Point-by-point responses to Reviewer 2

Factors contributing to the concentrations and stable isotopic ratios of sea-ice particulate organic carbon and nitrogen in the Indian sector of the Southern Ocean

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Thank you for reviewing and providing comments that helped improve our manuscript. All revisions including IAC:Chl *a* ratios and other points are explained in the following specific comments. The revised text is circled by quotation marks "" in this text.

Changes in the manuscript not mentioned by reviewers

In the estimation of POC_{Input}, we did not consider density of sea ice; the ice melt thickness in Table 2 was equivalent to the thickness "before" its melting (Komatsu et al. 2025). Therefore, the original sea-ice meltwater flux and estimation of POC_{Input} were slightly overestimated given that sea ice density is usually <950 kg m^{-3}. In the revised manuscript, we corrected the values by multiplying density of sea ice (ρ_i=900 kg m^{-3}) used in Komatsu et al. (2025). We added following equation in the text;

"The sea ice meltwater flux was multiplied by the POC_{VWM} concentration in sea ice to estimate the POC flux from sea ice to the water column per unit area (mg C m^{-2} $year^{-1}$).

$$POC_{Input} = H_i \times \rho_i \times POC_{VWM} \quad (6)$$

Here, H_i is ice melt thickness and ρ_i is the density of sea ice (0.90) used in Komatsu et al. (2025)."

We also noticed erroneous marks in Fig. 3f which indicated significant differences of $\delta^{13}C$ between the regions. We corrected the figure and checked these revisions did not change our discussion and conclusion.

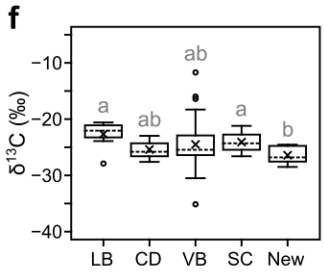

Figure 3f: Regional or ice-type (FYI/OI brash ice v.s. newly formed ice) differences in stable isotopes of POC (f).

Reviewer 2

The observational dataset presented in this manuscript is highly valuable and fills an important gap in measurements of summer brash sea-ice BGC in the Indian sector of the

Southern Ocean. Such observations are relatively scarce, and the dataset provides a useful contribution to our understanding of sea-ice BGC and the potential role of melting sea ice as a source of organic matter to the upper ocean. The manuscript is generally well written and the figures are clear. However, I have a several concerns regarding the justification of some methodological assumptions and the strength of several interpretations. In particular, the estimation of ice algal carbon, interpretation of POC:Chl *a* ratios, and several interpretations based primarily on correlation analyses would benefit from additional clarification and discussion of their associated uncertainties. Addressing the comments below would substantially strengthen the manuscript.

Specific comments

Line 25 – Remove “this”.
We revised as suggested.

Line 31- Seawater mean and SD aren’t given in main text.
We added mean and SD in Section 3.1;

"Seawater POM $\delta^{13}C$ and $\delta^{15}N$ were −28.8±1.0‰ (−30.9‰ to −27.0‰) and −0.5±1.4‰ (−3.1‰ to +1.5‰), respectively and no relationships were observed between them."

Line 32 – “indicating” seems a bit strong given that these are correlations rather than direct measurements. Perhaps consider changing “indicating” to “suggesting”, particularly for the inference regarding decomposition.
We agree to change it to "suggesting".

Line 54 – Include a reference here for seeding theory such as van Leeuwe et al 2022. Also, similarly for stratification influences and iron supply as there are papers on this.
We added following references;

Seeding theory: Garrison et al. (1987) and van Leeuwe et al. (2022)
Iron from sea ice: Sedwick and DiTullio (1997)
Stratification of water column: Wilson et al. (1986)

Line 67 – Remove “and accumulate data.”
We removed as suggested.

Line 68 – Does PN include both organic and inorganic N? If not, would PON be more appropriate for consistency with particulate organic carbon (POC).
Because of the limitation in analytical method, it is not possible to discriminate inorganic nitrogen (PIN) from PON. The measured data includes both organic and inorganic nitrogen and PON is termed PN in most sea-ice and ocean studies though inorganic fraction would be minimal. Since PN is most commonly used (e.g., Arrigo et al., 1999; Henley et al., 2012), we would like to use PN.

Line 80 – I am not sure what is meant by ‘when fast ice and pack ice floes are well established to land on ice’. please rephrase for clarity.

