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Interactive comment on "Ocean acidification from 1997 to 2011 in the subarctic western North Pacific Ocean" *by* M. Wakita et al.

Anonymous Referee #1

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Wakita and colleagues present a 14 year record of ocean CO2 system parameters in the western subarctic gyre of the North Pacific, combining data sets from Stations K2 and KNOT. They find that surface pH is declining slower than expected from atmospheric CO2 penetration alone, and suggest that enhanced dissolution of CaCO3, which raises total alkalinity, may be responsible for the discrepancy. They also find that subsurface waters (200-300 m depth) are exhibiting a more rapid pH decline than expected, perhaps due to enhanced organic matter remineralization.

Although this is an impressive dataset, the present analysis is only slightly (2 years) updated from previously published work from this group. Nevertheless, the authors have done a nice job of explaining the observed trends in pH and DIC and in apportioning the contributions of various physical and biogeochemical contributions to these trends.

There are three areas in which I think the authors need to make some important clarifications/improvements:

1) In Section 3.2, the higher rates of pH decline at depth can be a bit misleading; because pH is on a log scale and pH declines with depth, a given rate of change of pH at depth actually represents a greater rate of change of [H+] than the same rate would represent at the surface. The authors should consider whether their rates of pH decline at various depths actually represent different rates of [H+] accumulation.

2) In Section 3.4,the apportioning of DIC changes seems to leave out the potential for divergence of lateral transports (advection) of DIC. Is there evidence for net advective transport of DIC to or from this location?

3) In Section 3.4, changes to DIC driven by remineralization of organic matter (resulting in AOU) are considered. However, fixed stoichiometries of organic matter remineralization are utilized in the calculations. If there have been changes in community structure/ foodweb structure over the period of observation, these ratios may not have remained constant. Some quantitative effort to consider the potential effects of changing "Redfield" ratios on the DIC apportionment should be implemented.

Also I have a few minor issues to point out:

1) Para. 8286 line 3: Revelle Factor should be defined for the non-specialist. 2) Para. 8288 lines 14-16: There should be parentheses around the stoichiometric ratios for clarity. 3) Para. 8291, lines 2-3: "...favors the uptake of CO2..."; actually, CO2 is outgassing at this time of year, not being taken up. So instead of "favoring" CO2 uptake you are actually "suppressing" CO2 emission. 4) Figures 1,3, 4 and 7 need bigger font sizes to be readable. 5) Figures 2 and 5 need to be resized larger, AND given larger font sizing in order to be readable.

End of Review.

Interactive comment on Biogeosciences Discuss., 10, 8283, 2013.