Response to Reviewers Comments

We thank both reviewers for their insightful comments and for pointing out mistakes and omissions. We have tried to take their concerns into account in the revised manuscript and in our response below.

Reviewer #1

General comments:

• *There did not appear to be consistent use of total inventory change (Pg C), concentration change (umol/kg) and specific inventory change (mol/m^2). Moreover, the changes were not always provided in context of total inventory of the basin making it difficult to put the values in context.*

• We have made changes to the text and tables as per the reviewers specific comments below to provide better context. We also now alert the reader to the various units used.

• *The discussion of the different empirical back calculation methods is limited. The problems with the TrOCA method are well described and referenced but the some of the others methods are not. In specific the PhiCT method is mentioned in the tables in the Appendices but not discussed in text.*

• We have now added a more extensive description of some of these other methods, including the $\phi \mathrm{C}_\mathrm{T}^\circ$ technique.

• *The use of terminology differs for the different RECCAP papers. For instance, Ocean forward models mentioned are also listed as Ocean Global Circulation Models; column inventory is referred to as specific inventory in Wanninkhof et al 2012.*

• We were not aware of any requirement for the same terminology across all the RECCAP papers. For example, both “column inventory” and “specific inventory” are widely used in the literature and did not realize one was to be preferred over the other (we now mention that “column inventory” is the same as “specific inventory”). Similarly, OGCM is an accepted and well understood term in the oceanographic literature for a numerical model based on the primitive equations. As these are typically designed to be integrated forward in time they are also called - as we interchangeably do - forward (ocean) models, although not all forward ocean models are GCMs (e.g., ”box models”). Still, we recognize the need for consistency and have changed the text, although, unless there is risk of confusion we do occasionally use more than one term to prevent the text from sounding repetitive.

• *It is unclear why the Tables are in an Appendix rather then part of the main text. There is no preface to the Appendix.*

• Our reasoning was that unlike the two summary tables (which are, and in the published version should appear within, the main text), the tables that deal with uncertainty are not central to the flow of the article and were better placed in an appendix. Also, there is a sentence before the start of the appendix tables that states what is in the appendix. Unfortunately, in the BG typeset version this sentence got separated from the tables! We're sorry for the confusion!

• *At the end of the references there are the page # listed where they appear in the text. Its an interesting idea, but I have never seen this before.*

• This is automatically inserted by the BG latex typesetting style. Alas, we have no control over it.

Specific comments and technical corrections:

• *Page 8933, line 24: state absolute amount of uncertainty: 155 +- 31 Pg C (20%)*

• Done.

• *Page 8934, line 5: "currently sequestering" perhaps change to "which to date have sequestered"*

• Done.

• *Page 8934, line 15: delete "(unknown)"*

• We have deleted the word, but we believe it was important to emphasize that the preindustrial distribution of DIC is not known (not always appreciated).

• *Page 8935, line 19: Is "forward integrations of OGCMs" the same as "forward models"?*

• Yes, these are the same. Please see our response above.

• *Page 8937, line 6-14: perhaps mention other approaches (and their issues)*

• We have added a discussion of other approaches (specifically, $C^{\circ}_{\mathrm{IPSL}}$ and $\phi C^{\circ}_{\mathrm{T}}$), and their pros and cons.

• *Page 8938, line 3: "avoids the need for complex and uncertain biological corrections". This is a bit misleading perhaps rephrase that it a priori assumes that the biology is in steady state*

• *We have modified this sentence to add that steady state biology is assumed, but perhaps the reviewer can say why the rest of the statement is misleading. It is not simply the steady state biology assumption, but also the fact that certain parameters such as stoichiometric ratios are assumed known and spatially uniform.*

• Page 8939, line 7: "forward model" = "forward ocean model"

• *Text has been changed.*

• Page 8942, line 13: "preindustrial" = "preindustrial era"

• Text has been changed.

- *Page 8942, line 23: check the concentrations. I recall that the NAtl had up to 70 umol/kg based on the C* method(see Lee et al).*

    - If the reviewer is referring to Lee et al., GBC, 2003, they state that typical surface values were in the range 50-60 μmol/kg. This is not too different from the range we give. None of the figures in that paper suggest a value as high as 70 μmol/kg. Could the reviewer be referring to a different paper?

