

We are thankful to both reviewers for their thoughtful comments on the manuscript. After giving a careful reading to the review of Referee #1, we realized that our manuscript fell short of conveying to an expert reader either the motivation or the strength of our results. This was confirmed by the review of Referee #2. It is therefore clear that we will need to conduct significant revisions of the text, the figures, as well as the structure of the manuscript to achieve clarity. This will require some time, but in the meantime we wish to outline how we would like to respond, and to clarify some of the concerns raised by Referee #1 (who was the more critical of the two referees). We were particularly concerned with the comment of Referee #1, who stated that: *'There is no logical sequence of the individual parts of the paper. The investigation of what drives the seasonal cycle does not seem to be needed in later chapters, and it is not even clear whether the results would look much different if the volume transports were simply multiplied by an average DIC concentration.'*

Our study was originally motivated by some of the outstanding controversies surrounding the role of the Southern Ocean in the global carbon cycle. A wide number of mechanisms have been invoked in the climate literature to account for dynamical controls on the carbon cycle, including wind forcing, the thermohaline circulation, ice cover, to name just a few. While the three-dimensional character of the Southern Ocean is typically acknowledged, most analyses ignore physical processes occurring in the ocean interior. For studies that consider the biological pump, the focus is carbon uptake in the upper layer and its remineralization at depth. For dynamical studies the focus is often on the overturning of deep waters and mode/intermediate waters masses, and there typically much of the analysis describes surface interaction with the atmosphere. In both approaches, upper layer processes are the principal concern.

Our goal here is a scientific one: to understand the ocean role in climate. It is to this end that we set out to develop a new framework that accounts for ocean interior processes (mixing, entrainment, chemical and physical transformations, etc.) that may be at least as important as surface processes in controlling the carbon distribution in the ocean interior. Another way of framing the approach is to consider the critically important process of gas exchange at the surface, which determines the partitioning of CO<sub>2</sub> between the ocean and the atmosphere, and thereby climate. What interior three-dimensional processes are reflected in the sea surface pCO<sub>2</sub>?

Critically, it is known that winter surface properties imprint themselves on the ocean interior (the "Stommel Demon"), so by default the seasonal cycle and watermasses need to be invoked to connect surface and interior carbon cycling for process understanding. In other words, any account of the connection between surface and interior properties must be understood within a watermass framework that includes the full seasonal cycle. For the case of carbon, it was our intention that our analysis begin first and foremost from this well-established process understanding of air-sea interaction and subduction, and then to extend this to other aspects of the carbon cycle. We chose to do this using a three dimensional model, which despite its limitations (no model is perfect) allows us complete three-dimensional representation of the ocean state.

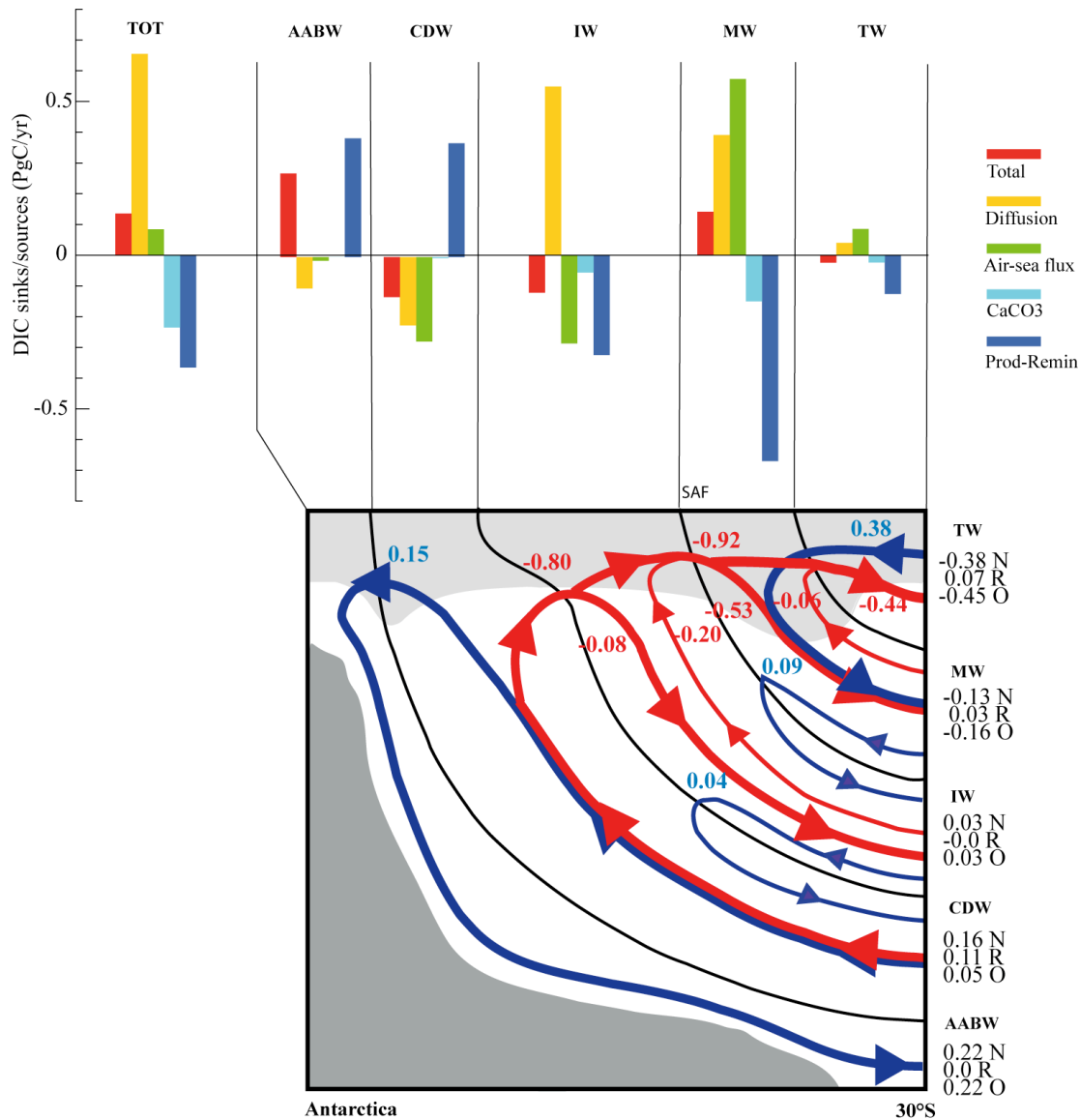
The modeling configuration we use consists of the OPA circulation model, and the PISCES biogeochemistry model. We specifically chose the circulation scheme of the published studies of Iudicone et al. (four Journal of Physical Oceanography papers), and the published version of the PISCES model (Aumont and Bopp, 2006). We wish to emphasize that this state-of-the-art model has a number of biases that are characteristic of all global ocean models, especially those that include alkalinity as a prognostic tracer. Our goal here is NOT to claim that the model run presented here is best understood as a perfectly realistic simulation of global biogeochemistry. Rather the model is meant to be understood as a tool for understanding processes, which will serve the larger community interests of interpreting measurements. In our humble contribution to the larger community efforts, our purpose is one of processes.

