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Interactive comment on "The dynamics of the carbon dioxide system in the outer shelf and slope of the Eurasian Arctic Ocean" *by* Irina I. Pipko et al.

Anonymous Referee #1

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This paper presents pCO2 data and associated air-sea flux of CO2 from the Eurasian sector of the Arctic Ocean for three years (2006, 2007 and 2009). Data in this region are extremely scarce due to the logistical difficulties involved. As such, this paper makes a valuable contribution to our understanding of CO2 exchange between the atmosphere and the Arctic Ocean at a time when the latter is undergoing rapid change. The authors have followed up various lines of thought to explain inter-annual and regional differences. I particularly liked the separation and apportionment of freshwater sources (MW and RW). The work presented is substantial, the analysis is very thorough and the paper is well structured and well written. The written style varies slightly between sections, probably reflecting the fact that different authors had written different

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section – a consistent writing style may slightly improve the manuscript in this respect. I enjoyed this paper and have no hesitation to recommend its publication. I have some specific comments which are outlined below. I would leave most of my comments at the authors' discretion, but I would urge them to address comments 6 to 9 in particular. Specific Comments:

1) Lines 79-84: A couple of useful references might also be: Mann et al., 2012, doi:10.1029/2011JG001798 and Mann et al., 2015, doi: 10.1038/ncomms8856

2) Line 129: At what depth was the intake for the pumped seawater?

3) Line 135: Please give batch numbers for carbonate CRMs

4) Line 146-147: Does the 30-minute averaging have an effect on accuracy? Over 30 minutes a moving ship may cross fronts, river-plumes, marginal ice zones etc. Averaging would therefore smooth if not obscure any gradients in pCO2.

5) Line 255: The high Oxygen supersaturation observed in the Barents Sea is intriguing. Clearly, temperature alone explains 84% of the variance in pCO2 and the authors are correct to point out the air-sea exchange may not have fully compensated for earlier biological drawdown of CO2. Typically, the turnover of the surface mixed layer CO2 via gas-exchange is in the order of months because of carbonate buffering. In contrast, Oxygen will re-equilibrate with the atmosphere in days/weeks. Simultaneous CO2 undersaturation and O2 oversaturation would therefore suggest very recent PP. Satellite Chlorophyll might give additional insight should the authors wish to expand their analysis.

6) Line 318-320: The authors state that "optically-active OM and suspended material... promotes the accumulation of solar radiation... which decreases the heat content [leading to further ice melt]". I don't think that OM and SPM contribute hugely to the heat content of surface waters. The big switch from high albedo with ice-cover to low albedo in ice-free water would have a much bigger effect than the absorbing constituents such

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as OM. Nevertheless, I do believe that OM is hugely relevant here since OM will undergo photolysis to CO_2 (e.g. Mann et al., 2012, doi:10.1029/2011JG001798). This is in addition to microbial OM mineralization which the authors have covered.

7) Line 330: In relation to pCO_2 supersaturation in the East Siberian Sea, the authors state that this was due to the atmospheric pressure gradient which diverted river water offshore. I presume that this would carry high OM to the ESS which would be further mineralized to CO_2, hence the elevated pCO_2. It would be worth stating this explicitly as the current text leaves it up to the reader to make that connection. If the reader fails to make the connection, then the message is lost.

8) Line 351-355: Regarding the flux of CO_2, the authors state that the highest influx coincided with high wind, while the highest $DpCO_2$ did not result in very high influx because of low wind. The authors have used the cubic relationship of Wanninkhoff and McGillis (1999) between k and wind speed for calculating the flux. There is a wide spectrum of Kw-wind relationships and the one used here returns k values at the upper end of the range. I wonder whether a middle of the range formulation might be better while the separate debate regarding the Kw-wind relationship goes on in the air-sea gas exchange community. Wanninkhoff, 1992 would certainly be ok here. On line 190 it is stated that the W92 formulation was used, but there is no further mention of it in the results (?).

9) Lines 363-364: The authors state that hourly wind speed improves the estimate of CO_2 uptake capacity (line 363-364). I have a technical objection to the use of the term "uptake capacity" (or "uptake intensity" on line 368). What do these mean? Surely, we are talking about "flux", so why not stick to that term and avoid ambiguity? Whether hourly wind-speed improves the estimate of the flux is somewhat irrelevant given that the flux depends so much on one's choice of k formulation. This alone makes a difference of 50%, if not more at high wind speeds. Each parameterization of Kw has its limitations and is calculated over different time-scales so it may or may not be appropriate to apply this to hourly wind data. I would simplify this discussion by not going

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into such details of air-sea exchange. In my opinion, it's fine to clearly state how the flux is calculated here and move on to the other sections. Statements regarding improvements of the flux by hourly vs. daily wind speed are beyond the scope of this paper.

10) Figure 1: It would be informative to also plot the 2007 sea-ice extent on panel b of Figure 1.

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