Author's answers to Referee #2 (Dr. Marta Álvarez)

This manuscript (MS) is a very nice piece of work dealing with real biogeochemical data in the Southern Ocean south of Tasmania. As an biogeochemical observationist I do really appreciate high quality sustained ocean observations in harsh regions as the SO. Maintaining the funding for such expensive long term programs is always difficult. State agencies always prefer quick, 3-4 years projects with high impact results for society or policy makers. The slow science done with time series, either fixed or oceanographic lines is precious to detect global change in a comprehensive way (Henson, 2014, dx.doi.org/10.1098/rsta.2013.0334).

Thank you very much for your comment. We really appreciate it and the very thorough review, which has added to the paper.

After the nice words, I do conclude that this MS should be accepted but before, it needs some MINOR improvements, they regard to two main points organization and data & calculations.

With regard to the organization of the MS:
- the resolution or uncertainty of the back-calculation method for CANT is given in page 16, line 16. It should be given in section 3.2.

The uncertainty of C_{ANT_BC} was already mentioned in section 3.2 (page 6, line 4).

- I would suggest including section 6 as Supplementary material, a slight change in the organization / order of this section, the general title is OK for me, but the sections would be:

Thank you very much for the advice. Nevertheless, we consider that section 6 should stay as it is for two main reasons:
    - When talking about C_{ANT} it is always good to have some reference values from other methodologies.
    - Explaining the sensitivity of the results completes the manuscript and explores the reliability of the results.

Besides, the scheme proposed for the inclusion of section 6 in the supplementary material does not change too much the structure of section 6 as it is right now.

The numerical model configuration, the OMP analysis and the particularities of the AT0 and *CDIS* estimates should be in the supplementary material to make the reading in the main text easier. In fact, we think that it could be better to upload this supplementary material in a separate file.

Before continuing, there are some points that we would like to clarify. There has been a misunderstanding with the methodology, mainly because we misled the sign in equation 3 and also because we need to clarify certain aspects of the methodology. Also, we think that there was a misunderstanding between this work and the previous work by Pardo et al. (2014), probably favoured by the error in the sign of Equation 3. We would like to clarify that we refer to Pardo et al. (2014) because the methodologies are similar. Since a similar methodology was applied before in the Southern Ocean we consider that it is fair to cite this reference. Nevertheless, we do not redirect the reader to that work because we prefer to explain the methodology again in order to help readers not familiar with back-calculation methods. We will try to clarify this in the following answers.

We answered the comments as they were made, i.e., following the proposed organization. We did not answer those comments that are just a title for the proposed organization scheme.

- CANT estimation

If this comment refers to moving section 3.2 to the supplementary material, we do not agree. We would like to maintain the methodology in the main text in order to help readers not familiarized with back-calculation methods.

- OMP analysis

- parameterization for TA0 and CDIS - biogeochemical model estimating $\delta CDIS$, please give numbers for this term, as far as I understand is not given in Table A1.

$\delta CDIS$ can be easily obtained through Eq. 4 (section 3.2):

$$CDIS^{\pi} = CDIS - \delta CDIS$$

and for this reason we do not list it separately (redundantly) in Table A1.

- DIC changes

- CANT estimated with other methods

- circulation and biological processes at steady state: here I have doubts & thoughts, the methodology to calculate DIC changes and attribute them to any oceanographic process (C_{ANT} increase, change in circulation, warming, higher upwelling,..) should be clarified in the main MS.

We agree with Dr. Álvarez in that there is a need to clarify more the estimates of the change in DIC explained in section 3.3. We will add the next text after the first paragraph of section 3.3:

"Respect to the total change of DIC ($\frac{\partial DIC}{\partial t}$), our goal is to disentangle the effects that solubility, circulation, biology and C_{ANT} uptake have on the variability of DIC. The total change of DIC ($\frac{\partial DIC}{\partial t}$) in a water mass is due to changes in the atmosphere-ocean exchange, biological processes and circulation processes. In order to account for the change in DIC due the atmosphere-ocean exchange and biological processes, we compare $\frac{\partial DIC}{\partial t}$ to $\frac{\partial C_{ANT\_BC}}{\partial t}$ and $\frac{\partial DIC^{BIO}}{\partial t}$, Eq. (2). The change in DIC not explained by $\frac{\partial C_{ANT\_BC}}{\partial t}$ or $\frac{\partial DIC^{BIO}}{\partial t}$ will then be due to circulation processes.

