seesaw during Termination II → stronger westerlies → enhanced Southern Ocean upwelling and subarctic Pacific deep water ventilation (AABW enhanced) → increased CO_2 concentrations→ amplified warming, likely sustaining elevated CO_2 into the early Last Interglacial. The relevant revision reads:

"Compared to the Penultimate Glacial Maximum (e.g., MIS 6), the increased meridional thermal gradient between the hemispheres during Termination II triggered an intensification of the Southern Hemisphere westerlies, promoting vigorous Southern Ocean upwelling and CO_2 outgassing. Concurrently, enhanced AABW export coincided with stronger deep-ocean ventilation, supporting elevated CO_2 outgassing in the subarctic Pacific. This positive climate-carbon feedback amplified global warming and appears to have extended into the early interglacial, as reflected in the synchronous variability of Antarctic temperature, Antarctic and subarctic Pacific export productivity, and atmospheric CO_2 (Figs. 4b-4d and 4f, 5a-5b)." Please see lines 351-359 in Main Text.

2) Why the Feedback Did Not Persist: To address the reviewer's question about why atmospheric CO_2 concentrations did not continue rising indefinitely, we have now proposed that this feedback eventually weakened, presumably because the source of easily ventilated carbon was finite or because winds and ocean circulation reached a new equilibrium, or Earth's orbital parameters altered. The relevant revision is as follows:

"Strengthened Southern Hemisphere westerlies during the late Last Interglacial enhanced Southern Ocean upwelling and carbon outgassing, likely forming a positive climate-carbon feedback that may have initially amplified warming or prolonged the warm period. However, the stabilization of atmospheric CO_2 at ~270-280 ppm, along with Antarctic cooling, suggests this feedback was eventually limited, possibly due to a new equilibrium in winds and ocean circulation, to the finite nature of readily ventilated deep-ocean carbon reservoirs, or adjustments in Earth's orbital parameters." Please see lines 412-419 in Main Text.

6. In terms of climate dynamics, I also have some comments to consider:
One aspect that could be emphasized is the parallel between the LIG scenario and the modern/future Southern Annular Mode (SAM) dynamics. The intensified, poleward-shifted westerlies the authors invoke are essentially a prolonged positive phase of the SAM (or a similar mode) under interglacial warmth. In modern observations and model simulations, a positive SAM (strengthened poleward westerlies) leads to enhanced upwelling of deep waters around Antarctica and reduced stratification, sometimes causing outgassing of CO_2 from the Southern Ocean. The authors allude to future implications (with SHW expected to intensify under global warming), which is excellent. They could bolster this by explicitly noting that their paleo-findings are consistent with SAM-like behavior on long timescales. This not only places the result in a familiar framework for climate community but also underscores that such ocean-atmosphere coupling is already observed on shorter timescales today. If not already mentioned, integrating a brief mention of SAM or analogous wind-pattern changes would strengthen the connection to known climate modes.

Response:

We sincerely appreciate the reviewer's comments highlighting the parallels between the Last Interglacial and modern/future Southern Annular Mode (SAM) dynamics as a framework for interpreting past and future climate behavior. We have revised the manuscript to incorporate SAM-like dynamics in three key aspects:

1) Link to SAM in Southern Hemisphere westerlies: During the late Last Interglacial, poleward-intensified Southern Hemisphere westerlies resemble a prolonged positive phase of the SAM. Such SAM-like circulation during the late Last Interglacial would have intensified the upwelling of Circumpolar Deep Water, delivering nutrients to the surface and contributing to elevated export productivity. Supporting evidence includes intensified Antarctic Circumpolar Current (ACC), increased nitrate supply, higher export productivity, and a southward shift of the Subtropical Convergence and ACC front. Please see lines 240-266 in Main Text.

2) SAM-driven upwelling impacts on atmospheric CO_2: Some studies propose the Antarctic temperature-driven processes for atmospheric CO_2 changes over glacial-interglacial timescales (Watson et al., 2015). Proxy records, however, reveal that this link weakened during the late Last Interglacial (125-113 kyr BP), as atmospheric CO_2 remained stable at ~270-280 ppm despite Antarctic cooling of more than 6°C. Our reconstructed export productivity records suggest that SAM-like wind circulation intensified CDW upwelling and sustained CO_2 outgassing. This wind-ocean-carbon linkage likely formed positive climate-carbon feedback that initially amplified warming or prolonged the warm period. However, the stabilization of atmospheric CO_2 suggests that this feedback was eventually limited, possibly due to a new wind-ocean equilibrium, the finite nature of readily ventilated deep-ocean carbon reservoirs, or orbital variations. Please see lines 351-374 and 404-419 in Main Text.

3) Implications for future SAM and CO_2 dynamics: Climate projections indicate that Circumpolar Deep Water will spread poleward and downward by 2050, while a more positive SAM phase is expected, strengthening poleward Southern Hemisphere westerlies. Stronger westerlies are projected to enhance Southern Ocean upwelling, contributing to increased atmospheric CO_2. Although on shorter timescales, this scenario may parallel Last Interglacial conditions, providing insight into potential polar ocean-atmosphere feedbacks under future warming. Please see lines 424-441 in Main Text.

