Reviewer 1 (RC1)

We thank the reviewer for the careful thought and suggestions, which we think have strengthened the clarity and rigor of our manuscript. We hope that we have been able to address all these points to the reviewer's satisfaction below and in the revised manuscript.

General comments

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

Reviewer: Unfortunately, I find the title beginning "Mechanisms..." to be a disappointing overreach as rather than including quantification of the solubility, transport, and biological pump mechanisms, the authors rely entirely on the empirical seasonal relationship correlation of dSST/dt and cursory analysis of mixed layer entrainment as metrics of model mechanisms

Response: We apologize for giving this impression. This study is based on a recently published mechanistic framework that used the seasonal cycle of $dpCO_2$ and FCO_2 as a mode to diagnose mechanistic differences between models and observations (Mongwe et al., 2016). We, regretfully, neglected to provide a detailed description of how we separated the terms contributing to the total DIC surface layer changes and how we compare these to temperature. We have clarified this part in the revised manuscript. The total rate of change of DIC $\left(\frac{\partial DIC}{\partial t}\right)_{Tot}$ in the surface layer consists of the contribution of air-sea exchanges, biological, vertical and horizontal transport-driven changes (eq. 1).

$$\left(\frac{\partial DIC}{\partial t}\right)_{Tot} = \left(\frac{\partial DIC}{\partial t}\right)_{air-sea} + \left(\frac{\partial DIC}{\partial t}\right)_{Bio} + \left(\frac{\partial DIC}{\partial t}\right)_{Vert} + \left(\frac{\partial DIC}{\partial t}\right)_{Hor} \qquad \text{(eq.1)}$$

Because we used zonal means from medium resolution models, we assume that the horizontal terms are negligible.

Furthermore, in order to constrain the contribution of temperature on changing pCO_2 and FCO_2 we derived a DIC equivalent term $\left(\frac{\partial DIC}{\partial t}\right)_{SST}$ defined as the magnitude of DIC change that would correspond to a change in pCO_2 driven by a particular temperature change. In this way the ΔpCO_2, driven solely by modelled or observed temperature change, is converted into equivalent DIC units, which allows its contribution to be scaled against the observed or modelled DIC change (Eq.1).

This calculation is done in two steps: firstly, the temperature impact on pCO_2 is calculated using the Takahashi et al., (1993) empirical expression that linearizes the temperature dependence of the equilibrium constants.

$$\left(\frac{\partial pCO_2}{\partial t}\right)_{SST} = 0.0423 \times pCO_2 \times \frac{\partial SST}{\partial t} \qquad \text{(eq. 2)}$$

Though this relationship between dSST and $dpCO_2$ is based on a linear assumption (Takahashi et al., 1993), this formulation has been shown to hold and has been widely used in literature (e.g. Bakker et al., 2014; Feely et al., 2004; Marinov and Gnanadesikan, 2011; Takahashi et al., 2002; Wanninkhof et al., 2010). We show in the supplementary material that the extension of this expression into polar temperature ranges (SST < 2°C) only introduces and additional uncertainty of 4 -5%.

Secondly, the temperature driven change in pCO_2 is converted to an equivalent DIC using the Revelle factor $\left(\gamma_{DIC} = \frac{DIC}{pCO_2}\frac{\partial pCO_2}{\partial DIC}\right)$.

$$\left(\frac{\partial DIC}{\partial t}\right)_{SST} = \frac{DIC}{\gamma_{DIC} \times pCO_2}\left(\frac{\partial pCO_2}{\partial t}\right)_{SST} \qquad \text{(eq. 3)}$$

Although we used a fixed nominal polar Revelle factor of 14, we show in the supplementary material that this does not alter the phasing or magnitude of the relative controls of temperature or DIC on the seasonal cycle of pCO_2 (Fig. 1).

