Review of Schneider et al. (2025): "Air-sea gas exchange measurements helped derive in-situ organic and inorganic carbon fixation in response to Ocean Alkalinity Enhancement in a temperate plankton community"

The paper by Schneider et al. investigates the ecological and chemical consequences of non-CO2-equilibrated Ocean Alkalinity Enhancement (OAE) using a mesocosm experiment in a temperate fjord. The study's overarching hypothesis is grounded in existing literature, particularly the "white vs. green ocean" framework (Bach et al., 2019), and addresses known gaps in empirical OAE studies. Specifically, it aims to test whether varying levels of added total alkalinity (TA) using silicate- and calcium-based minerals alter carbonate chemistry, air-sea CO2 fluxes, calcification, net community production (NCP), and zooplankton respiration. This hypothesis is both relevant and timely, as most OAE research remains conceptual or model-based. Some explicit strengths, use of Gafar et al.'s (2018) CaCO3 Production Potential (CCPP) bridges cellular physiology with mesocosm-scale data. Estimation of zooplankton respiration as the difference between oxygen-based and DIC-based NCP is innovative and revealing.

2. Experimental Design and Methodological Soundness

The methodology needs to be improved and supplemented with the method description (and results) currently missing.

- One of the biggest limitations is that there was only one direct DIC sampling point for validation introduces uncertainty into TA-pH derived DIC estimates. Such uncertainties have likely propagated through time but that has not been addressed in the paper. In addition, NCP is derived from changes in DIC — so uncertainty compounds over time. This needs to be addressed and evaluated.
- Methodology of how preparing different feedstock solutions is not described in sufficient details. Artificial separation of silicate and TA effects may not represent real-world OAE mineral additions. How were the concentrations of all the feedstocks measured to assure that the concentrations at the end were correct?
- How much NO3-, Si and Si(OH)4 and Ca2+ were added, mention specific numbers. Why was NO3- added to up to 4umol/kg, which is at least 4-5 times higher than in a fjord, creating completely artificial conditions for those communities inhabiting fjord? And subsequently, how do you know that this is a natural response of the communities acclimatized to low nutrient levels, instead of artificial response that might be out of scope if OAE without the added nutrients would happen? Can you decouple this effect somehow and include this in the discussion and results section?
- What depth was N2O taken, up to 20 m or the surface, not clear from the text.
- Respiration measurement description is missing.
- The description or the reference to the flow cytometry analyses is missing.

3. Data Collection and Analytical Approach

Analytical concerns:

- The pH measurements required dye corrections due to potential impurity artifacts—highlighting the fragility of spectrophotometric pH at high alkalinity. Can you comment and revise?
- Assumption of 1:1 O2:C ratio in NCP calculation may oversimplify complex respiration dynamics. How do you rectify this? In which range does this ratio hold? Could that be different for the respiration of the micro vs large zooplankton (above 280um)?
- Data on respiration is missing entirely.
- The large variability of DIC upon the nutrient addition is overwhelming and not well explained, also not matching the trends in the other parameters. Provide better explanation.
- Where did you take the 95% for full equilibration from?
- No coccolithophores or diatom data presented??? It is literally impossible to draw some of the results and conclusion in this paper unless there is data available for this.
- Are there any taxonomic or metagenomic assessments to resolve zooplankton community and why the decision on cutting it at 280um?

3. Results and Interpretation

Calcification: Coccolithophore calcification followed an optimum curve relative to pCO2, with a peak around 250 µatm and suppression at extremes. However, in the figure S4a, calcification is below 0 for the two highest treatments, which is not explained anywhere in the text. Does this indicate dissolution. Even less severe treatments are still just hardly above 0, especially before the addition of nutrient part, which signifies lack of calcification overall, and only just happening in the first three treatments. How does this align with the NCP, can you correlate? And how does it align with the CALC, could it have any effect on the TA? Is this species-specific, could it be due to any other calcifiers (not just the autotrophs)? In general, the drawback of this is also that no other potential calcifiers have been implicated in the CALC, only the autotrophs. Are there any data available to support this, or discount for the impact of zooplankton on the CALC?

Net Community Production (NCP): NCP was significantly higher in silicate treatments post-fertilization, with no direct pCO2 effect. But the effect of the pH was not investigated and should be included in ANOVA. Also, why is NCP related to Si and not to Ca2+ treatment- again, data on diatoms and phytoplankton are absolutely essential, otherwise this all on the level of inferences.

Also, how does Chla correlate with NCP and Calcification (Figure S4a-c and S1f)?

Zooplankton Respiration: Respiration declined with decreasing pCO2 and was lower in Si treatments, but in general, this aspect is largely underexplored and insufficiently presented. Much more effort needs to be put in explaining respiration data and how it links to suggested trophic-level complexity. Present the data on respiration beyond 2 days, 2-day data is insufficient, compare the pre and post nutrient treatment.

The results of respiration are also fundamental in explaining some of the effects and should be put in the Results, not Discussion, section.

Discussion:

In general, this study is really divided in two parts:

- Pre-nutrient treatment that is represented of the fjord environment under OAE and post-nutrient that is NO LONGER representative of the oligotrophic fjord conditions, whereby the used communities were not acclimated to such increases in nutrients and is just a mesocosm trial of OAE with nutrients. In such systems, the communities and species could react completely differently than under such artificial conditions. This aspect is now touched upon in the results and discussion and I would like the authors to fully dedicate the effort on the potential confounding effects due to such nutrient addition and how different the fjord system response to OAE would be if such strong nutrient artificial addition was not present- Fco2 still high, but NPC insignificant, what about respiration etc?
- In addition, no evaluation of the longer-term dynamics to capture seasonal or successional effects is presented.