7. Also, the idea of ocean-atmosphere feedbacks could be expanded. The authors have essentially identified a feedback loop: warmer climate (and/or changing insolation) → poleward-intensified westerlies → increased Southern Ocean upwelling → CO_2 release → further warming globally. It would be valuable to articulate this clearly. It's also worth discussing the time lag or sequence of events: Did the westerlies intensify because of the initial LIG warmth (e.g., due to a reduced equator-to-pole temperature gradient), thus later sustaining CO_2 levels even as Antarctic temperatures started to drop? If so, we are observing a hysteresis or time-lag effect where wind-driven CO_2 feedbacks prolong the warmth. Any insight the authors can provide (or speculate on, based on timing in their data) about the sequence – e.g., did Antarctic EP start rising before or just as CO_2 started rising at Termination II, and did subarctic EP falter only after the Antarctic winds revved up? – would be intriguing context.

Response:

We have expanded the ocean-atmosphere feedback loop as: warming by bipolar seesaw during Termination II and early last Interglacial) → poleward-intensified westerlies → enhanced Southern Ocean upwelling and subarctic Pacific deep water ventilation (AABW enhanced) → increased atmospheric CO_2 → amplified warming and orbital forcing change (late Last Interglacial) → positive SAM-like westerlies → increased Southern Ocean upwelling (Circumpolar Deep Water upwelling) → stable CO_2). We also discuss its temporal evolution across three phases:

1) Termination II and early Last Interglacial: Antarctic warming, driven by the bipolar seesaw mechanism, steepened the hemispheric temperature gradient and intensified the Southern Hemisphere westerlies. This enhanced Southern Ocean upwelling and AABW export, thereby increasing nutrient and CO_2 supply from the deep Southern Ocean both locally and remotely. Regionally, this sustained high productivity and CO_2 outgassing in the Southern Ocean. Remotely, the enhanced export of AABW advected into the subarctic Pacific, increasing abyssal nutrient content. This, together with enhanced deep-ocean ventilation, supported elevated productivity and CO_2 outgassing in subarctic Pacific. Collectively, these processes in the Southern Ocean and subarctic Pacific have contributed to rising atmospheric CO_2, forming positive climate-carbon feedback that further amplified warming. Please see lines 220-238, 306-322, 351-359 in Main Text.

2) Mid- to late LIG evolution: As obliquity declined, steepened the meridional temperature gradient further reinforced the Southern Hemisphere westerlies (Ai et al., 2020). Proxy evidence indicates that poleward strengthened ACC and weakened AABW export (Wu et al., 2020; Huang et al., 2020). Thus, wind-driven Circumpolar Deep Water upwelling remained active, supporting regional productivity and CO_2 release. However, weaker AABW export and a poleward ACC shift likely limited nutrient supply to the subarctic Pacific. Together with a weakened upwelling index, this likely constrained surface productivity and ocean-atmosphere CO_2 exchange. The wind-driven feedback likely prolonged warmth, yet the stabilization of atmospheric CO_2 at ~270-280 ppm, suggests this feedback was eventually limited by new wind-ocean equilibrium, orbital variations, or the finite nature of readily ventilated deep-ocean carbon reservoirs. Please see lines 240-266, 324-337, and 404-419 in Main Text.

3) Relevance to future climate: Model projections suggest that AABW-driven abyssal circulation may slow by more than 40% by 2050, accompanied by a poleward and downward expansion of Circumpolar Deep Water and a more positive SAM with poleward-intensified westerlies. As a result, Southern Ocean upwelling is projected to intensify, potentially enhancing CO_2 outgassing and amplifying global warming. In contrast, a slowdown of AABW, together with a concurrent poleward migration of the ACC, would be likely linked to reduced deep-water ventilation in the subarctic Pacific and limited ocean-atmosphere CO_2 exchange, echoing the spatial decoupling observed during the Last Interglacial. These potential parallels offer valuable insights into how Southern Ocean circulation has consistently influenced atmospheric CO_2 levels in both past interglacial periods and projected future climate conditions. Please see lines 424-441 in Main Text.

8. The manuscript touches on large-scale modes indirectly by discussing "abyssal circulation" and upwelling differences. To further integrate with global-scale ocean circulation, the authors might consider the role of the meridional overturning circulation (MOC) in their scenario. A subtle point: if Southern Ocean deep upwelling was enhanced, was there any compensating change in North Atlantic Deep Water production or global MOC strength? Some studies of past warm periods (and future projections) suggest that freshening or warming can alter North Atlantic circulation, which in turn might influence the volume of deep water reaching the Southern Ocean and eventually the North Pacific. While the focus here is rightly on Southern Ocean winds, acknowledging these broader connections would show comprehensive thinking. For instance, if the authors suspect a slowdown in some component of the MOC (hence "weakened abyssal circulation"), they could tie it to known possibilities like reduced Antarctic Bottom Water formation in a warmer climate. If any PMIP Last Interglacial model outputs or previous studies (e.g., on Atlantic vs. Pacific ventilation) align with their findings, they should be referenced to give the reader confidence that the proposed dynamics are realistic.

Response:

In the revised manuscript, we have included a clearer discussion of the meridional overturning circulation (MOC) accordingly, particularly regarding the link among Southern Ocean upwelling, NADW formation, ACC dynamics, and AABW export.