- *Page 8943, line 24: As I recall Graven et al 2012 only compare two models and while they state that the models are a lower and upper bound, I would be cautious stating it as such as it implies that these are the actual upper and lower bounds of uncertainty. You in essence state this concern in the first paragraph of page 8945.*

    - We have rewritten this sentence.

- *Page 8946, line 14-18: switching form umol/kg to mol/m2 makes comparison difficult*

    - The switch is somewhat unavoidable because the first paragraph discusses concentration errors, while the next one discusses errors in the column inventory (as indicated by the first sentence of that paragraph).

- *Page 8949, line 24: "uses observations" "uses transient tracer observations". It is important to emphasize that the TTD and GF approached do not use inorganic carbon parameters, or use them as a weak constraint*

    - The text has been modified to state this explicitly.

- *Page 8951, line 18: "assuming . . . increases proportionally to perturbation of atm CO2". It must be stressed that this is a fundamental assumption*

    - This assumption only applies to the OIP estimates and is justified because of the manner in which the basis functions were computed. This is now stated in the text.

- *Page 8952, line 29: perhaps mention papers by Brewer et al., and Keeling and Peng who were some of the first to assess carbon transport across 26 N Brewer, P.G., Goyet, C., Dyrssen, D., 1989. Carbon dioxide transport by ocean currents at 25 N latitude in the Atlantic Ocean. Nature 246, 477-479. Keeling, R.F., Peng, T.-H., 1995. Transport of heat, CO2 and O2 by the Atlantic's thermohaline circulation. Phil. Trans. R. Soc. Lond. 348, 133-142.*

    - These were indeed among the first papers to assess carbon transport, but as they deal with the transport of total carbon rather than C_{ant}, we're not clear as to how relevant they are to the present discussion.

- *Page 8955, line 5: "transport" = "inorganic carbon transport"*

    - Done.

- *Page 8959, line 12: it would be helpful to put these numbers in context, also note change of units* *in storage rate from page 8958 line 12.*

• We now also mention the relative uncertainty and provide the actual storage rates in the corresponding table as per the reviewers comment. We have also added a sentence at the end of Sec. 4.1 to alert the reader to the different units.

• *Page 8960, line 25: It might be worth a few words how these estimates for the marginal seas were determined. Are these more or less "wild guesses" based on area?*

• As discussed in Sec. 2.2, these estimates are based on either the ΔC^* or TTD methods. We now mention this again in Sec. 5.

• *Page 8961, line 17: "variability" = "variability and trends"*

• Done.

• *Table 1: Why is the difference between the GLODAP region and model domain not constant for the CCSM models (the difference between number and number in brackets ranges from 9 to 11.*

• These are different realizations of the CCSM model, i.e., run with different forcings and parameter values. Thus the relative (or absolute) difference between the GLODAP region and model domain is unlikely to remain constant.

• *Table A2. Why isn't Green function uncertainty listed here?*

• We're sorry for the omission. This has been added.

• *Table A4: include the average transport and location*

• We have added this information.

• *Table A5: include average storage rate*

• We have added this information.

Reviewer #2

General comments:

• *1. The major issue that should be clarified in a revised manuscript is the relation- ship between the anthropogenic CO2 uptake inferred by the GF approach and that simulated by the ECCO model (as shown in Figures 2, 3, 4, and 6). The estimates show a remarkable similarity, although as I currently understand it the GF estimate and the ECCO estimate are totally independent. Are the two estimates independent? Or was the ECCO circulation somehow used as a prior for the GF estimate? If they are totally independent, why is there such excellent agreement between the two estimates? What is the RMS difference between the ECCO and GF estimates? I would also like to see GF-ECCO differences for the column inventory (Figure 2), the basin profiles (Figure 3) and the depth profiles (Figure 4) for only the GLODAP region (I assume that the ECCO points in* *the North Atlantic plot in Figure 4 include non-GLODAP regions and that is the reason why the ECCO estimate is uniformly higher than the GF estimate there).*

