Dear Editor and Reviewers,

We addressed all reviewers' comments. In particular, we clarified the limitations and biases associated with our analysis:

- **the use of fluorescence that is only a qualitative indicator of biological production**
- **the sampling bias of frontal areas by lagrangian drifters**
- **the Lee-based approach to compute alkalinity that underestimates the DIC uptake associated with the formation of calcite.**

We also added a figure to address a point raised by reviewer 1 concerning the seasonality of the dominant processes.

Finally, we modified and enhanced most figures so the main features are clearer.

We hope the manuscript is now acceptable for publication in Biogeosciences.

Please find below the point by point response to the reviewers' comments as posted on the Biogeosciences discussion website.

<u>Response to Reviewer 1:</u>

I recommend the publication of this paper with minor revisions consisting mostly of clarifications as outlined below.

Scientific and Technical Comments: 1) Page 13855, TITLE: The Southern Ocean is a vast place with highly variable processes controlling carbon chemistry. If the space al- lows, the title should be more geographically specific: for example . . . seasonal variability of the CO2 system in the northern sub-polar frontal zone of the Southern Ocean"?

The region covered by our observations extends from south of the polar front (55°S) to north of the subtropical front (40°S). It is therefore difficult to name the region as the " northern sub-polar frontal zone". We chose to leave the title as it is but clarified that point in section 2.1 (Methods-observations) as follows:

"Between 2003 and 2009, 9 autonomous lagrangian CARbon Interface OCean Atmosphere (Carioca) drifters were deployed in the Southern Ocean, acquiring an extensive dataset of 80537 individual observations. The region covered by our observations extends from south of the polar front (55°S) to north of the subtropical front (40°S), with most observations acquired in the Subantarctic and Subtropical Zone of the Southern Ocean (Fig. 1 a)."

2) Page 13857, lines 1-26: The large differences among the various independent estimates for the air-sea CO2 flux are not entirely due to the scarce CO2 observations as the authors imply in the Introduction. The differences are largely attributable to the definition of the Southern Ocean: some consider south of 50?S, Gruber et al (2009) use 44?S as the northern boundary, and Takahashi et a. (2009) use 30?S. Since the most intense CO2 sink zone is centered around 40?S between 30?S and 50?S, the "Southern Ocean" CO2 uptake flux is sensitive to the choice of the northern boundary.

We agree that the definition of the Southern Ocean differs between studies. We modified the paragraph as follows:

"The Southern Ocean is a key region for the global carbon cycle and the climate system. It accounts for about 25-30% of the total anthropogenic carbon uptake, although its spatial extension differ in the literature, (Orr et al., 2001; Mikaloff Fletcher et al., 2006; Gruber et al.,

2009; Takahashi et al., 2009). "

This is also the reason why we gave CO2 fluxes published in studies compiling different estimates over the same region. For example, Gruber et al. (2009) compares estimates from the Mikaloff Fletcher et al. (2006) inversion and from the pCO2 climatology of Takahashi et al. (2008) recomputed over the same region (south of 44°S). The discrepancies we discuss in this paragraph are hence not related to the region. We now specify that line 29:

"The recent syntheses of Gruber et al. (2009) and Lenton et al. (2013) compared the CO2 uptake by the Southern Ocean south of 44°S estimated from various models (ocean biogeochemical models, inverse atmospheric and oceanic models) with those derived from observations. ..."

3) Page 13860, Eq 1). What do the author mean by "max" (X (...)) and "min" (X (...)? I assume that the authors are looking for the amplitude of variation within the 20 day period. Do the "max' and "min" values are difference between the single max and min values? Or, do they indicate some sort of mean amplitude? The authors should explain it more clearly.

The text was not clear on that point. It was rephrased as follows:

"At each point t of the trajectory, SV100km is defined as the amplitude of variation (maximum sampled value - minimum sampled value) in a 100x100 km box centered on the point."

4) Page 13862, line 25: Here a biological quantity "Fluo" is introduced. Unlike SST and DIC, which can be defined explicitly, "Fluo" (which I assume fluorescence measurements) is a measure of biological activities, but is not quantitatively related to the primary production as evidenced by a number of papers on this subject. I realize that the authors use "Fluo" as an indicator for primary production because of the lack of any other biological parameters (such as change in nutrient concentrations), and support its use. However, I would like to see a short statement explaining caveat in using "Fluo" as a primary production indicator. Even if it reflects "qualitatively" the gross primary production, it is NOT an indicator for the net community production" which the authors wish to have. If "Fluo" indicate the gross production, then its use for the indicator for net community production would tend to over-estimate the contribution of the biological contribution.

