

## ***Interactive comment on “Temporal variability of the anthropogenic CO<sub>2</sub> storage in the Irminger Sea” by F. F. Pérez et al.***

**F. F. Pérez et al.**

Received and published: 16 October 2008

Dear Reviewer:

We appreciate very much your detailed and careful comments. They have been very valuable in the process of improving the paper. An appendix has been added outlining the equations, error analysis and main concepts behind the  $C_{ant}$  reconstruction method used in the manuscript. An ancillary new figure to complement the large amount of data presented in Table 2 has also been added. This new figure (Figure 3 in the revised manuscript) also provides graphical evidence for the dependence described in the text between the  $C_{ant}$  saturation concentration and AOU for the main water masses in the Irminger basin. Also on the graphical side of the manuscript, Figure 2 has been reorganised and enlarged as much as possible to facilitate its reading.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



The authors hope you will find that this version is clearer as it solves more of your remarks and the passages where the main message was unclear have been rewritten. Again, we would like to thank you for all your sensible comments.

Yours truly,

The authors.

General comments:

Page 1589, introduction: It is worth to point out that although the study by Sarmiento and Le Quéré (Science, 2001) found profound decrease of Cant uptake in a reduced shut-down scenario) of the thermohaline circulation (THC) from a GCM, they noted that this might be offset by the downward flux of organic carbon. Now we know that the “new production”; is significantly larger than the Cant flux, i.e.  $\sim 11$  PgC/y (Duce et al., 2008, Science, and references therein) for the new productions vs. the 2.2 PgC/y of Cant uptake. It is thus very likely that the transfer of carbon from the atmosphere to the ocean would increase significantly if the THC did shut down. Although this might not yet well quantified, I feel that the statement by Perez et al. should reflect this possibility.

The paragraph has been modified as follows:

*“... However, regarding the oceanic sequestration of  $C_{ant}$  it must be noted that the possibility of the MOC shutting down and its consequences are not free of ambiguity. Joos et al. (1999) and Duce et al. (2008) have argued that the predicted enhanced downward organic carbon fluxes in MOC shutdown scenarios could actually compensate for and even increase the oceanic uptake of  $C_{ant}$ .”*

Page 1591, Dataset and method: It is difficult to judge the validity of the method used to estimate the Cant, since the authors refer only to a manuscript in review (Vázquez-Rodríguez et al.), the authors should provide a more detailed description of the method.

An appendix has been added to the paper where the  $\varphi C_T^o$  method is described, the main assumptions behind the equations to calculate  $C_{ant}$  are out-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

lined, and the associated uncertainties are given. At the moment, the paper is still under revision. However, there is a paper in Biogeosciences Discussions (that can be cited) where an intercomparison of  $C_{ant}$  methods, including the  $\varphi C_T^o$ , is performed in the North Atlantic. The paper can be consulted at <http://www.biogeosciences-discuss.net/5/1421/2008/bgd-5-1421-2008.pdf>

Page 1591, Methods: The few lines on uncertainties leave the reader with some questions on how this was done (presumably this information is available in the ms. In press). Furthermore, the errorbars in figure 3 (and table 2) are much smaller than +/- 5.2  $\mu\text{mol}/\text{kg}$ . If the authors used a different method to calculate those uncertainties, then this should be described. I guess that the error-bars only refer to the uncertainties in the averaging procedure, which only reflect the variability of the water-column, not any real biasing errors due to the Cant inference method. The uncertainties should reflect the total uncertainty in the accuracy.

The uncertainties of the method are assessed by propagating random errors over the precision limits of the various measurements required for solving the  $C_{ant}$  estimation equations, and this has produced an estimated overall uncertainty of 5.2  $\mu\text{mol}\cdot\text{kg}^{-1}$  (more details on  $A_T^o$  and  $\Delta C_{dis}$  uncertainties on the Appendix).

With respect to the error-bars in Figure 3 (Figure 4 on the revised manuscript version), it is now specified on the last paragraph of section 2 that the final error reflects the accuracy of the bottle measurements:

*“... Regarding the specific inventory estimates, errors were estimated using a perturbation procedure for each layer and the total water column. They were calculated by means of random propagation with depth of a 5.2  $\mu\text{mol}\cdot\text{kg}^{-1}$  standard error of the  $C_{ant}$  estimate over 100 perturbation iterations, and are given in Table 2. Assuming that the uncertainties attached to the  $C_{ant}$  estimation method are purely random and do not introduce biases, the final error included in Figure 4 is calculated by propagating the individual errors associated to the samples. They include both measurement and*

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

*parameterization errors.”*

Furthermore, even if we allowed for the possibility that a bias could be introduced by the chosen  $C_{ant}$  reconstruction method, such bias would be the same for the whole time-series and, therefore, it would not have an effect on the calculation of the rates, which is our final aim.

Page 1594, Results: The authors claim (without showing the figure with the relationship that they are referring to) that the AOU can be used with some confidence as a Proxy for Cant, at least in the LSW, since there is a correlation between these two quantities. However, it could also be a direct result of the method used to calculate the Cant. It is difficult to judge such a causal relationship from the sparse description of the method that the authors provide. It might be a valid statement, but I would like to see some discussion on the impact on how the methodology to calculate Cant relates to the AOU/Cant correlation. It would also be nice to show the figure of the AOU / Cant correlation.

New Figure 3d shows the described AOU vs  $\%C_{ant}$  sat. dependence for the main water masses in the Irminger basin.

