

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 region 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

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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 McGillis (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.

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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 pHT 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 pHT₂₅, AT and inorganic nutrients data using CO₂SYS..." In addition authors should say which constants for sulfate and borate (KSO₄ and KBorate) 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

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Sea in the fall season.”

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, pHT25 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 make 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 $\Delta p\text{CO}_2$ 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”.

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