In order to compare $\frac{\partial DIC}{\partial t}$ to $\frac{\partial C_{ANT\_BC}}{\partial t}$ we need to consider how the change in $\frac{\partial DIC^{BIO}}{\partial t}$ and DIC^{π} ($\frac{\partial DIC^{\pi}}{\partial t}$) (terms of Eq (1), section 3.2) affect the changes in C_{ANT_BC} ."

We believe that all the following comments also belong to this same point: "- circulation and biological processes at steady state". We will answered them individually following the assigned numbers.

1) On one side, the BC method for CANT assumes steady state in the circulation / biology (= constant stoichiometric ratios), the main reason behind is that biology activity or ocean circulation/mixing is not affected by the CANT increase.
2) On the other side, when talking about DIC is completely untrue that is not affected by changes in circulation (higher/lower upwelling, higher/lower transport of water masses).

3) Any change on circulation would be detected in AOU (as nutrients are always more problematic due to precision and exactitude issues).
4) But also in transient tracers that you do not show (this is an important caveat of the whole analysis!!).
5) Please clarify in this section which processes affect CANT and which DIC and how your method deals with them.

1) We agree and we stated that the C_{ANT_BC} assumes steady state in the circulation /biology (section 3.2):
"Back-calculation methods assume the ocean is in steady state for dynamical and biological processes."

2) We agree. We never stated that the DIC is not affected by the changes in circulation. In section 3.3 we say:
"We assume that the changes in DIC^{BIO} due to circulation do not affect the amount of DIC in the layer. This assumption is one of the caveats of the methodology, since we cannot know how much of the change in DIC is associated to changes in circulation, i.e., how much of the change in DIC is a change in non-anthropogenic DIC. We will discuss this more in section 4.4.2."

This comment made us realize that there is an error in the citing of section 4.4.2, which should be "*section 6.2*". We'll correct this.

We hope that the new text clarifying which processes drive the changes in DIC (please, see answer to the point: "- circulation and biological processes at steady state") will help with this misunderstanding.

3) We agree and had already explained this aspect of AOU (section 3.3, page 8 lines 10-11):
"...since part of the changes in AOU with time reflect changes in circulation that we cannot separate from those in remineralization."

We also agree with the comments on nutrients and, as well as the problematic precision of nutrients, we had already noted that upper ocean processes also impact their utility for trend detection:
"The detection of long-term trends of nutrients at upper layers of the ocean can be masked by short time scale physical processes such as changes in the mixed layer depth, mesoscale activity and advection." (Section 6.2, page 16, line 32 and line 1 of page 17).

4) Unfortunately, using transient tracers is beyond the scope of this manuscript for several reasons:

- There are only CFCs available for all the cruises considered. Other tracers have been measured but not in all the cruises. CFC_S alone are not the most reliable tracers when stablishing water mass ages in young surface waters, places of frequently deep convection or in bottom waters that are eventually ventilated, because of their decay in the atmosphere.
- Most importantly, our main goal in the study is to make use of most data from the SR03 and also to consider the input of AABW in terms of C_{ANT}.

5) We will add a new text after the first paragraph of section 3.3 for further clarification. See answer to the comment on point: "- circulation and biological processes at steady state".

- changes in the rates of export of POC: POC export is related to primary production in the upper layer, higher POC production implies higher primary production. If this POC is mineralized AOU would increase and DIC increase as well, only if we keep the same circulation. An intensification of circulation

for the same POC input would mean a lower AOU as bacteria would have less time to work. I insist that without transient tracers distangling the influence on DIC from circulation and biology is difficult.