1) Broadly speaking, the MOC consists of two interconnected cells: an upper cell driven by NADW formation and Southern Ocean wind-driven upwelling, and a lower abyssal cell maintained primarily by AABW formation. Our proxy reconstructions for Termination II and the early Last Interglacial indicate a marked intensification of Southern Ocean upwelling and Pacific ventilation consistent with enhanced AABW export (Huang et al., 2020; Hayes et al., 2014). This occurred concurrently with substantial weakening of the Atlantic Meridional Overturning Circulation (AMOC; Böhm et al., 2015), primarily due to freshwater forcing and surface cooling in the North Atlantic, which suppressed NADW formation (Govin et al., 2012). Thus, the upper cell weakened while the abyssal cell strengthened, consistent with the bipolar seesaw hypothesis (Broecker, 1998).

2) During the late Last Interglacial, the enhanced interhemispheric insolation contrast amplified the meridional temperature gradient. At the same time, declining obliquity further steepened this gradient, reinforcing the Southern Hemisphere westerlies. Proxy

evidence shows stronger ACC speed (Wu et al., 2020), and enhanced Southern Ocean upwelling. At the same time, NADW recovered while AABW export declined (Hayes et al., 2014; Böhm et al., 2015; Huang et al., 2020). Together, these changes imply a transition toward an MOC dominated by a stronger NADW-driven upper cell and wind-induced Southern Ocean upwelling (strengthened ACC), while the abyssal AABW cell weakened.

3) As already outlined in response to earlier comments above, climate models project a reduced but stable AMOC (Baker et al., 2025) and ~42% weaker AABW export by 2050, accompanied by a poleward and downward expansion of Circumpolar Deep Water (Li et al., 2023). Elevated greenhouse gas concentrations are also projected a more positive SAM, with poleward-intensified Southern Hemisphere westerlies (IPCC, 2007), further enhancing Southern Ocean upwelling (Chen et al., 2008). These projected changes suggest a future MOC structure resembling the late LIG, with a stronger wind-driven Southern Ocean upwelling (strengthened ACC) and stable NADW upper cell, and a weakened abyssal AABW cell.

We have added this discussion and relevant references to the revised manuscript. Please see lines 306-322, 324-330, and 424-436 in Main Text.

Reviewer #3 (Comments to Author (shown to authors):

This is a very interesting study that produced a stacked record of Antarctic Southern Ocean sediment proxy records of export productivity and compared this productivity stack with Antarctic temperature, atmospheric CO_2, and Subarctic North Pacific export productivity variations over the last 160 kyr, with a specific interest in the Last Interglacial period. The study examines temporal variations in the relationships between these climatic parameters through comparison of time series and through direct correlation analysis and highlights that the positive linear correlation between atmospheric CO_2, Antarctic temperature, and Antarctic and Subarctic productivity seem to break down during the late Last Interglacial. Based on this analysis, the authors conclude that the Last Interglacial period may have been associated with poleward migration of the Southern Hemisphere westerlies, which enhanced nutrient upwelling, productivity, and deep carbon release in the Antarctic Southern Ocean. In contrast, the authors suggest that the North Pacific experienced reduced abyssal upwelling, which caused a reduction in subarctic productivity and a decoupling between subarctic productivity and atmospheric CO_2.

This work tackles the dynamic mechanism behind global carbon cycling in warmer-than-modern climate conditions, which is an interesting problem on its own and very relevant for accurate predictions of future climate behavior associated with anthropogenic warming. I appreciate the analytical approach of this study and commend the authors for the very good data accessibility provided in their Materials and Methods and supplemental tables. Although this is not the first study to support poleward migration of the westerlies during warmer-than-modern paleo-intervals, this is the first one to my knowledge that directly connects this mechanism to disparate changes in Antarctic and subantarctic export productivity across the Last Interglacial. However, I do have a few suggestions, including additional analysis of the data, that I believe would strengthen the impact of this work. Therefore, I recommend that the authors be given the opportunity to address the following comments before finalizing a publication decision for Nature Communications.

Response:

We thank the reviewer for the constructive feedback and the recognition of our analytical approach and our data, and the contribution of linking poleward Southern Hemisphere westerlies to contrasting Antarctic/subantarctic export productivity during the Last Interglacial. Following the reviewer's suggestions, we carried out additional analyses of the data. Specifically, we compiled five independent subarctic Pacific export productivity records and constructed two composite stacks using the same methods as for the Antarctic records, confirming their regional representativeness. We further performed linear regressions extended to the Holocene, showing that MIS 5 exhibits a distinct interhemispheric productivity pattern, with the largest deviation during the Last Interglacial. Additional regressions of Antarctic temperature vs. Antarctic productivity, Antarctic temperature vs. CO_2, and subarctic Pacific productivity vs. CO_2 over the past 160 kyr reveal a pronounced decoupling during MIS 5, highlighting its distinctive characteristics relative to other glacial-interglacial intervals and strengthening the overall impact of this work.