We completely agree with the reviewer that fluorescence is only a qualitative indicator and that it is not linearly corrrelated to the uptake of DIC by primary production. However, it is not such a shortcoming for the present analysis. Indeed, Principal Component Analysis requires that all fields are normalised: the mean is removed and the anomaly is then divided by the standard deviation. The PCA only correlates the relative variations of DIC with the relative variations of SST and Fluorescence without taking into account the amplitude of variation or the mean value. We modified the text concerning the PCA as follows:

"Surface DIC concentrations generally vary in association with physical and/or biological factors, such as vertical and lateral dynamical transport or biological uptake by photosynthesis. Fluorescence and SST are only qualitative indicators of photosynthesis and dynamical processes respectively. However the objective here is not to directly explain variations of DIC by variations in SST and fluorescence (Fluo) but to relate the small-scale spatial variability in DIC to either dynamical or biological factors and identify where and when those variations are dominant. The PCA is a useful tool in that context as it correlates the relative variations of DIC with the relative variations of SST and Fluorescence after having centered and normalized the variables relative to their mean and standard deviation respectively."

5) Page 13868, line 10: Here, the authors make an important statement relating the large-scale

and small scale variability: " ... the strong signature of large-scale patterns on the variability of smaller spatial scales" . However, the large-scale patterns are not explained. Around 40? latitudes in the northern and southern hemisphere oceans, there is a zone where the effect on pCO2 of the seasonal change of temperature is compensated by changes in DIC, and the amplitude of seasonal change of pCO2 be- comes zero (Takahashi et al., DSR-II, 2002). In the southern hemisphere, this occurs in a zone between STF and Sub-polar front, where this study was made.

We clarified what we meant by large scale patterns in the text in the result section:

"In this region, PC1 opposes SST and DIC i.e. a decrease in DIC is correlated to an increase in SST (Fig 4 c and e). This negative correlation is consistent with the opposed large-scale horizontal and vertical gradients of SST and DIC: SST decreases while DIC increases with increasing latitude and depth."

The discussion on how large scale patterns cascade to small-scale structures is now presented in the discussion:

"Modeling studies showed that both horizontal and vertical advection associated with small-scale structures can affect the distribution of DIC at small spatial scales and hence the surface pCO2 (Mahadevan et al., 2004; Resplandy et al., 2009). Horizontally, the ocean circulation advects large-scale gradients down to small spatial scales. It is now widely accepted that mesoscale eddies, which dominate the variability at scales of the order of 100 km, horizontally transport and stir tracers such as DIC and temperature, resulting in a cascade of tracer variance to smaller scales (Abraham et al., 2000; Lévy and Klein, 2004)."

6) Page 13868, lines 16-20: The authors point out that "It is interesting to note that the dominance of biological activity depends more on the region than a on the season of sampling". This is really puzzling. Is it possible that phytoplankton stop fluorescing when they are exposed to strong lights beyond a certain threshold level, that might occur in summer? Since I have only limited experience with fluorometer, I would advise the authors to consult with experts. I might speculate that, only when the light levels are reduced in winter, phytoplankton community responds to fluorometer.

We performed a more qualitative diagnostic of the dominance of processes depending on the season. The dominance of the biological activity is indeed higher in summer than in all other seasons. However, the contribution of biology is non negligible in winter, fall and spring. We think that this point is interesting and therefore rephrased this sentence and added the figure below (Fig 7 in the manuscript) to address it:

"As expected, the contribution of biological activity is higher during summer (Fig 7). However, it should be noted that biological production is also identified as a dominant process for a large number of observations during fall, winter and spring (Fig 7). The major reason is that seasonal variations of DIC, and hence the variations associated with the seasonal bloom, have been filtered out from this analysis. What is highlighted here is the role of biological activity in introducing variability on scales of 100 km."

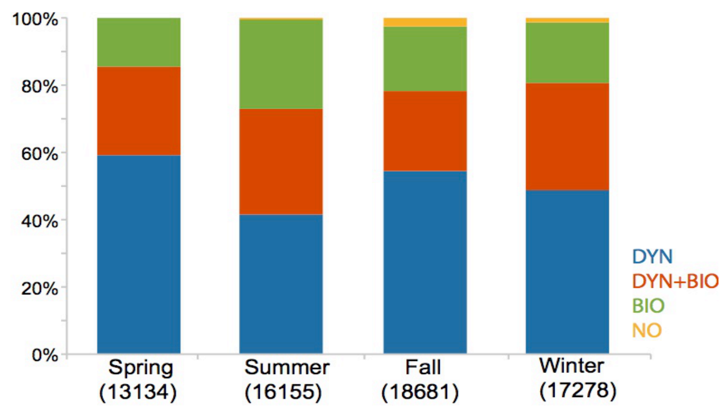

Fig 7: The proportion of dominant processes identified for each season (in %). Note that there is a sampling bias toward the winter and fall seasons, spring being less sampled (the number of observations is indicated in parenthesis).

