Meyer et al. Ocean Science Reviews 10/26:

We thank reviewers 1 and 2 for their constructive comments. We believe they have helped improve the manuscript and hope our changes are satisfactory. Our responses to the reviewers are below in bold.

Reviewer 1:
I thank the authors for their responses and for the changes made to the manuscript. Several of my previous concerns have been adequately addressed, particularly the discussion of methodological uncertainties, the spatial and temporal mismatch between HPLC sampling and sediment-trap measurements, and the more cautious interpretation of the haptophyte signal in relation to Phaeocystis antarctica.

I continue to consider this a valuable dataset from an important and relatively undersampled region. However, after reading the revised manuscript, I think that two of the main issues raised in my previous review still need to be addressed.

1. PCC and POC export

This remains my main concern. The authors have now included additional caveats regarding the small number of observations and the limitations of the comparison. However, the main interpretation has not changed sufficiently. The PCCratio–POC relationships are based on only four sediment-trap deployments, and the relationships in Fig. 4 are not statistically significant ($p > 0.05$). Despite this, the revised manuscript still refers to a "strong relationship" and concludes that the potential predictive capacity "appears strong and holds across oceanic systems". I do not think these statements are supported by the present results. Acknowledging the small sample size does not resolve this concern if the manuscript still concludes that the relationship is strong or has predictive capacity across oceanic systems. The statement in the Abstract that the dominant phytoplankton groups "explain the majority ($R^2 = 0.37–0.82$) of carbon export" should also be revised. Besides the limitations of $n = 4$ and $p > 0.05$, an R^2 of 0.37 does not represent a majority of explained variance. I am not suggesting that this analysis should be removed. I think the relationships are interesting and worth reporting, but with the present number of observations they should be treated as exploratory and hypothesis-generating. The Abstract, Discussion and Conclusions should clearly distinguish between an interesting observed relationship and demonstrated predictive capacity.

We appreciate the reviewer's concern and have dampened some of the language to highlight these are initial (opportunistic) findings that require further investigation. See lines 22-24, 350-353, 368-371, and Lines 410-412:

Lines 22-24 (Abstract): "*Taken together, the relative biomass of the three dominant phytoplankton groups (39.7% diatoms, 30.2% haptophytes, and 14.9% cryptophytes) help to explain ($R^2 = 0.37 – 0.82$) the magnitude of carbon export to 150 m.*"

Lines 350-353 (Discussion): "*The substantial ecological differences between the Kramer et al. (2025) dataset and this study highlight the potential utility of this relationship to predict the magnitude of POC flux in differing ecosystems. While further in situ Antarctic sampling and analysis is clearly required, even the limited number of samples shown here suggest that a noteworthy relationship may exist.*"

Lines 368-371 (Discussion): "*However, this result suggests that it is not solely the relative diatom, haptophyte, or cryptophyte abundances that are a predictor of carbon export, but that diatoms + hacrobia together are the most*

robust metric at multiple depths and should be evaluated together against POC flux in remote sensing products and global models."

Lines 410-412 (Conclusions): "*While more research is needed to robustly test this relationship through space and time, particularly in multiple bloom stages and during a Phaeocystis bloom, the potential predictive capacity of diatom + photosynthetic hacrobia community composition to estimate the magnitude of POC flux is noteworthy and appears to hold across oceanic systems."*

2. Regional literature and novelty

I also think that my previous comment regarding the regional literature has only been partially addressed. The authors have added some references, but the revised manuscript still states that these data represent "one of the only HPLC datasets and one of the only phytoplankton community composition datasets from the Weddell Sea continental shelf". I am somewhat surprised that recent and directly relevant HPLC/pigment-based studies from the northern Antarctic Peninsula and adjacent Weddell Sea are still not adequately considered, particularly because this point was explicitly raised in the previous review. For example, Senger et al. (2026) includes an extensive field dataset from the northern Antarctic Peninsula, including Continental and Oceanic Weddell Sea sectors, and uses HPLC pigments, CHEMTAX and microscopy to investigate phytoplankton community composition and its contribution to carbon-cycle processes. Of course, sea–air CO_2 exchange and downward POC export are different processes, and I am not suggesting that this study previously demonstrated the PCCratio–POC relationship investigated here. However, its geographical, methodological and biogeochemical relevance to the context of the present study seems clear. My point is not that Senger et al. (2026), or any other particular paper suggested in my previous review, must be cited. Rather, if the scarcity of previous observations is used to establish the novelty of the study, the most recent and directly relevant regional literature should be adequately represented.

I also think the manuscript remains too attached to the idea of being "one of the only" datasets from the region. In my view, this is unnecessary and somewhat distracts from the actual value of the results. The study provides new observations from an important region, describes relevant spatial patterns in phytoplankton community composition, and combines pigment-derived community information with sediment-trap measurements. These results already provide sufficient novelty without the need to emphasize that this is "one of the only" datasets from the region.