The choice of a different  $C_{ant}$  method or a different O:C Redfield ratio ( $R_C$ ) does not invalidate the AOU vs  $\%C_{ant}$  sat. dependence found. If  $\%C_{ant}$  sat. is calculated using the TrOCA method (Touratier et al., 2007), the AOU vs  $\%C_{ant}$  sat. dependence still holds true:

$$\%C_{ant} \text{ sat. (TrOCA)} = -0.026 \cdot \text{AOU} + 1.78 ; (R^2=0.92)$$

The difference in the slopes in the AOU vs  $\%C_{ant}$  sat. relationship between the TrOCA and  $\varphi C_T^o$  fits is due to the different “zero- $C_{ant}$ ” references and  $R_C$  ratio taken by each method. The  $R_C$  ratio is known to have both vertical and horizontal variability, but virtually all back-calculation methods to date have used constant ratios. The appropriateness of the chosen  $R_C$  to a given area in the ocean can be relevant in terms of  $C_{ant}$

## BGD

5, S1965–S1971, 2008

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



estimates. The TrOCA method uses an  $R_C \approx 1.36$  (Körtzinger et al., 1998), while the  $\varphi C_T^o$  uses an  $R_C=1.45$  (Anderson and Sarmiento, 1994). If an  $R_C=1.36$  is introduced in the equations of the  $\varphi C_T^o$  method, then the following relationship is found:

$$\%C_{ant} \text{ sat. } (\varphi C_T^o) = -0.026 \cdot \text{AOU} + 1.35 ; (R^2=0.94)$$

From the similar  $R^2$  and slopes obtained for the TrOCA method it can be deduced that the choice of  $R_C$  ratios does not influence the obtained AOU vs  $\%C_{ant}$  sat. relationship depending on the  $C_{ant}$  reconstruction method applied. Alternatively, this result also indicates that the effect of the AOU/ $R_C$  term in the  $\varphi C_T^o$  back-calculation equations represents a very small weight in the covariance of AOU vs  $\%C_{ant}$  sat. As explained in the manuscript, the correlation found is due to:

*“... The term  $\%C_{ant}^{sat}$  is independent of the atmospheric  $CO_2$  increase since it is referred to the  $C_{ant}^{sat}$  concentration in the corresponding sampling year. In this sense,  $\%C_{ant}^{sat}$  is comparable to oxygen, whose atmospheric concentration is stationary. Hence, it is expected that recently equilibrated (young) waters will have low AOU and high  $\%C_{ant}^{sat}$  values, while the opposite is expected in older waters that have undergone large remineralization of organic matter”.*

Anderson, L. G., J. L. Sarmiento, Redfield ratios of remineralization determined by nutrient data analysis. *Global Biogeochemical Cycles* 8, 65-80, 1994.

Körtzinger, A., Ludger Mintrop, Jan C. Duinker, On the penetration of anthropogenic  $CO_2$  into the North Atlantic ocean. *Journal of Geophysical Research* 103, 18681-18689, 1998.

The conclusions on the way the anthropogenic carbon inventory in the Irminger Sea has increased in a step-like fashion are very interesting, as are the speculations over the causes of the non-linear increase. It would also be interesting to see a number of the inventory of  $C_{ant}$  in the whole Irminger Sea, i.e. an extrapolation of the data

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



to cover the whole basin (admittedly, this would be a somewhat uncertain number, but not worse than any other basin wide extrapolation of Cant inventories). Although this is a regional study, it would be nice to see a little more discussion on the possible implications for the North Atlantic Cant sink based on the results from the Irminger Basin sections.

The following paragraph has been included at the very end of section 3:

*“... The above data was extrapolated to cover the whole Irminger basin ( $0.58 \cdot 10^6 \text{ km}^2$ , taking the FOUREX section and the Denmark Strait as the southern and northern boundaries, respectively) and give estimates of total inventories, admittedly of the uncertainties attached to this practice. They have been estimated in  $0.26 \pm 0.02$  and  $0.38 \pm 0.01 \text{ Gt C}$  for the years 1981 and 2006, respectively.”*

Specific comments: Figure 2: This figure comes out very small in the pdf -format, and is difficult to read.

The figure has been re-structured, re-oriented and expanded to the limit of the page. Font types have also been made bigger whenever possible to facilitate value reading. The  $\theta$  plots include now the isopycnals (thick red lines) that separate the main water masses in the Irminger basin (presented in Figure 1b) to give an idea of the extent of convection. The grey dots indicate the spots where bottle data was available and was used in calculations.

Page 1593: The reference to Olsson 2001 could be complimented with these, more easily accessible, publications; [Olsson, et al., 2005; Tanhua, et al., 2008].

Thank you for the advice. They have been included.

Page 1593: The increase of Cant in the DSOW and the NEADW does not necessarily imply more vigorous mixing south of the Greenland-Scotland sill, but might also reflect changes in the source waters (either as different source water composition, or that the increase in Cant is also reflected in the source waters themselves).

Although the DSOW is not present in the Greenland-Scotland sill we agree with your argument. At the end of the second paragraph in section 3 the following has been added:

*“... The observed increase of  $C_{ant}$  in the NEADW and DSOW can derive from vigorous mixing processes, but it might also reflect the increase of the  $C_{ant}$  content in the Arctic source waters.”*

Table 2: There is a wealth of information in this table. It would be nice to see a figure similar to Figure 3, but for AOU (or even theta and salinity for that matter) in addition to the table. Such a figure might be easier to grasp than the table.

This is the content of new Figure 3. Thank you for the input.

---

Interactive comment on Biogeosciences Discuss., 5, 1587, 2008.

## BGD

5, S1965–S1971, 2008

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

