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

Anonymous Referee #2

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The paper presents an interesting summary of pCO2 data collected in different regions around Iceland (close Irminger Sea, Iceland Sea, and ice-free polar waters off or in the EGC), and in particular discusses the air-sea fluxes, in particular in winter. The ocean data are from different years, either from long time series stations or from seasonal surveys (with continuous pCO2 sampling) in 2006-2007. Then, theauthors discuss what could be the alkalinity properties and how water from the Arctic can cause a local large CO2 sink in the Iceland Sea.

I find the presentation of the pCO2 data sufficient and relevant. I would however object to the use of 'Irminger Sea' when discussing the results from the repeated station (IRM) southwest of Iceland. The station is located within the Irminger Current, in a region of often deep winter mixed layers, conditions that are far from common in the Irminger

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Discussion paper

Sea, even though other localized deep convection areas happen in its southwestern part, but only in specific years (see Fröb et al., 2018 and 2019), and involving different processes and water masses.

See also Reverdin et al. (2018) for a summary of conditions and trends in areas further southwest in the eastern Irminger Sea (rather close to Reykjanes ridge) and we show for some winters conditions favorable to a CO2-sink, albeit not for all, and with a tendency for a change from sink to source from the early to mi 1990s to the mid-2000s (the changes in the trends are more thorughly discussed in Leseurre et al., 2020)

I would also add some comments on interannual anomalies which are not really described, but certainly 2006-2007 are fairly remarkable years (see the red curve on Fig. 6, and also indications of anomalies in SST, temperature and probably winter mixed layers in the area southwest of Iceland).

My main concern with this paper is with the discussion on total alkalinity, which I dont find satisfactory. Definitely, the are complicated balances and processes happening in the Arctic and related to the exchanges with sea ice (either during its formation or later melt), and specific modes of primary production that take place iether in the ice, under the ice, or after its melt. Also, Arctic rivers can be high in TA, albeit by far the largest values are for the Canadian rivers, and this should not be such a large share of the fresh water flowing in the east Greenland Current, and furthermore entering the Iceland Sea. Thus, this component of the freshwater budget should not contribute to end members as high as 1700 micromol/kg. See for example the approach in Sutherland et al (2009) paper, and what is used in other more recent) papers combining alkalinity with water isotopes to investigate the freshwater budget both in the Arctic proper or in the East Greenland Current. Indeed the Nondal et al (2009) paper which find this resul is based on data from I.B. Oden in May 2002 (mostly within the dirfiting sea ice). This is a period of extensive (and thick) sea ice drifting from the Arctic, and thus the water is strongly influenced by the brine releases, and other Arctic processes (the interesting Rysgaard et al., 2007 study is not directly comparable to what is observed here). We expect the sea ice to have a rather low alkalinity which would compensate the larger alkalinity (compared to salinity) of the brine enriched waters. The main issue is what part of the EGC water influences the surface Iceland Sea: is it the winter brine-enriched water or is it also the sea ice. Depending on that the result on the alkalinity properties of the surface water (independent on biological activity) is going to be very different.

Also, for the Iceland Sea, one would expect from Nondal et al (2009) that the 0-intersect is 582 micromol/kg for S > 34.5. This relationship is used for example in Fröb et al. (2019) for a nearby area in the Irminger Sea. This contrasts only slightly with Reverdin et al 2018 who find an intersect at 713 micromol/kg, but that's including data closer to Newfoundland and when there is no (or little) sea ice (the slightly higher values are coherent with the export from the Canadian Arctic, which freshwater component should be higher in TA). There is also plenty of new TA data in EGC for different seasons (not in winter) both in Nordic seas or Irminger Sea and east Greenland Current that could be looked at to get a better estimate of what Ta might be in inflowing polar water and in the Iceland Basin or nearby areas that could help on this issue (notice that the lower salinity there is not just resulting from input from the Arctic through exchange of freshwater and sea ice with the EGC, but also from excess precipitation in large parts of the Nordic Seas. The surface waters of the Icland Sea are also influenced by heat loss that will contribute to its undersaturation (this could be quantified, and discussed following different studies on the pCO2 budget in the Nordic Seas by the UIB group (A. Olsen, Bellery, Nondal...).

Finally, I had a hard time with the discussion of Fig. 7 and the two hypotheses formulated. It seems that only the horizontal arrow is discussed. I feel that the change of SSS and admixture with the EGC waters (both freshwater and melting sea ice) has to be associated with both changes in DIC and TA (not just TA). It is not fully clear to me how this would work out. To be convincing a simple box model should be established at least to provide an order of magnitude of what is proposed there, and whether this can explain the under-saturation, compared with other hypotheses.

Detailed comments: For Arctic water, results are interesting and show the all season strong undersaturation of these waters (in 2006-2007). Can maybe be related to studies in the Arctic proper? I would question a little bit the atmospheric value used here to estimate the atmospheric pCO2 in this area. More likely be higher atmos pressure than Reykjavik? but maybe CO2 in the northeasterlies closer to Greenland (if those are the conditions encountered) is a little lower than further east or south in Iceland Sea or Irminger Sea? (even if winds coming from Greenland...). Now I dont think that resulting differences would exceed 10 microatm... Also, it seems by reading the paper that the reference pCO2 (or fCO2) is not always the one measured in Iceland... There can be seasonally significant differences with other stations. How are these taken into account. Furthermore, I think that it is important to point that the measurements are only made in ice-free areas. This will seasonally vary, and not be always typical of the EGC (in particular in winter and spring). Either because these are situations when the sea ice could have melted (see above discussion of alkalinity) or on the other hand has been flushed away... At lest, this should be acknowledged, and taken into account in the discussion of the data.

l. 86: NAC waters derived from the Gulf Stream (I would add that they are highly transformed in the subpolar gyre by air-sea fluxes, but also admixture of subpolar gyre water; maybe less so at IRM end of the section)

l. 92: the mention of Arctic fresh water. Should add that only part of freshwater from Arctic (and in particular river input) brought back south by EGC/EGCC... (and in particular EGC...) l. 189: not c lear how the fluxes are estimated at this point although this is explained later. But there is a question on the error resulting of the use the monthly V**2 and instantaneous pCO2 measurements (which are linearly interpolated in time between successive cruises, if I understood correctly).

L. 193: replacing Westmann Islands values with Mauna Loa when CO2-ICE missing. Why Mauno Loa. What are the SLP values used then (Reykjavik values)? These changes can make big differences. WHen was CO2-ICE missing?

Arrows of plot 1 for currents: a bit schematic (in particular near Reykjanes Ridge and south of Iceland), but probably OK for the purpose

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