• The GF estimate presented here and ECCO are not completely independent (they were in previously published estimates). We should have noted this key point in the manuscript and thank the reviewer for raising it. The two are related in the following way. In computing the prior, we fit a 1-d inverse Gaussian form to CFC-12 data. In previous versions, we used a width/mean ratio of "1". In the estimates used here, we used a TTD simulated by an annual mean ECCO circulation with an impulse boundary condition at the surface of the ocean (no spatial variation). This simulated TTD was then spatially averaged over 20 deg x 20 deg boxes and a width/mean ratio computed for each box. This ratio was then used when computing the prior. The reasoning behind this procedure was to arrive at an improved prior (as a width/mean ratio of 1 is not always justified), yet reduce its dependency on a model. (Information from the model about mixing, i.e., where water at any given location comes from, or time scales, is not used.) (The eventual goal is to use priors derived from state estimates such as ECCO, but, as discussed by Holzer et al. (JGR, 2010), averaged so as to not introduce detailed information about circulation that is not expected to be accurate.) This dependency could in part explain the similarity, but we also note that this procedure leads to results that are very similar to one in which a ratio of 1 is used. Another explanation is that both ECCO and GF are heavily constrained by observations. We now discuss this at length in the manuscript (at the end of Sec. 2.1) and also note the similarity (and minor differences) based on the new plots requested (end of Sec. 2.2).

The RMS difference (in 2010) between ECCO and GF is $\sim$3 μmol/kg.

We attach the requested figures to this response (we take it the reviewer did not necessarily want them included in the paper). The reviewer is correct that the North Atlantic difference in the basin profiles is reduced when the GLODAP mask is applied to ECCO (but less than expected since the area concerned - primarily the Nordic Seas - is relatively small).

• *2. Page 8938, lines 11-12: Only the mean of the TTD was estimated from CFC-12 data. The mean/width ratio was specified.*

• The reviewer is right. Only one parameter is estimated in the TTD method. This is now mentioned.

• *3. Page 8941, first paragraph: Should also note that negative values have been removed from the* ΔC^* *method, resulting in* $\sim$*10% higher inventory than if they are left in.*

• This is now mentioned in the text.

• *4. Page 8944, lines 11-13: Minor point, but how does one know whether the errors are due to errors in the CaCO3 cycling model or in the circulation? It seems likely that the circulation (too* *weak ventilation) is the main problem, given the discussion in Graven et al (JGR, 2012).*

• The reviewer is right that it is hard to attribute this solely to the CaCO3 model. Our reasoning here in trying to explain problems with simulated surface C_{ant} values was that the CaCO3 model impacts the surface buffer factor. This could have an effect on both surface C_{ant} concentrations (as well as interior ones in waters ventilated from that region). On the other hand, errors in circulation tend to cause primarily the deep ocean C_{ant} to be wrong, whereas the surface ocean values are much less affected. Thus, depending on where one looks, either of these or both play a role. We have modified the text to clarify this point.

• *5. Page 8946-7, last paragraph (and Table A3): The degree to which mapping errors have been quantified deserves some comment. As I understand it, the errors in Table A3 do not include mapping error, except perhaps in some very ad-hoc way. For the GF estimate, a Monte Carlo approach was used to determine the uncertainty. Was the mapping error taken into account in this estimate (e.g. by producing new maps of T, S, CFCs etc. using an objective mapping procedure for every member of the suite of MC simulations)? Or were the observational errors assumed independent (in which case their effect would largely cancel out upon globally integrating).*

• This is a good point. In the GF method the errors are estimated very crudely by simply perturbing at each point the various tracer data, i.e., observational errors are assumed independent. The more sophisticated approach of generating multiple realizations of the observational field that the reviewer suggests would clearly be a better way to estimate errors. We note, however, that the GF method is applied to the gridded GLODAP dataset which was produced using an objective mapping analysis. That analysis also produced error fields which were used in the above MC procedure. The ΔC^* and TTD estimates were gridded using objective mapping. This is now discussed in the text (Sec. 2.3).