General Comments:

1.Broadly, the comparison between Antarctic export productivity records (synthesized into a stack of 15 different proxy time series from 12 different sediment cores) with the subarctic North Pacific export productivity records (indicated by two barium-based proxies from a single sediment core at Site 882) seems unbalanced. How well does export productivity at Site 882 reflect the entire subarctic North Pacific? The Antarctic records exhibit some degree of spatial variability (for example, ODP 1094 looks very different from U1537), would be expect the same in the North Pacific? Even if there are not additional North Pacific productivity records covering this interval (which I am not sure about), this manuscript would benefit from a discussion of how representative Site 882 is of the region based on more recent intervals, where additional productivity data is more likely to be available.

Response:

To address the reviewer's concern regarding the representativeness of export productivity for the broader subarctic Pacific region, we constructed two composite subarctic Pacific productivity stacks from five independent subarctic Pacific records using the same methods applied to Antarctic records (inverse standard deviation weighting method and PCA). The records spans both the western (e.g., ODP Site 882, MR98-05-3PC) and eastern (ODP887 and Juan de Fuca Ridge stack) sectors, and the Bering Sea (e.g., JPC17), covering diverse water depths (2209-5507 m) and proxies (biogenic barium concentration and $^{231}Pa/^{230}Th$; Table r1).

PCA shows that the first principal component (PC1) captures 61.3% of the total variance (Fig. r4), closely aligns with the stack generated using the inverse standard deviation weighting method (R = 0.97; Fig. r5), as well as the published subarctic

Pacific productivity records (R> 0.7; Fig. r6), confirming regional representativeness. We also incorporated the high-resolution, astronomically tuned Ba/Al record from ODP Site 882 as a complementary dataset to better resolve millennial-scale variability.

We have incorporated the analyses into the revised manuscript, along with supporting Figs. S10-S12, Table S1, and text in Lines 136-157 and 587-638.

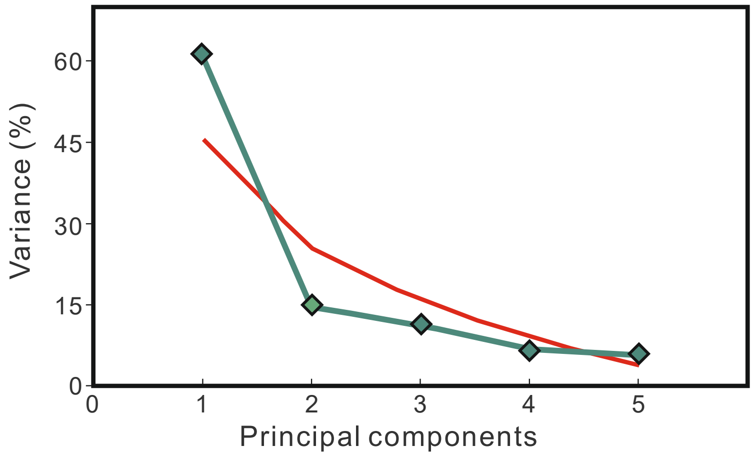

Fig. r4 (new Fig. S11 of the revised manuscript) | The variance explained by each principal component, along with the broken stick curve (red).

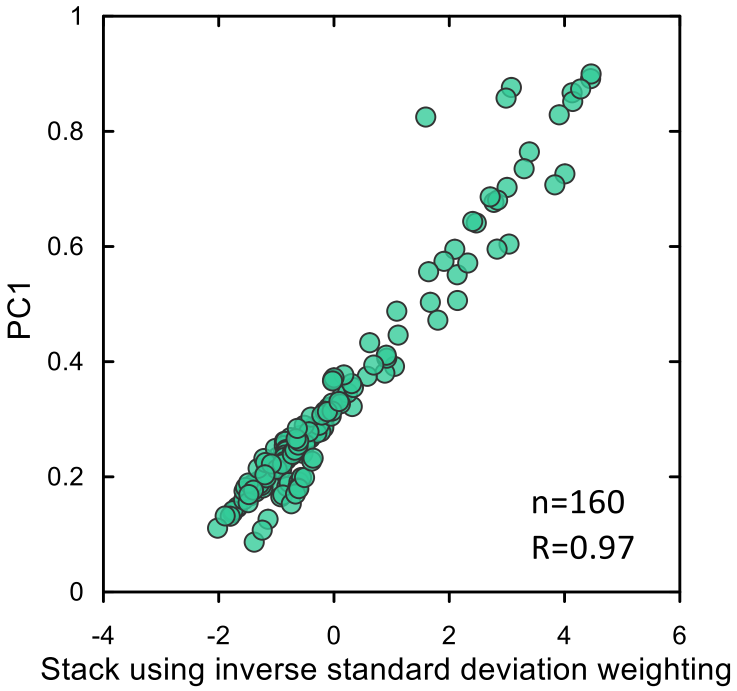

Fig. r5 (new Fig. S10 of the revised manuscript)| Correlation of PC1 with Subarctic Pacific export productivity stack using the inverse standard deviation weighting method.

