

Interactive
Comment

***Interactive comment on* “Seasonal pH and aragonite saturation horizons in the Gulf of Alaska during the North Pacific Survey, 1956–1957” by S. McKinnell and J. R. Christian**

Anonymous Referee #1

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This short paper on pH data from the mid 50s is a gem in revealing how well some carbonate system measurements were made 50 years ago. While it is certainly possible to take issue with the accuracy of these data, the author is undoubtedly correct in suggesting that the trends observed are meaningful and reveal much about biogeochemistry and circulation in the Alaska Gyre.

I am less enthusiastic about using this data to calculate the degree of aragonite saturation and the comparison with the saturation states determined by Feely et al (2008) along the North American Continent. In this regard there several chemistry issues that I believe are not well enough acknowledged:

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(1) It is appropriate to spend a significant amount of time discussing the pH scales used, but that is not the only potential problem with data like this. The pH measurements were almost certainly made using a glass electrode although this is never actually acknowledged in the paper. Glass electrodes have liquid junction potentials that effect accuracy because the ionic strength of the buffers used is different than seawater. This causes a significant accuracy error that should have at least been acknowledged here.

(2) Another serious problem with the saturation calculations is the estimation of the alkalinity using the salinity-temperature regressions. (What regression was used?) If you take a look at alkalinity data from the North Pacific published by Wong et al in this decade you do not see nice trends with salinity in surface waters, presumably because it is an area of CaCO₃ formation. Thus, extrapolating a trend from this area today to 50 years ago could easily be very wrong, depending on the extent of CaCO₃ formation in the surface waters at these two times.

(3) Finally, I am not sure of the usefulness of comparing data at sigma theta 26.2 in near surface waters of the subarctic pacific with similar data on this density horizon along the coast of North America. What is the surface origin of waters that upwell on the coast? How different is the history of waters from these two regions? It is likely that the respiration history of these two waters is different, which would make their carbonate ion concentrations different - even without the effects of anthropogenic CO₂.

In spite of these criticisms, I find this paper very interesting for the historical content and for the insight it brings to the carbonate chemistry of the region around one of the world's most important time series locations.

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