• *6. Table A3: It would be nice to see the uncertainties given as percentages as well, to allow for easier comparison*

• We have added percentage uncertainties to the table.

• *7. Page 8952, line 8-10: This deserves more comment - why do some of the OIP and EnKF models have much stronger storage and transport in the Southern Ocean?*

• In the OIP, one of the models showed a much larger Southern Uptake. In fact, the model that found this extreme anthropogenic uptake in the Southern Ocean also over estimates CFC uptake in this region compared with observations quite badly, pointing pretty clearly to model error. Due to the fact that the reported mean of the ocean inversion is weighted by a skill score based on CFCs, this model doesn't have much influence on the reported mean result. In case of the EnKF, these much larger uptake rates in the Southern Ocean have to do with the too vigorous convection of the

Bern3D model. This vigorous convection was needed in order to get enough deep water formation. This is a consequence of the very coarse model resolution in the Southern Ocean making it difficult to fully represent the different water masses and their ventilation timescales. So, these uptakes rate are an artifact of the Bern3D model. This is now discussed in detail in the text.

• *8. Figure 6, northward transport for GF: How are the northward transports for the Indo-Pac and Atlantic separated? One knows the flux in from the atmosphere and the storage rate, but what about the flux in from the Southern Ocean? And what about the northern boundaries?*

• It should be pointed out that for all methods the same approach of integrating the continuity equation was used to compute transports. This is true for the GCMs as well since we did not have access to the explicit lateral fluxes computed by the models. So the reviewers' point applies to all the estimates.

At the northern edge, a no-flux boundary condition was applied and the continuity equation was integrated southward starting from the northern boundary. Specifically, northward transport at the southern edge of a grid box is equal to the storage + transport at the northern edge of the box - the air-sea flux into the box. The integration (and the plots) stop at 45 deg S (about as far south as one can get before running into the problem the reviewer noted).

The reviewer raises a good point about the northern boundary condition. In many of the models used here there is no Arctic so a no-flux boundary condition is justified (or at least consistent). CCSM does include the Arctic, but we don't know (i.e., have access to) what the transports into and out are. However, assuming a Bering Strait outflow into the Arctic of 1 Sv and an average C_{ant} concentration of 40 μmol/kg gives a northward transport of $\sim$0.015 PgC/y. This would shift all the Indo-Pacific curves upward by that amount. The Atlantic curves would – in steady state – shift downward by a similar amount. We have added the above points to the text.

• *9. Page 8961, last sentence: I don't see any "important differences" between the ECCO Cant estimate and the GF Cant estimate. See #1 above.*

• This statement refers not just to ECCO relative to GF, but to other data-based estimates as well (which are weighted equally in arriving at our final "best estimate"). We have changed the text in light of the difference figures the reviewer asked for.

• *10. The conclusion (particularly the last sentence) is rather weak. The "multiple approaches" discussed in the manuscript have not reduced the uncertainty on Cant uptake, so it seems that "multiple approaches" is not necessarily the answer. It would be nicer to discuss some ways in which the uncertainty and biases of the model and data-based methods could be reduced so as to arrive at better Cant estimates.*

• The reviewer is right that the existence of multiple approaches hasn't necessarily resulted in a narrower range. But it has made it possible to assess different methods and put bounds on the distribution of ocean anthropogenic CO_2. In that sense, the availability of different method can be argued to have led to a more robust understanding and remain necessary. That said, the uncertainty and biases of the data-based methods should go down as newer datasets such as GLODAP version 2 come out, and with better observational coverage (both in space and time). As for models, improved and higher resolution (eddy-permitting) physical state estimates as well as joint physical-biogeochemical data-assimilation that uses both in-situ and space-based measurements seems to offer scope for improvement. We have reworded the conclusions and briefly mention the above points, but a more detailed discussion seems beyond the scope of the present paper. The last two paragraphs of the Conclusions section now read:

