

Interactive
Comment

Interactive comment on “Interannual variability of air-sea CO₂ fluxes and carbonate system parameters in the East Siberian Sea” by I. I. Pipko et al.

Anonymous Referee #3

Received and published: 11 May 2011

The manuscript presents hydrographic and carbonate system measurements from three field studies that took place in the East Siberian Sea during August 2003, 2004 and 2008. The primary conclusions are that the carbonate system (magnitude and distribution) is strongly influenced by organic matter degradation (the organic matter primarily originating from river runoff and coastal erosion), river inflow and meteorological forcings. The authors support these conclusions by examining correlations between, for example, pCO₂ and suspended particulate matter and by qualitative descriptive associations. The data are interesting but have apparently already been reported in a previous paper – this needs to be clarified or the paper withdrawn. My other primary concern with the paper is that there is no reason to believe the calculated pCO₂, upon

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive
Comment

which most of the conclusions are based. This arises because the $p\text{CO}_2$ was calculated from pH measured with glass electrodes calibrated using NBS buffers. These buffers are not appropriate for electrode calibrations for seawater measurements (see DOE 1994) and can lead to large systematic errors. There are no uncertainties (accuracy or precision) quoted for the pH. The only way the accuracy can be rigorously established is by independent measurement of another carbonate parameter such as dissolved inorganic carbon, by pH measurement of an appropriate seawater buffer (tris) or by direct comparison with spectrophotometric pH measurements. A thorough discussion of the accuracy of the calculated $p\text{CO}_2$ must be provided. I also agree with the other reviewer that overall the paper is poorly organized and needs to be rethought and corrected for grammatical errors. There are paragraphs that have no logical order and many redundant sentences. Other specific comments are as follows:

P. 1231, paragraph 10: refer to Figure 3 to make this more clear. p. 1233, 15-20: $E_x =$, $E_m = ?$ this is wavelength but it is not obvious. p. 1234, 5: why is Schmidt number mentioned here? Figure 4: the legend has fine print that is not easy to see. simplify these figures Plot and compare Alkalinity versus salinity curves for the three years p. 1238, 15: low light levels are frequently mentioned but PAR data are not shown. Show profiles of PAR from the 3 years, in different areas, to support the arguments p. 1240 (top): report the actual change in $p\text{CO}_2$, not the temperature coefficient (also, it's Wallace, not Wallase). p. 1240 (bottom): here Alk-S is mentioned with a non-zero offset on the following page? Show these correlations. p. 1241, 20: the riverine DIC and Alk endmembers are less than the seawater levels, but that is not what is said here. Perhaps you mean the salinity-normalized values? However, there is still much less inorganic carbon in the freshwater source.

Heterotrophy of allocthonous organic matter is cited as the source of the high $p\text{CO}_2$ yet is it possible to have high rates of remineralization in these cold waters? These statements need to be supported by literature measurements of respiration. Are there other possible sources of high $p\text{CO}_2$ (e.g. loss of alkalinity relative to DIC, or when you

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

mix the river and seawater endmembers conservatively, can this result in high pCO₂?). Please address these other possibilities.

p. 1246, 25: The atmospheric pressure pattern needs to be more clearly connected to the observed biogeochemical distributions. It leaves a lot up to the reader to make these connections. In this same paragraph SPM in 2008 is mentioned but no SPM data from 2008 are presented. p. 1248 (top): It's helpful if gas fluxes are also reported with units of mol/m²/yr for comparison to annual rates in other areas, even if there is ice cover for most of the year.

There are a number of papers that describe the carbonate system of the Chukchi Sea (Bates et al. papers, etc) that provide important end member information for the eastern ESS. These should be cited and discussed.

Interactive comment on Biogeosciences Discuss., 8, 1227, 2011.

BGD

8, C913–C915, 2011

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C915

