Review of the manuscript “Sea ice loss translates into major shifts in the carbonate environmental conditions in Arctic Shelf Sea” by Hauri et al., 2023

Summary:
The manuscript reported time-series observations of a suite of hydrographic and biogeochemical parameters (T, S, pCO_2, pH, NO_3) in the subsurface water (at 33 m depth) in the Chukchi Sea from 2016 to 2020. The dataset is very interesting and valuable for better understanding the rapid changes in carbonate chemistry, ocean acidification, as well as the impacts on Arctic organisms and the ecosystem. The authors focused on examining seasonal and interannual variability and discussing the controls of the carbonate system. They found that the subsurface waters have very high CO_2, low pH and Ω in summer and fall, which is due to remineralization of organic matter. They further identified two periods (spring and late fall) with relatively lower CO_2 and higher pH and Ω, called “ocean acidification relaxation events”. The time-series data well explained the causes and drivers for these events, which provides new insights into seasonal variations in the carbonate system in the Chukchi subsurface waters.

However, I cannot recommend the manuscript for publication in its current version because I have several concerns that need to be addressed. I listed my comments below:

Major comments:
Title:
I find that the title of this manuscript is a little bit too ambitious. One mooring location may not represent all Arctic Shelf Seas. Maybe consider changing “Arctic Shelf Sea” to “the Chukchi Sea”.

Salinity-normalized data
Although the authors pointed out that freshwater can affect the inorganic carbon system and they attempt to normalize DIC and TA to a reference salinity, they didn’t clarify the freshwater end-member they used for salinity normalization. Since normalized data are critical for further quantitative analysis, they should be clearly presented.

In addition, it is not clear to me if the authors used normalized data in Results and Discussion sections because they presented changes in DIC, TA, and NO_3, not nDIC, nTA, nNO_3, thus it is really hard to evaluate their data interpretation.

Minor comments:
Line 25 Need to define “pCO_2” the first time it appears.

Line 37 I feel that “CO_2-depleted surface water” is not a very accurate description. The authors need to explain how they define “depleted”. Maybe “low pCO_2 surface water” is good enough.

Line 37 I feel that ice formation-induced water convection in late fall may also play a role.

Line 56 The citation is not complete.

Line 67-71 Please carefully check throughout the entire text and modify the terms as "$CaCO_3$", "CO_2", CO_3^{2-}, "pCO_2", and "Ω_{arag}". I cannot list all the wrong spell here.

Line 95 Need to define CEO at the first time it appears

Line 190-192 Should Move to 2.5 CTD and Oxygen section. How about DO data collected from Aug 2017 to Aug 2018?

Line 220 What is CEO-2? Need to explain.

Line 220 …mooring near the HydroC…

Line 264 I don't understand why the r^2 for the validation part is even higher than that of the training part. Why select data from 15/9/2017 to 14/3/2018 as the training dataset?

Line 253-255 The parameter symbol of α (not a) should be consistent throughout the text and equation.

Line 288-289 TA, DIC, and Ωarag (Figure 2 i-k) were calculated based on data from the HydroC pCO2, pHest and S, T, and pressure (P) from the SBE16.

Line 306 ~~(~~Sulpis et al. (2020) found that…

Line 323 Please cite the literature for supporting this statement "also at times by the Mackenzie River outflow from the eastern Beaufort Sea and the large Russian Arctic rivers located to the west of the Chukchi Sea."

Line 331 $nP = (P - P(s=0))/S^* Sref + P(s=0),$ (2)

Line 336 Not clear how the authors determine the freshwater endmember for DIC, TA, pCO_2, and NO_3. I suggest listing all estimated freshwater end-member in a table in the supporting information.

Line 337 I don't think pCO_2 can be directly normalized by salinity because it doesn't change linearly with salinity.

Line 336-338 How does the brine rejection process during ice formation affect seasonal variations in these variables? Especially during the winter.

Line 338 and 345 Should be (Figure S3) and (Figure S4)

Line 344 $pCO_{2,NT} = pCO_2 * \exp(0.0423(T_{ref} - T))$ (3)
Should list the equations in a number order

Line 346 …surface waters were entrained to…

Line 348 "pCO_2"

Line 357 Please explain why selected 51% of sea ice concentration as a threshold. People normally use <15% as a threshold for open water.

Line 375 Needs to define "springtime" in this study.
"higher pH and Ω_{arag} and lower pCO_2" compared to what? The overall means?

Line 377 I don't see the spikes in June of 2020

Line 384 If the oxygen data is already known as bad data as shown in Fig. S2, how can the author trust the oxygen changes can be used for quantitative analysis?

Line 388 Not clear how the authors estimated TA increase with the assumption of NO3 consumption. No observation of NO3 in the 2018 spring. Need more explanation.

Line 404 If the water column is well-stratified, does the observation at 33 m reflect the surface mixed layer or bottom layer at this period? If it is reflecting the variation of the surface mixed layer, the CO2 air-sea exchange should be considered. If it is reflecting the variations of bottom layer, how $CaCO_3$ mineral dissolution in the surface layer increases TA in the bottom layer?

Line 407 Needs to define "fall" in this study
…drops in pCO_2 ?

Line 431 There is no figure (Figure 7) showing changes in Ω_{arag}

Line 458 DIC changes or nDIC change?

Line 491 NO3 decreased below 10 µmol kg^{-1} shown in Fig. 2f

Line 514 the authors need to clarify that they are comparing the acidification rate at the surface vs. in the water columns

Line 515-516 I didn't see the result of the statement "The weaker trend was calculated with data starting in 1994, whereas the stronger trend used data starting eight years later." in Qi et al. (2022). Need to explain how the author concluded that.

Line 520 Please clarify if the model results were derived from the depth of the CEO.

Line 604 …pH (~~e.g.~~ Gianguzza et al., ~~(~~2014).

Line 625… and lower pCO_2, were driven by a combination

Line 627-629 It is not necessarily and exclusively to be sea ice algae, because the water column is not well-stratified in the spring, the phytoplankton is also able to grow at the subsurface or to be entrained into the subsurface.
Please check Arrigo et al., 2017 and Ouyang et al., 2022

Arrigo, K. R., Mills, M. M., van Dijken, G. L., Lowry, K. E., Pickart, R. S., & Schlitzer, R. (2017). Late spring nitrate distributions beneath the ice-covered northeastern Chukchi Shelf. Journal of Geophysical Research: Biogeosciences, 122(9), 2409-2417.
Ouyang, Z., Collins, A., Li, Y., Qi, D., Arrigo, K. R., Zhuang, Y., ... & Cai, W. J. (2022). Seasonal Water Mass Evolution and Non-Redfield Dynamics Enhance CO2 Uptake in the Chukchi Sea. Journal of Geophysical Research: Oceans, 127(8), e2021JC018326.

Line 693 “dataset”

Line1273 I suggest enlarging Figure 2 in the y-axis scale. Since this is the most important and valuable figure in the manuscript, it should be made more readable.
Also, it is really hard to see the measured pH (grey line) in Fig. 2c. Suggest changing it to another color.

Line 1286-1287 Please enlarge the green diamond and its error bars in the figure to make it more readable.

Supplement

Line 7-8 the format of the date should be consistent with the label in the figure.

Figure S3 Please enlarge the labels to make them more readable. Also, suggest changing the normalized data to a more readable color.

Figure S4 Suggests changing the temperature-normalized data to a more readable color.