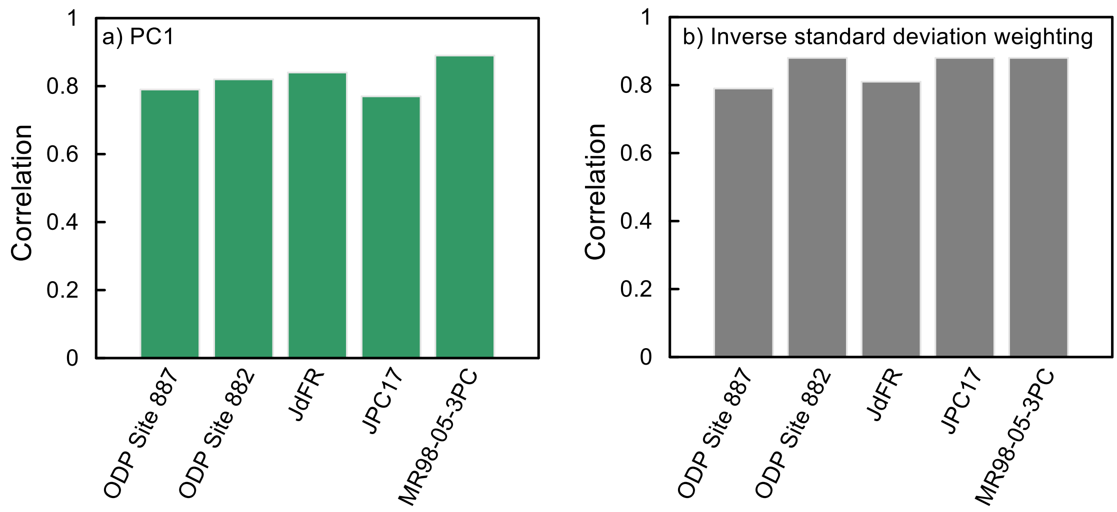

Fig. r6 (new Fig. S12 of the revised manuscript) | Correlation of the related Subarctic Pacific export productivity records on PC1 (a) and export productivity stack using the inverse standard deviation weighting method (b).

2. The argument that the dynamics observed during the Last Interglacial period represent a unique climate regime would be more compelling if the study also included analysis of how the Antarctic temperature, atmospheric CO_2, Antarctic export productivity, and subarctic export productivity records compared across the Holocene/current interglacial period. The Antarctic productivity stack that this study has generated is well suited for this analysis because it covers the last 160 kyr completely. It seems the Site 882 productivity record does not span the Holocene, so perhaps another North Pacific productivity record could be used here for the comparison?

Response:

We agree that extending the analysis to the Holocene is valuable. Accordingly, we compiled five subarctic Pacific export productivity based on biogenic barium and $^{231}Pa/^{230}Th$ proxies covering the past 160 ka, thereby including the Holocene (Table r1). Using this expanded dataset, we constructed composite productivity stacks based on two independent methods consistent with those applied to Antarctic records (inverse standard deviation weighting and PCA). Both stacks show strong consistency with published subarctic Pacific records, enabling robust comparisons with Antarctic productivity and climate proxies during the Holocene. These new analyses and figures have been added to the revised manuscript (Fig.2, Figs. S10-S13, Lines 136-157 and 587-638).

3. I appreciate the inclusion of the correlation plots in Figure 4 as a way to examine the data beyond the time series comparisons illustrated in Figures 2 and 3, with respect to differences between records during MIS6, T2 and the LIG. Panel 4d is especially helpful for illustrating the decoupling between Antarctic temperature and atmospheric CO_2 during the LIG and Panels 4e and 4f very clearly suggest that atmospheric CO_2 concentrations seem to be much more driven by Antarctic Zone productivity than by North Pacific productivity.

However, the circled LIG data in panels 4a and 4b look to me more like a regime shift than a complete decoupling between Antarctic temperature and Antarctic productivity or between Site 882 productivity and Antarctic productivity (e.g., the data still look to be linearly correlated, though perhaps with a different slope and intercept). What correlation coefficients do you get when you examine just the LIG data, rather than the full 150-113 ka record? Are the LIG-labelled data points that fall closest to the 150-127 ka trendline the ones closest in age to T2, or are they from the middle of the LIG? It would also be helpful to see how data from MIS 3, 4, and 2 would plot on these graphs (at least in the supplement if it makes the main figure too crowded), to see how representative the MIS 6 and T2 relationships are of the last glacial cycle as well.

Response:

We thank the reviewer for this insightful comment. While the circled LIG data in Fig. 4a-b appear consistent with a regime shift, our regressions indicate otherwise. Across the full LIG (128-113 ka), correlations are very weak (Antarctic Temperature vs. productivity $R^2 = 0.07$; Antarctic vs. Site 882 productivity $R^2 = 0.0008$). A stronger fit (Antarctic Temperature vs. productivity) appears in the late LIG (125-113 ka; $R^2 = 0.66$). Thus, the prevailing pattern during the LIG interval is a decoupling, rather than a simple change in regime shift.

To place the Antarctic productivity-temperature and Antarctic-subarctic Pacific productivity relationships in a broader context, we added new plots in Fig. r7, covering MIS 1-6 (including MIS 5e, LIG). From these plots, MIS 6 points fall close to MIS 2, 3, and 4, indicating that they are representative of glacial variability, while TI and TII data largely overlap, reflecting deglacial changes.

