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Discussions

Interactive comment on "Enhancement of the North Atlantic CO_2 sink by Arctic Waters" *by* Jon Olafsson et al.

Anonymous Referee #1

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Review of the manuscript submitted to Biogeosciences, MS No.: bg-2020-313. "Enhancement of the North Atlantic CO2 sink by Arctic Waters" by Olafsson et al.,

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General comment:

The Ocean is a major CO2 sink, representing about 25% of the total anthropogenic emissions (Le Friedlingstein et al 2019), but how this carbon sink varies at interannual to decadal scales is still subject to large uncertainties. Even the mean global ocean CO2 sink is still subject to uncertainties. In a recent study Watson et al (2020) re-evaluated a global ocean carbon sink that would reach 3.7 PgC/yr in 2018, or 4.2 PgC/yr for the anthropogenic ocean CO2 sink, i.e. 36 % of the total emissions in 2018

(fossil fuel + land used of 11.5 PgC/yr, Friedlingstein et al 2019) a number close to the results of 31 % based on CO2 interior inventories but for the period 1994-2007 (Gruber et al 2019). At the end of the abstract authors ask the question: "Will the North Atlantic continue to absorb CO2 in the future as it has in the past?" Interestingly, Gruber et al (2019) suggested a reduction of the inventory change in the North Atlantic but this is somehow limited to available data. As recalled by Olafsson et al, it is well-known that the high latitudes in the North Atlantic, north of 50°N represent a strong CO2 sink, both in term of air-sea CO2 flux and anthropogenic CO2 inventories (e.g. Peng et al 1987; Takahashi et al 1993, 2002, 2009; Sabine et al 2004; Khatiwala et al 2013). The results presented by Olafsson et al confirm these previous findings, a large seasonality of fluxes (or pCO2) and contrasting ocean carbon sink in North Atlantic Drift, Polar and Arctic surface waters. Here they focus on these waters observed around Iceland using historical (over 30 years) and more recent pCO2 observations and suggest that ALK input from the Arctic explain part of the contrasting CO2 flux estimated in these water masses. The analysis is mainly based on 1994-1995 time-series and 2006-2007 pCO2 underway observations conducted around Iceland, and extended to a synthesis of 30 years of observations in this region (although it is not clear what data are used over 30 years). The paper is organized in two parts. First authors evaluate the seasonal and interannual (over 30 years) air-sea carbon dioxide fluxes in 3 waters masses characteristic of this region. Second they investigate the origin of the differences of the flux and based on ALK budgets suggest that excess alkalinity from Arctic is a significant source contributing to the North Atlantic CO2 sink.

The first part of the analysis (seasonality and CO2 fluxes) is not really new, although I think it is the first time the air-sea CO2 fluxes are calculated with the data obtained in 2006-2007 around Iceland (these data were added this year in SOCAT-v2020). What is new concerns the impact of Alkalinity (ALK) to explain the difference of DpCO2 and CO2 sink/source. It is suggested that excess alkalinity derived from Arctic sources contribute significantly to the CO2 sink in this region. This is an interesting analysis, as coupled climate/carbon models generally failed in reproducing correctly the ALK fields and this might impact on future CO2 sink estimates especially in the North Atlantic (e.g. Lebehot et al 2019), not only linked to future changes in E/P at large scale but also potential dramatic physical and biogeochemical changes in the Artic in the future (Terhaar et al 2020). The topic addressed in this paper is in line for Biogeosciences. However, the manuscript needs revision, especially regarding ALK observations. Nowhere in the manuscript have the authors presented ALK data that would support the analysis. They mostly refer to previously published ALK/salinity relationships and mean ALK in Arctic rivers. Also, Supplementary Figures are missing in the file (?) and this would probably help to follow the discussion for the main new results presented. In the conclusion, a schematic view of the ALK input in the region (with uncertainty) would be nice and show how this is linked to the CO2 sink around Iceland.

Overall, the study by Olafsson and co-authors by revisiting historical observations and presenting new data (2006-2007) presents interesting results in this region that open new questions regarding the oceanic carbon sink in relation to water masses and alkalinity changes and is suitable for publication after major revision.

