We thank Christina Schallenberg for her constructive review of our manuscript and appreciate the time and effort she has devoted to carefully evaluating our work. Our point-by-point responses to her comments are provided below. Reviewer comments are presented in blue, our responses in black, and corresponding changes we will make to the manuscript are outlined where appropriate.

General comments:

This is a fine paper describing a thorough analysis of a regional dataset (from the northwestern Weddell Sea), while drawing on a wider data set to put the findings into context. It is interesting, well written, the analyses are clear and meaningful, and the results make a valuable addition to the body of knowledge. I only have very minor comments for improvement. The paper was a pleasure to read.

RESPONSE: Thank you for your positive feedback. We are very glad you enjoyed reading our paper ☺

Scientific comments:

L111 and 115: it's definitely 0.1 um? In the introduction it's 0.2 um (L46).

RESPONSE: Yes, we have double-checked, and 0.1 µm polycarbonate filters were used during the PICCOLO cruise (see also page 251 of the PICCOLO cruise report). However, the use of 0.2 µm polycarbonate filters to distinguish between particulate and dissolved matter is more common (as was the case during the AMT cruises). This is why 0.2 µm was mentioned in the introduction, in the context of the typical filter pore sizes used.

L137: So data between 0.0001 and 0.001 were kept, but if equal to 0.001 they were excluded? That doesn't sound right.

RESPONSE: We recognise this was not clear. We will therefore modify the sentence as follows, to improve clarity.

"*Quality control was performed on Chl-a measurements. Records with missing or non-positive total or size-fractionated Chl-a values were excluded, along with those with total Chl-a < 0.001 mg m^{-3} or size-fractionated Chl-a ≤ 0.0001 mg m^{-3}. Values of exactly 0.001 mg m^{-3}, used to indicate measurements below the detection limit in the 2016-2017 dataset, were also excluded. Records with comments indicating unsuccessful sample collection were also removed*."

L146-152: For the cruises that had the "wrong" pore size for the micro fraction (i.e. 5 or 10 um rather than 20), only the pico-data can be used, correct? Could that skew your data set, i.e. because you have way more pico data than any other size class? Or asked differently, how different would the results be if you only used data from the AMT cruises with the all the correct filters?

RESPONSE: Yes, this is correct, and you raise an interesting point. To investigate this, we repeated the fitting of the AMT data using only concurrent samples (i.e. samples for which both pico- and nanoplankton data were available; N = 769). It is worth noting that this issue only affects the picoplankton fits (parameters C_p^m and D_p), rather than the combined nano- and picoplankton fits (parameters $C_{p,n}^m$ and $D_{p,n}$), for which concurrent samples were already used.

For the picoplankton fits, the sample size was reduced from *N* = 1476 (NB: we corrected the numbers in Table 1) to *N* = 769. The results of these additional fits are presented in Table R3-1. We found no substantial differences in the estimated picoplankton parameters, with the uncertainties (±1 standard deviation) around the parameter estimates overlapping between the two datasets.

We therefore conclude that restricting the analysis to AMT cruises with the appropriate filter sizes would not substantially alter our results, and that the larger number of picoplankton observations in the original dataset does not appear to introduce a significant bias.

Table R3-1: Fitted parameters for picoplankton from fitting the three-component model of Brewin et al. (2010) to AMT data (as in Table 2 of the manuscript) and separately to concurrent datapoints where both pico- and nano-phytoplankton were available.

Parameter	AMT All data (*N* = 1476)	AMT concurrent samples (*N* = 769)
C_p^m	1.092 ± 0.089	1.051 ± 0.096
D_p	0.747 ± 0.007	0.754 ± 0.010

L235 (eqn 16): It would be good to add a reference for this equation (is it the Mobley (1999), or is that just for the value? It's unclear), and also a word of explanation (e.g., what is corrected with the 0.028 factor?). And is the Rrs then directly comparable to satellite Rrs?