Excluding MIS 5 greatly improves fits ($R^2_{\text{Antarctic Temperature vs. productivity}} = 0.90$; $R^2_{\text{Antarctic-subarctic Pacific productivity}} = 0.91$), whereas excluding MIS 1 and deglacial yields results similar to the full record ($R^2_{\text{Antarctic Temperature vs. productivity}}$: 0.76 full record vs. 0.80 excl. MIS 1 and deglacial; $R^2_{\text{Antarctic-subarctic Pacific productivity}}$: 0.58 full record vs. 0.56 excl. MIS 1 and deglacial). MIS 5 data begin to deviate starting around 125 ka and reach a maximum deviation around 115-113 ka.

Taken together, these analyses indicate that MIS 5 as a whole, represents a unique interval of decoupling between Antarctic productivity and temperature or between Antarctic and subarctic Pacific productivity. Please see Fig. 3

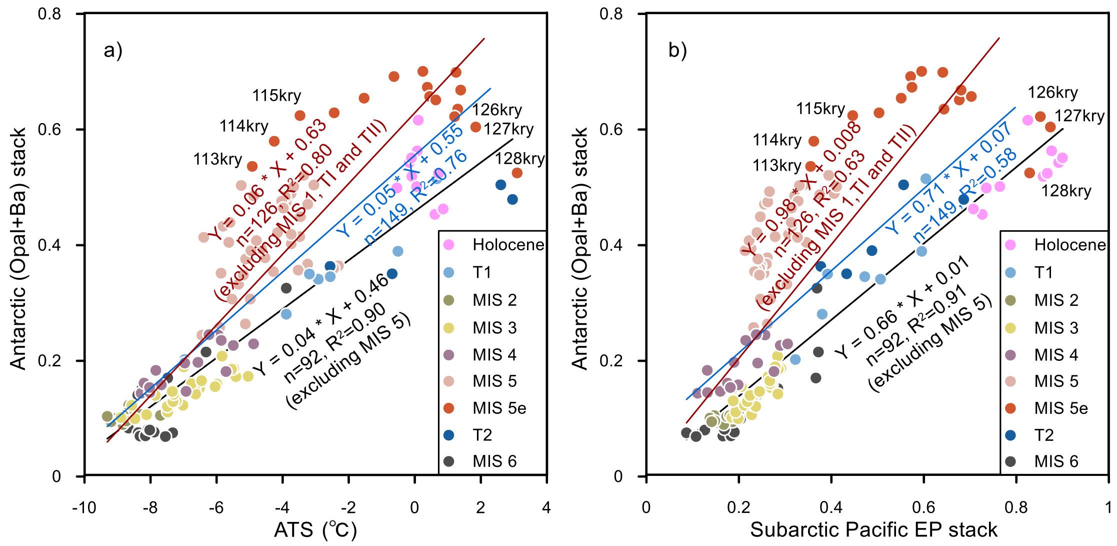

Fig. r7 | Cross plot of the Antarctic (biogenic opal+ biogenic barium) stack versus ATS (a) and the subarctic Pacific export productivity (EP) stack (b), respectively.

Specific Comments:

1. Line 34: Suggest to rephrase to "…abrupt decline in the Antarctic air temperature stack (ATS)…" so unfamiliar readers will understand where the S is coming from in the acronym.

Response:

We have rephrased the text as "over 6 °C Antarctica cooling" to make it easier for all readers to follow. Please see line 31 in Main Text.

2. Lines 37-40: I think the conclusion of this sentence would be easier to follow if an additional step in the logic were explained (for example, please specify here why a tight coupling between subarctic Pacific export productivity and Antarctic temperature indicates suppressed abyssal upwelling).

Response:

We agree that specifying the mechanistic link would clarify the logic. In the revised abstract, we have added a concise statement indicating that altered the Southern Ocean deep-water supply to the subarctic Pacific likely suppressed upwelling, limiting CO_2 release. A more detailed discussion of this mechanism is provided in the main text. Please see lines 33-35 and 294-337 in Main Text.

3. Line 91: Suggest to clarify here, rather than waiting until the methods section, that the 15 export productivity records are from 12 sediment cores (especially as there are only 12 orange dots in the map figure).

Response:

We have revised the sentence to improve clarity. It now reads: "To assess regional differences in export productivity and carbon cycling, we synthesize 15 records from 12 sediment cores in the Antarctic zone and 5 records from 10 sediment cores in the subarctic Pacific." Please see lines 81-85, Figure 1, and the related caption.

4. Lines 139- 141: Suggest to include the correlation coefficients for the full record here between the Antarctic proxy stack, the ATS and the subarctic records for consistency in comparison across time periods.

Response:

To improve consistency across time periods, we have now included the linear regressions for the full record among the Antarctic export productivity stack, the ATS, and the subarctic Pacific export productivity records, which capture these relationships more comprehensively than simple correlation coefficients. Please see Figure 3, lines 169-175, 207-210, and 273-277 in Main Text.

5. Lines 164-168: Suggest to rephrase to "in contrast, iron fertilization does not appear to have been impactful in the Antarctic Zone, which exhibits lower export productivity during glacial periods" to clarify this point.

Response:

We have rephrased the sentence as suggested to clarify this point. Please see lines 190-192 in Main Text.

6. Lines 176-178: It is my understanding that Ref 32 does not assert that temperature is the primary control on export productivity in the Antarctic zone, rather they suggest that low glacial productivity in this region may be caused by reduced wind-driven upwelling associated with a northward migration of the Southern Ocean fronts. Consequently, I suggest not using this reference to justify the first part of this sentence.