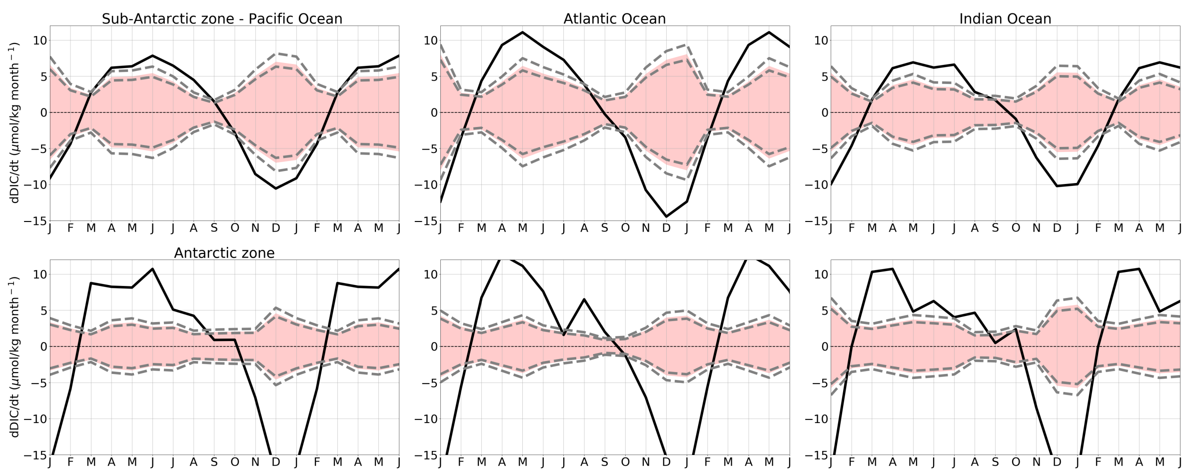

Figure. 1 Seasonal cycle of the rate change of surface total DIC $\left(\frac{\Delta DIC}{\Delta t}\right)$ *black line*, and the estimated solubility DIC driven rate of change $\left(\frac{\partial DIC}{\partial t}\right)_{SST}$ *shaded area*, for monthly data given in µmol kg^{-1} $month^{-1}$ at the 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 in the Southern Ocean (γ_{DIC} = 12, 15.5).

This methodology (Mongwe et al., 2016) is now fully described in the revised manuscript, before proceeding to compare directly the influence of temperature with the estimated total surface DIC changes (eq. 3), The anomaly of the temperature contribution to pCO_2 change to total modelled or observed DIC change, expressed in equivalent DIC units, is set out in Eq. 3) below where a positive anomaly points to ΔpCO_2 being thermodynamically controlled and a negative anomaly points to DIC control:

This isolation of the role of solubility is the first step in our analysis, we then proceed to also look at the role of vertical DIC (entrainment) due to changes in the mixed layer depth and biological processes: now, in addition to the initial inclusion of biomass (chlorophyll), we have expanded it to include Net Primary Production (NPP), carbon export and oxygen to examine how DIC changes are driven by biological process. The main caveat is that we focus on processes that drive CO_2 variability on the vertical scale and, for now, have neglected the horizontal scale fluxes. This assumption is thought to be reasonable given the large-scale zonal averages that we work with where the seasonal flux variability is likely to be dominated by vertical length scales as well as and medium resolution models we used for this analysis.

We were sorry to read that our analysis of the estimated DIC change at the base of the mixed layer to examine surface DIC changes driven by subsurface/bottom DIC variability was perceived as cursory. These estimates are based on annual mean DIC profiles and seasonal MLD due to the availability of three-dimensional DIC data in the standard set of CMIP5 variables. In order to reduce this uncertainty, we also ran an additional model (section 2.4) with comparable spatial resolution and verified that our conclusions were valid despite the use of annual means of the DIC distribution.

Thus, we re-emphasize that our approach is not correlation based, but does examine the variability of the main drivers of CO_2 at the seasonal scale i.e. DIC and temperature, which we thought could be useful in showing first order sources of the apparent CMIP5 FCO_2 seasonal cycle biases. Using this approach, we were able to show that overestimated warming and cooling rates were the main bias in group B CMIP5 models, while exaggerated primary production is the main bias in group A models. This finding is an important consideration for the ability of Earth System Model to predict long-term changes in the oceanic CO_2 sink. It indicates that this ability is likely dependent on the model's capability to represent realistic seasonal changes in temperature and not just the mean state and ranges, because this has marked implications on DIC and pCO_2 solubility. This is especially important during the spring season where primary production (uptake) and solubility (surface warming) have an opposing effect on the direction of the CO_2 flux. The relative rates are critical to understanding the climate sensitivity of the model in respect of air-sea CO_2 fluxes. We recognize that these points were not clear enough in our submitted manuscript. To better match with the revised content of the paper, we also propose to change our title to "The Seasonal Cycle of CO_2 fluxes in the Southern Ocean: Diagnosing Anomalies in CMIP5 Earth Systems Models".