The original text suggested thick and (usually) undeformed ice where ice coring can be conducted safely. We clarified this statement;

"To date, much of the POM data have been obtained from sea-ice core samples collected during spring, when fast ice or pack ice floes are thick enough to conduct ice coring and observations safely on ice."

Line 105-106 – Were all cores (even for bio) stored at -20 before analysis and approx. how long was this frozen period? Or was this only on physical cores – clarify.
We added duration in text; All sea ice was stored frozen for not longer than 12 months in the dark prior to melting. Cold room (−20°C) is recommended to halt biological activity (Miller et al., 2015) and freezing ice for months are found in biogeochemical studies of Antarctic sea ice (van der Merwe et al., 2009; Sahashi et al., 2022; Nomura et al., 2023; Johnson et al., 2024) if onboard processing was not feasible.

"The collected sea ice was immediately packed into polyethylene bags and stored in the dark at −20°C (Miller et al., 2015) until analysis for not longer than 12 months."

Line 108 – What was the seawater filtered through GF/F's for… POC/N?
Yes. We clarified it was for POC/PN and isotopes;

"For POC/PN concentrations and $\delta^{13}C$ and $\delta^{15}N$, a total of 1.0 to 2.3 L of seawater was filtered through..."

Line 123-124 and 130 are a repeat of description for filtering for Chl *a*, choose one to keep.
We removed an overlap in method descriptions in Section 2.2. and Section 2.3.;

"The filters were soaked in *N*,*N*-Dimethylformamide (Suzuki and Ishimaru, 1994) and stored at −20°C in the dark."

"The pigments on filters were extracted with *N*,*N*-Dimethylformamide for at least 24 h and Chl *a* concentrations were measured using a pre-calibrated fluorometer (Welschmeyer, 1994)."

Line 149-150 – Did you use the sea ice nutrient data from these papers? The data aren't cited then, rather they are used/sourced? Rephrase.
We used sea-ice salinity, nutrient, Chl *a* values reported from those studies, however, not all data were published in open access data system. Therefore, all data including nutrients etc. were articulated in a single file and archived in the system (URL is included in Data availability section). For clarity, we moved this statement to Section 2.1. and revised as follows;

"For UM18-08, UM19-08, JARE61, and KH19-01 sea-ice salinity, nutrients and Chl *a* concentration of some samples are published in the previous studies (Takahashi et al., 2022; 2023; 2025, Nomura et al., 2023). Along with these datasets, we compiled unpublished data (e.g., POC, PN, and stable isotopic ratios etc.) and archived in the Arctic and Antarctic Data Archive System (ADS)."

Line 154-155 – Brine convection and exchange also influence bulk concentrations and doesn't necessarily dilute nutrients. This process may also be worth acknowledging here.
We included the following remark;

"It should be noted that this method focuses on the seasonal changes in nutrient concentration (i.e., changes since sea-ice formation) and does not completely rule out the effect of brine convection or exchange with seawater which could dilute/supply nutrient in sea ice."

Section 2.6 – I found the calculation of IAC somewhat unclear. Could the authors clarify exactly how IAC was estimated and where the IAC:Chl *a* ratio originates? Line 166 states that this ratio was estimated from "field observations", but it is not clear whether these observations come from the present study, a previous publication, or a larger independent dataset. If the IAC:Chl *a* ratio is independent of the present dataset, please clarify its source and justify its applicability to the samples analysed here. Given the known variability in algal C:Chl relationships with species composition, physiological state, and environmental conditions, it would be helpful to discuss the uncertainty associated with applying a single mean conversion factor. As this conversion is applied in several subsequent interpretations, additional justification and discussion of its limitations would strengthen the subsequent interpretations.
The IAC:Chl *a* data are independent from this study except those in Figure S1c (Takahashi et al., 2023), which is the same samples as St. KH19-1. We added IAC and Chl *a* plots with linear regressions determined from the 3 previous publications as a supplement (Supplementary Fig. S1, shown below). The mean and standard deviation was calculated from the slopes of regressions (91.7, 32.5, 39.0).