The joint Eulerian-Lagrangian diagnostics presented here are thus intended to elucidate *processes* controlling the oceanic carbon cycle, and ultimately the partitioning of carbon between the oceanic and atmospheric reservoirs (climate). Such diagnostics have not previously been applied to the oceanic carbon cycle. Again, the tool that we have used to develop these diagnostics (a three dimensional model) is a “work in progress” with a number of biases that are common to all global biogeochemical modeling efforts, but the diagnostics and process understanding presented here is independent of the imperfect state of global biogeochemical modeling.

As was pointed out by both Referees, the manuscript we submitted was quite long and difficult to follow. Our goal in revising the manuscript will be to both rewrite and restructure much of the text for the sake of clarity. We understand now that it is particularly important to emphasize in the Abstract, Introduction, and Concluding section that this study with a widely used and widely documented biogeochemistry model is intended to elucidate processes, rather than to tune the existing model to improve the representation of the carbon cycle. Additionally, significant efforts will be dedicated to focusing the individual sections.

Perhaps most importantly, we will much more clearly explain the underlying background understanding required to understand the Lagrangian tools, and how these contribute to process understanding. In particular, instead of organizing the presentation of the work by *method*, we will merge the Eulerian and Lagrangian results in sections organized by *ocean process*, with, first, a section of the role of the overturning in the redistribution of the tracer and, secondly, a section on the processes other than the overturning (diffusion, etc). To give an idea of the possible outcome, the results on this last subject will be presented in one unique figure (Figure I below) that merges the Lagrangian analysis of sinks/sources (previously Figure 15c) and the budget of each sink/source per water mass (previously Figure 14b).

Thus we ask the Editor if he/she agrees that our revising the manuscript would be encouraged, in the hopes of publishing in BGD.



**Figure 1.** Upper panel: the Eulerian net budget (in PgC/yr) per water mass for each process involved in the evolution of the DIC. Lower panel: net Lagrangian DIC budget (in PgC/yr) per pathway (see the text for more explanations). The case of the pathway of TW transformation into MW illustrates well how different and complementary are the two informations. The Lagrangian analysis shows that TW → MW implies a gain of 0.38 PgC/yr but little occurs to DIC in the TW class (Eulerian analysis above). The interpretation of this result is that most of this gain occurs after the TW was transformed into MW. Further, we can also appreciate that the gain in that density class is due to both air-sea fluxes and diffusive processes and that these processes overcome biology for this specific pathway. Finally, if we consider only the surface layer, it is clear from the previous Fig. 14c (see manuscript) that diffusion is actually the most important process acting on the tracer associated to this pathway before the water injection into the interior.

## **A crucial issue is the anomalous alkalinity values and distributions.**

The modeling configuration that we used for this study consists of the OPA circulation model, and the PISCES biogeochemistry model. We specifically chose the circulation scheme of the published studies of Iudicone et al. (four Journal of Physical Oceanography papers plus more details in Iudicone Thesis, which is available), and the published version of the PISCES model (Aumont and Bopp, 2006). No changes have been made to any of these configuration. The circulation model itself has been extensively evaluated with a wide number of tracers (CFCs, C14, He3, hydrography, mixed layer depth, sea surface height), it has been used in the 4 JPO papers and it represents the best of the set of runs presented in Dutay et al. (2009). Just to mention CFCs, the inventory along the Ajax (Atl) and Pacific section is very good, far better than most of similar models while we overestimate the ventilation in the Indian Ocean by a factor two. In summary, the limits and positive aspects of the physical simulation are well known and have been published before.

The biogeochemical scheme PISCES has also been extensively evaluated (see Auxiliary Materials of Aumont and Bopp, 2006). Nevertheless, we wish to emphasize that this state-of-the-art model has a number of biases that are characteristic of all global ocean models, especially those that are run up a few thousand of years to equilibrium and those that include alkalinity as a prognostic tracer (alkalinity is prognostically computed based on a variety of processes, carbonate production and dissolution, organic matter production and remineralization, ...see eq. (42) of Aumont and Bopp 2006 – Auxiliary Material for a complete equation).

In no way did we intend to cover or hide problems in the modeled Alkalinity fields (as they were already mentioned in the original manuscript). The figure was in black and white simply because the division of labor amongst the coauthors led to different plotting routines to be used for some of the figures.

