# *Interactive comment on* "Ocean carbonate system variability in the North Atlantic Subpolar surface water (1993–2017)" *by* Coraline Leseurre et al.

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This contribution presents the trends in a very impressive set of data from the western subpolar North Atlantic, collected within the framework of the SURATLANT program. While the paper presents the data and trends more or less adequately, it fails in properly attributing them to (climatic + oceanographic + biological) drivers. These aspects are unclear, speculative, and somewhat confused. As such, major revisions are required.

My major comments are:

1. Samples are collected two to four times a year. My sense is that with such rare sampling one might be very prone to getting false trends because of differences in the timing of the data collection, also in combination with timing of the spring bloom. For

example, the strong increase in region C+D+E summer DIC in 2001 -2008 might be generated by earlier sampling/later spring bloom.

While some effort seems to have been taken to deal with this; reconstructed data; it is not quite clear how this is done, and how well it works. Some improvement is needed in this description (page 5 lines 1-19). Is it possible to test this scheme by using data from a year with sampling in all of the months used (e.g. Jan-March)? At least for SST and SSS this can be done as there should be continuous TSG data available, and for fCO2 there are data available from the VOSs Nuka Arctica and Atlantic Companion, which crosses the study area. These can be retrieved from SOCAT and used to test the method.

2. More broadly, it would be interesting to know how representative the data are for large-scale interannual phenomena. This can be tested. For example, the trends in SST can be compared with objectively analysed SST from NOAA. The trends in SSS can be compared with some renanalysis model output. And again, for the time representativeness, the continuous data from the TSG, which collects data on all crossings, should be used. It might also be worthwhile to look at remotely sensed Chl a, to evaluate if there are concominant trends in surface ocean primary production, e.g. a loss in production from 2001 -2008 such as the DIC data seem to indicate and was suggested in the manuscript.

3. The attribution section is not well done. In particular, I strongly suggest the authors to explicitly account for salinity changes in the pCO2 driver decomposition following the method by Keeling et al. (2004) and recently used in the subpolar North Atlantic by Fröb et al. (2019). The reason is that dillution may completely overwhelm increase of DIC expected from uptake of anthropogenic CO2.

4. Further, showing trends in salinity normalised DIC and TA is worthwhile, but make sure to use the correct method for salinity normalising as described in Friis et al. (2003).

5. Also, there is a lot of mentioning of the change in the air-sea CO2 difference. But this

is not illustrated, which leaves a lot to the readers imagination. I therefore recommend to show the actual air-sea fCO2 difference in Fig. 3. You can make room for this by removing one of the panels for Omega (Calcite/Aragonite), as there is no need to show both.

6. Uncertainties are not properly dealt with. For example, the uncertainty in the SST of 0.1 degrees C results in an fCO2 uncertainty of 2 microatmospheres. There are also uncertainties in DIC and TA. These errors need to be propagated to calculated fCO2, pH etc. This can be done using the most recent CO2SYS from James Orr as this includes error propagation. It is available from GitHub. The errors in fCO2 (and the others) can be propagated to the trends using Monte Carlo.

7. I am in particular concerned with the fact that the large summer fCO2 increase in region B 2001-2008 are basically caused by the 'reconstructed' data of 2001 and 2008. The actual observations are pretty steady. How confident are you in these reconstructed values?

8. As the other reviewer, I think this contribution confuses anthropogenic and natural CO2. The trends that are observed in pCO2 does not say any on anthropogenic carbon uptake. Further, the North Atlantic is not a big sink of anthropogenic CO2 because the air-sea flux is large. The air-sea flux is a combination of natural and anthropogenic CO2 fluxes. Horizontal advection is likely a big source of anthropogenic CO2 to the North Atlantic. Hence, the data that are presented only informs about the changes in surface pCO2 and air-sea CO2 flux, not on the North Atlantic sink for anthropogenic CO2. This needs to be considered both in the introduction and in the discussion.

9. I think the dicussion is pretty dissapointing. It is basically a recap of the results + some more exploration of these, combined with some speculation based on published litterature. There are no attempts to analyse the relation between the observed trends and likely drivers (for which data exist) – such as NAO or AMV indices, winter/summer mixed layer depths (Argo data), primary production (remotely sensed ocean color), and

SGP strenght (SPG index). This should be done.

10. Finally, Metzl et al (2010) suggested deep mixing as the cause for the sharp winter fCO2 trend in 2001-2008. Fröb et al (2019) demonstrated unequivocally that deep mixing leads to strong increases in winter fCO2 in NAO positive years because of intensified deep mixing (bringing remineralised DIC to the surface). In light of this, it is interesting that the smallest winter fCO2 trends from SURATLANT are observed in the period 2008-2017, as it is well known that this has been a period of rather large deep mixing compared to 2001-2008. I am not sure how this can be reconciled, i.e. the large fCO2 trend in the period of little deep mixing 2001-2008 vs the small fCO2 trend in the period of frequent deep mixing in 2008-2017. It seems worthwhile to dvelve into this. While doing so, keep in mind that deep-mixing events seemed (in Fröb et al., 2019) to cause year-to-year anomalies and not so much an anomalous trend. Therefore, I recommend to reconsider the use of three periods - the trend in each of these can be strongly affected by the fCO2 in the start and/or end year, which might just be an anomalous year. Therefore the authors might be doing themselves a disfavor by sticking to the three periods, which are largely defined because of 'historical'/'traditional' reasons related to sampling and previous SURATLANT papers.

