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 #2

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

The paper illustrates the surface pCO2 distributions in the Arctic Ocean and the associated air sea CO2 fluxes within wide and shallow shelves of the Eurasian sector, which can be affected by intense exchanges at the air sea interface. In addition, spatial and temporal variabilities are presented together with different drivers of the marine carbonate system in one of the most sensitive regions to climate change and ocean acidification. The region has been undergoing rapid changes for the last decades. The collected data refers to three seasonal campaigns, conducted in late summer/fall 2006, 2007 and 2009, characterized by different meteorological conditions. The spatio-temporal variability and the different drivers are thoroughly analyzed and well discussed, while results are clearly presented. In my opinion the objectives of this study are clearly presented and fully reached. The paper can add valuable contribution to the knowledge of CO2 fluxes in a polar region, where dearth of data is limiting. I enjoyed the paper, in particular the introduction and the discussion on the response of marine carbonate system to the different drivers well enlightening the complexity of the system. I believe it is worth of publication. Nevertheless I would recommend some minor revisions summarized in the specific comments.

Specific comments:

1) It seems to me that title does not fully mirror the focus of the paper, mainly addressed to the upper layer properties, distributions and dynamics. . . . If you agree would you mind suggesting this even in the title?

2) Line 30: more caution should be used about “a growing CO2 evasion occurs . . .” as the estimated fluxes from the sea to the atmosphere (in Tab 1) are really very low! Wanninkhof and McGilis (1999) are reported to underestimate fluxes at low wind speed, that seems the case. I don’t mean to open discussion about the best parameterisation (for instance Nightingale et al. 2000 might be suggested). I accept the author’s choice but please be cautious about results. I rather would say that uptake was strongly weakening under 2007 environmental conditions as surface seawater appears in equilibrium with atmosphere . . .

3) Paragraph 2.2.2: author should provide the temperature conditions of analysis. Titration has been performed at constant temperature ? and which one ? Due to the variety of analytical methods and measurement units, the international community working on marine carbonate system has decided to adopt common protocols (requiring the analysis at constant temperature, and common measurement units) Protocols reported by Dickson et al 2007 that authors cite, are recommended.
4) Paragraph 2.2.3: indicate the scale of the pH measurement and again the temperature. The international community working on CO2 fluxes, ocean acidification and impacts, has decided to adopt common protocols and common measurement units in order to increase utilization of data among different scientific communities. This uniformity would increase a wider utility of the paper. Protocols reported by Dickson et al. 2007 that authors cite, are recommended. Please refer to them for units and scale. Line 140-141: authors should provide the accuracy of the method, for consistency with TA. This can be done by calibration against the reference materials (CRM’s supplied by Dickson) and using CO2SYS for calculating the pH of CRMs at the temperature of analysis.

Specific comments at point 3 and 4 are necessary also for the next paragraph (2.2.4), where CO2SYS programme is mentioned. This could be useful to non-expert (of carbonate system analysis) readers.

5) Paragraph 2.2.4, lines 148-149: in order to prevent misunderstanding and not confuse direct continuous pCO2 measurements (by SAMI CO2 sensor) with the calculated pCO2 from discrete samples (collected by Rosette), I suggest to specify “At oceanographic stations surface pCO2 values were calculated, on discrete samples, from pH\textsubscript{25}, AT and inorganic nutrients data using CO2SYS…” In addition authors should say which constants for sulfate and borate (K\textsubscript{SO4} and K\textsubscript{Borate}) have been chosen in the CO2SYS programme.

6) Lines 367-374: rephrase the two paragraphs as “In order to compare our estimates with those calculated by Lauvset et al. (2013) which carefully assessed the seasonal cycle of air-sea CO2 fluxes in the Barents Sea, daily wind speed and quadratic parameterization of gas transfer velocity (Wanninkhof, 1992) were used for calculating CO2 fluxes in the northern Barents Sea. The CO2 uptake during the 2007 fall season reached an average … As the dataset by Lauvset et al. (2013) did not cover the north of the sea comprehensively, the data obtained during our cruise adds information enabling more accurate estimation of the absorption capacity of the whole Barents Sea.”

7) Lines 408-409: again I feel necessity of a clear indication that pCO2 data of the selected transect, reported in fig 10, are calculated for discrete samples (from AT, pH\textsubscript{25} and inorganic nutrients data) by means of CO2SYS programme.

8) Fig 10 seems underutilized in the text, as only surface data are compared without any further discussion about vertical distributions. As the figure is very informative could you please comment a bit more?

9) Line 421: I find a bit “dangerous” using here the word “supersaturation” as this makes me to wonder if supersaturation has been really computed (as

10) Line 422: I find not fully proper to say that CO2 outgassing into the atmosphere was observed … (Fig 10), as the calculated fluxes for the Laptev and Est Siberian seas were really very low (see Tab 1). I would prefer rephrase as “Thus \( \Delta p\text{CO2} \) conditions (Tab 1) favouring CO2 outgassing into the atmosphere were observed”

11) Line 456-458: I suggest authors to rephrase as “… resulting in an increase of the area where seawater pCO2 was in equilibrium with atmosphere and consequent reduction of CO2 adsorption in the East Siberian Arctic seas”.