Reviewer: I suggest the authors quantify the role of SST change on DIC solubility to be able to confidently assess whether the role of temperature in the temperature correlated.

Response: This analysis does in fact quantify the role of SST change on pCO_2, which is then converted to DIC equivalents as explained above. Strictly speaking, SST cannot change DIC in a closed system so we proposed the use of the DIC equivalent, which reflects the magnitude by which DIC would have to change if it were to make the same change as SST on pCO_2. We are here referring to an equivalent DIC change resulting from pCO_2 change by solubility, this is to scale up the solubility component to total DIC changes (eq.1). The reviewer comments, however, highlight that our methodology was not adequately explained. In the revised manuscript we provide an expanded explanation of our methodology (see also the response above), which clarifies this point among other improvements.

Reviewer: The authors should include some quantification of model biases relative to observational uncertainty, and include several of the available observationally constrained products to assess that uncertainty

Response: Since our analysis is based on the Landschützer et al., 2014 data product, we have used the mean monthly FCO_2 (1998 – 2011) to compute the standard deviation of the seasonal cycle of FCO_2. This is not strictly a measure of data uncertainty but more an estimate of the interannual variability that was used to compare against CMIP5 models variability in Fig. 2 & 3 of the original manuscript. For the annual means in Table 1, we use the uncertainty magnitude provided by Landschützer et al., 2014 (±0.31 Pg C yr^{-1}) when comparing observations estimates against models.

We examine the uncertainty further by adding more data products, as suggested. We now compare Landschützer et al., (2014) with the more recent Gregor et al (2017) data product, which uses Support Vector Regression (SVR) and Random Forest Regression (RFR), as well as Takahashi et al (2009) for pCO_2 climatology in the supplementary material. We use pCO_2 instead of FCO_2 firstly, because Gregor et al., (2017) only provided fugacity and pCO_2, and also being mindful that the choice of wind product and tranfer veolocity constant in computing FCO_2 would increase the level of uncertinity (Swart et al., 2014). Secondly, while the focus of the paper is on the evaluation of FCO_2 biases, the major part of our diagnostic analysis is based on pCO_2, which determines the direction and part of the magnitude of the fluxes.

Fig. 2 below shows the seasonal cycle of pCO_2 in the Sub-Antarctic zone and Antarctic zone with interannual standard deviation between 1998 – 2011 and their corresponding FCO_2 climatology. All three datasets mostly agree in the phasing of the seasonal cycle of pCO_2 in the Sub-Antarctic, but show differences in the magnitude. Tatakahashi et al. (2009) shows an amplified impact of primary production 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 pCO_2 observations was still limited and strongly biased towards summer. In the Antarctic zone these three observationally-based datasets 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 few observations or it is due to a more marked CO_2 seasonal cycle in the Antarctic zone (relative to Sub-Antarctic) that can be captured with less observations.