Other comments (including minors), suggestions and questions are listed below:

;;;;;;;;;;;;;;;;;;;;; Specific and minor comments

C-01: Page 2, Lines 57-62. Authors write: “Estimates of long term trends for the North Atlantic CO2 sink due to changes in either DpCO2 or wind strength are conflicting, particularly the Atlantic Water dominated regions (Schuster et al., 2013; Landschützer et al., 2013; Wanninkhof et al., 2013). The drivers of seasonal flux variations are inadequately understood (Schuster et al., 2013) and a mechanistic understanding of high latitude CO2 sinks is considered incomplete (McKinley et al., 2017).” Well, I think compared to other regions, the seasonal variations (Peng et al 1987; Takahashi et al 1993, 2002) and long term trends of pCO2 are relatively better observed in the North Atlantic, although it has been recognized that annual trends are also sensitive to interannual variability (including linked or not to NAO or AMO/AMV) and the trends

better detected during winter when productivity is low (e.g. Metzl et al 2010; Frob et al, 2019). This is probably the reason why authors used winter data over 30 years to produce figure 6b.

C-02: Page X, Lines 62-65: Authors write: “It is common to many large scale flux evaluations, modelled or from observations, that they are based on regions defined by geographical borders, latitude and longitude, e.g. between 49°N and 76°N for the high latitude Sub Polar North Atlantic (Takahashi et al., 2009;Schuster et al., 2013).” The choice of latitudinal bands was mainly selected based on the “big boxes” used in atmospheric inversions (TRANSCOM) and compare ocean estimates versus inversions (e.g. specific boxes for RECAPP, Schuster et al 2013). However, for ocean purpose, many products are now based on “biomes” definition (Fay and McKinley, 2014) and used for air-sea CO2 fluxes calculations (and trends) from reconstructed pCO2 fields (e.g. SOCOM project, Rödenbeck et al 2015; Landschützer et al, 2016; Denvil-Sommer et al 2019). The region investigated by Olafsson et al, is at the boundary of biomes NA-ICE and NA-SPSS (see Fay and McKinley, 2014) but results in biome NA-ICE are often omitted due to sparse data coverage (Rödenbeck et al 2015). However, methods are now able to add pCO2 climatology in the Arctic considered as a single “biome” (Landschützer et al, 2020). It might be interesting to compare the seasonal view presented here (Figure 6) with most recent pCO2 climatology around Iceland (maybe for another paper).

C-03, Page 4, Line 84 and Introduction: Concerning the circulation in the high latitudes in the north Atlantic (and freshwater input from the Arctic) I would suggest to refer to Holliday et al (2020) who described the observed recent changes in salinity in this region, that probably also impact on surface waters properties observed around Iceland (including TALK and pCO2 and CO2 sink ?).

C-04: Page 4, Line 106: Would be interesting to show NAO (and AO) index, e.g. for the full 30 years period (1982-2012, to add in figure 6 ?) and discuss if you find a link (or not) with NAO as this is still debatable. This is optional as variability of NAO is not really discussed in the manuscript (excepted for 1994-1996 period).

C-05: Page 4-5: Figure 2 shows the underway pCO2 data for 2006-2007. It might be useful to note here that these data were recently qualified in SOCAT-v2020 (Bakker et al 2016, 2020). Why the data in 2008-2010 obtained by the same group and also available in SOCAT not included in this analysis ?

C-06: General: Why not adding other data available in SOCAT in this region to extend the analysis after 2012. In particular, a freshening has been observed in recent years in the subpolar zone (Holliday et al, 2020). This signal would decrease alkalinity and increase pCO2 (i.e. opposed to the effect of ALK discussed in the manuscript).

C-07: Page 6, Line 142: Takahashi et al 1993b: In reference Takahashi et al 1993a,b is listed twice.

C-08: Page 6, Line 155: The accuracy of 2 μatm is impressive for such analysis (ashore, correction to SST, etc…). In SOCAT the pCO2 data for 2006-2007 cruises have been assigned with a flag "C" (accuracy better than 5 μatm not 2 μatm).

