SURATLANT: a 1993-2017 surface sampling in the central part of the North Atlantic subpolar gyre
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The paper presents a dataset of temperature, salinity, carbon dioxide variables, nutrients and water stable isotopes in surface waters between Iceland and Newfouland since 1993. The procedure of validation, accuracy and characteristics of the data are discussed together with the resulting seasonal and interannual cycles.

Even when there are many adjustments in the data set, justified in the text, one data experimentally determined is of great interest and should be recorded and archived. One of my major concerns in this paper is why the authors do not show temperature data (unless a Hövmüller diagram), seasonal and interannual trends. Temperature controls most of the variables here considered and discussed and it is complex to explain the results without that figure. Moreover, they compute variables of the carbonate system (pH and fCO$_2$) where this variable is crucial and, when they also compare their estimation with VOS line data presenting both a great (even too high) agreement. Moreover, due to the range in salinity along the region and the seasonal variability, carbonate variables (Alkalinity and total dissolved inorganic carbon) should be presented normalized to a constant salinity. They provided a relation between AT and S that should be used as indicated in Friss et al., 2003, included in their manuscript. Therefore, present your discussion indicating the seasonal trends for the normalized values of DIC and At. I consider a moderate/major revision should be done in the manuscript before acceptance.

Other comments:

Line 65. 2001-2008 is an important number of years for some parameters. However, for those with an important seasonal variability and with a high error of estimation such us the pH in this case, consider including some indication about the short period o time in the referred paper.

Line 97. Here and along the text, the transect is defined as AX02 and in others AX2 (also in Figure 1). Homogenize

Line 126. I do not consider this method of sampling as one to be used for calibrating any data to, at least, the required accuracy.

Line 143. Even when they tried to explain it, please remove psu in any figure and in salinity values.

Line 218. Does it mean you select the data giving you a better figure? If you have done an important data treatment to remove, improve and homogenize the experimental data, why did you use different data sets?

Line 238. Present the data for At and DIC normalized to a constant salinity following Friss et al., 2003.

Line 243. The formation of organic matter reduced dissolved nutrient concentrations but also DIC (not increase)

Line 244. Do you see a decrease in alkalinity following this same argument? There is a small decrease in AT in the figure, but without normalization and with the error in the estimation, it is difficult to assure this conclusion.

Line 252. What about deep convective mixing in the area with important interannual variability?

Line 267. Clarify what Snorth means.

Line 275. It is not clear what SST values were used to compute these values.

Line 281. Again, in this region SST plays a strong influence in fCO2 seasonal variability. Therefore, it will be important to show SST to see how much of this variability is related to SST and how much is due to biological effects. You could also present the data following Takahashi et al., papers.

Line 340. Higher trends are found in Bates et al, 2014 paper for Irminger region and Olafsson et al. 2010. Please compare. Could this be in part to your lower DIC trends?

Line 344. How do you calculate the significance? You say in line 346 large uncertainties, small number of years ….

Line 355. Please include reference for Alert station

Line 385 and the full paragraph. I do not see this as a conclusion. Please, move up

Line 459. You used CRMs for your analysis. Why should your values be adjusted by this important value? What about alkalinity values?

Line 480-498. The interannual trend in DIC of 0.7 in a period of 24 year means an increase of 16.8 umol/kg in the full period, that is less than twice the indicated errors in the DIC values. Moreover, with an error of 10 units in DIC, computed pH values could be affected with an error as high as 0.02-0.03 units while in fCO2 could be close to 30 uatms. The resulting very low average difference between computed and VOS data and high error (line 493, -3.6 +- 12.4) indicates positive and negative deviations in DIC and/or different sign in the deviation of At and DIC. Conclude from this intercalibration exercise that used only experimental values what it is indicated in line 497 looks too much.

Line 508. The LOCEAN DIC values were always lower than other values, as indicated above. Have you corrected the data for this bias?