We thank the reviewer for the detailed review of our manuscript. The review was indeed helpful in strengthening the manuscript. We hope we have addressed all comments satisfactorily.

All grammar corrections where done as suggested with minor changes where necessary. All are detailed in an annotated version of the document.

**General comments**

**Reviewer:** I think the methodology could be better explained. Perhaps the logic of equations 1-3 is explained more thoroughly in the previous paper by Mongwe et al 2016, but this critical reference is missing from the reference list. Assuming they mean the paper in Ocean Modelling 106: 90, I will agree that equations 1 and 2 can be derived from equations 3.2-3.4 in that paper. But the LHS of equation 2, which does not appear in the previous work, is physically speaking, a fairly nebulous quantity. The Takahashi et al 1993 estimate of 0.0423 $K^{-1}$ mainly expresses the change in partial pressure due to changing temperature for a given concentration of $CO_2 ([CO_2^*])$, with a small contribution from the partitioning of DIC among $CO_3/HCO_3^-/CO_2$ due to the temperature dependence of the equilibrium constants. DIC does not change as a result of changes in temperature, except indirectly through gas exchange.

So what we have here is an observed change of $pCO_2$ with changing temperature, convoluted into a change of DIC by application of several highly empirical conversion factors (more about this below), as an estimate of the changes in DIC not attributable to biological uptake/remineralization (and therefore primarily attributable to gas exchange). This in itself might be inoffensive, but I would prefer if its relationship to actual physical processes were better explained. The equations that are taken as a starting point are highly empirical, and we should not invest rearrangements of these with an outsized significance.

0.0423 $K^{-1}$ is intended to be an average value for a broad range of ocean conditions, but it is stated to be valid for salinities from 34-36 and temperatures from 2-28°C (Takahashi et al 1993). In the Southern Ocean one will encounter conditions outside, or on the far edges of, these ranges. What are the implications of this for the analysis shown here? This seems like something that could be evaluated. Similarly, the calculations assume a constant Revelle factor, but it should be quite straightforward to calculate Revelle factors from the model.
outputs, giving a range for the range of environmental conditions characteristic of the study area. The conclusions are probably robust to these assumption, I see no reason why not tested.

Response: The reviewer points out an overarching request to clarify our methodology, which indicates that it was not clearly presented. This is partly because the link to our previous paper (Mongwe et al., 2016), where we explained this methodology, was poorly made. A more detailed synthesis from Mongwe et al., (2016) was necessary as suggested. This was compounded by some inadvertently missing references, including the Mongwe et al., 2016. Having used referencing software, I neglected a thorough check of the bibliography, which was a mistake and I apologize for this mistake; all references in the revised manuscript have been corrected.

This reviewer’s comment on the methodology is partly connected to the next comment asking how we ascribed the influence of temperature to DIC and the partitioning of the other DIC terms. We have deferred this part of the response to the next comment, in which this question is raised in a more specific way. In this first part we focus on the question about the uncertainties linked to the constants of the Takahashi et al., (1993) empirical expression as well as the Revelle factor in polar waters. We thank the reviewer for raising the question on limits to the validity of the empirical relationship used to calculate temperature-driven changes to pCO₂ observations and model data.

We examined the applicability of the Takahashi et al., (1993) linear approximation

\[
\left( \frac{1}{pCO₂} \frac{\partial pCO₂}{\partial SST} \approx 0.0423°C^{-1} \right)
\]

in our region of study. In the Sub-Antarctic zone, surface water has a temperature range of ~4°C – 12°C, which is within the limits (2 – 28°C) provided by Takahashi et al (1993). Since surface temperatures go below 2°C in the Antarctic zone, we have tested whether this relationship can be extrapolated down to -2°C. We do so by comparing the dependence of pCO₂ on temperature for a range of temperature values (4°C to -2°C : 0.1°C intervals) using a carbonate equilibrium model and the Takahashi et al., (1993) linearization. Ancillary variables DIC, TA, phosphate, silicate and salinity are fixed and the carbonate equilibrium model CO2SYS (Pierrot et al., 2006) was used with K1, K2 from Mehrbach et al., (1973) refitted by Dickson and Millero, (1987), which are the same parameterizations used in the majority of CMIP5 models. We used the mean climatological pCO₂ (Landschützer et al., 2014) and the seasonal mean for nutrients (silicate and
phosphate), salinity, TA and DIC from the same Antarctic region in GLODAP2 as was used in the models.