We have also compared anthropogenic C_{ant} simulated by forward ocean biogeochemical models with the data-based estimates. Substantial regional differences exist between ocean forward model C_{ant} fields and data-based estimates, as exhibited by the CCSM model variants that tend to underestimate global C_{ant} inventory. The forward model biases reflect ongoing issues with forward ocean model physical circulation. The data-constrained physical state estimation as exhibited in the ECCO simulation improves the spatial patterns of the simulated C_{ant} field, although some differences with the various data-based estimates remain. This suggests the use of physical state estimates as a weak constraint, such as in the computation of priors required by the Green function approach (with suitable averaging so as to not introduce detailed information about model circulation that is not expected to be accurate (Holzer et al., 2010)). Nevertheless, the experience suggests that forward OGCMs can be improved through careful model-data comparisons and process level studies. Forward OGCMs also offer opportunities to help interpret climate-driven variability and trends as well as projecting future behavior of ocean carbon storage.

Lastly, a compilation of inventories based on different methods gives us a "best" estimate of about 155 PgC for the global ocean inventory of anthropogenic carbon in 2010. The uncertainty on this estimate is $\sim \pm 20\%$. The large range in various estimates (Table 2), and our comparison of different methods suggests that multiple approaches, each with its own strengths and weaknesses, remain necessary to quantify the ocean sink of anthropogenic CO_2. Future progress in reducing this uncertainty is likely to come from newer datasets such as GLODAP version 2 and better observational coverage (in both space and time), as well as the development of improved and higher resolution physical state estimates and combined physical-biogeochemical data-assimilation systems that can exploit these data.

Technical corrections

- *Equation (1): should define symbols x and t*

- These are defined just before the equation.

- *Page 8939, line 6: define RECCAP*
  - Done.
- *Page 8939: State the resolution of the CCSM*
  - All of the CCSM-3 simulations are at a resolution of 0.9–1.9°latitude by 3.6°longitude with 25 vertical levels. This is now stated in the text.
- *Page 8940: State the resolution of ECCO*
  - The ECCO model has a horizontal resolution of 1°with 23 vertical levels. This is now stated in the text.
- *Page 8940, lines 10-11: Estimating the Circulation and Climate of the Ocean*
  - Fixed. Thanks!
- *Page 8948, line 10: The assumption of the* ΔC^* *method is more properly stated as "no mixing"*
  - We have changed the text, but note that this approach can (and in some implementations does) account for mixing between different water masses when, for instance, computing the air-sea disequilibrium term. (This is now mentioned in the text.) What it does not account for is the distribution of times since last contact with the surface (which, as the reviewer noted, implies no mixing).
- *Page 8953, line 16: "Remarkably robust" seems to be overstating it - maybe just "robust"*
  - The text has been modified accordingly.
- *Page 8954, lines 24-25: The text refers to four red lines, but there are only two red lines in Figure 6.*
  - Thanks for pointing out the inconsistency. The text has been corrected to say "two red lines".
- *Page 8957, line 15: "evidenced" (typo)*
  - Fixed. Thanks!
- *Page 8957, lines 23-27: This is confusing. The "earlier" study is that of Sabine et al. (2008) while the study of Waters et al. (2007) is referred to as more recent. Furthermore, somehow the 2007 study is based on data from 2010. Typo?*
  - This was a typo. It should have been Waters et al. (2011). Thanks!