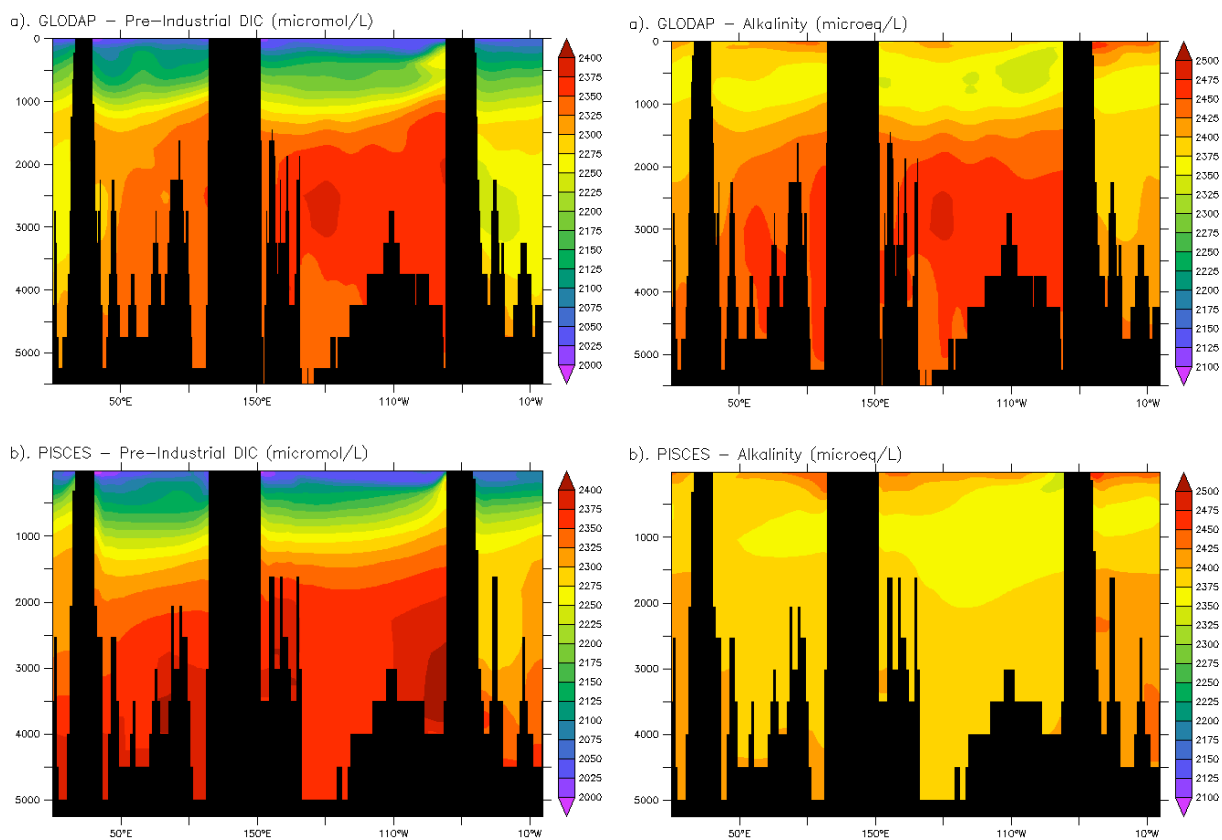
Importantly, a number of the coauthors of the manuscript have broad experience with measurement collection, processing, and interpretation. Thus we would like to emphasize (in response to the comments of Referee #1) that care has been taken in the interpretation of GLODAP data. In fact, conversations with Robert Key (the author of the GLODAP paper) had confirmed our understanding that GLODAP is not intended to skillfully represent DIC gradients in the upper 200 meters due to strong seasonal biases in the underlying measurements as well as the data processing. Conversations with Robert Key had also confirmed our supposition that mapping errors are expected to be non-trivial in the Southern Ocean, due to data sparsity issues. For this reason, the GLODAP estimate of pre-anthropogenic DIC should be expected to have non-trivial errors in its representation of the zonal mean. Last but not least, even if a perfect state estimate of pre-anthropogenic DIC were available, one would need to zonally average using a streamfunction or density to avoid problems due to the non-zonal character of the Southern Ocean frontal system.

As a result of these considerations, we now compare DIC and ALK sections at 30°S (color figures attached) which correspond to WOCE cruise tracks and therefore are not compromised by significant mapping errors. As is mentioned in the manuscript, we do in fact clearly underestimate alkalinity at depth (by as much as 75  $\mu\text{mol/L}$ ) in the Pacific and Indian basins. On the other hand, deep ocean DIC concentrations in all three basins, and Alkalinity in the deep Atlantic ocean, suffer from significantly smaller biases. Reviewer #1 had commented that “the vertical DIC gradient is 50% too high in the model, and vertical alkalinity gradient is far too low”. Although we concur with regard to the Alkalinity gradient in the Pacific and Indian basins, the reviewer is mistaken with regard to the DIC gradient. In fact this was largely the fault of the authors – in the text of the submitted manuscript, we had erroneously stated that the DIC gradient is as much as 50% too large, but this is certainly NOT the

case everywhere in the Southern Ocean (see attached figure). This was clearly a point of concern for Referee #1, and we apologize for this misunderstanding.

As for the reasons for the models biases, the deficiency in Alkalinity is due to the fact that there is too little carbonate production and export in the version of PISCES used here (the version used in the reference publication of Aumont and Bopp [2006]). This bias is known, and in fact in a more recent study this has been adjusted for the case where both calcite and aragonite production are considered (Gangsto et al. BG, 2008). However, as we would clearly state in the revised manuscript, the important surface-to-surface gradients in the version of PISCES used here do in fact capture the important gradients down to 1000 meters depth seen in the observations, and that these are the critical gradients over the critical scales that regulate sea surface pCO<sub>2</sub> through the effect of upwelling. In this sense, our study should not be understood as being a model validation exercise. We wish to underscore that our intention is to use the model to understand processes that determine the distribution of carbon in the ocean and the exchange with the atmosphere.

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**Figure II:** Pre-industrial DIC (micromole/L) and Alkalinity (microeq/L) concentrations, at 30°S, obtained from the GLODAP database and the PISCES model. Pre-industrial DIC was obtained by subtracting anthropogenic DIC to total DIC.