We agree that this disentanglement is challenging, and accordingly in section 6.3 we stated that:
> *"Our results show that the increase of DIC in mode and intermediate waters is fully explained by the uptake of atmospheric CO_2, which could indicate that there was no detectable change in the rate of export of POC over the 1995-2011 period."*

Moreover, we considered the change in the rate of export of POC as a possible reason for the increase of DIC in deep waters. That is why we considered the results from deep sediment traps:
> *"Estimates of POC export in the SAZ (~3800 m), the SAF (~3100 m) and the PFZ (~1500 m) using moored sediment traps.."* to check how important a change in the rate of POC export could be on the DIC change found for deep water masses.

Nevertheless, we agree with the comment and we will clarify this further by changing the text at the end of the second paragraph of section 6.3 into:
"This increase is close to the uncertainty of the total DIC increase estimated for the UCDW layer, which means that in order to generate an increase in DIC similar to that found in the UCDW layer, the rate of POC export should be ~10 times higher than the observed rates. This change should be certainly noticeable in $\frac{\partial DIC^{BIO}}{\partial t}$ in surface waters but most probably in deep waters as well, which we do not see".

- stochiometric ratios

- the title of the MS is "carbon uptake and biogeochemical change", so I understand is not only CANT changes, it is mainly about DIC changes and the processes causing them, of course, one of them the CANT increase.

I do follow section 3.2, but I do not follow section 3.3. In fact I think section 3.3 should be introduced in first order compared to section 3.2.

This is part of the misunderstanding we were talking about before. We believe that section 3.2 is necessary to understand section 3.3, and therefore prefer not to reorder the text. We hope that with the explanations given it is clearer now.

And I question in the current section 3.3 the explanations in page 8 to estimate CANT /DIC changes and the reasoning behind. It seems that two types of CANT are in the ocean, the back calculated and another one with something else. IT seems as well that CANT changes could also contain DIC changes related to AOU (circulation/biology) which is clearly separated in section 6. As section 3.3 is difficult to follow the results in Tables 4 & 5 too. I will comment more about this issue in the data & calculation section of this review.

There are not two types of C_{ANT}. C_{ANT_BC} is directly estimated using the back calculation method and $\frac{\partial C_{ANT}}{\partial t}$ is obtained by correcting $\frac{\partial C_{ANT\_BC}}{\partial t}$ for the effects of solubility and change in DIC^{BIO}. $\frac{\partial C_{ANT}}{\partial t}$ is the best approximation for the change in C_{ANT} (as we already stated in section 3.3):

"We consider the best approximation for the change in C_{ANT} as a range depending on the possible effect of biology and circulation processes on $\frac{\partial DIC^{BIO}}{\partial t}$. If the value of $\frac{\partial DIC^{BIO}}{\partial t}$ is due to the variability in the remineralization rates and the change in solubility is considered, the estimate $\frac{\partial C_{ANT\_BC}}{\partial t}$ will be the lower limit of the range, (lower limit of $\frac{\partial C_{ANT}}{\partial t} = \frac{\partial C_{ANT\_BC}}{\partial t} + \frac{\partial DIC^{\pi}}{\partial t}$). For the upper limit of the range, we

And we also repeated this during the explanation of the results (section 4.1.1) in order to help the reader.

