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Interactive comment on "Observed small spatial scale and seasonal variability of the CO_2-system in the Southern Ocean" *by* L. Resplandy et al.

Anonymous Referee #2

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This study used a pioneering data set of pCO2 from autonomous CARIOCA floats deployed in the Southern Ocean in the period 2004 – 2006 to examine the drivers of short spatial scale variability of derived DIC. The authors use useful approaches to analyze the characteristics of the variability both in the spatial scale as well as contrasting the seasonal cycle. DIC is derived from a Lee-based method to calculate Total Alkalinity (TA). After removing the low frequency variability they use a combination of spatial variability within moving 100km blocks along the float tracks and PCA to analyze the variability and the contributions of ocean dynamics (DYN) and phytoplankton productivity (BIO) on the variability of DIC.

In principle this is all good and useful to put low frequency derived variability into context but 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 variability 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?

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

Interactive comment on Biogeosciences Discuss., 10, 13855, 2013.