## DETAILED RESPONSE TO REFEREE #1

### **p. 3396, I.8 circular argument**

It will be changed.

### **p. 3396, I.25 integrated, not averaged!**

Integrated, of course. We apologize for the error.

### **p.3398, I.11 How is the modeled pre-industrial CO<sub>2</sub> flux compared with the industrial observational estimate of Takahashi et al.?**

The comparison is presented briefly in Section 3. The Takahashi field will be added as a figure to ease the comparison. The same will be true for the Mikaloff-Fletcher et al. (2007) estimate of the natural carbon fluxes.

### **p.3399, I.18. What is the interior diffusivity in the model**

The model background vertical diffusivity increases from the surface to the bottom in order to mimic the effects of decreased stratification and increased small-scale turbulence near the bottom. (Values ranges from  $0.12 \cdot 10^{-4} \text{ m}^2\text{s}^{-1}$  in the first 1000m to  $1.2 \cdot 10^{-4} \text{ m}^2\text{s}^{-1}$  at 5000m.)

### **p.3399, I.20. Why should there be a "finite" number of water masses? Aren't there an infinite number of watermasses?**

"In his original brief monograph, Helland-Hansen (1916) introduced the concept of a water mass as being defined by a temperature-salinity (T-S) curve. He found that over a large area of the eastern North Atlantic a "normal" T-S curve could be drawn. He showed that variations from this curve could be attributed to the intrusion of alien water masses that had originated elsewhere. The use of the T-S diagram has been almost universal in physical oceanography since Helland-Hansen introduced it. It is not only a powerful descriptive tool, but observers at sea routinely plot T-S diagrams and use them as a check on the tightness of their sampling bottles and the correct function of their thermometers. The term "water mass" has been very loosely used by numerous authors. According to Sverdrup, Johnson, and Fleming (1942), a water mass is defined by a segment of a T-S curve, and a "water type" by a single value of temperature and salinity that usually falls on a T-S curve. Thus a T-S curve is made up of an infinite number of "water types. These definitions will be adhered to in this chapter as far as is possible." (From L. V. Worthington "The Water Masses of the World Ocean: Some Results of a Fine-Scale Census" in [Warren, Bruce A.](#), and [Carl Wunsch](#), eds. [Evolution of Physical Oceanography](#). Cambridge, MA: [The MIT Press](#), 1981. ISBN: 0262231042.). In literature there are several definitions of water masses, always defined as a classification of water types into a finite set (e.g., Defant, *Physical Oceanography*, Pergamon Press, 1961, pag. 216). This finiteness in fact made the concept of water mass so powerful and so widely used in oceanography since its very beginning. It is clearly a practical concept. As in most recent papers, we define a water mass as a body of water with a common formation history. This means that, locally, the water mass characteristics are also the result of the subsequent transport and mixing in the ocean interior and any choice of a tracer range of values to identify it will suffer of some limitations. Our choice is to use neutral density as a framework for the definition of the Southern Ocean water masses, for the reasons illustrated in Trevor McDougall's papers (resumed in Iudicone, Madec and McDougall, 2008) and coherently with most previous studies on the Southern Ocean (e.g., Sloyan and Rintoul, JPO, 2001).

### **p.3400, I.8. Is a two-week mean sufficient to conserve adiabatic processes?**

Yes, for a non-eddyding ocean forced with monthly-varying surface fluxes. For the computation of

water mass transformations, two-week sampling does introduce noise into the computation of water mass transitions, and this was addressed in the previous study of Iudicone, Madec, and McDougall (2008). For the Lagrangian analysis, previous work (through the European TRACMASS Project) has clearly demonstrated that the sampling of model output should resolve the frequency of the model forcing (in this case monthly).

**p.3400, I.9-11. I do not understand what you want to say by this sentence.**

We will rewrite this to avoid confusion. In the submitted version of the manuscript, we did not linearize the computation of the air-sea fluxes. We agree completely with the belief of the reviewer that linearization is not appropriate, and for this reason we chose an approach that is respectful of nonlinearities in the system.

**p.3400, I.20, Is "t" temperature? which units? Degrees Celsius? What are the units of wind speed (and at which height is it assumed)?**

T is temperature in degrees Celsius. Wind speed is in m/s and a standard 10m height is assumed. We do apologize for the missing information.

**p.3400, I.24, Is "T" temperature? Which units? Kelvin?**

Yes, T is temperature in Kelvin. This is being corrected in the text.

**p.3401, I.9ff Why are you interested in the relative contributions? Why should the reader be interested in these?**

There are two reasons:

- 1) Water mass properties are set at the moment of the formation and lately altered by mixing and biogeochemical processes. It is then absolutely important to resolve the seasonal cycle of the surface properties (mixed layer, currents, tracer values, etc) to understand the relevant processes. The formation generally occurs at the end of winter and thus the winter surface properties are of great interest. This is for example clear with AAIW. In summer, the density class of the AAIW includes the Antarctic Surface Water (lying above the CDW) and thus extends much further south with respect of the region of AAIW formation. Therefore, if one is interested to the processes that set the AAIW water mass formation, the local winter air-sea fluxes have to be considered as the main local forcing on DIC. At the same time, it is important to underline that the Southern Ocean water mass subduction is characterized by a large contribution from the lateral transport across the mixed layer, which occurs in all the seasons. It has to be made clear that the analysis here presented constitute a description of the processes involved in the DIC cycle in the Southern Ocean which is made of pieces only partially connected and it is not easy to link one analysis to the other. We are now very well aware of this and raises a clear need to improve the manuscript. This fragmentation is also due to the fact that there are at present no theoretical tools that allows for a comprehensive analysis. Such a tool (or algorithm) should allow for a quantitative Lagrangian analysis of the dynamical evolution of the DIC in a model. This tool simply does not exist at the moment and only qualitative (yet, very interesting) steps have been made in this direction (Verdy et al., GBC, 2007). Nevertheless, the approaches here presented push our ability in understanding the DIC dynamics and we strongly believe they represent a significant step in the right direction.
- 2) Recently there has been growing interest in the driving mechanisms for the large amplitude seasonal cycle in air-sea CO<sub>2</sub> fluxes over much of the extra-tropical ocean. Thus our intention here was to connect the main results of our analysis to this important research question.