We will add a new figure to improve the understanding of this differences between $\frac{\partial C_{ANT\_BC}}{\partial t}$ and $\frac{\partial C_{ANT}}{\partial t}$:
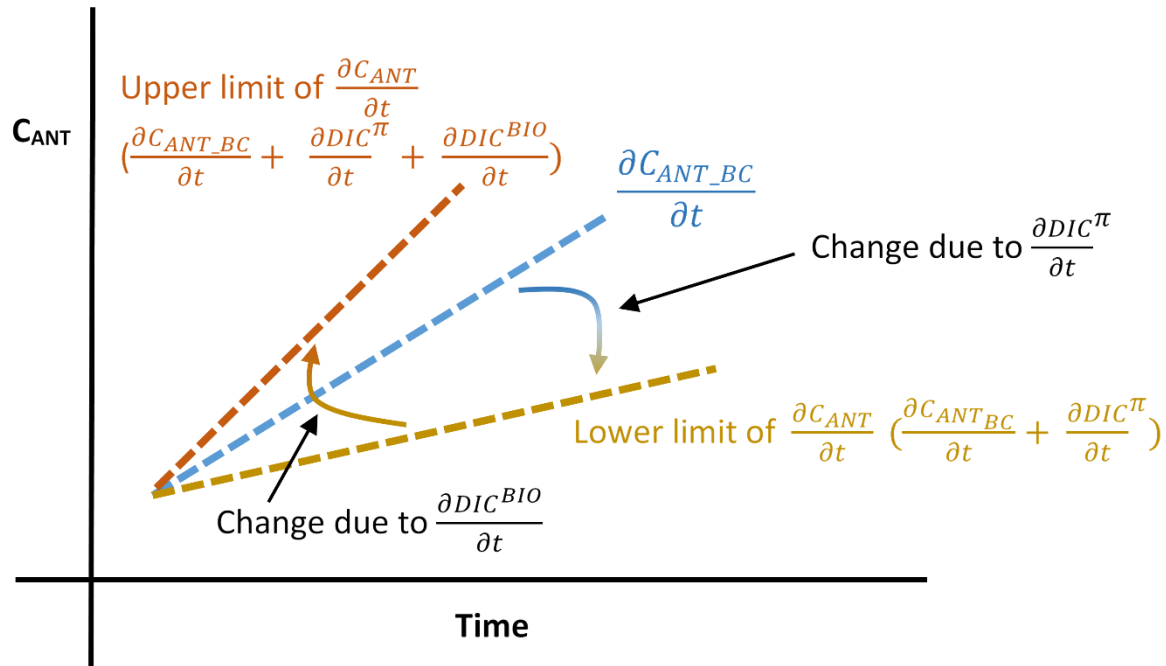

Figure: Estimation of the ranges in $\frac{\partial C_{ANT}}{\partial t}$ from $\frac{\partial C_{ANT\_BC}}{\partial t}$, depending on the possible effect of biology and circulation processes on $\frac{\partial DIC^{BIO}}{\partial t}$ (section 3.3).

- I do miss some figure with the temporal evolution of DIC & CANT & DICBIO at the different layers.

The temporal evolution is presented in Table 4 with the errors and RMSE. The trends in the table offer the same useful information that the figures of the temporal evolution.

- Table 6 should be included in the corresponding section of the Sup information if changes!

We've decided not to change the structure of the manuscript.

With regard to the data & calculations:
- Cruise data: please state that those cruises were included in GLODAPv2 and therefore checked, DIC for the 2008 was corrected. Please confirm that there is no transient tracer data (CFCs or/and SF6).

We stated that the data are included in GLODAPv2 (page 5, lines 17-18) but we did not clarify the QC done to the variables. We'll change the phrase to:

"The section data are available through the Global Ocean Data Analysis Project (http://cdiac.ornl.gov/oceans/GLODAPv2; Key et al., 2015; Olsen et al., 2016). The original data for the different cruises were corrected following the QC recommendations in GLODAPv2."

We already answered the comment about tracers.

It is very very surprising to report oxygen accuracy and precision as 1%, please correct me if I am wrong, so for O2 in deep bottom water, 230 umol/kg.. the estimation error would be 2.3 umol/kg, it means an error in DICBIO of 1.6 umol/kg. Quite high.

We agree. It was an error. The accuracy and precision for O_2 are 0.3% .

- CO2SYS calculation: please state that you calculate pHT and omega Ar in situ from DIC and TA. Which is the borate constant?.

We will make that clearer by changing the previous phrase (line 19 page 5):
    "DIC and TA measurements allow estimates of other variables of the dissolved CO_2 system."
To:

    "Other variables of the dissolved CO_2 system were calculated from the DIC and TA measurements."

And also the second sentence in the paragraph to:

    "We calculate pH_T and Ω_{Ar} (from measured DIC and TA) using the CO2sys program ..."

The reference for the borate constant is: Uppström. (1974). It will be added in the text.