We thank the reviewer for this comment. We have changed the language around being "one of the only studies". However, we have maintained that there are not many HPLC data from the Weddell Sea continental shelf. This is important for two reasons: 1) HPLC data are used to ground-truth remote sensing algorithms, so while other datasets (e.g., microscopy, imaging) are valuable, they are less relevant to the remote sensing connection we establish in the paper. 2) The West Antarctic Peninsula and the open-ocean Weddell Sea are quite different hydrographically and ecologically from our study region. We argue that phytoplankton communities and overall findings from these regions should not be considered representative of the Weddell Sea continental shelf. That being said, we have added relevant references, including Senger et al., as suggested. Please see lines 21-22, 58-61, 225-226, and 399-400:

Lines 21-22: "These are rare pigment-based community composition data from this key region for deep-water formation and carbon transport."

Lines 58-61: "Phytoplankton community composition sampling in the ice-heavy Weddell Sea is sparse, with most of our knowledge coming from several

independent cruise programs occurring broadly in the Weddell Sea sector of the Southern Ocean, over varying months and years (Kang and Fryxell, 1993; Schloss and Estrada, 1994; Wright et al., 2009; Xi et al., 2020; Flynn et al., 2023; Senger et al., 2026).”

Lines 225-226: “As a rare HPLC dataset from the Weddell Sea continental shelf, this study presents valuable observations to characterise the phytoplankton community and how it relates to the magnitude of POC flux in this region.”

Lines 399-400*: “As a rare HPLC dataset from the Weddell Sea continental shelf, this study presents valuable observations to characterise the phytoplankton community and how it relates to the magnitude of POC flux in this region.”*

Reviewer 2:
Overall, I acknowledge the improvements the authors have made to the manuscript. However, there are still a few minor points to improve referencing, some of the figure layout and clarity of writing (see below), As written before I think too little results are presented in the main manuscript, e.g. part of the supplementary material should be moved to the main manuscript, especially S4 should be integrated into Figure 2 and S5 should be a stand-alone figure in the main manuscript. Some figures require improvement, I made some more suggestions below.
We agree with the reviewer and have completed the suggested changes, moving S5 into the manuscript as Figure 5 and added Figure S4 to Figure 2.

Detailed comments:
1.) Lines 127ff: I acknowledge adding the information in the manuscript for motivating the Phytoclass method. However, the reasoning that the CHEMTAX method has no adaptation for the Southern Ocean is simply not correct, as there have been many studies using CHEMTAX and adjusting the matrices for the Southern Ocean. Here are some additional ones to the ones your cite: Alderkamp et al. 2012 doi: 10.1016/j.dsr2.2012.03.005, , Gibberd et al. 2013 10.1016/j.dsr.2013.04.007, Trimborn et al. 2017 DOI: 10.1016/j.dsr.2014.12.010).

We have added text to the methods acknowledging that *phytoclass* is not the only Southern Ocean-tuned chemotaxonomic algorithm. However, we maintain that is an appropriate algorithm to use for this analysis, based on its nearly identical formulation to CHEMTAX, which the reviewer notes here has been used many times in the Southern Ocean. See lines 114-116:

Lines 114-116: *“phytoclass uses the same basis and statistical assumptions as the CHEMTAX method (Mackey et al., 1996) but is an emergent method and one of several methods which are tuned for the Southern Ocean (Alderkamp et al., 2012; Trimborn et al., 2017).”*

2.) Chapter 2.4: Still misses a detailed description of the method how POC concentrations were determined from the samples.
We apologise for this omission and have added a description of the methodology. See lines 130-135:

Lines 130-135: *“POC concentration was calculated by dividing the mass of the particulate element by 600 ml. POC concentration was calculated by dividing the mass of particulate element by 600ml. Rates of POC flux were calculated according to the following equation (2):*

$$Flux = \frac{POC}{A * d}$$

Where M is the total POC mass calculated using POC concentration multiplied by the total canister volume (6.2 L), A is the area of the canister opening

(0.012m2) and D is the duration (days) the trap was drifting."

3.) Figure S3: requires revision, the lines of the temperature, salinity and fluorescence profiles are far too thin. **Thank you for pointing out this error with our figure printing. We have corrected it.**

4.) Line 236: Add a "," after "However". **Corrected as the reviewer suggests. See line 194.**

5.) Line 237_ states "the pigments exhibited strong variability, with haptophytes dominating…" it is not clear which pigments are meant here, as later in the sentence it is mentioning the derived phytoplankton groups only. Please revise to improve clarity. **We have added language to clarify which pigments. See Lines 196-198:**

Lines 196-198: "*Overall, the accessory pigments exhibited strong spatial variability, with haptophytes dominating at the Larsen-C Ice Shelf locations while diatoms dominated in the ice core and interface samples and at most non-Larsen water column locations (Figure 3G-J).*"

6.) Figure 2 has different y-axis ranges for all the subplots, which sometimes even do not show well the variation of Chl-a conc. among the different station data from the same depth (range). I suggest to use for all subplot (except the ones from the ice stations) the range until 3.5 or 4 mg/m2 and have the same style in labelling ticks to enable easy intercomparison for readers. (Mind now to integrate as columns in different color the results from Figure S4). **We agree with the reviewer and have standardized the axes between subplots for chlorophyll *a* and accessory pigments, leaving the ice samples as an independent axis. See revised Figure 2.**