C-09: Page 6, Line 158: For underway pCO2 cruises listed in Table S4 might be useful to specify the Expocodes (like in SOCAT, easier to see what cruises have been used).

C-10: Page 6, Line 159: Fig S1 ? (there is no figures in Supp Mat, this would help to see the way data in various water masses were selected).

C-11: Page 7, Line 169: Takahashi et al 1993b: In reference Takahashi et al 1993a,b is listed twice.

C-12: Page 7, Line 170: Authors use "pCO2" in the manuscript. In SOCAT, the data are in fCO2. For clarity, specify if you use pCO2 or fCO2 in DpCO2 calculations and figures (e.g. Figure 2).

C-13: Page 7, Line 171. I am not sure that Olafsson et al (2010) described the pCO2 underway observations in 2006-2007. Please clarify.

C-14: Page 7, Lines 173-175: Not sure to clearly identify when and where were conducted the cruises in 1983-2012 for the "Polar water collection". Are you using the discrete samples taken for pCO2 only or also use TALK and DIC data (and calculate pCO2) ? Maybe add a table in Supp Mat specifying date, location and what property was measured (pCO2, DIC, TALK). Are data obtained in 1983-1985 around 64N-28W included (Peng et al 1987) ?

C-15: Page 7, Line 175: "The data provide pCO2 for calculation of Delta pCO2 in Fig. 4." Data for figure 6 (not figure 4) ?

C-16: Page 7, Line 183: Equation number "1" used in previous section.

C-17: Page 8, Line 196: Maybe recall how Pw is calculated (e.g. Weiss and Price, 1980)

C-18: Page 8, Lines 1998-211: Not sure that all details on the way the wind were selected is useful. The most important results related to wind in this study is the gas transfer coefficient presented in Figure 4a (and should be also presented in figure 5 and 6, see comment below).

C-19: Page 9, Results: Figure 3 (if still published): add units on Y-axis (m2/s2). However, is figure 3 really useful here ? Maybe add this figure in Supp Mat. I think the "Results" section could start with the description of Figure 4.

C-20: Page 9, Line 240. Also discussed by Peng et al (1987).

C-21: Page 9, Line 240. Takahashi et al (1993). 1993a and b are the same in references.

C-22: Page 11: Table 1: Would be useful to add a column with climatological value in the same region (e.g. from Takahashi et al 2009). The climatology range is between 0 and -5 molC/m2/yr around Iceland, same range as listed in Table 1. Thus the regional difference depicted in Table 1 appears a permanent feature that supports the second part of the analysis (ALK).

C-23: Page 11, Lines 276-277: "possibly due to a weaker stratification (Fig 4b)". Figure 4b does not show change in stratification. Maybe add a reference here or show MLD variations.

C-24: Page 12, Line 280: typo: "fluxes"

C-25: Page 12 Line 281: "despite very different physical conditions". This is not specifically shown. Maybe recall the NAO shift and inform on observed SST, SSS, MLD variations in 1994-1995 ?

C-26: Page 12, Line 283: is the difference due to resolution of observation or real change between 1994-1995 and 2006-2007. Figure 5 shows much less seasonality in Arctic waters compared to Figure 4c. Is the difference of fluxes due to difference in pCO2 or wind or both ? Might be relevant here to present in Figure 5 (like for Figure 4) the gas transfer coefficient and DpCO2 (not only the fluxes).

C-27: Page 12: Line 296: Supp Fig S1: Fig S1 is not in the Supp Mat. Thus it is not easy to follow the selection of the data in Polar Water (see also comment C-10).

C-28: Page 12, Line 299: Why comparing the flux in Polar waters with the mean flux north of 50°N ? Might be more relevant to compare with the climatology from Takahashi et al (2009) in the same region, I.e. -3.5 to -4.5 molC/m2/yr around 68°N northeast off Iceland and seems coherent with the flux calculated with the pCO2 observations in 2006-2007 (see also comment C-22)

C-29: Page 12, Line 300: Authors write: "We evaluate the long term pCO2 characteristics of the three water masses from other data assembled over about 30 years". Which other data are used ? Is there a simple way to show which data are used in the 3 water masses ? Would be also interesting to use all SOCAT data available in this region to extend the analysis after 2012.