As shown in Fig. 1, the response of pCO$_2$ to temperature below 2°C can still be described with the Takahashi et al., (1993) linear relationship. Thus we can extrapolate the Takahashi et al., (1993) linear dependence of pCO$_2$ to temperature for the estimation of temperature solubility changes to equivalent DIC changes in the Antarctic zone as explained above.

Figure 1 (S4 in the revised manuscript) in the revised supplementary material) Comparison between of dependence of pCO$_2$ to temperature changes according to the Takahashi et al., (1993) empirical the constant (0.0423°C$^{-1}$) and the computed ratio of temperature dependence, from the carbonate system equations (CO2SYS, Pierrot, et al., 2006) using mean climatological data from GLODAP2 (Salinity, TA, DIC, silicate & phosphate) and pCO$_2$ from Landschützer et al (2014) for the Antarctic zone and a temperature range of -2.0°C to 4°C (0.1°C intervals).

In a comparable way, we examined the uncertainties arising from a fixed polar Revelle factor in our calculations. We recomputed the Revelle factor in the Sub-Antarctic and Antarctic zones using annual mean climatologies of TA, salinity, surface temperature nutrients. Firstly we examined DIC changes for the nominal range of pCO$_2$ change (340 – 399 μatm, 1 μatm intervals) and then used this dataset to derive the Revelle factor ($\gamma_{DIC} = \frac{DIC}{pCO_2} \frac{\partial pCO_2}{\partial DIC}$).

The calculated Revelle factors in the Southern Ocean range between $\gamma_{DIC} \sim 12 – 15.5$ with an average of $\gamma_{DIC} = 13.9\pm1.3$. This justifies our use of $\gamma_{DIC} = 14$ for the conversion of the
equivalent solubility driven pCO$_2$ change to DIC throughout the analysis. As an addition, we 
now provide the uncertainty in this conversion as it translates into the temperature 
constraint, by using the upper and lower limits of the Revelle factor ($\gamma_{\text{DIC}} = 12 – 15.5$) in the 
model framework. Below we show an example for observations in the Sub-Antarctic and 
Antarctic zone, this shows that extremes of the Revelle factor values ($\gamma_{\text{DIC}} = 12 – 15.5$) do not 
alter the phasing or magnitude of the relative controls of temperature or DIC on the 
seasonal cycle of pCO$_2$ (Fig 2).

Figure. 2 Seasonal cycle of the rate change of surface total DIC for the Landschützer et al 
(2014) data product (black line) for the Landschützer et al (2014) data product and the estimated 
temperature driven DIC rate of change ($\frac{\partial \text{DIC}}{\partial t}$)$_{\text{ST}}$ shaded area, for monthly data given in 
$\mu$mol kg$^{-1}$ month$^{-1}$ at the Sub-Antarctic zone i.e. Pacific Ocean (first column), Atlantic Ocean 
(second column) and Indian Ocean (third column). The dotted line shows the uncertainty 
boundaries for the Revelle factor extremes accounting to range in the Southern Ocean ($\gamma_{\text{DIC}} = 
12 – 15.5$).

Reviewer Finally, isn’t the total DIC variability by definition the sum of the various 
components? So I’m not clear why the temperature driven component should ever be larger 
than the total. I find equation 3 and the discussion on 304-311 to be the most confusing 
part. We have an observed rate of change of DIC (which is never actually defined), which 
one would think would be the sum of the contributions from gas exchange, biological uptake 
/remineralization and entrainment. But in this case, the index that is considered is that the 
total is either greater or less than one of these three components (whose physical meaning
is nebulous). To confuse matters worse, we have a reference to "the total DIC seasonal cycle (dDIC/dt)" (306-307). Doesn’t dX/dt imply an instantaneous rate-of-change that will itself vary over the annual cycle? I really do not understand what is being asserted here. (Also, the text should say something about exactly what sort of discretization was used in calculation of trends, e.g., does delta-X/delta-t for November represent a value for Nov. 1 based on a difference of October and November means, or is it something else? If this is the case, figure axes should indicate that calculated values are for the first of the month and not the mid-month.)