- Table 3: which is the meaning of "*" for the 2011 cruise in the SAF region?

The explanation will be added to Table 3 caption:

" The range in the location of the SAF for the 2011-cruise could be related to a diversification of the PF or SAF into different jets (Sokolov and Rintoul, 2009) but also to crossing the meander of the front twice."*

- CANT estimation: either you include here current section 6.1 or you keep the CANT-OMP method as the only one, and the others in the Supl. Material

In order to understand the results we need to maintain the two different notations. $\frac{\partial C_{ANT}}{\partial t}$ is the best approximation we can offer to compare to $\frac{\partial DIC}{\partial t}$. We already explained the differences between $\frac{\partial C_{ANT}}{\partial t}$ and C_{ANT_BC} (or CANT-OMP, as you call it). We hope that adding the new Figure will help to understand this better.

With regard to the CANT-OMP method, the reference paper is Pardo et al. (2014), I suggest to use the same notation.

This is another misunderstanding. We would like to say that we use Pardo et al. (2014) as reference because the methodologies are similar but we never redirect the reader to that work. We already explained that we rewrote the methodology in order to help those readers not familiar with back-calculation methods.

Similarly to what we did before, we are going to answer the following comments using the assigned numbers.

1) Your Eq 1 defining CANT-OMP is different from Eq 4 in Pardo2014.
2) I always understood CANT as the difference in preformed conditions from now (or whenever) to preindustrial times. So what is the meaning of DICπ in current Eq1, it should be DIC 0 π.
3) Current Eq 3 is also different from Eq 3 in Pardo2014.
4) The MS DIC π is different from DIC 0 π ??. However current Eq 4 equals Eq4 in Pardo2014.
5) TAble 1 in Pardo2014 and Table A1 & A3 here should be comparable.
6) Maybe in Table 1 2014 there is a typo ΔCdis π should just be ΔCdis. I am very confused about the CANTOMP calculation for the disequilibrium term and the info given in Table 1 here and that in Pardo2014. Please clarify
7) And please give in the section the error for CANT-OMP.

1) Equation 4 in Pardo et al. (2014) is not the definition of C_{ANT_BC} (or CANT-OMP as you call it). Probably you refer to Eq. 1 in the present manuscript compared to Eq. 1 in Pardo et al. (2014). These equations are not exactly the same. Nevertheless, if you follow both texts a little, it is easy to find the similarity.

2) You understood correctly, CANT is the difference from preformed conditions. DICπ is always a preformed variable. The notation in Pardo et al. (2014) relates to that used by Chen (1982)*. The "origin" of CANT for a water mass is: at preindustrial times and in its region of formation, which is the surface ocean. This means that DICπ is always going to be a preformed value whatever the notation chosen. There is a small problem with the notation "0" in DICπ. While Chen (1982) does not consider the disequilibrium term we do, thus the real "preformed" term should be DICπSAT. That is why we decided to avoid the notation "0" this time. Nevertheless, to avoid more confusion, we will add "preformed" in the text when explaining the term DICπ: a) in page 6 line 17: *"...and DIC^{π} is the preformed concentration of DIC in preindustrial times."* and b) in page 6 line 28: *"The preformed preindustrial term, DIC^{π} ,…".*

*Chen, C.-T. A.: On the distribution of anthropogenic CO2 in the Atlantic and Southern Ocean, Deep-Sea Research, 29, 5, 563-580, 1982.

3) Thank you very much for noticing. We made a mistake with the sign of Eq. 3 and it will be corrected.

$$DIC^{\pi} = DIC^{\pi}_{SAT} + CDIS^{\pi}$$

It was just a typo in the equation, the estimates were done correctly.

4) Yes, those equations are and should be the same. All is okay.

5) The tables are not equal but are comparable.