**p.3402, eq.4 What are  $G^{ZZ}$  and  $G^{MM}$ ?**

$G^{ZZ}$  and  $G^{MM}$  represent excretion terms from microzooplankton and mesozooplankton, respectively. This will be added to the equation description.

**p.3402, I.21 Eq.5=Eq.4? The entire section 2.2 does not add any new information.**

We agree with the referee. The section will be eliminated.

**p.3404, I.9 vs 13. Do the authors consider results of previous modeling studies as "observations"?**

Data inversions are somehow midway between modelling exercises and observations but we agree with the referee that the title has to be changed.

**p.3404, I.24, and many other places. The sign is wrong. Air-sea flux describes the flux from the air into the sea, not vice versa.**

OK

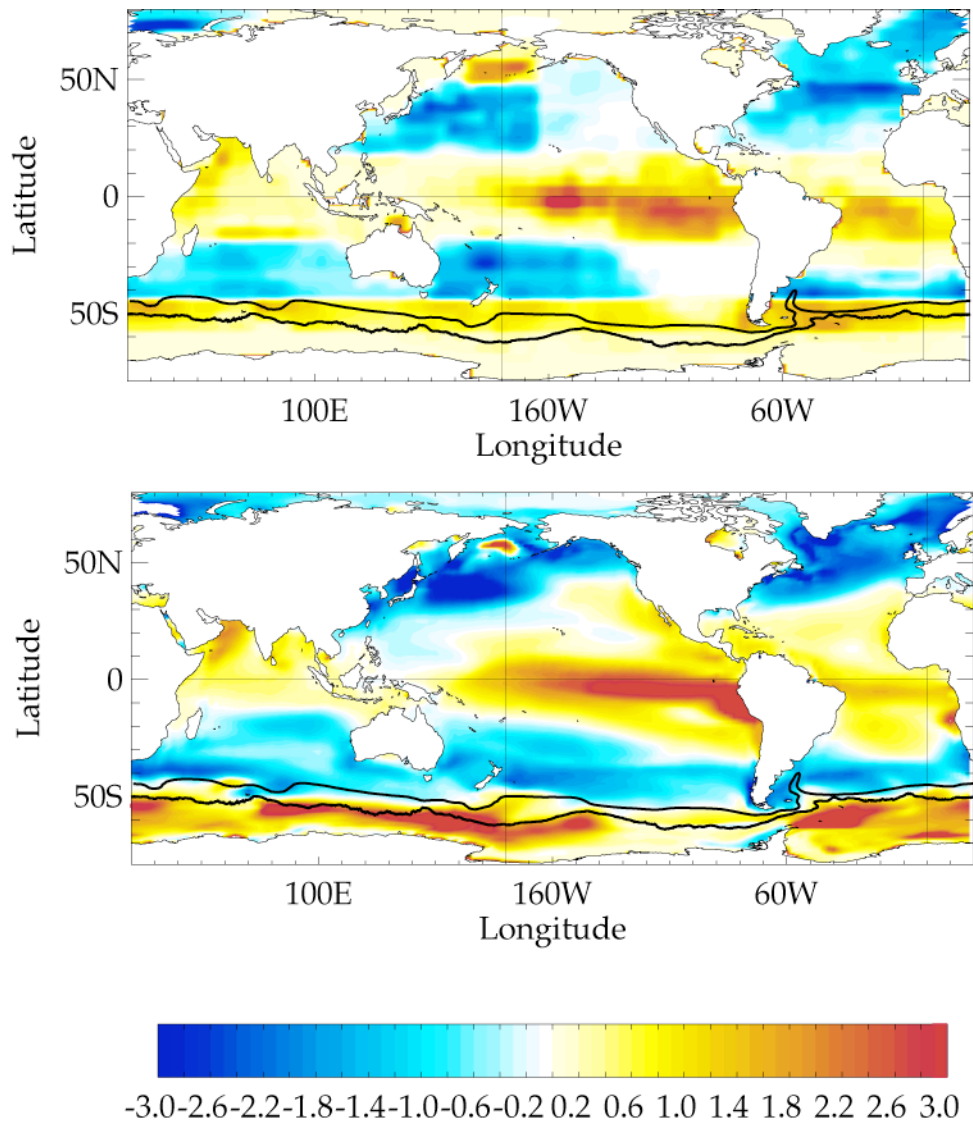
**p.3406, I.21 Does the Lagrangian analysis account for this significant residual eddy transport? Presumably this can be done by accounting for the bolus velocity inherent in the eddy parameterization. Has this been considered?**

Yes.