**Response:** We thank the reviewer for this comment, it was important to clarify this point. We apologize for giving this impression. We, regretfully, neglected to provide an adequate description of how we separated the terms contributing to the total DIC surface layer changes. We have clarified this part in the revised manuscript. The total rate of change of DIC \[\left(\frac{\partial \text{DIC}}{\partial t}\right)_{\text{tot}}\] in the surface layer consists of the contribution of air-sea exchanges, biological, vertical and horizontal transport-driven changes (eq. 1).

\[
\left(\frac{\partial \text{DIC}}{\partial t}\right)_{\text{Tot}} = \left(\frac{\partial \text{DIC}}{\partial t}\right)_{\text{air-sea}} + \left(\frac{\partial \text{DIC}}{\partial t}\right)_{\text{Bio}} + \left(\frac{\partial \text{DIC}}{\partial t}\right)_{\text{Vert}} + \left(\frac{\partial \text{DIC}}{\partial t}\right)_{\text{Hor}}
\]  

(eq. 1)

In our method, we assumed that the horizontal term can be neglected because we used zonal means from medium resolution models.

The discretized form of the total rate of change is written as:

\[
\left(\frac{\partial \text{DIC}}{\partial t}\right)_{T_{\text{Tot}}} = \left(\frac{\partial \text{DIC}}{\partial t}\right)_{n,l} = \frac{\Delta \text{DIC}_{n+1,l} - \Delta \text{DIC}_{n,l}}{1\text{ month}}
\]  

(eq. 2)

where n is time in month, l is vertical level (in this case the surface, l=1). We here take the forward derivative such that November rate the is difference between December the 15th and November the 15th, thus being centered at the interval between the months.

In order to compare the role of temperature in scalable terms with DIC in pCO\(_2\) and FCO\(_2\), we convert the instantaneous pCO\(_2\) changes driven by solubility \(\left(\frac{\partial \text{pCO}_2}{\partial \text{DIC}}\right)\) using 0.0423°C\(^{-1}\) from Takahashi et al (1993) to an equivalent DIC change using the Revelle factor \(\ln(p\text{CO}_2)\ln(DIC) \approx \gamma_{\text{DIC}}\).

\[
\frac{\partial \text{DIC}}{\partial t}_{\text{SST}} = \frac{\text{DIC}}{\gamma_{\text{DIC}} \times p\text{CO}_2} \left(\frac{\partial \text{pCO}_2}{\partial t}\right)_{\text{SST}}
\]  

(eq. 3)
This equivalent DIC rate of change (eq. 2) driven by temperature (solubility) allow us to convert the influence of solubility into DIC units, which can then be directly compared with the other terms in eq. 1. We hope that this now clarifies the construction of our indicator metrics,

\[ M_{T-DIC} = \left| \left( \frac{\partial DIC}{\partial t} \right)_{SST} \right| - \left| \left( \frac{\partial DIC}{\partial t} \right)_{Tot} \right| \]  
(eq. 4)

which is used to compare the estimated change of (equivalent) DIC as driven by temperature-controlled solubility with the actual DIC change simulated by the models and obtained from the observational data product. When \( M_{T-DIC} > 0 \) indicates that the pCO₂ variability is dominated by the rate of change of temperature and when \( M_{T-DIC} < 0 \) indicates that the pCO₂ variability is controlled mainly by DIC changes.

We have now revised manuscript to better reflect the underlying methodology.

**Reviewer:** The discussion of entrainment is also confusing and poorly connected to actual physical processes. Equation 4 does not have the units of a flux, but rather of a rate of change within the surface layer. The proper quantity here is not DIC concentration at MLD (T+1) but rather the difference between DIC at MLD(T+1) and at MLD(T).