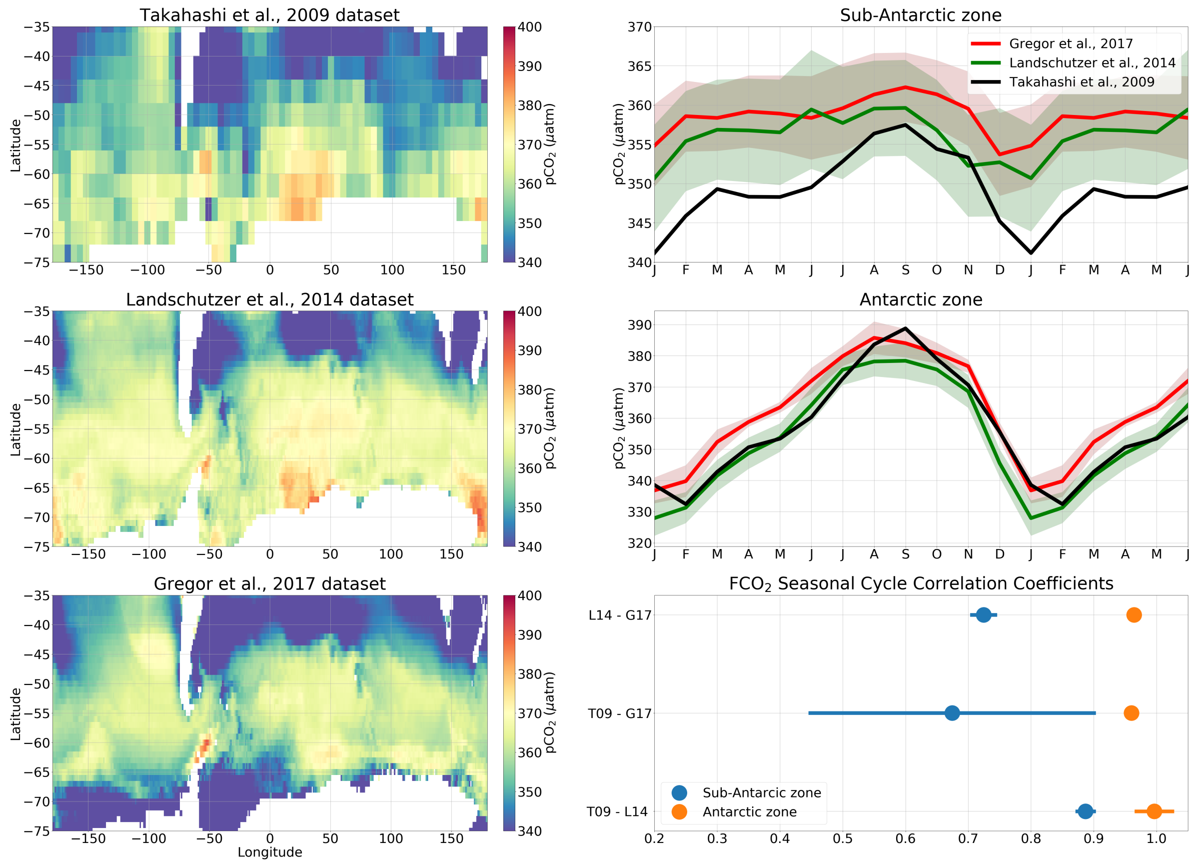

Figure. 2 pCO_2 (µatm) spatial (climatology) and seasonal cycle differences in Landschützer et al (2014), Gregor et al (2017), Takahashi et al (2009) datasets in the Southern Ocean. The seasonal cycle climatology of pCO_2 in the Sub-Antarctic and Antarctic zone is based on the period 1998 – 2011. The shaded areas show the standard deviation of the interannual variability of the seasonal cycle for this period. The uncertninity in the correlation coeffecient is based on the correlation coeffient of the mean plus stardard deviations seasonal cycle(s).

Reviewer: Interior budgets could be constructed, leading the authors to developing a simple box model of mixed layer DIC to be able to reproduce the various GCM results through the combination of gas exchange, thermal, transport, and biological mechanisms. While such a more mechanistically based box model analysis could prove very valuable in uncovering the mechanistic differences between the models, it is probably outside the scope of the present manuscript

Response: This is an excellent suggestion to isolate different drivers of FCO_2, but it will be a separate study as it is out the scope of this analysis. Nevertheless, we hope that our revised manuscript clarifies our approach and its usefulness as an analysis of the mechanisms of the main drivers of CO_2 at the seasonal scale.

Specific Comments

We hope that all grammatical and spelling challenges have been addressed with minor changes where necessary.