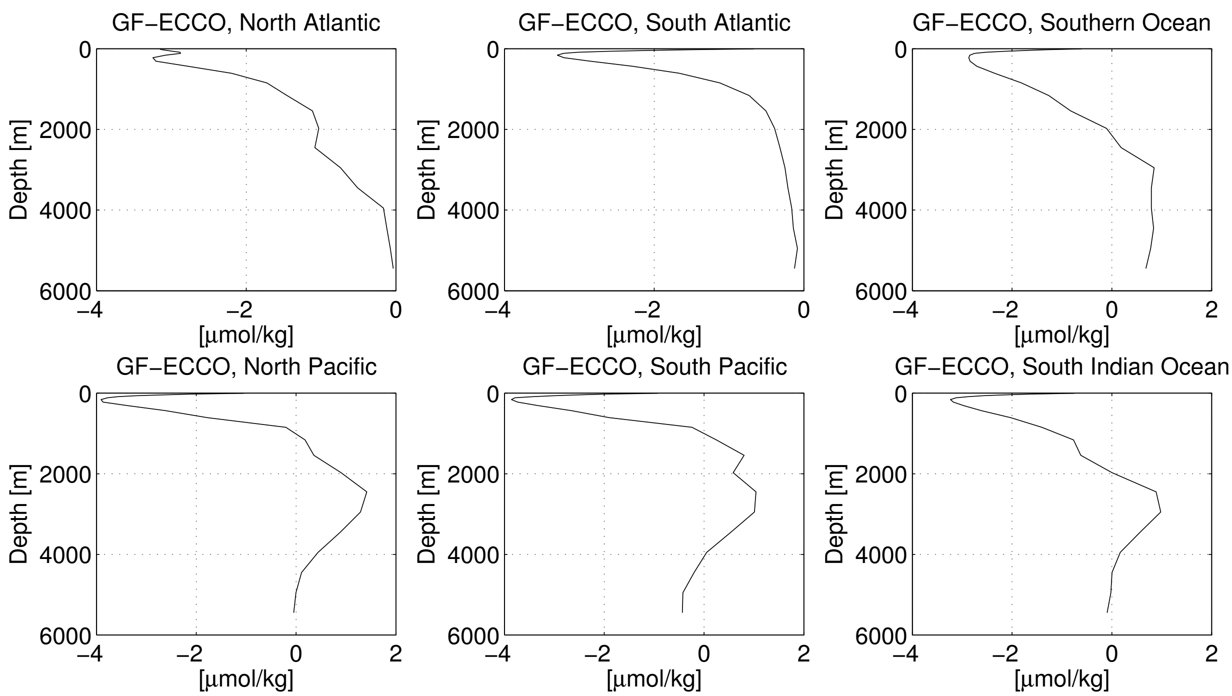

Figure 1: GF-ECCO profiles.