**Response:** The reviewer is correct in this regard and we made this correction in the revised text. The definition of entrainment at the base of the MLD has been improved using a more appropriate notation (please refer to the discretization in the answer above that is now included in the revised manuscript). Entrainment is physically considered as advection of preformed DIC at the base of the mixed layer. It is therefore based on the advection term.

\[ RE = U_e \left( \frac{\partial DIC}{\partial z} \right)_{MLD} \]  
(eq. 5)

\[ RE_n = \left( \frac{\Delta MLD_n}{\Delta t} \right) \left( \frac{\Delta DIC}{\Delta z} \right)_{n,MLD} \]  
(eq. 6)

\[ \Delta MLD = \frac{MLD_{n+1}-MLD_n}{1 \text{ Month}} \]  
(eq. 7)

\[ \left( \frac{\Delta DIC}{\Delta z} \right)_{n,MLD} = \frac{DIC_{n,MLD_{n+1}} - DIC_{n,MLD_n}}{\Delta z} \]  
(eq. 8)
In which \( U_e \) is an equivalent velocity based on the rate of change of the mixed layer depth. This approximation of vertical entrainment is necessary as it is not possible to compute this term from the CMIP5 data because the vertical DIC distribution is only available as annual means. We clarified that we use the estimated rate of change of DIC at the base of the mixed to examine surface DIC changes driven by subsurface/bottom DIC changes. We also updated \( F_{DIC} \) estimate with GLODAP version 2 dataset.

Reviewer: Why use only ten years of model output (124)? The results could be biased by internal variability; the more usual averaging period would be 20 or even 30 years. With a reference year of 2000, this would require using emissions scenarios, which is perhaps a reason not to do it, but the differences among scenarios are very small in 2005-2015 (because the scenarios are constructed precisely around the assumption that there is some inertia in human societies and abrupt changes are unlikely). I think the authors should (a) pick one model, recalculate the results for 1990-2010 and 1985-2015, and estimate the potential error associated with aliasing of internal variability. And (b) if this error turns out to be large, repeat the calculation for the full suite of models.

Response: The choice of the period was to match a period closest to the available observational data product (Landschützer et al (2014), 1998 – 2011). However, the reviewer is correct to highlight that we assumed that the seasonal cycle of CO\(_2\) does not vary significantly on decadal timescales. We have now investigated this assumption for a few models (here we present HadGEM2-ES and CanESM2), comparing the mean seasonal cycle climatology of FCO\(_2\) at the Sub-Antarctic zone for 30 years (1975 – 2005) and 10 years (1995 – 2005) of the historical scenario (Fig. 3). It shows that the seasonal cycle of FCO\(_2\) remain the small (\( R = 0.99 \)) in both HadGEM2-ES and CanESM2 over 30 year.

We have now added this sentence in the method section: “The choice of the 10 year period was done to match the simulated pCO\(_2\) values to the period of the observations. We tested the interannual variability of the seasonal cycle over a period longer than the reference 10 years for a few models and found no significant variation in the monthly standard deviation.”
Figure 3. Compares the seasonal cycle of Sea-Air \( \text{CO}_2 \) fluxes over 30 year (1975 – 2005 and 10 years (1995 – 2015). The shaded area shows the standard deviation.

Reviewer: The use of chlorophyll as a proxy is not really explained, when primary production and export production are generally available as model output fields. One might justify this by saying that observations are available only for chlorophyll, but this should be stated explicitly. There are also observation-based estimates of primary production available (see below Terminology).