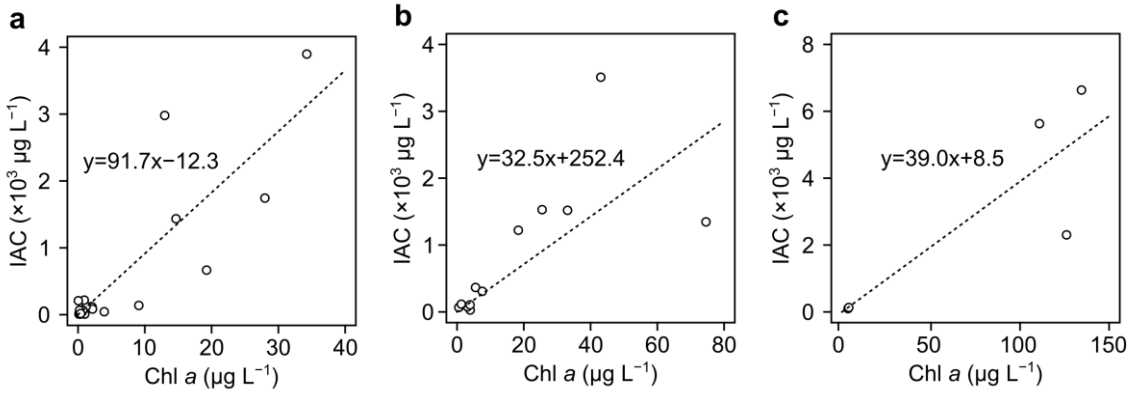

Figure S1
The relationships between ice-algal carbon (IAC) and chlorophyll *a* (Chl *a*) concentration of Antarctic sea ice. The plots in Fig. S1a–c are created from the data published by Becquevort et al. (2009), Meiners et al. (2011), Takahashi et al. (2023), respectively. All IAC data were obtained from microscope count and conversion to carbon content from bio-volume.

We agree that several factors influence IAC:Chl *a* ratios and added discussion and limitation on this point;

"This ratio still has uncertainty which affects our estimation of POC_{algae} because of the variabilities among studies (Fig. S1). Natural ice-algal assemblages change their cellular carbon or Chl *a* content under the alleviated light condition (Gosselin et al., 1990)."

Line 170 – Define F. Is this the mean IAC:Chl *a* 54.4?
Yes, we added definition;

"Here, *F* is the mean IAC:Chl *a* ratio."

Line 179 – What is the resolution for thickness data? I'd also add in a short sentence about the low sea ice concentration influencing why you don't have DSF for some stations. I see it's in the caption of Table 1 but would be good to state in this section 2.7.
We don't have a specific resolution for thickness data, like the direct measurements (e.g., ice coring or electromagnetic induction). Ice thickness was determined by salt budget analysis using CTD data collected during melt seasons, assuming that salinity deficits in water column reflects the amount of ice melt (Komatsu et al., 2025).

We added an explanation why some stations are missing DSF in Section 2.7;

"Because sea-ice motion was estimated using cross-correlations of pixels in a 60 km×60 km window, the trajectories and DSF data are missing in Sts. KH19-1 and ROV which lacked valid sea-ice data around the sampling site."

Results & Discussion
Line 228 – Add specific panel you reference… 'a' to Fig 2.
We revised as suggested.

Line 234 – Add maximum value found at St S35.
We revised as suggested.

Figure 2 – Include regression coefficients on the figure panels a-c, if significant like Fig 6.
We added spearman's rank coefficients and *p*-values in Fig. 2a-c.

Figure S1, S2, S3 – I would add that the panel labels "B, C, D, KH20-1, RAS, CD2" are station names in the captions to be clear for the reader.
We added "St." before each station name.

Section 3.5 heading – suggest removing "production in the".
We revised as suggested.

Table 2. What is 'b'? I would also suggest re-arranging the footnotes alphabetically. I assume 'e' is supposed to be 'b'.
We're sorry that caption was incorrect. It should be "[b]Estimates from Komatsu et al. (2025)", which denotes that ice melt thickness was cited from this study. We revised the caption.

For the meltwater calculation, where does the time unit come from? Table 2 reports a mean ice melt thickness of 0.82m, yet the resulting POC input is expressed as mg mg^{-2} yr^{-1}. Is the

Komatsu et al. estimate an annual cumulative sea-ice melt thickness? If so, reporting this as 0.82 m yr^{-1} (or stating explicitly what it represents) would make the calculation much clearer.
Yes, the unit is per year. We revised it in Table 2.

Line 269: The reported value -24.6 and -24.4 for δ13C differs from the value in the abstract. Please check for consistency.
We corrected the values in the abstract and text;

"The $\delta^{13}C$ and $\delta^{15}N$ values of sea-ice particulate matter were −24.4±3.2‰ and +1.5±2.2‰ (mean±standard deviation), respectively..."

"The mean (±SD) $\delta^{13}C$ and $\delta^{15}N$ values for sea ice excluding newly formed ice were −24.3±3.2‰ and +1.6±2.2‰, respectively (−24.4±3.2‰ and +1.5±2.2‰ for all sea ice)."

