

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 #2

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The manuscript reports a data set of measured pH and alkalinity (AT), calculated pCO₂ (from pH and AT) and air-sea CO₂ fluxes in the East Siberian Sea in September 2003, 2004 and August-September 2008. Based on these data, the authors suggest that the ESS can be divided into two distinctive biogeochemical regions: the western part of the ESS represents a river- and coastal erosion-dominated area that is heterotrophic and a source for atmospheric CO₂, while the eastern part of the ESS is a Pacific water-dominated area that is autotrophic and a sink for atmospheric CO₂. Interannual variability in parameters of carbonate system is also discussed. When I first read this manuscript, I thought it was nice to see a new data set in a marginal sea of high latitude (particularly a multiple years' data set), where becomes more and more important

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on estimating the global oceanic CO₂ uptake, due to the impact of climate change. However, when I went back to one of the authors' earlier publications (Semiletov et al., 2007; JMS), I have a concern on if this manuscript is appropriate to be published in Biogeosciences. Data of September 2003 and 2004, which have been published in Semiletov et al. (2007), are used for this manuscript again. Figures 9 (a) and (b) in this manuscript are almost the same as Figures 11(A) and (B) in Semiletov et al. (2007). Data of 2003 and 2004 in Table 2 of this manuscript are very similar to Table 4 in Semiletov et al. (2007). Please note that air-sea CO₂ flux is the key information provided in this study as indicated by the title. Most importantly, after carefully went through this manuscript again, I still get an impression on that the authors present the data in a manner as they are all new and have never been published. Overall, I think the authors have something interesting to contribute, but the whole manuscript needs to be rewritten with an explicit explanation on the relationship between the present study and the authors' previous publication(s). I suggest the authors to develop a fully independent paper with new data (2008 data), in which they can compare the new data set with their previous work(s) in depth. I think that it would be more helpful and healthy to our community. Because of the major concern I identified here, I do not provide detailed editorial suggestions.

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