Response: Our initial choice for chlorophyll was because of the availability of an observational data product for comparison and that it was available for most models (9 out of 10) in the CMIP5 portal. In the revised manuscript, we added net primary production (NPP), carbon export and oxygen for the models with available data. This addition of NPP, carbon export and oxygen was indeed useful in examining impact of biological driven DIC changes in particular it helped isolate/constrain the role of primary production on the seasonal cycle of p\( \text{CO}_2 \).
Reviewer: I think the authors should acknowledge that the FCO\textsubscript{2} data product is not really ‘observed’ in the sense that pCO\textsubscript{2} is. I think they should compare modelled and observed pCO\textsubscript{2}, and then discuss what this means for modelled estimates of CO\textsubscript{2} flux, without referring to the Landschutzer FCO\textsubscript{2} estimates as observations. CO\textsubscript{2} fluxes in models and data products like this are actually quite different conceptually. When you estimate CO\textsubscript{2} flux from observed pCO\textsubscript{2}, the errors in the flux are a linear function of errors in wind speed (or \(u^2\), assuming a quadratic parameterization) and the piston velocity. In numerical models, pCO\textsubscript{2} and DIC self-regulate to dampen these errors when monthly averaged fluxes are considered, e.g., if both the wind speed and the DIC are too large, the enhanced outgassing flux will reduce the pCO\textsubscript{2} and DIC error. Higher wind speed or piston velocity will tend to drive pCO\textsubscript{2} towards atmospheric, and not necessarily towards the ‘correct’ value if over- or undersaturation exists, so there is no straightforward way to correct for this difference. But I think that the authors should acknowledge that it exists, and that in comparing modelled and ‘observed’ fluxes they are to some degree comparing apples and oranges. (With regard to point (4) above, if there is no observed primary production, there is no observed CO\textsubscript{2} flux either: both of these are extrapolated from the primary observed field using models of unknown accuracy.)

Response: It is indeed true that FCO\textsubscript{2} observations we use are estimates, data products derived from empirical methods of gap filling. We now refer to them in this answer and in the revised manuscript as “data products”. We tried to make the point much clearer in the revised manuscript and also provided the uncertainty as given by Landschützer et al. (2014) and used the interannual standard deviation over 14 years when comparing the seasonal cycle. However, it also important to note that the majority of our analysis is based on pCO\textsubscript{2} estimates rather than FCO\textsubscript{2} in particular. Thus, we do acknowledge the role of wind in providing the kinematic forcing for sea-air fluxes and although the wind influences the magnitude of the fluxes, the direction is determined by delta pCO\textsubscript{2}, which is here considered the primary driver of the seasonal cycle of FCO\textsubscript{2}. We are mindful that differences in the parameterization of the Sea-Air interactions and wind products chosen are likely to affect the resulting FCO\textsubscript{2} (Feely et al., 2004; Swart et al., 2014).

The uncertainty in the data products used for the assessment is an important point that we neglected to highlight in our first version of the manuscript. We now acknowledge the limitations of the data products and have further addressed the issue of uncertainties as
We have examined the uncertainty of our reference data set by comparing Landschützer et al., 2014 data product with the more recent Gregor et al. (2017a) data product, which uses Support Vector Regression (SVR) and Random Forest Regression (RFR), as well as Takahashi et al. (2009) to derive a seasonal climatology pCO₂ in the SAZ and AZ of the Southern Ocean in the supplementary material (Fig. 5). Part of the reason we focus on pCO₂ in these data products instead of FCO₂ is firstly, because Gregor et al., (2017a) only focuses on fugacity and pCO₂. Thus we mindful that the choice of wind product and tranfer velocity constant in computing FCO₂ is likely to increase the level of uncertainty for the compared data products (Swart et al., 2014). Secondly, while we evaluate FCO₂ biases as the final aim of the paper, the major part of our diagnostic analysis is based on pCO₂ rather FCO₂. Fig. 4 below shows the climatology of the seasonal cycle of pCO₂ in the Sub-Antarctic zone and Antarctic zone with interannual standard deviation between 1998 – 2011. All three datasets mostly agree in the phasing of the seasonal cycle of pCO₂ in the Sub-Antarctic, but show significant differences in the magnitude. Takahashi et al. (2009) shows an amplified impact of primary production on pCO₂ in summer. We see this as a bias in the Takahashi et al., (2009) dataset arising from a period when the space – time coverage of pCO₂ observations was still limited and strongly biased towards summer. In the Antarctic zone all three observationally-based data products agree in both phasing and amplitude. At this stage it is not clear whether this agreement is due to all the methods being equally exposed to the same limited observations or if it is due to a more marked CO₂ seasonal cycle in the Antarctic zone (relative to Sub-Antarctic) that can be captured with less observations.
**Fig. 4** $p$CO$_2$ (μatm) spatial (climatology) and seasonal cycle differences in Landschützer et al (2014) (L14), Gregor et al (2017) (G17), Takahashi et al (2009) (T09) datasets in the Southern Ocean. The seasonal cycle climatology of pCO$_2$ in the Sub-Antarctic and Antarctic zone is based on the period 1998 – 2011. The shaded areas show the standard deviation of the interannual variability of the seasonal cycle for this period. The uncertainty in the correlation coefficient is based on the correlation coefficient of the mean plus standard deviations seasonal cycle(s).