In Pardo et al. (2014) Table 1 is shorter than Table A1 (present manuscript) because there was a previous manuscript describing the OMP analysis (Pardo et al., 2012) done for the whole Southern Ocean, which was the base for the study of CANT in Pardo et al. (2014). Nevertheless, we'll add the uncertainties of the parameters of definition of the SWTs to Table A1 in order to make them more comparable

In the present manuscript, we changed some things in the OMP analysis with respect to Pardo et al., (2012) because this is a local study. In a local study it is really important to consider the local varieties of the water masses in order to maintain the reliability of the OMP. This is why we have to show the whole definition of the SWTs in Table A1.

With respect to Table A3 (present manuscript) comparison to Table 1 in Pardo et al. (2014), we consider that this is just a matter of how you present the equations. In detail, the way we estimated AT^0 and CDIS in the current manuscript is similar but not identical to how it was done in Pardo et al. (2014).

6) There is no typo in Table 1 of Pardo et al. (2014 and ΔCdis can be estimated with the equation 4 given in Pardo et al. (2014).

7) The error for C_{ANT_BC} is already given there (page 6, line 4).

The reference for Thacker 2012 is not included in the references.

The reference is already in the previous manuscript.

Silicate is included as a non-conservative variable in the OMP, but no Ratio is given.

Silicate is included as a conservative variable and no Ratio is needed. We mentioned that in section 6.4: *"The residuals of SiO4 did not change significantly for any value of R_{Si} and we consider SiO4 as a conservative variable."*

But we forgot to mention it in section A.2 where it should be. Thanks for noticing. We would add this information in section A.2.

I think is more coherent to write $\Delta O2$ in the section A.2.

We agree. We'll change ΔO to ΔO_2.

In TAble A1 the analytical error, ϵ, is not given, page 21 line 6.

Thanks for noticing. We'll change the table A1 to:

	θ (°C)	S	SiO_4 (µmol/kg)	NO_3^0 (µmol/kg)	PO_4^0 (µmol/kg)	$O_2^{0\,*}$ (µmol/kg)	TA^0 (µmol/kg)	DIC^{π}_{SAT} (µmol/kg)	CDIS (µmol/kg)	$CDIS^{\pi}$ (µmol/kg)	Fractions uncertainties (%)
SWT_{STW16}	16 ± 0.06	35.1 ± 0.07	0.9 ± 0.2	1.2 ± 0.2	0.04 ± 0.3	243 ± 2	2290	1990	-19	1	0.04
SWT_{STW15}	15 ± 0.06	35.66 ± 0.07	0.6 ± 0.2	0 ± 0.2	0.12 ± 0.3	247 ± 2	2328	2026	-22	-2	0.04
SWT_{AASW}	-1.85 ± 0.006	33.8 ± 0.005	45 ± 2	30.7 ± 0.2	2.10 ± 0.3	360 ± 4	2289	2137	-23	-19	0.06
SWT_{SASW}	5 ± 0.008	33.8 ± 0.03	3 ± 0.2	23.3 ± 0.3	1.55 ± 0.5	310 ± 3	2264	2064	-13	-4	0.06
SWT_{HSSW}	-1.91 ± 0.08	34.71 ± 0.006	80 ± 1	28.3 ± 0.08	2.02 ± 0.03	300 ± 3	2351	2188	-21	0	0.08
SWT_{SAMW}	8.8 ± 0.02	34.63 ± 0.03	6 ± 0.6	13.2 ± 0.6	0.92 ± 0.8	280 ± 7	2290	2053	-10	2	0.03
SWT_{AAIW}	4 ± 0.01	34.35 ± 0.02	34 ± 2	29.2 ± 0.4	1.97 ± 0.9	220 ± 8	2299	2099	-16	-6	0.04
SWT_{NADW}	3.28 ± 0.008	34.91 ± 0.003	28 ± 1	27.5 ± 0.3	1.19 ± 0.7	220 ± 4	2355	2152	-27	-10	0.08
SWT_{CDW}	0.65 ± 0.006	34.707 ± 0.003	115 ± 7	30.8 ± 0.1	2.12 ± 0.1	220 ± 3	2351	2168	-23	-2	0.03
SWT_{PIDW}	1.44 ± 0.008	34.75 ± 0.005	125 ± 3	34.2 ± 0.01	3.4 ± 0.02	96 ± 2	2360	2168	-24	-4	0.04
SWT_{AABW}	-0.6 ± 0.006	34.66 ± 0.006	130 ± 5	30.7 ± 0.08	2.13 ± 0.03	259 ± 3	2355	2181	-22	-2	0.05
Weights	20	10	0.5	1	1	1					
SDR	0.004	0.003	6.0	0.50	0.04	2.0					
r^2	0.99	0.99	0.98	0.99	0.99	0.99					