7) Page 13889, Fig. 7: The Satellite SST along the float (Fig. 7-b) is compared with the Satellite SST 2007/03/28 (Fig. 7d). Fig. 7-b shows SST values ranging 6 to 7.5?C, whereas Fig. 7-d shows about -0.5?C. I suspect that one of them is labeled incorrectly. I assume that the red curves represent the float tracks, but the color does not correspond to the color scale. Please clarify.

There was indeed a mistake in that figure that has been corrected.

Editorial Nature: 1) Page 13858, line 19: Correct typo to read “interpretation”. 2) Page 13869, line 7: Correct typo to read “. . .. these patches of biological activity were located along the SAF. . ..”. 3) Fig. 4: Define and explain the color scale values. Are they correlation coefficients? 4) Fig. 6: Define and label the gray curves. 5) Fig. A1, A2 and A3: Define and explain the color scale values. Are they correlation coefficients?

Typos have been corrected. PCA loadings are indeed correlation coefficients, which is now specified in the captions of all figures. Grey curves indicating the Polar, Subantarctic and Subtropical Fronts are now labeled.

<u>Response to Reviewer 2:</u>

... I have two issues, which I would like clarified by the authors:

1. One of the difficulties of using surface floats to sample the ocean, particularly the Southern Ocean is that they tend to be concentrated along the frontal jets by the convergence driven by flow continuity. This is likely to create a data analysis bias in respect of generalizing the findings such as the importance of DYN beyond those frontal regions. How do the authors view this problem and the implications that it has for this kind of analysis?

2. One of the main finding of this study is the strong role attributed to DYN in understanding how the regional large-scale DIC gradients are cascaded into the fine scale. However, I wonder if this is not an artifact of the calculation of DIC from TA which itself integrates the large-scale salinity and temperature gradients. A Lee-based approach to deriving TA from t,S is fine because the errors from imposed regional scale gradients are small but once TA is used to understand the drivers of vari- ability through derived DIC then the regional cascaded t, S signal is likely to emerge. I am particularly challenged on this issue by the contrasting correlations between temp and DIC (high) and tem and pCO2 (low) in Fig. 6 even though DIC is probably the key proxy for pCO2. It suggests that DIC has modes of variability that exaggerate the role of DYN vs BIO. Please could this be clarified?

The two issues raised here by the reviewer are indeed crucial to this analysis. Although these two points are distinct issues, they could lead to a bias towards the overestimation of the dynamical contribution. We therefore wrote a paragraph to address both of them in the discussion:

“It should be noted that the dominance of dynamical and biological processes inferred from this analysis could be biased in two ways. First, drifters tend to be concentrated along frontal jets, where the dynamical advection is particularly intense. This could exaggerate the contribution of dynamical advection, in particular in the Atlantic sector between the Polar and Subtropical Fronts (Fig 5 d and e). Second, DIC is calculated from alkalinity with a Lee-based approach, which itself integrates the large-scale salinity and temperature gradients. This method adapted to studies at the regional scale does not account for the biologically-driven changes in alkalinity that could occur on small spatial scales, such as in a productive eddy. Biologically-driven changes in alkalinity are largely associated with the

uptake of CO_3^{2-} when calcite is formed, the impact of other nutrients (nitrate, phosphate, silicate etc.) being negligible. We expect this bias to be relatively small in most regions sampled by the drifters, the production of calcite being very low in the region sampled by the drifters except north of 45°S (see Sarmiento and Gruber, 2006, chap 9). Hence, this approximation could impact the results in the subtropical Indian sector, where the formation of calcite could represent ~10% of the biological uptake of carbon. This could lead to an underestimation of DIC variations of ~10% in small-scale structures where calcite is formed. However, we don't expect this bias to change the main result of the present study, which already identifies the subtropical Indian sector as the region with the highest biological contribution (Fig 5 d and e)."

Some more general comments:

1. Some of the figures (particularly Fig3) are hard to understand because of their size, small text and dense data 2. The co-location of fronts and the float data is critical because of the convergent tracking of the frontal jets by the floats 3. Aghulas is actually Agulhas 4. There are quite a few typos which can be cleaned out by a careful read

1)We modified most of the figures so the features are clearer.
2)Fronts vary interannually. Their climatological position gives a usefull indication but can not be considered as their "effective" location when the drifters passed by.
3)Indeed.
4)I hope we did.