- Review Report -

General Comments

This manuscript presents the first in situ observations of surfactant accumulation in the sea-surface microlayer (SML) of the Fram Strait during the onset of Arctic sea ice melt (spring 2023), coupled with CH_4 and N_2O measurements. The study addresses an important knowledge gap: the influence of short-term surface processes on climate-relevant trace gas fluxes in polar oceans. The novelty lies in linking surfactant dynamics, algal bloom development, and greenhouse gas (GHG) emissions, providing new insights into how the SML may act as a natural regulator of CH_4 and N_2O fluxes in rapidly changing Arctic environments.

The manuscript is generally well-written, logically structured, and supported by robust datasets from a challenging field campaign. Figures are clear and informative, and the interpretations are scientifically sound. Overall, the study is valuable and has strong potential for publication, but the current manuscript requires several important clarifications and improvements as detailed below.

Specific Comments

1. Abstract (p.2, lines 24–31): The conclusion that surfactants reduced CH_4 and N_2O emissions is compelling but somewhat overstated. Please add quantitative uncertainty ranges for this reduction with specific numbers.

2. Introduction (p.3, lines 79–87): The introduction nicely describes CH_4 and N_2O sources/sinks in the Arctic. However, the information about the role of EPS and algal-derived surfactants is likely to be insufficient here. Please provide more information about the background of EPS in polar environments and their potential to form slicks, and etc.

3. 2.3 section (p.5, lines 135–136): Please clarify why poisoning was done with different $HgCl_2$ volumes for CH_4 (125 µL) *vs* N_2O (50 µL).

4. 2.3 section (p.5, lines 143–147): The time lag between sampling at leads/ice holes and sample poisoning is critical. I consider this the major weakness of the manuscript, and the authors should provide scientifically robust evidence to validate this assumption.

5. 2.5 section (p.7, lines 187–188): The thresholds of >200 and >1000 μg Teq L^{-1} originate from studies conducted in non-Arctic conditions (e.g., Wurl et al., 2011; Mustaffa et al., 2020). Since physical conditions in Arctic leads differ substantially, the authors should provide justification for applying these temperate thresholds to Arctic environments.

6. 4.2 section (p.13, lines 340–350): The authors interpret the June 5 CH_4 increase and N_2O decrease as possibly driven by shifts in microbial pathways. However, this explanation remains speculative without direct evidence (e.g., microbial rate measurements or isotopic signatures). The authors should present this more cautiously and highlight the need for the validation.

7. Table 2 (p.9): Negative CH_4 fluxes were observed at some stations. These are important and should be discussed in more depth.

8. Fig. 5 (p.10): The extremely high SAS concentration observed on June 5 is remarkable, approximately an order of magnitude higher than typical values. Moreover, the discrepancy between filtered and unfiltered samples is unusually large. This phenomenon requires a more thorough explanation.

9. 4.1 section (p.11, lines 265–275): The authors apply the parametrization of Butterworth & Miller (2016), which was developed for open oceans and marginal ice zones. Please elaborate on the limitations of using this parametrization in semi-ice-covered Arctic leads, where turbulence, wind fetch, and ice-edge effects may differ substantially.