This paper reports on a suite of biogeochemical parameters measured during an ice drift experiment in the Fram Strait. It is an interesting paper, because the conditions under which the measurements were made are challenging, and therefore under-represented in the literature. The suite of measurements is also rare, with these particular measurements rarely measured together. So... we have a paper with with a rare combination of measured variables, in a rarely measured environment.

It is also very well written. The paper follows a logical flow, and the results and discussion are well supported with good tables and figures. The authors clearly understand the limitations of their study, and do not attempt to overstate the significance of their results.

The only negative comment I have about this paper is that the connections between the different data types are not particularly strong. For example, there is clearly a theoretical link between dissolve greenhouse gas measurements and measurements of surface film concentration. But, with no direct flux measurements (e.g. by a chamber or eddy covariance) there is no analytical connection between GHG concentrations and SAS concentrations, and so we don't actually learn anything new about how these things interact. Similarly, there are interesting measurements of chlorophyll, nutrients, and phytoplankton abundances, all of which may impact GHG concentrations and SAS concentrations... But again, there is no strong analytical link between the two datasets.

So really, there is nothing wrong with this paper. And as I mentioned before, it is to the author's credit that they don't try to over-interpret the data and claim links that are unsubstantiated. But the lack of connections will limit the impact of this paper, and the editors will need to decide if it will have enough impact to be published in a strong journal like The Cryosphere.

I would certainly recommend publication, and only have a few minor comments that the authors might wish to address before publication:

Figure 1 and Table 1: I did not find it very easy to cross-reference between these two paper elements. It was difficult to understand where (spatially) certain measurements had been collected because the only reference was through date... Something like station names probably would have been easier.

Lines 170-175: I would have appreciated more detail on both the sampling and analysis of SAS. Here are some of the things I found confusing about this section:
-How certain are we that the glass plate method is successful in collecting all of the surface film? It seems that some materials might not adhere to it
-It wasn't clear why the samples were split into filtered and unfiltered bottles
-I don't understand what the voltammetry technique does... What is the fundamental principle of measurement that allows us to understand the SAS?
-I also don't understand the principle of addition of Triton X-100.

Figure 3: It is not clear whether all three variables reported here are averages (if so, over what period)? max, min, etc.

Figure 5: The categories in the caption (e.g. Bulk Unfiltered) did not match the description in the text (ULW).

Lines 265-275: This paragraph needs to be revised. At times, it seems like the authors have forgotten that they didn't actually measure a flux, they estimated a flux. For example, they write "the k660 of 2.5 cm/hr reported by Prytherch et al. (2024) is in good agreement with the average kSIC in this study".... This seems to imply that they measured k, when they didn't, they just calculated it. A similar issue emerges when they write "the average reduction in this study was 33%, which is close to the 30% reported by Prytherch and Yelland (2021). The authors just seem to sort of lose the point here a little bit, and I would strongly recommend revising this paragraph.