RESPONSE: Thanks for highlighting the need for further clarification. We will modify the sentence preceding Equation 16 to explicitly reference Mobley (1999), as follows:

"*Following Mobley (1999), R_{rs} is calculated as…*"

The factor of 0.028, often referred to as the *ρ* (rho) factor, accounts for the proportion of sky radiance ($L_i(\lambda)$) reflected at the air–sea interface and contaminating the measured total radiance ($L_t(\lambda)$) when the radiometer views the sea surface. Subtracting this contribution from $L_t(\lambda)$ provides an estimate of the water-leaving radiance ($L_W(\lambda)$) which we divide downwelling irradiance ($E_s(\lambda)$) to derive R_{rs}.

The resulting R_{rs} is broadly comparable to satellite-derived R_{rs}, although a full normalisation has not been performed. We have not applied a bidirectional reflectance distribution function (BRDF) correction. This is because measurements were predominantly made under fully overcast conditions, when illumination is highly diffuse and relatively uniform across viewing directions, unlike the predominantly direct illumination under clear-sky conditions typically encountered during satellite observations. Although this correction can influence the magnitude and spectral shape of R_{rs}, its effect on spectral shape is generally relatively small. This is particularly relevant to our study, as we are primarily interested in deriving chlorophyll-a concentrations using band-ratio algorithms (Section 2.4.2).

We will modify the text following Equation 16 to clarify the meaning of the 0.028 factor, as follows:

"*...where λ represents wavelength (nm), and 0.028 represents the proportionality factor (following Mobley 1999) used to account for the contribution of reflected sky radiance to the total radiance measured when the radiometer views the sea surface*."

L275: what was the maximum time elapsed between CTDs that were matched in this way?

RESPONSE: For the 19 CTD matchups, the median time difference was 4.04 h (mean difference: 5.05 h), with a maximum difference of 12.02 h at the Supersite station. Including underway samples too, for the 34 matchups, the median time difference was 4.45 h (mean difference: 5.78 h), with a maximum difference of 21.35 h at a mooring station.

In addition to comments raised by Reviewer 1, we will now add the following sentences to Section 2.5

"*For the 34 matchups, the median depth difference was 2.14 m (mean difference: 0.91 m), with a maximum difference of 26.83 m, corresponding to depths of 70.17 m (SFF) and 97.0 m (HPLC). The median time difference was 4.45 h (mean difference: 5.78 h), with a maximum difference of 21.35 h at a mooring station. Additional details on the number of matched observations are provided in Table 1*."

L359-363: suggest merging this content into the paragraph L340-352

RESPONSE: Nice suggestion. The section will now read

"*Coincident R_{rs} and total Chl-a data from PICCOLO revealed a clear inverse relationship between Chl-a and the maximum-band ratio (R_{rs} (443 > 490 > 510)/R_{rs}(555)), which is characteristic of the three empirical algorithms tested (Figure 3a). This pattern is also evident in the validation plots, with all three algorithms showing high correlations between retrievals and in-situ Chl-a (Figure 3b–d; r ≥ 0.84). The globally tuned algorithm of O'Reilly and Werdell (2019) systematically underestimated Chl-a when compared with in-situ measurements (Figure 3a–b). For a given Chl-a, the maximum-band ratio is consistently higher (i.e., waters appear optically bluer) in the PICCOLO dataset than in the global relationship of O'Reilly and Werdell (2019) (Figure 3a). The underestimation of remotely-sensed Chl-a using a globally tuned algorithm (Figure 3a,b) in the northwestern Weddell Sea has also been reported by Salyuk et al. (2025). They attributed this bias to low chlorophyll-specific phytoplankton absorption (driven by high pigment packaging and the presence of large cells), low non-phytoplankton particle absorption, and low coloured dissolved organic matter absorption, relative to global ocean averages*."

L430: reword to "less nutrient limited"? Under nutrient replete conditions I would expect microphytoplankton dominating?

RESPONSE: Good point. We will now reword to "*less nutrient-limited*".

L424-442: This paragraph is not entirely convincing as it doesn't currently explain why nanophytoplankton dominate in the Weddell Sea (Southern Ocean) in the low-Chl regimes, rather than picophytoplankton (as observed on AMT). It offers some general concepts but doesn't mention iron limitation (which is likely shaping phytoplankton communities in the regions of interest) at all, so that's one point to be addressed. And a more direct explanation of the nanophytoplankton dominance would also be useful, even if it's just in the form of a hypothesis.

