The evolution of back-calculation methods is necessary and certainly not new. New pieces have been progressively and slowly added over the initial concepts of the preformed $C_T$ method (which used surface data to parameterize $A_T^o$ and $C_T^o$) from Brewer (1978) or Chen & Millero (1979) that have remained to a greater or lesser extent at the core of the new-coming methods. The milestone set by the old $\Delta C^*$ approach in the work from Gruber et al. (1996) came from being the first to formally deal with the $\Delta C_{dis}$ problem, and had its main strength on the use of CFC data as a mean to give a first approximation to it, some thirteen years back. There are several works acknowledging and documenting the caveats associated with the $\Delta C^*$ approach (Matear et al., 2003; Lo Monaco et al., 2005; Matsumoto and Gruber, 2005), which stem mainly from the surface data used to obtain the parameterizations and the non-linear behaviour of CFC mixing and water mass dating for ages greater than ~25 years (Doney et al., 1988).

The advancement of back-calculation methods is an ongoing and inevitable process as the understanding of the ocean carbon system improves considerably (Levine et al., 2008; Rodgers et al., 2009 in press) from the increasing bottle data coverage available, repeat sections, etc. This and the known shortcomings of existing methods is what motivated the upgrades proposed in our work. The changes introduced in the procedure might seem subtle at first, but they are indeed significant in any case. Out of the several modifications proposed the most important ones are: *a)* Having used the subsurface layer to obtain significantly better parameterizations (based on the understanding of how winter surface properties propagate into the ocean interior); *b)* Avoiding the use of CFC data in $C_{ant}$ estimation. The concentration of CFCs is not needed as an input parameter in any of the proposed equations, but it had to be used (reasonably appropriately in subsurface waters not older than 25 years) to obtain the fit for one of them (see below).

The following are answers to specific comments:

1. *"…in the end they got more or less the same answer that existing approaches have already published."*

Although the general $C_{ant}$ concentration fields certainly share some general trends and similarities, the discrepancies are important enough so as to generate differences of up to 8 GtC in terms of Atlantic $C_{ant}$ inventory (about 15% of the inventory) (Gerber et al., 2009; Vázquez-Rodríguez et al., 2009), and this is not only with respect to the old $\Delta C^*$ method but also with respect to methods more recently proposed like the one from Lo Monaco et al. (2005) or the TTD.

2. *"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."*

Several ranges were tried, from 50-150 to 150-250 m, but the 100-200 m range showed to work particularly well in the Atlantic, showing the lowest seasonal variability and having the closest average values to surface late wintertime ones, when water masses typically form in high latitudes. The challenge in selecting an appropriate range relied in the fact that the selected layer had to be as free as possible from the direct influence of surface seasonal and short-term variability, and yet be not too influenced by underlying older waters. Adding to this, the availability of at least four different levels of bottle data in the GLODAP dataset for this particular range meant an added value to establish the 100-200 m boundaries for the subsurface Atlantic layer. Doing so, the parameterizations benefited from the higher number of data and spatial coverage from the numerous spring and summer cruises in GLODAP.

3. *"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?"*

We know that the winter mixed layer depth (WMLD) generally increases polewards from the Equator, with known exceptions like in the Southern Ocean. In the Equator, for instance, where the strong upwelling brings up to the surface waters slightly older than 20 years, there are no significant water mass formation processes. Even if there were some, Figs. 3a, 3b and 3e show how in this region (and in the Southern Ocean) the vertical variability of relevant tracers for the parameterizations of $A_T^o$ or $\Delta C_{dis}$ in the 100-200 m layer is rather negligible, and the distributions are quite homogeneous, contrary to the $\Delta C_{dis}$ computed subsurface from subsurface $C_T$ data (Fig. 3f), which actually corresponds quite well to the age distribution in the Southern Ocean (Fig. 3d). In the Equator, the obtained $\Delta C_{dis}$ subsurface values close to zero (Fig. 3f) are due to the strong upwelling of very old, $C_T$ enriched waters that would therefore be close to $C_T$ saturation with respect to the present atmospheric $pCO_2$, while the highest disequilibrium in Southern ocean waters is mostly due to ice cap hindering of air-sea fluxes and the way oxygen disequilibrium interferes with the way $\Delta C_{dis}$ is defined and calculated in back-calculation approaches (Lo Monaco et al., 2005).