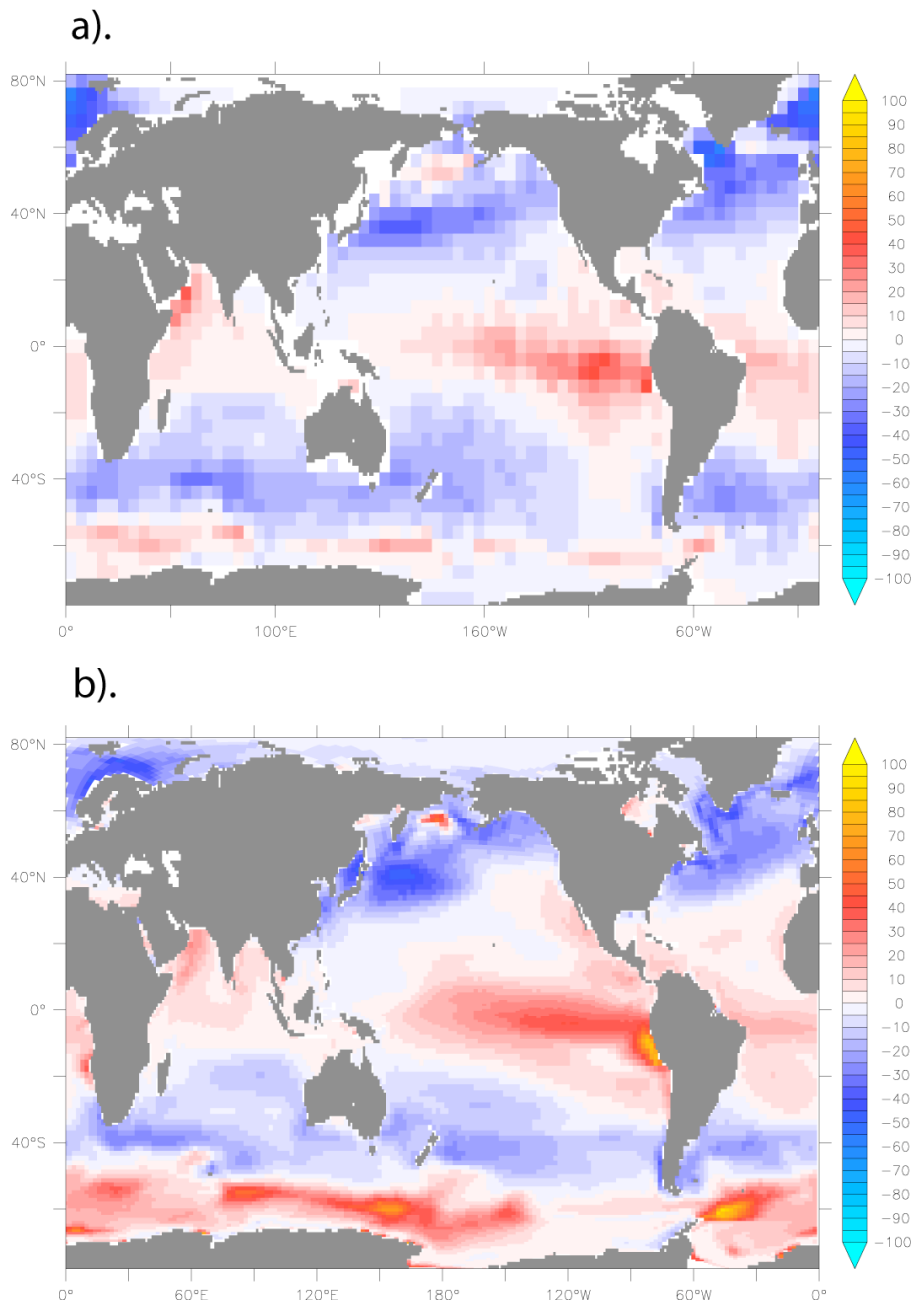
**p.3407, I.1 What is the residence time of upwelled water at the surface? If it is not long compared to the equilibration time scale of CO<sub>2</sub>, then there is no reason to expect that upwelling and outgassing should be correlated.**

We agree that the sentence was too strong and needs to be clarified. In general, in the Southern Ocean the residence time is from one to a few years (see Ito's papers and Iudicone, Blanke, Madec, Speich, JPO, 2008). The characteristic time scale for air-sea CO<sub>2</sub> equilibration of the mixed layer is on the order of a year. Therefore, we agree with the referee that they should not coincide. First, because the net northward transport is significant and, in fact, the summer outgassing is downstream of the upwelling regions (assuming a north-eastward Ekman transport). Secondly, because the air-sea gas transfer coefficient depends on wind and SST and they are both higher north of the upwelling region. Finally and most importantly, because of the large winter ice cover (sometimes overlooked in literature). Nevertheless, they are correlated to the regions of upwelling and to the frontal dynamics more than in previous estimates, as can be seen in fig. 3 and it is clear in the figure below (Fig. III), where the lower panel is the same as in fig. 4 while as upper panel we have added the natural carbon flux estimated in Mikalkoff-Fletcher et al. (2007), which, as matter of fact, is the best estimate of the natural fluxes currently available (see also Gruber et al, GBC, 2009). The comparison with Takahashi et al. (2009), discussed in the text but not shown, is also reported (Fig. IV).





**Figure III.** Upper panel: annual air-sea fluxes (natural carbon) from Mikhalkoff- Fletcher et al. (2007) (mol/m<sup>2</sup>/yr). Lower panel: PISCES annual air-sea fluxes (mol/m<sup>2</sup>/yr).



**Figure IV.** Upper panel: annual air-sea total fluxes from Takahashi et al. (2009) (mol/m<sup>2</sup>/yr). Lower panel: PISCES annual air-sea fluxes (mol/m<sup>2</sup>/yr).

There is an important point that has to be considered and which was rarely discussed before. Most of the GCM underestimate the summer mixed layer depth in the Southern Ocean frontal regions, north of the upwelling. This could impact the equilibration time scales. The amount of such an impact is currently investigated in the framework of an on-going sensitivity study.

**p.3407, I.4. If I understand correctly, Fig.4 compares simulated air-sea exchange and observed frontal positions. What does this imply? Why not use simulated frontal positions? Are they similar to observed ones?**

See discussion just above. The positions of the boundary winter outcrop position in the model compares rather well with the SAF position, as can be seen in fig. 8. A detailed validation and discussion of the physical model frontal structure has been presented in Ludicone (PHD Thesis, 2007) and resumed in Ludicone, Madec, Speich and Blanke (2008). For the upwelling, we have chosen to represent the density isoline which separate the (upwelling) CDW from the AAIW. The northern boundary of the upwelling region is represented by the polar front by the actual extent of the upwelling is limited to some regions south of it.