C-30: Page 12, Line 300: Typo "water masses"

C-31: Page 12, Line 304-306: Figure 6b also suggests that over 30 years, oceanic

pCO2 follows the atmospheric increase. A look at the monthly mean fCO2 in this region (from SOCAT-v2020) indicates an oceanic fCO2 trend of around 2.3 μatm/yr (here only for winter season and period 1982-2019).

C-32, Page 13, Figure 6a. Might be interesting to compare the DpCO2 seasonal cycle in "Polar water" with the climatology of Takahashi et al (2009) who probably used part of the same data to construct the climatology (?).

C-33, Page 13, Lines 320-322: Are you sure that Polar waters have higher TALK/DIC ratio (maybe a typo, higher DIC/TALK ?). Here, it would be interesting to show ALK/S and ALK/DIC ratios in various water masses (e.g. using your data and GLODAPv2 data)

C-34, Page 13, Lines 323-325: Lee et al (2006) did not discussed ALK in Arctic. Here, I would refer to Broullón et al (2019). Suggestion for a synthetic view of riverine ALK concentrations in Arctic: Fig S4 in Broullón et al (2019); they also conclude the difficulty to reconstruct ALK fields in this region.

C-35: Page 13, Line 325: When listing and discuss different TALK/S relationships it would be useful to show these relationships in a figure (and if possible colored by region). See also comment C-37. C-36, Page 14, Line 330. Maybe recall that Takahashi et al (2014) calculated the relation for PALK not ALK.

C-37: Page 14, Lines 332-333: Authors write: "However, the intercepts indicate considerable variability, they are higher than the average alkalinity of Arctic rivers and the intercepts are high in upstream regions of the East Greenland Current". Not easy to follow this statement. A plot of regional TALK/S relationship from both data and climatology would help.

C-38: Page 15, Line 350: ALK = 1048 μmol/kg. Recall this is a mean value (Cooper et al 2008). It might be relevant to associate an uncertainty to this value for the ALK flux estimates.

C-39: Page 15, Lines 353 and 359: Supplement ? there is no information in Supp Mat. (only Table S1-S4). Please check the file when resubmit the MS.

C-40: Pages 14-15, Lines 346-377: there are the main new results in this paper with calculation of ALK advected in the north Atlantic. I think authors could explain in more details their calculations (e.g. recall equation from Nondal et al 2009). A table with the ALK flux estimates (for the two calculations) and a schematic figure with boxes indicating Input (Sv), TALK concentrations and TALK budget would help to follow the discussion.

C-41: Page 15, Line 356: How the ratio DIC/TALK of 0.85 was chosen ? Based on the climatology (Takahashi et al 2014), the ratio DIC/TALK is always higher in this region (range 0.9-0.95). For a reader not familiar with this topic, why not showing a map of the DIC/TALK ratio in this region ?

C-42: Line 400: Authors write: “The Atlantic Water seasonal pCO2 variations we observe (Fig. 2c), are primarily driven by regional thermal and biological cycles”. Probably Figure 4b not 2c. This is a well-known signal (e.g. Peng et al 1987; Takahashi et al 2002). Is it important to recall this in the conclusion ?

C-43: The conclusion is somehow very broad recalling pCO2 seasonality and anthropogenic CO2 in the North Atlantic (same as in the introduction, although anthropogenic CO2 is not discussed in the manuscript). In the conclusion authors should highlight the main findings and suggest what new observations or modeling experiment should be conducted to confirm their results or reduce the uncertainty in the calculations. For example are the Arctic and North Atlantic biogeochemical models include the effect of Arctic ALK changes to better simulate pCO2 and air-sea CO2 fluxes or is it a secondary process ?

C-44: Page 17: After “Authors contributions” add a section “Data availability” and list the links to the data used in this study (e.g. SOCAT, CARINA, GLODAP, other ?).

;;;;;;;; In references:

Line 577 and 580: Takahashi et al 1993 listed twice.

;;;;;;;; Reference in this review not listed in the manuscript

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;;;;;;;;; End review

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