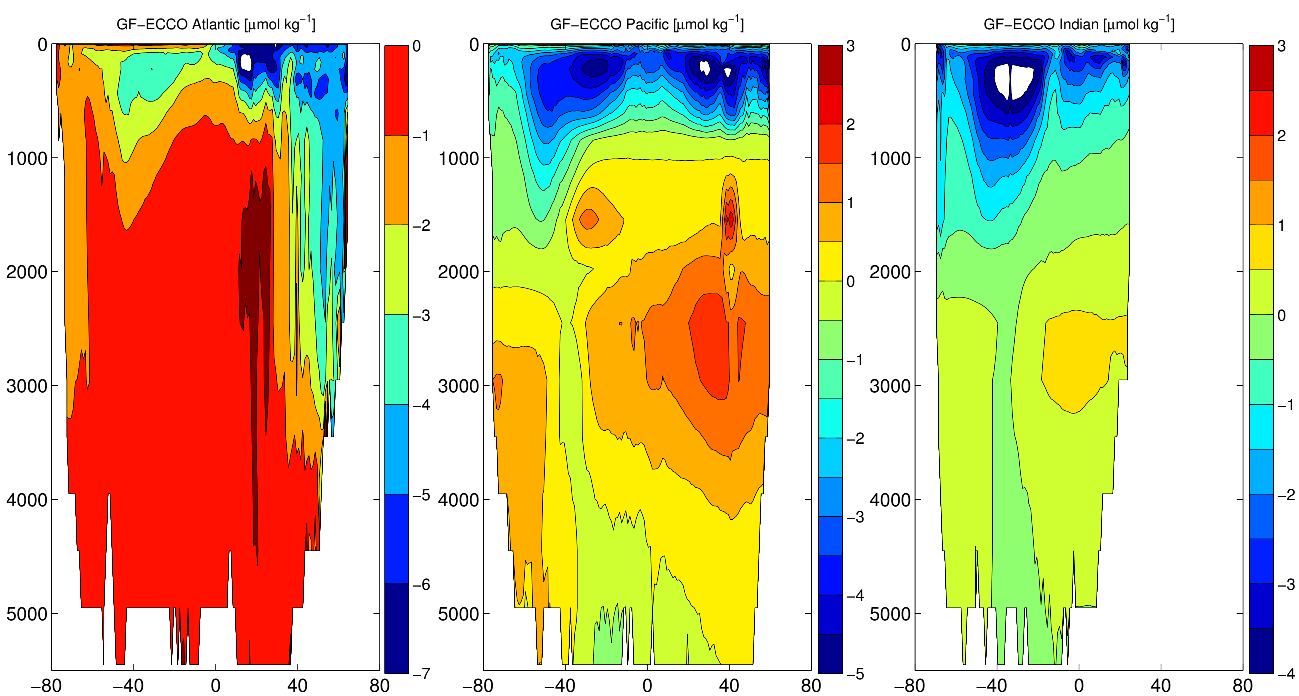

Figure 2: GF-ECCO zonal mean.

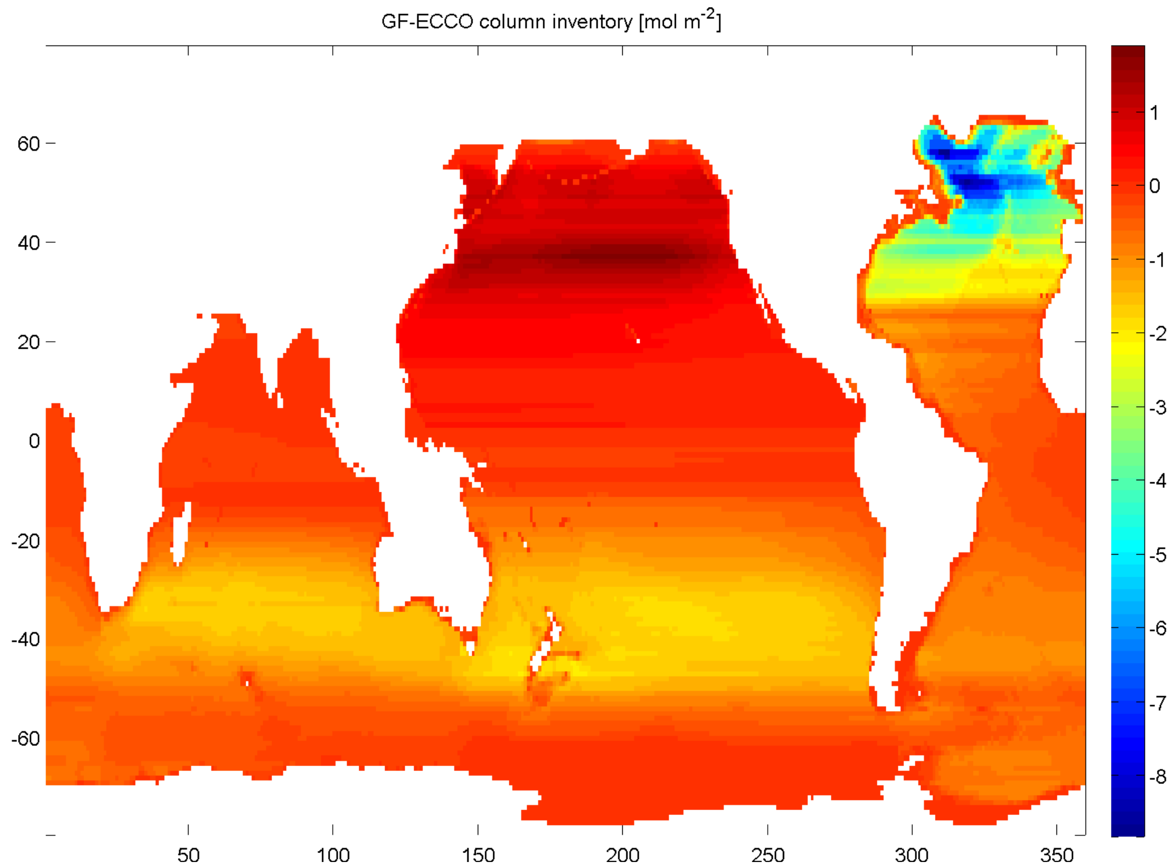

Figure 3: GF-ECCO column inventory.