We included the range and variation originating from IAC:Chl *a* ratios in the discussion, where the input of ice-algal carbon were compared between this study and Ackley et al. (1979), following the comment.

Line 313 – Does the 27.9 Tg yr^{-1} MIZ value account for both open water and ice, or just ice-derived primary production? I couldn't find the value in the Arrigo paper. Are you taking a SIZ calculation and applying it to only the MIZ? I see in the footnotes in Table 2 it contains MIZ and MIZ shelf… do you consider this to be equivalent to the SIZ?
The value 27.9 Tg yr^{-1} is solely the open water primary production estimated just after seasonal sea-ice retreat (14 days after sea ice concentration dropped <10%). To match with our sea-ice data in 20–160°E, we summed the averages of annual primary production within MIZ and MIZ shelf (MIZ in shallow coastal regions) in the South Indian Ocean (20–90°E) and Southwest Pacific Ocean sectors (90–160°E). The data were cited from Table 5 of Arrigo et al. (2008).

Line 339 – should be "Fig. 3c, b".
We revised the figure citations in the order they appear in Fig. 3;

"...while the Chl *a* and POC concentrations in this study were 0.2–4.0 μg L^{-1} and 246.1–1354.0 μg L^{-1}, respectively (Fig. 3b, c)."

Line 329-330 – Could the authors clarify/expand why higher DSF is interpreted as indicative of greater light limitation? As ice ages and decays, floes may become smaller and more fragmented, potentially increasing light exposure.
We removed this statement for the discussion on light limitation was not appropriate here; mean cumulative PAR (irradiance that sea ice experience since its formation) remains relatively constant as ice grows older (Raymond et al., 2009) and sea ice breakup was not considered.

Fig 4. The C:N ratio appears higher than redfield ratio. It may be worthwhile assessing whether this could reflect the accumulation of carbon-rich matter over time (e.g., EPS, lipids, fatty acids), nitrogen limitation or other factors influencing orgranic matter composition.
We added the following discussion on this point in Section 4.1.;

"In accord with the previous studies, POC:PN ratio was generally higher than that in seawater and the median for Antarctic sea ice is around 10 (Dalman et al., 2025). This is partly owing to ice algae which have high C:N ratio even in nutrient-replete condition (Gleitz and Thomas, 1993). Under nutrient (NO_3^-) limitation, POC:PN could increase to >20 (McMinn et al., 1999) and it is more pronounced in productive spring season (Roukaerts et al., 2016; Dalman et al., 2025). EPS are rich in carbon and may accumulate in older sea ice as suggested by comparison of FYI and MYI (Meiners et al., 2003). Although there was no significant relationship between DSF and POC:PN (Spearman's rank correlation test, p=0.49), inferring the combination of nutrient status and EPS affected our results."

Section 4.4 acknowledges the potential influence of seasonal sampling on the regional POC estimate. The discussion could acknowledge additional assumptions underlying this first-order regional calculation, such as the representativeness of the sampled brash ice, the use of a regional mean melt thickness, and the comparison of POC supply with primary production estimates.
We added discussion on the potential variabilities in estimating POC supply and its contribution to production in the MIZ in Section 4.4.;

"It should be acknowledged that these estimates are averaged ones and the spatial or temporal differences are expected owing to variations in ice melt thickness (0.2–1.6 m, Komatsu et al., 2025), sea-ice POC concentration (Fig. 3c), and primary production in the MIZ (21.4–37.2 Tg year^{-1}, Arrigo et al., 2008)."

Line 456 – The value of 924 mg m-2 yr^{-1} is different in Table 2. Since Ackley et al. estimated carbon input using Chl data, would it be more appropriate to compare that estimate with the calculated ice algal carbon?
We corrected the value in the text. Although Ackley et al. (1979) took their estimate as POC (including non-algal carbon), we also calculated POC flux using Chl *a* data, considering that Ackley's estimation is ice-derived carbon;

"If we use our Chl_{VWM} value and POC:Chl *a* ratio (37) used in Ackley et al. (1979), the POC_{Input} was estimated to be 327.7 mg m^{-2} year^{-1}. The input of ice-algal carbon (IAC_{Input}) using Chl_{VWM} and IAC:Chl *a* was 535.3±188.1 (278.8–812.1) mg m^{-2} year^{-1} and our most conservative estimate was greater than that by Ackley et al. (1979), suggesting a significant underestimation caused by the measured sea-ice Chl *a* concentrations and the conversion factor from Chl *a* to POC."

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