RESPONSE: Thanks for these helpful comments. We agree that the distinction between macronutrient availability and iron limitation is important when interpreting phytoplankton size structure in the region. Regarding the dominance of nanophytoplankton in the low-Chl-a regimes of the Weddell Sea, we will expand the discussion to consider how differences in environmental conditions, particularly iron availability, light and temperature, may influence the relative competitive advantages of different size classes. We will also introduce the

possibility that the nanophytoplankton community in the Weddell Sea comprises taxa with physiological adaptations to these conditions. We acknowledge that this remains a hypothesis, as our observations do not provide sufficient taxonomic or physiological information to identify the mechanisms directly. Specifically, we will add the following text to this section

"*However, the relationship between nutrient availability and phytoplankton size structure may be more complex in the Weddell Sea, where macronutrients are generally abundant but iron availability can limit phytoplankton growth. Iron limitation, together with low light availability and low temperatures, may influence the relative competitive advantages of different phytoplankton size classes. The dominance of nanophytoplankton in the low-Chl-a regimes of the Weddell Sea observed here, rather than picophytoplankton as observed in the oligotrophic AMT regions, may reflect differences in the physiological and ecological characteristics of the communities occupying these environments, with certain nanophytoplankton taxa particularly well adapted to the low-temperature and iron-limited conditions. However, this interpretation remains speculative in the absence of detailed taxonomic and physiological observations*."

L469-470: I think this sentence should be qualified by "at Chl concentrations <2 mg m-3" (or thereabouts), I and also suggest referring to Figure 5 here as it illustrates the difference between the Weddell and the lower-latitude regions

RESPONSE: Thanks, excellent suggestion. The sentence will now be revised to state:

"*At Chl-a concentrations <2 mg m^{-3} (or thereabouts), and relative to lower-latitude regions where picophytoplankton contribute substantially to the Chl-a standing stock, the northwestern Weddell Sea shelf and the broader region are dominated by nanophytoplankton (Figure 5).*"

L480-481: For someone not familiar with the concept of these slopes, this is difficult to follow. Maybe insert a sentence to explain the concept a bit? Does the slope relate to all biomass, not just phytoplankton?

RESPONSE: Yes, a normalised biomass size spectrum generally describes the biomass distribution across all organisms included in the spectrum, potentially spanning multiple trophic levels, rather than phytoplankton alone.

As suggested, we will modify the text to explain the concept more clearly, specifically we will now state

"*Therefore, a shift from pico- to nanoplankton dominance may provide a more efficient pathway for energy transfer from primary producers to metazoans by reducing the number of trophic steps. The normalised biomass size spectrum describes the distribution of biomass across organism size classes, with its slope indicating how biomass changes with increasing organism size. This shift may be reflected in a less steeply negative slope, indicating a more even distribution of biomass across size classes, with implications for more efficient energy transfer through the food web.*"

L503-505: As above, please qualify at what Chl concentrations this is true

RESPONSE: Agreed. We will now correct the sentence to state

"*Medium-celled phytoplankton (nanophytoplankton) dominate Chl-a at concentrations <2 mg m^{-3} in most of the northwestern Weddell Sea*."

Minor technical comments:

L94: refer to table 1 as well? That's the only place where the time of year gets mentioned

RESPONSE: Now done.

Figure 1: Consider giving the ice shelf a colour different from land? Make the land grey?

RESPONSE: We will now colour land a light grey shade in Figure 1. The new figure will look like this (see Figure R3-1 below)