Response:

Following the reviewer's suggestion, we have removed Ref 32 from the statement on temperature and revised the text accordingly. Please see lines 202-205 in Main Text.

7. Lines 247-249: Suggest to continue to specify that "temperatures" refers to "Antarctic temperatures" here, to avoid confusion associated with asynchronous millennial-scale warming between Greenland and Antarctic temperature records (e.g., Blunier et al., 1998).

Response:

The text has been revised to specify "Antarctic temperatures" to avoid confusion. Please see lines 362-364 in Main Text.

8. Line 560 (Figure 3 caption): More detail is needed here regarding the upwelling index denoted (e.g., suggest to specify that it's the subarctic upwelling index based on U1343 and Site 1012). Since the paper typically indicates what geochemical proxies are used to infer each parameter for productivity, ACC strength, etc…, I suggest to include the proxy basis of the upwelling index here as well (or perhaps where this is discussed in the main text).

Response:

We have revised the main text and the caption of Figure 4 to clarify that the upwelling index, which represents the upwelling in the subarctic Pacific, is calculated as the normalized opal MAR at Site U1343 minus the normalized $\delta^{15}N$ difference between Sites U1343 and 1012. Please see Fig. 4 caption and lines 317-320 in Main Text.

9. Figure 5: I found this figure very helpful for visualizing the different suggested circulation scenarios, but I can't find a reference to this figure anywhere in the main text.

Response:

We have added a reference to Figure 5 in the main text. Please see lines 359,406, and 411 in Main Text.

10. Lines 599-601: Please explain here why you included these two cores into the productivity stack, although they do not span the LIG range targeted by this study.

Response:

Although these two cores do not fully cover the targeted LIG interval, they include detailed productivity records from the Holocene and millennial-scale intervals. Including these records thus allowed us to compile a more comprehensive Antarctic productivity dataset, thereby enhancing the robustness and representativeness of our productivity stack. We think this approach strengthens the validity of our analysis and conclusions. Please see lines 450-454 in Main Text.

11. Line 678: I assume records were normalized by dividing each by its mean. Could you please specify the exact approach used so unfamiliar readers can better follow along?

Response:

Each record was using Min-Max normalization, defined as $X_{norm} = (X_{sample}–X_{min})/(X_{max} – X_{min})$. This transformation rescales the data to the range [0, 1], allowing for direct comparison of the magnitude and timing of changes in export productivity across different records. We have now explicitly stated this normalization procedure in the revised manuscript. Please see lines 532-537 in Main Text.

12. Lines 686-691: After looking through Figures S7 and S8, I wonder if this inverse dispersion weighting is artificially removing legitimate high frequency signal from the stacked record (leading to an over-smoothing). There are many Antarctic sediment cores in which sedimentation rates vary largely between productive climates and low-productivity climates (ODP 1094 is a good example), such that the ability of each record to capture millennial-scale changes varies temporally. There are also clear difference between sediment cores in their ability to capture high frequency variations in export productivity (for example, TN057-14PC4 captures much more resolution than PS75-072-4 during MIS3), does that mean TN057-14PC4 receives a lower weighting than PS75-072-4? Should it? Would the stacked export productivity records look very different if you weighted each record by sedimentation rate across a given interval, rather than inverse dispersion?

Response:

We appreciate the reviewer's careful consideration of our weighting method. Our inverse-dispersion weighting assigns higher weights to data with lower standard deviation and lower weights to more variable data, ensuring that stable and reliable records contribute more to the stacked record. For example, during MIS 3, TN057-14PC4 provides much higher resolution than PS75-072-4, yet it receives greater weight (standard deviation $\sigma = 0.1683$; w=0.0903) than PS75-072-4 ($\sigma = 0.3084$, w=0.0492 for opal flux;σ=0.2241, w=0.0678 for Ba/Fe). This approach does not diminish the role of high-resolution records. A principal component analysis of the compiled records shows that the first principal component (PC1) correlates strongly with our stacks (Fig. r8), supporting the robustness of our approach.

Following the reviewer's suggestion, we also tested sedimentation rate weighting. This method increases the amplitude of many peaks, likely because high-sedimentation intervals capture higher-frequency variations, including potential noise. In contrast, inverse dispersion weighting prioritizes more stable signals, providing a robust representation of overall export productivity trends. Importantly, both weighting methods produce relatively consistent large-scale patterns (Fig. r8), demonstrating the reliability of our approach.

