The authors report surface-layer profiles of pCO_2, salinity, and temperature, which reveal pronounced gradients in these variables. They also present direct air-sea CO_2 flux measurements using an open-path eddy covariance (EC) system. I have many experiences with both open-path and closed-path EC systems. As also noted by the first reviewer, it is well established in the air-sea EC CO_2 flux community that open-path systems are sensitive to humidity cross-sensitivity (Landwehr et al., 2014; Blomquist et al., 2014; Nilsson et al., 2018).

The results presented here further suggest this issue: the strong correlation between the EC CO_2 flux and the heat flux (Fig. S7) is a typical indicator of cross-sensitivity effects. In addition, the back-calculation approach used by the first reviewer to infer an unrealistic pCO_2 value is, in my view, persuasive. Consequently, I do not have sufficient confidence in the EC CO_2 flux data presented in this study.

On the other hand, I find the profile measurements themselves to be very interesting and valuable. The data clearly demonstrate pCO_2, temperature, and salinity gradients from ~0 meter to ~10 meters in sea-ice melt regions, and notably show distinct vertical structures during different ice-melting periods. Even if the open-path EC CO_2 flux results were excluded, a manuscript focusing on these profile observations alone could still make a meaningful contribution to the community.

Minor comments:

- **Line 32:** Please define CO_2 at first use.
- **Line 34:** The assumption of homogeneity implies no vertical gradients. I understand that you may be referring to a linear gradient within the waterside mass boundary layer; however, the current wording could be misinterpreted as implying a linear gradient from the surface to several meters depth. I suggest revising to: *"The bulk approach assumes homogeneous surface conditions and no vertical pCO_2 gradients in the bulk seawater."* The sentence in line 36 can be revised accordingly by removing the word "non-linear."
- **Lines 37–38:** This sentence is unclear. Do you mean waters *at* 1 m depth? Please clarify.
- **Line 67:** You may consider citing Miller et al. (2019) here (https://doi.org/10.1029/2018GL080099).
- **Line 79:** Please add an appropriate reference.

- **Line 85:** "Most" may be more appropriate than "many."
- **Line 94:** For greater rigor, this statement could be revised to: *"Dong et al. (2021) illustrate that high-latitude CO_2 fluxes calculated using the bulk method (based on measurements at 6 m depth) differ significantly from those measured using direct eddy covariance in sea-ice melt regions."*
- **Line 135:** This figure originates from Liss and Slater (1974, *Nature*). You may want to indicate that it is adapted from Liss and Slater (1974) and Wanninkhof et al. (2009).
- **Line 176:** Please place the left bracket before the year and remove the comma.
- **Lines 212–213:** I understand the motivation here, but I suggest emphasizing that ensuring the robustness of the measurement technique should be the priority. Butterworth et al. (2025), cited later, demonstrate the feasibility of long-term CO_2 flux observations using a tower-based closed-path EC system.
- **Lines 253–254:** A reference appears to be missing.
- **Line 268:** Figure S2 has not yet been introduced in the text.
- **Line 452:** the derived skin temperature should be shown in the main text since this is a core parameter for the flux correction. I personally don't think the cool skin effect could be a significant factor that can affect the bulk flux for the data shown here. Because the cool skin effect is typically less than 0.2 K (Donlon et al., 2002), which is much smaller compared to the temperature gradients shown in Fig. 5. And 0.2 K will result in ~3 uatm decrease in the Δ*p*CO2 (Dong et al., 2024, Sci. Adv.), which is also much smaller than the *p*CO2 gradients.