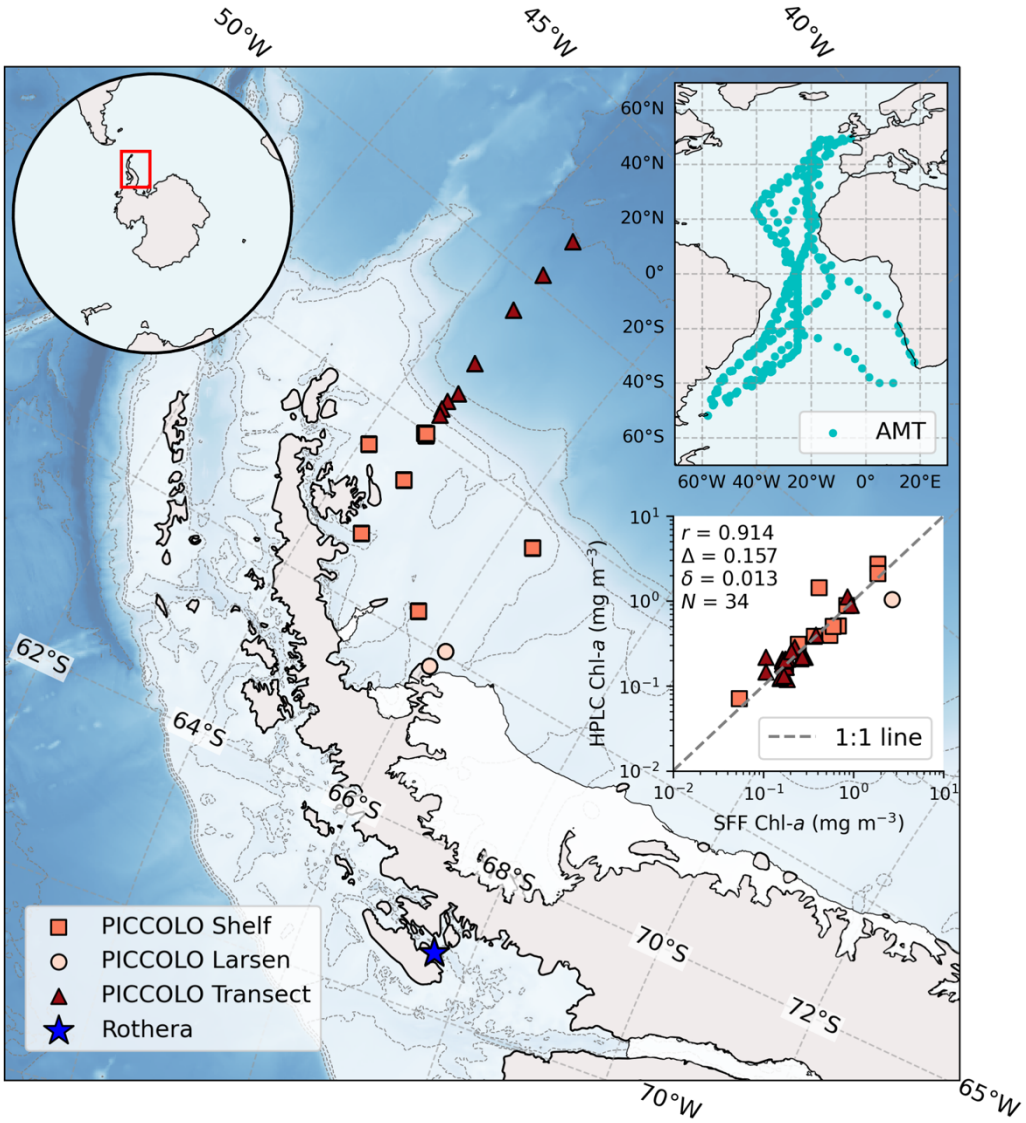


Figure R3-1: Updated Figure 1.

L163: include "and" after (Fn)

RESPONSE: Now done.

L167: Remove "be" between "can" and "then"

RESPONSE: Good spot. Now done.

L169: “derive” not “derived”

RESPONSE: Now done.

L187: missing a verb between “reconstructed” and “the weighted”?

RESPONSE: Now corrected.

L197, L204, L219: “…in each size class…”

RESPONSE: Good spot (thanks!). Now done.

L211: “…albeit with a different…”

RESPONSE: Good spot (thanks!). Now done.

L216: remove “to”

RESPONSE: Now removed.

L245: “…using a commonly…”

RESPONSE: Now corrected.

L253: remove “the” before “Southern Ocean waters”

RESPONSE: Now removed.

L333: specify “picophytoplankton” (replace “phytoplankton”)

RESPONSE: Now corrected.

L336: “Methods” is the wrong word at the beginning of the sentence; do you mean “Correlation coefficients”?

RESPONSE: Apologies this was not clear. We will clarify the sentence to state

“*DPA methods often produced systematically higher Chl-a for one size class (typically microphytoplankton) and consequently systematically lower Chl-a for another (typically nanophytoplankton, but picophytoplankton in the case of Chase et al. (2020)).”*

Thanks again for reviewing our paper.

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