Reviewer: 135 The assertion that "The seasonal cycle of the ocean-atmosphere CO2 gradient dpCO2 is considered to be the main driver of the seasonal variability of FCO2" …… Is true in a regional sense but is certainly not true in a temporal sense in most regions where wind variability can dominate like in the equatorial Pacific: The delta pCO2 argument was that is you average over large enough scales, the mixed layer equilibration time of CO2 was short enough (about a year) that CO2 fluxes were determined by the net balance of biology and thermal factors rather than the wind. On a seasonal scale, ignoring the role of wind seems like a fatal flaw. Rather, the authors should argue that the wind variability in this region is small before disregarding it. This is likely true in the Southern Ocean where winds are strong in all seasons.

Response: This an important point, which may have other implications elsewhere outside the Southern Ocean. Here, while it is correct that winds provide the variability in kinematic forcing for sea-air CO_2 interactions, the weak seasonal cycle in wind stress in the Southern Ocean(Young, 1999) means that the impact on FCO_2 is largely in the intra-seasonal (synoptic) scales. The impact of wind in the mixed layer dynamics can also be amplified or suppressed depending on the mesoscale and sub-mesoscale characteristics of the surface ocean(Mahadevan et al., 2012; du Plessis et al., 2017) which may also play a role on the onset of and variability of primary production and entrainment (and thus FCO_2). In contrast, ΔpCO_2, which sets the direction and also contributes to part of the magnitude of the flux, is regulated by the strong seasonal modes of solar warming, which drives SST and mixed layer depth (MLD) that influences the seasonal extremes of spring-summer productivity and winter convective entrainment in the Southern Ocean.

However doing this analysis has some complexity because different wind products result in different FCO_2 responses (Swart et al., 2014), which both highlight a strong sensitivity of pCO_2 to winds and a challenge for choosing reliable wind product. For this analysis, as the reviewer pointed out, because winds do not have a strong seasonal variability in the Southern Ocean, we don't anticipate a strong seasonal impact on FCO_2 and it was excluded from the main text. We make this point in revised manuscript. Nevertheless we recognize that evaluating the impact of winds on FCO_2 in both setting the mean-state and inducing fine scale dynamics important CO_2 at the seasonal scale remains an important aspect and will be considered for a future study.

Reviewer: 181 – While I am glad the authors are considering mixed layer entrainment, it seems remiss here to ignore the biological and other circulation terms such as upwelling and consider them all lumped together as "DIC" terms.

Response: Once more, we recognize that the description of the separated terms was short, and they were all referred to as DIC drivers together. We also only relied on surface chlorophyll, which is indeed a measure of standing stock, to explain the biological CO_2 uptake. In the revised manuscript and in the answer to the first main comment above, we have clarified this point by showing all the terms and how we consider them to contribute to the surface layer changes of DIC (eq. 1). We also explain that we neglect the horizontal term as we make a regional average over the whole sub-Antarctic and Antarctic regions. While we don't provide an explicit estimation of vertical transport, we use the discretized DIC changes at the base of the mixed layer to provide an estimate of surface DIC changes driven by winter convective entrainment.

In the revised manuscript, we added net primary production (NPP), surface oxygen and carbon export to help constrain the role of biological DIC changes from entrainment fluxes. The addition of NPP and Carbon export improved our separation of the biological terms for DIC changes from entrainment.

Reviewer: 184 A brief description of the Orsi definition should be provided here.

Response: A brief description of Orsi definition was added,

"In this study, we partition the Southern Ocean into 2 zones using the criteria proposed by Orsi et al., (1995). It is defined as the ocean south of the Sub-tropical front (STF: 11.3°C isotherm at 100 m) [Orsi et al., 1995] and divided into two main domains, the Sub-Antarctic Zone between the STF and the Polar Front (PF, 2°C isotherm at 200 m) the Antarctic Zone south of the PF. We further partition the domain into the three main basins of the Southern Ocean i.e. Pacific, Atlantic and the Indian Ocean."

Reviewer: 249 – The statement that the models "do not capture any of the basin-specific features" is a fairly strong, but non-quantitative statement. This should be much more specific – like, the observational reanalysis shows a stronger flux in the Atlantic than the Indian and Pacific while the models show similar fluxes in each basin.

Response: We thank the reviewer for this suggestion. As suggested in the main comments above, we have now made the analysis more quantitative in general, and here in particular, we added a measure of how different the seasonal cycles from the various models are from the observational data products by using the correlation coefficient (Fig.2, revised Figure 4 in the new manuscript). We have also corrected the sentence as suggested.

