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Biogeosciences Discussions

Interactive comment on "An upgraded carbon-based method to estimate the anthropogenic fraction of dissolved CO_2 in the Atlantic Ocean" *by* M. Vázquez-Rodríguez et al.

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While I found this to be an interesting article, I guess I don't really see what is new and innovative about it. This approach adds a few additional "correction factors" to what are essentially existing techniques and in the end they got more or less the same answer that existing approaches have already published. Obviously I have over simplified things in this statement, but it represents the take home message I got from this article. There already exists at least 5 different techniques for calculating Canth inventory. All of the back calculation techniques have problems, but 10 years ago there were no historical data sets to do anything better. With all of the repeat cruises available now there

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are many more options for assessing anthropogenic carbon in the ocean that should allow us to move on from these back calculation techniques.

One of the proposed changes with this method is the use of data from the 100-200m depth range for calculating the preformed alkalinity. It is true that one can get a better fit in this range, but the depth range seems rather arbitrary. What if the winter mixed layer is only 100m? Then the chosen range does not represent that winter's properties. From figure 3d, we see that some of these waters have ages in excess of 20 years so how can the authors claim that this represents the recent winter values?

OM has a relatively small but real effect on alkalinity so it does not represent the true preformed values. To get around this the authors have had to use potential alkalinity but this adds an additional complexity to the calculations that likely offsets the value of the slightly improved fit particularly with the problems in getting good nutrient measurements. Also, this may work in the North Atlantic, but how does it work in the North Pacific where there is a strong salinity maximum in that depth range representing a totally different watermass from the deeper waters? The authors propose a correction to the AT0 to correct for a predicted decrease of preindustrial AT due to $CaCO_3$ dissolution changes and SST shifts. This correction is based totally on theoretical estimates and seems very tenuous at best.

What concerns me most is the use of the 100-200m DIC values to determine the disequilibrium values. I actually found this section very confusing to read. If I understand correctly, they describe a two step approach for calculating the disequilibrium term with equations 2 and 3. First they use the Gruber approach that requires a watermass age. It is not clear to me how they got watermass ages from CFC12 for cruises in the 2000s when CFCs were not increasing during that timeframe. This is further complicated by taking the equation 2 disequilibrium values and fitting them with multiple linear regressions. They had to break the dataset into 7 regions and still got R squared values ranging from 0.18 to 0.62. Even the best fits only constrain a little more than half of the variability. On top of all this they apply a temporal correction to the disequilibrium term that is also full of short cut calculations and assumptions, then averaged for the whole Atlantic.

These approaches are used for all waters with a temperature greater than 5C. For colder waters (I did not see why 5C was chosen as the cutoff), they used an optimum multi-parameter fitting routine. They also had to apply a specific separate set of equations for the Mediterranean Waters.

From all of this the authors claim an uncertainty of 5 umol/kg, but they do not show how they determined their uncertainties.

The bottom line is that all these added complications and additional steps seemed to result in a distribution that was somewhere between the various existing estimates and within the uncertainty of the various approaches. The main differences were in the Southern Ocean and the Nordic Seas. The Gruber and Lee estimates did not include any Nordic Sea data so it is difficult to comment on these differences. For the Southern Ocean, Lee and Gruber both acknowledged the challenges in that region particularly since the all of these calculations had very little data south of 50S making it very difficult to properly characterize these waters. The TrOCA and LoMonaco's calculations already showed higher values relative to the C* so this isn't a new finding.

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