Other comments:

Page 2, line 4. Takahashi's estimates of air-sea CO2 flux cannot be equated to anthropogenic CO2 flux.

Page 2, line 6. I don't think any has ascribed the variations in the North Atlantic CO2 sink to climate change. Carfel with such statements.

Page 2, line 21. 'based on Total Alkalinity (TA) and Dissolved Inorganic Carbon (DIC) observations,..'

Page 3, line 7. 'which', not 'witch'.

Page 3, line 14-15. 'to this end', not 'to this aim'.

Page 3, line 15. 'Iceland', not 'Island'

Page 3, line 17. These acronyms have already been defined.

Page 4, equation 1. I think it would be useful to recap the accuracy of this relationship, and how well it works for the data that are presented here.

Page 4. Atmospheric $fCO_2$ calculation. What atmospheric pressure was used?

Page 4, decomposition equation. As mentioned above, please use the method that explicitly accounts for the effects of dillution/concentration. Note also, that the equation as written is wrong. The $dX/dt$ term should be $dX/dz$, where z is the driver in question. You further need to explain what values you used for these sensitivities, and how they were derived.

Page 5, line 5. How many times were data excluded from box B because of SSS being outside the 34-35 range?

Page 5. Also, please consider the grouping of these regions for the trend analysis From Figure 2 SSS and TA, E and D appears quite similar while C and B both appear different. My sense this that combining E and D, while keeping C and B seperate might be the best approach.

Page 5, lines 10-19. Please collect this information in one section. Please also write that these are the reconstructed values in Fig. 3 (If I understand correctly the e.g Jan/March values adjusted to February are the 'reconstructed' values in Fig 3).

Page 6, lines 2-3. As mentioned above I am not convinced that these are 'pluriannual' trends. You might be doing yourself a disfavor by splitting the data into three time periods. It might make more sense to look at the timeseries as a whole and instead look at anomalies from the long-term trend. In particular this should be done from 2001 onwards. Consider to relate anomalies to mixed layer depths similar to Fröb et al. (2019).

C5

Page 6, lines 7 onwards. Here a panel showing air-sea $fCO_2$ difference, as suggested above would help, as it will make the discusssion more quantitative.

Page 6, line 20. I think 'near-stagnation' is the wrong word here. In winter the increase appears significant, at 1 uatm/yr. Generally, please make the summary of the results quantative.

Page 7, line 11. Remove 'Thereafter'

Page 7, line 15-16. Enclose 'much faster than the atmospheric signal', with commas. And... 'suggest larger productivity in the beginning of the period than at the end'. BTW this can and should be checked with Chl a data.

Page 7, line 19, replace 'than' with 'to'

Page 7, line 21-22. It is interesting that the slower increase in $fCO_2$ is associated with strengthening of the winds and enhanced deep mixing. As noted above, Metzl et al., (2010) suggested deep mixing as the cause for the larger increase in the 2001-2008 period. Recently Fröb et al. (2019) found anomously high $fCO_2$ during years of deep mixing. What is suggested here, is thus at odds with these papers. This needs to be explored or revised.

Page 7, line 25. Set the '2' as subscript.

Page 8, line 9 -10. The link between the trends in the carbon system and NAO+AMV that are described here are not backed up with any statistics. It comes across as very speculative. The statements needs to be backed up with for example correlation analyses.

Page 8, line 11. Fröb et al (2018) shows in particular a large increase of anthropogenic DIC inventory in deep mixing years, and tendencies for a loss of natural carbon. Fröb et al. (2019) shows a outgassing of $CO_2$ during deep mixing years. The coupling between the inventory changes and the variability in fluxes has yet to be made. Some discussion around this would be interesting.

C6

Page 9, line 5. 'Makes it difficult to predict the evolution of CO2 uptake..' I suggest to read the paper by Li et at 2016, on decadal predictions of North Atlantic CO2 uptake, this might provide some relevant information.

References (not cited in the paper) Fröb, F., Olsen, A., Becker, M., Chafik, L., Johannessen, T., Reverdin, G., and Omar, A.: Wintertime fCO(2) Variability in the Subpolar North Atlantic Since 2004, Geophys Res Lett, 46, 1580-1590, 2019.

Li, H. M., Ilyina, T., Muller, W. A., and Sienz, F.: Decadal predictions of the North Atlantic CO2 uptake, Nat Commun, 7, 2016.

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