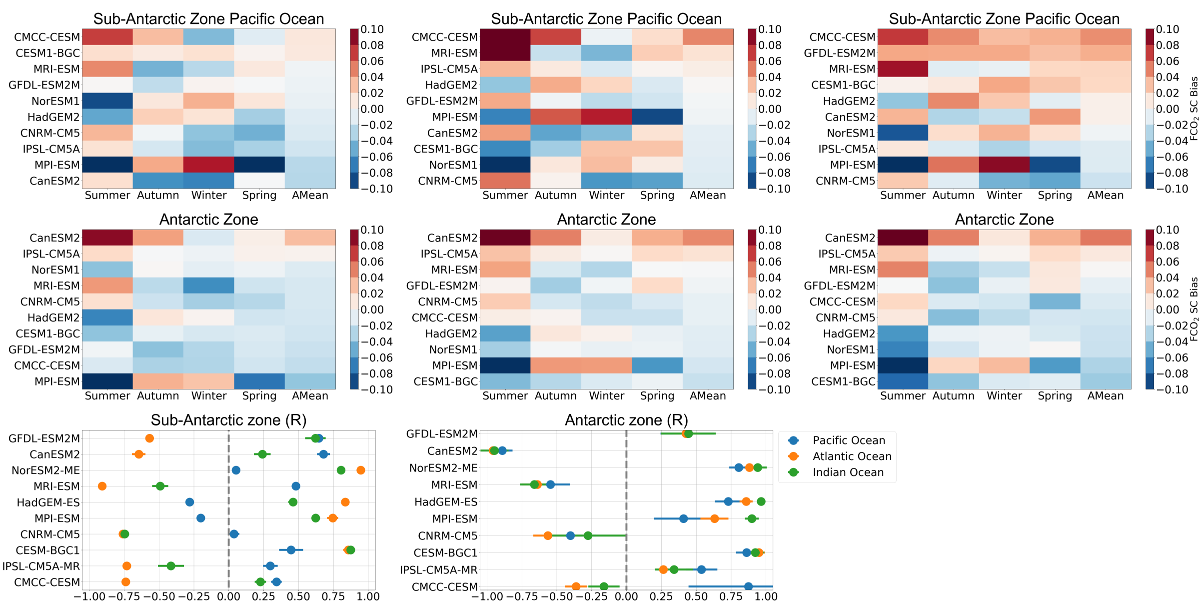

Figure. 2 Sea-Air CO_2 Flux mean seasonal and annual biases with respect to observations (gC m^{-2} yr^{-1}) for the Sub-Antarctic and Antarctic zones in the Pacific Ocean (first column, a and d), Atlantic Ocean (second column, b and e) and Indian Ocean (third column, c and f). CO_2 out-gassing biases are in red, while blue color intensity shows in-gassing biases. The models are sorted according to the annual mean bias presented in the last column (Amean). The quantitative assessment of the difference between the seasonal cycle of the data product and models is based on the correlation coefficient and its confidence interval.

Reviewer: 319 – "justifies our a priori separation" comes across as inappropriate self- congratulation. The salient point is that the separation quantifies the separation be- tween the two classes of models in terms of the relative dominance of SST-Flux correlation.

Response: We thank the reviewer for pointing out this issue and suggesting a proper sentence. We changed the text as follows;

"The model groupings that emerge from the calculated $M_{T\text{-}DIC}$ for CMIP5 models and observations quantify in terms of the relative role of temperature the a priori separation between group A and group B fluxes in the Sub-Antarctic zone that we proposed in section 3.2. It shows that the CO2 flux in group A models (HadGEM2-ES, NorESM2 and MPI-ESM) is mainly biologically driven while all group -B models show a stronger temperature control (solubility is the main mode driving pCO_2 seasonal variability), particularly in the Sub-Antarctic zone (Fig. 6a-c). In the Antarctic zone $M_{T\text{-}DIC}$ magnitudes are relatively more similar between CMIP5 models and observations, which is supported by FCO_2 seasonality."

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