**Reviewer:** I would like to see some discussion of the possibility that the apparently greater temperature control in the Pacific sector (259-263) is a real effect that arises from iron limitation. Because terrestrial sources of iron are much greater in Atlantic sector and the western half of the Indian sector (see e.g., Graham et al 2015 DSR I 104: 9; Tagliabue et al., 2012 Biogeosciences 9: 2333), it seems logical that the effect of seasonal biological drawdown on pCO$_2$ would be greater than in the Pacific and in the eastern half of the Indian sector. These regions also overlap the regions where the wind speed and the amplitude of its annual cycle are greatest (e.g., Trenberth et al., 1990, JPO 20: 1742), which will also tend to reduce the influence of biological uptake.

**Response:** Thank you for this important suggestion, it is indeed likely that differences in wind, iron supply, primary productivity and MLD across the basins are most likely responsible for contrasting variability in the three basins. We have added this discussion in
the revised manuscript;

“The observed differences in the seasonal cycle of FCO$_2$ across the three basins is likely due to differences in the basin properties of the Southern Ocean. Recent studies have highlighted significant basin scale differences in pCO$_2$ and FCO$_2$ ascribed to large-scale differences in temperature (Landschutzer et al., 2015), winds (Gregor et al., 2017b and primary production as reflected in surface ocean phytoplankton biomass (Thomalla et al., 2011). The relatively higher chlorophyll biomass (Graham et al., 2015; Thomalla et al., 2011) in the Atlantic Ocean, is likely linked to lower wind speeds (Trenberth et al., 1990) and higher supplies of iron from continental shelves (Thomalla et al., 2011; Boyd and Ellwood, 2010; Tagliaabue et al., 2014). This is likely associated with longer periods of shallower MLD (Dec – Mar, Fig. 7), which favor sustained primary production leading to a stronger CO$_2$ sink in the Atlantic Ocean with respect to the Indian and Pacific Ocean (Figs. 3a-c; 6a-c). In contrast, shorter periods of shallow MLD and lower iron concentrations in the Pacific Ocean, as pointed out by Tagliaabue et al. (2012), likely account for lower chlorophyll biomass and stronger thermal control of the seasonal cycle of pCO$_2$ and FCO$_2$ (Fig. 6b, Fig. 3 here). In the Indian Ocean stronger wind speeds are likely responsible for the early deepening of the MLD (Fig. 6c), and thus chlorophyll biomass are lower (Fig. 9). In the Indian Ocean, stronger wind speeds (Trenberth et al., 1990) are likely responsible for the early deepening of the MLD (Fig. 7c), limiting primary production and lower rates of change of temperature (Fig. 5c), ultimately resulting in a relatively constant FCO$_2$ for about half the year (Dec – Jun). Our plots indicate that CMIP5 models mostly don’t show these basin-specific features highlighted in observational products (Landschutzer et al., 2015; Gregor et al., 2017a and Thomalla et al., 2011) with the exception of three group B models (i.e. CESM1-BGC, CanESM2 and CMCC-CESM) in the Indian Ocean (Fig. 2, 3 a-c). This poses a challenge to the new generation of Earth Systems Models in CMIP6”
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