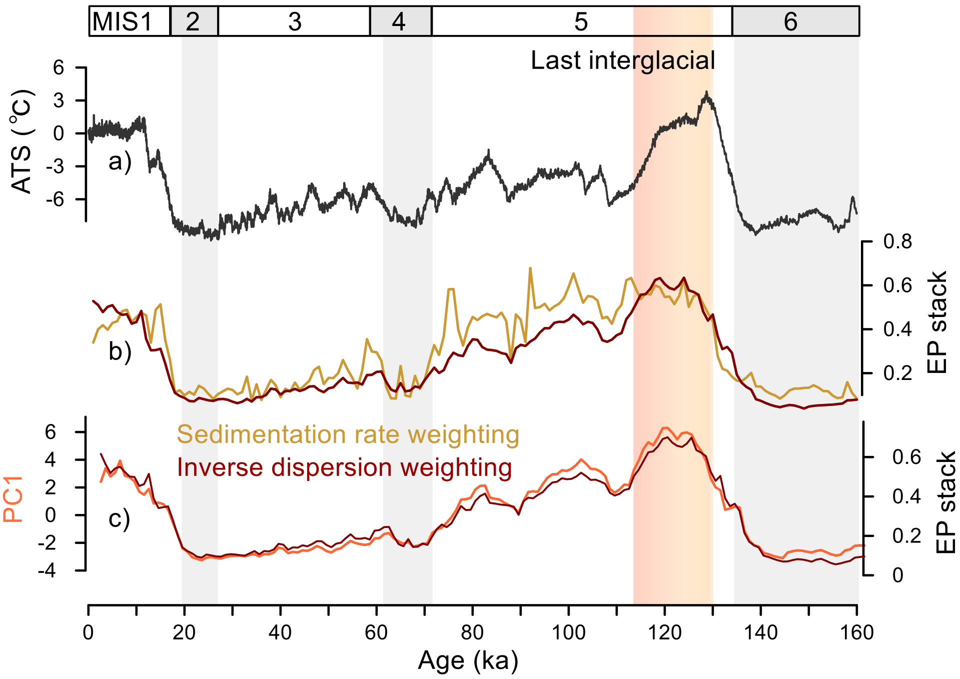

Fig. r8 | Comparisons of Antarctic (opal+Ba) stack with different methods. a) ATS; **b)** Antarctic (opal+Ba) stack using sedimentation rate weighting and inverse dispersion weighting; **c)** Antarctic (opal+Ba) stack using inverse dispersion weighting with the first principal component (PC1) conducted by principal component analysis.

13. Also please note that, in general, Marine Isotope Stage labels are used throughout the figures, but do not appear to be mentioned or explained in the main text.

Response:

We have now added a brief explanation of the Marine Isotope Stage (MIS) labels in the main text to ensure consistency with the figures and improve clarity for readers.

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Point-by-point response to reviewers' comments

Reviewer #1 (Comments to Author (shown to authors):

I have reviewed the authors' responses and the revisions in the manuscript and believe that the authors have made a great effort to respond to all of the reviewers' comments. Also, I find both the changes to the text and the new figures to be very appropriate. There is only one point that requires a small, but relevant, clarification. Fragilariopsis kerguelensis is not generally considered an indicator of high productivity (as indicated in the response to the reviewers). Other diatom species tend to dominate in highly productive environments such as coastal and shelf systems of the Antarctic continent. This does not mean that changes in the abundance of this species cannot be related to an increase in productivity. According to some studies (e.g. Nair et al., 2020 and other references in the text) increases in the flux of F. kerguelensis (the main vector of silica to the ocean floor) appear to be associated with greater nutrient input. Therefore, my only suggestion would be to clarify this point better in the text. The authors could indicate something like "changes in the abundance of F. kerguelensis can be taken as an indicator of productivity changes in the pelagic Southern Ocean" or something along those lines. Please also specifically mention in the text if authors are referring to absolute abundance and/or relative abundance of F. kerguelensis in the manuscript. I have no further suggestions from my side and would like to congratulate the authors on this very interesting work.

Reference (please note that references are just a suggestion, there is no need to include more references if authors do not considered relevant)

Nair, A., Ghadi, P., Mohan, R., Manoj, M. C., Crosta, X., Shukla, S. K., and Thamban, M.: Glacial-interglacial flux and size variability of Fragilariopsis kerguelensis and Thalassiosira lentiginosa from the Indian sector of the Southern Ocean, Deep Sea Research Part II: Topical Studies in Oceanography, 178, 104746, https://doi.org/10.1016/j.dsr2.2020.104746, 2020.

Response:

We appreciate the reviewer's constructive comment and helpful clarification. We have revised the text accordingly to clarify that Fragilariopsis kerguelensis can be used as an indicator of productivity changes in the pelagic Southern Ocean, rather than of high productivity in coastal or shelf environments. We have also specified that our discussion refers to its relative abundance. The revised sentence now reads as follows:

"Independent palaeoecological data on the relative abundance of Fragilariopsis kerguelensis within the permanent open-ocean zone south of the Polar Front in our study area, which can be used as an indicator of productivity changes in the pelagic Southern Ocean, closely track the stacks (Fig. S9), further supporting that the interpretation that the reconstructed stacks capture key signals of Antarctic export productivity."

Reviewer #2 (Comments to Author (shown to authors):

Thanks authors for hard working on revising the manuscript. The authors' revisions address all concerns. I recommend acceptance in Nature Communications.

Response:

We greatly appreciate your encouraging comments and positive recommendation for acceptance. We truly value the time and effort dedicated to reviewing and supporting our work.

Reviewer #3 (Comments to Author (shown to authors):

I am satisfied with the changes made in the revised manuscript and applaud the authors for their thorough incorporation of reviewer feedback and discussion. I believe these changes have made the manuscript stronger and, as such, I recommend this manuscript for publication.

Response:

Thank you very much for the supportive comments. We are pleased that our revisions have fully addressed your concerns and appreciate your acknowledgement that the manuscript is ready for publication.