Table A1. Properties of the SWTs characterizing the water masses of the SR03 section with the correspondent accuracies (ε). All SWTs are defined with preformed values of the variables (°). *The values of preformed oxygen (O2°) are not in equilibrium for end members representing waters from the Antarctic shelf or old deep waters. The uncertainties in the fractions of the SWTS, the weights given to each variable in the OMP analysis, the Standard Deviation of the Residuals (SDR) and the square correlation coefficient (r2) between the observed values and the OMP estimates are also listed.

- DIC changes: as suggested previously I suggest to start the methods sections with the proposed estimation of DIC changes and driving factors.

$\Delta DIC = \Delta DIC^{BIO} + \Delta DIC^{CANT} + \Delta DIC^{phys}$

please see Álvarez et al. (doi:10.1029/2010JC006475, 2011)

ΔDIC with your real measurements
ΔDIC^{BIO} will be computed as your Eq2, this term contains changes in mineralization associated or not with changes in circulation / biology BUT correlated with AOU / nutrients for sure. As you lack of transient tracers information you cannot account for changes in the transport, circulation independently.
ΔDIC^{CANT} with the OMP method (forget about the dis terms)
ΔDIC^{phys} as the difference between DIC- DIC^{CANT}-DIC^{BIO}, this term would contain any changes in DIC not associated with AOU, mixing / Ventilation…. not accounted for in the CANT-OMP.
Following this methodology the blurred assumptions in page 8, lines 1 to 22 are avoid.

and of course I think it would help to explain the results.

We really thank you for the suggestion, however we consider that the proposed estimation of the DIC changes would not really allow us to avoid the lines in section 3.3, since somewhere we must address the issue of non-steady-state circulation and biological processes, and we believe the current paper structure is the best way to do this. As we mentioned before, we'll add some clarifications in the text at the beginning of the section (see answer to the point : "- circulation and biological processes at steady state").

To respond specifically to your suggestions, the problem with using the proposed estimation of the change of DIC in the present manuscript is that:
- we have a range of values for ΔDIC^{CANT} (which corresponds to $\frac{\partial C_{ANT}}{\partial t}$ of the present manuscript)
- your recommended ΔDIC^{phys} is going to have as well a range of values that we will have to explain in the same way that we did with $\frac{\partial C_{ANT\_BC}}{\partial t}$ and $\frac{\partial C_{ANT}}{\partial t}$. This may well create more confusion to the reader. That is why we think that adding the new text to the beginning of section 3.3 will help, since at the end of this additional text we'll mention that:
"The change in DIC not explained by these two quantities is due to circulation processes." (see the complete text in the answer to the point : "- circulation and biological processes at steady state").

Besides, in section 5 we discuss about the different drivers (solubility, biology or circulation) that are responsible for the change in DIC not explained by the change in C_{ANT} (see page 12 from line 9 to the end and lines 1-6 in page 13).

The use of the notation proposed in Álvarez et al. (doi:10.1029/2010JC006475, 2011) was good for their study. They use Transient tracer distributions (TTDs) to estimate C_{ANT} (TTDs rely on tracers to stablish the amount of C_{ANT} in the deep ocean) and a numerical model to resolve the circulation effect on the AOU term. Thus, the terms with the different drivers affecting the variability of DIC were fully determined. The study was focussed on SAMW, which allowed for the TTD to give reliable values even using just one tracer (CFC-12), because the residence time of CFC-12 is efficient for waters of the age of SAMW. But this is not our situation, and we have previously explained why transient tracers are beyond the scope of this work.

I hope to have been helpful. Yes, of course. We thank you for your thorough review.