**p.3407, I.7 "flux patterns" instead of "fluxes"**

OK

**p.3407, I.16ff. This is difficult to understand. Why use winter surface density distributions in the analysis of the seasonal cycle?**

As discussed above (and in many details in Ludicone, Madec, Speich, Blanke, JPO, 2008), the seasonality of the surface density field is very large. Therefore, if the interest is in linking surface processes and 3-D water masses distribution, the natural framework is the winter outcrop, which connects the surface fields with the 3-D interior and, thus, with the frontal structure of the southern ocean. For instance, summer values in the AAIW range corresponds to the region of CDW upwelling (Ludicone, Madec, Speich, Blanke, JPO, 2008). Similarly, especially because of the ice cover, the patterns of summer and winter air-sea fluxes differ considerably and thus the seasonal cycle of the air-sea fluxes is much more complex if analyzed following the meridional movement of the surface isopycnals.

Notably, as most Southern Ocean observations were collected during Austral summer, for a long time southern ocean fronts were defined by the value of selected isotherms at depth as a way to filter out the summer surface bias.

The assumption here is that the water stays in the winter outcrop region during the whole year. This is not completely true because of the significant northward Ekman transport. However, the seasonal meridional migration of surface density isolines is in fact much more rapid than the Ekman transport and therefore an analysis done following the seasonally varying field would be difficult to interpret. Again, only a Lagrangian approach would fully take into account the different processes but this tool is not available (while, as illustrated in the manuscript, a Lagrangian computation of the budget can be easily and profitably done).

I hope the referee will appreciate our intention to be as much as possible respectful of the dynamics, which is complex, and our specific aim to go beyond standard latitudinal analyses, which in such system have a limited interest.

**p.3408, I.26, I could not find the 27.2 isopycnal in Fig.8, only 26.0 and 27.8 are shown.**

It will be added to the figure.

**p.3409, I.7 What is meant by "resembles very closely"?**

The main pattern corresponds. Because the comparison is presented and discussed, we will eliminate this sentence.

**p.3410, I.10ff. How is this done? Referring to the Methods section did not help me. As I understand, the method is a linearization about some mean, so the results will depend on which mean is chosen. Is this some annual mean map, or an annual mean areal average?**

An annual mean areal average. In each case the seasonal fluxes are recomputed using the fully non-linear equations while keeping one parameter constant (ie, using its annual areal average). For

instance, in the case of Spring, only when DIC is kept constant over the year we have a significant change in the estimate of the fluxes. Therefore, it is the dominant term and without considering the DIC seasonal cycle the ingassing of MW would be stronger and the IW outgassing less intense. The description of the method used will be much improved, as requested also by referee #2.

A very similar approach has been used in: <http://www.biogeosciences-discuss.net/7/745/2010/bgd-7-745-2010-discussion.html>.

**p.3410, I.18. Why should the impact of alkalinity be significant only in some regions?**

Because the seasonality of alkalinity is significant only in some regions.

**p.3411, I.28/19. What is meant by "in both three direction"?**

This should read: "In the three directions"

**p.3411, I.24. Is this about the amplitude of the seasonal cycle in the CO<sub>2</sub> flux or about the annual mean flux?**

About the amplitude of the seasonal cycle.

**p.3413, I.18 The previous chapter discussed seasonal anomalies of the CO<sub>2</sub> flux, not the surface flux of CO<sub>2</sub> itself.**

Yes. With reference to the processes that produced the seasonal variations, including the vertical diffusion and advection of DIC, supported by the 3-D circulation.

**p.3414, I.19, distinguish between advective and total transport.**

The new sentence will read: "the advective transport across the surface."

**p.3415, eq.7,8. are the two phi the same?**

Not exactly. The second corresponds to the first when the integral is performed from the South Pole to a given latitude. This will be clarified in the text.

**p.3415, I.17ff. Where is the steady state assumption required here? How does this apply to a seasonally cycling ocean?**

The steady state assumption is that the integral at the left hand side of eq. (6) is zero. In a seasonally varying ocean this is not exactly zero. For this reason we discuss the comparison between the diapycnal transport and the source/sink terms.

The text was intended to relate this approach to previously published approaches, in order to give the reader a better appreciation of the theory. It is possibly misleading and it will be changed. We could have used annual averages (as in Greatbatch et al., 2007) but we have chosen to fully include the non-linearities of the seasonal cycle.

**Fig.2 arrow air-sea flux should be from atmosphere into the ocean**

OK

**Fig. 4 Units?**

mol/m<sup>2</sup>/yr. The units will added to the legend.

**Fig. 5 & Fig. 6 pre-industrial DIC?**

Yes. Pre-industrial DIC has been obtained by subtracting GLODAP anthropogenic DIC from GLODAP natural DIC. This will be added to the figure legend.

In addition, we will combine Fig 5&6 and compare pre-industrial DIC and alkalinity only at 30°S. See first comment for justification.

**Fig.7 Units? something like "per density class" is missing**

Units are in the y-axis label (too small, actually) and are PgC/yr. They are not in units per density class but actual flux across the surface area identified by the density ranges of each bin.

**Fig.8 How were the frontal positions determined/derived?**

Red lines are the fronts as derived from observations, as in figure 4.

**Fig.9 what is meant by amplitude per season? Is this the amplitude of the annual cycle times the phase? This does not seem to be consistent with the fluxes shown in Fig.7 where, for example, winter and summer anomalies tend to have opposite signs throughout.**

It is the amplitude of the seasonal cycle. The plot shows the difference between the DIC and T contributions and thus who is more important of the two. The comparison with figure 7 is thus not straightforward.

**Fig.15 what is the meaning of red and blue lines? I do not understand the difference between panels b and c.**

We have used the color convention used in the work presenting the Lagrangian analysis of the overturning circulation (Iudicone et al., 2008). The red lines represents pathways of watermasses experiencing a net densification over the Southern Ocean and the blue ones pathways experiencing a net gain of buoyancy. This will be clarified in the text.

Figure 15b refers to the absolute DIC transport at 30S per water mass. Figure 15c refers to the gain/loss of DIC experienced along each pathway. It thus illustrates, for the first time, the integrated effect of the different source and sinks (biogeochemistry and diffusion) over the DIC decomposed into the different physical pathways. We will clarify the text and make much more readable figures. At our knowledge this is the first large scale quantitative analysis of a biogeochemical tracer evolution along pathways (not only of DIC).