4. *"The authors propose a correction to the AT0 to correct for a predicted decrease of preindustrial AT due to CaCO3 dissolution changes and SST shifts. This correction is based totally on theoretical estimates and seems very tenuous at best."*

The proposed correction for the effect of temperature on $A_T^o$ estimates is entirely based on recent empirical observations ($NPA_T$ vs T correlations), and the estimated temperature increase of surface waters is taken from the latest IPCC report and several other authors cited in the manuscript. On the other hand, ocean acidification and $CaCO_3$ dissolution are well-documented processes known to be taking place nowadays that affects $A_T$, thus making a correction necessary for preindustrial $A_T^o$ estimates from present subsurface data. The vast majority of published works and ongoing research are either mesocosm experiments or numerical models (for larger scales, usually). This is the reason for having referred to Heinze (2004) results to come up with the proposed minor correction.

5. *"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."*

Correct. First, $C_{ant}$ is indirectly estimated with a shortcut method (as in Gruber et al. 1996) using CFC12 age estimates in order to obtain subsurface $\Delta C_{dis}$ estimates (from equation 2) that can be later parameterized, in a "second step" (it must be noticed that this is something that does not need to be done again by future users. They will only need to apply equation 3 to get $\Delta C_{dis}$ in the water column). Only very few of the considered cruises were conducted in the 2000s (NSeas and A25 in 2002, and A16N in 2003). However, this would have no effect for subsurface water masses older than 5 years found during these cruises.

6. *"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."*

This further fitting is necessary to make $\Delta C_{dis}$ computable from measurable conservative tracers (θ, S, NO and PO). The "chopping into blocks" of the ocean into several regions for the purpose of minimising the residuals of the fits is a common practice (Sabine et al., 2002 did it for the Pacific; Lee et al, 2003 in the Atlantic). This practice suits even best procedures that include resolving water mass mixing via an OMP analysis, as in our case, since there is no method to date that is able to resolve the Atlantic Ocean mixing. Moreover, the goodness of the fit ($R^2$) and standard errors of the estimate are indicated in the manuscript. One can get low $R^2$ values in regions of low variability and this would still be acceptable in terms of final $C_{ant}$ uncertainties, like in the South Atlantic for instance. Most importantly, the lowest $R^2$ values correspond to warm intertropical waters with comparatively small relevance in terms of volume (and hence little impact on inventories) with respect to the whole Atlantic.

7. *"…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"*

The reasons for establishing this 5 ºC boundary are not arbitrary. Warm waters below the 5 ºC isotherm represent approximately 86% of the Atlantic volume and store about 50% of the Atlantic $C_{ant}$ content. The 5 ºC limit roughly separates the upper waters from the region where Arctic and Antarctic waters meet and the mixing can be solved with a classical OMP.

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

The way uncertainties were calculated is far well documented and has been previously used in other $C_{ant}$ estimation works (Gruber et al, 1996; Sabine 1999; Lee et al., 2003), as stated in the manuscript. The analytical errors of the variables involved in the parameterizations that are randomly propagated are also given in the manuscript. The resulting lower uncertainty, compared to older $C_{ant}$ estimation methods, comes from the higher quality of the modern $A_T$ data (CRM calibrated) and the better $A_T^o$ fit here proposed.

The bottom line is that, from what has been discussed here and in the manuscript, there is still plenty of room for improvement in back-calculation techniques, which is what is intended with the proposed upgraded method. It is understandable from the point of view of the modeller's community that having a single data-based model to calculate $C_{ant}$ would be optimum, but that would be as difficult as much as desirable. It would also be inappropriate to leave behind and not to profit from all of the ongoing carbon system fieldwork without implementing it to move forward in the back-calculation techniques. The improvement of data-based methods and numerical models is not only necessary, it must be complementary, in continuous feedback and, as much as possible, parallel in time.