## DETAILED RESPONSE TO REFEREE #2

**p. 3396, line 20: I've read this sentence many times and am not certain what it is saying. "... mass exchanges among them because of transformation processes will be used in this context ..." Something is missing here. Additionally, What is transport capacity? What is the word "latter" referring to?**

Former sentence: "The analysis of their paths and fate, as well as, of the mass exchanges among them because of transformation processes will be used in this context to characterize their transport capacity for tracers and the transformation of the latter within them or through exchange among them."

Possible new sentence: "The analysis of the water mass pathways and of the associated water mass transformations will be used to characterize the tracer redistribution promoted by the overturning and by tracer-specific processes such as biology, diffusion and air-sea exchange."

**p. 3402, eq (4): What are the  $G^{\{Z\}Z}$  and  $G^{\{M\}M}$  terms? Presumably they are grazing terms, but this should be stated.**

Yes, they are the grazing terms and the new text will state it. We do apologise for this inconvenience.

**p. 3404, line 10: Please give a brief summary of Iudicone et al. 2008c and Dutay et al. 2009, highlighting aspects that are relevant to ventilation.**

This will certainly be included in the manuscript revisions.

**p. 3405, line 10: Drop the words "very well". It doesn't make sense to make such a claim when it cannot be quantified.**

We will certainly follow the suggestion of the reviewer.

**p. 3405, lines 15-25: Please show difference plots. Also, GLODAP units are  $\mu\text{mol}/\text{kg}$ . Please state how you converted to stated units of  $\mu\text{mol}/\text{L}$ . Are you comparing to GLODAP's  $\text{TCO}_2$ , or have you removed GLODAP's estimate of anthropogenic DIC?**

The new figure is in the main comments above. The plots are in color and the different variables are clearly defined.

**p. 3407, lines 25-28: It is confusing to direct the reader to Figure 7 at this point, but not explain the dashed and dotted line until page 3410.**

We will change the text.

**p. 3408, line 1: The stated separation is not 'clearly evident' to me. Explain what you mean by this.**

The sentence is unclear and, in fact, not necessary. It will be eliminated.

**p. 3408, lines 6-8: Add area of the regions to Table 1, to justify this remark.**

OK

**p. 3408, lines 10-12: From figure 7, how can you infer that SAMW fluxes are from lateral mixing of TW.**

It is not possible. The referee is right and this sentence is in the wrong place because anticipate some of the results of the following sections. It will be reformulated.

**p. 3410, lines 3-13: Please explain, preferably with formulas, how the different terms of are computed. For instance, how do you remove the DIC effect? Without this expla-**

**nation, it is difficult to follow the discussion of your results.**

In each case the seasonal fluxes are recomputed using the fully non-linear equations (the same used for the full fluxes) while keeping one parameter constant (ie, using its annual areal average). For instance, in the case of Spring, only when DIC is kept constant over the year we have a significant change in the estimate of the fluxes. Therefore, it is the dominant term and without considering the DIC seasonal cycle the ingassing of MW would be stronger and the IW outgassing less intense. The description of the method used will be much improved, as requested also by referee #1.

A very similar approach has been used in: <http://www.biogeosciences-discuss.net/7/745/2010/bgd-7-745-2010-discussion.html>.

**p. 3410, line 18: Given the poor representation of alkalinity in your model results, e.g. weak gradients, how meaningful is this result?**

A discussion on alkalinity is presented in the main comments above. Here we add that the surface pattern of alkalinity in the southern ocean, and its variation over the seasonal cycle, is not well known and thus it is difficult to estimate the error given by the model.

**p. 3410, lines 26-28: Please connect Gloor et al.'s assertion to your physical simulation. Do their results hold in your model?**

One important point here is that in our physical simulation has a non-zero net heat flux that cools the surface waters of the Southern Ocean. The actual net air-sea exchange of heat (and even its sign) is under debate, since it varies largely among the different climatologies (eg, SOC, ECMWF, NCEP etc). (This is discussed in Ludicone et al, 2008a.). Therefore we should expect that our simulation should overestimate the role of the cooling and thus give a relatively small net outgassing. In fact this is not so. One main reason for this is that we used a fully coupled ice-ocean model and thus the cooling effect is often reduced locally because of the ice-cover. In other words, more than the global net balance, it is local match/mismatch between cooling and DIC excess is important in setting both the annual and seasonal air-sea fluxes.

**Section 4.3.1: With Figure 10 so small, it is hard to follow the discussion.**

It will be changed.

**Section 5.1.1: Please define more clearly the region  $V_{\{\gamma\}}$ , preferably in the manuscript text. The only definition is in the caption for figure 11, which states  $V_{\{\gamma\}}$  is sandwiched between two surfaces  $S_{\{\gamma\}}$ . Firstly, I would expect the volume to need two  $\gamma$  values, not one.**

Water masses analysis tools are very powerful but they are not really used by the community, a part of a restricted group of researchers. In fact, the water mass analysis is a difficult subject for any reader that is not used to it, as our experience with referees of previous papers further confirmed. Since one of our aims is to introduce and diffuse this tool in the biogeochemical community, we will thus improve the comprehension of the theoretical part as much as possible.

Specifically, on the referee question, the computation of the transformation implies the integration of the tendency terms from the densest surfaces (i.e. from the physical boundaries) up to the selected  $\gamma$ -surface. This integration will give the diapycnal across that surface (the water mass transformation in fig. 12 and the associated tracer transport in fig. 13). The difference between two diapycnal transports (the  $\Delta$  in the text) gives the net water mass formation (in physical space) and the net convergence of the tracer into the water mass layer due to the tracer diapycnal transport.

**Also, the figure only has one surface  $S_{\{\gamma\}}$ . Is the open boundary correspond to 30S of your analysis. If so, please make that clear.**

Yes, exactly. And we will change the figure and improve the text.

All the technical correction will be introduced. We thank the referee for the effort.