

1 **Review 2 (RC2)**

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

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

7 **General comments**

8 **Reviewer:** I think the methodology could be better explained. Perhaps the logic of equations
9 1-3 is explained more thoroughly in the previous paper by Mongwe et al 2016, but this
10 critical reference is missing from the reference list. Assuming they mean the paper in Ocean
11 Modelling 106: 90, I will agree that equations 1 and 2 can be derived from equations 3.2-3.4
12 in that paper. But the LHS of equation 2, which does not appear in the previous work, is
13 physically speaking, a fairly nebulous quantity. The Takahashi et al 1993 estimate of 0.0423
14 K^{-1} mainly expresses the change in partial pressure due to changing temperature for a
15 given concentration of CO_2 ($[CO_2^*]$), with a small contribution from the partitioning of DIC
16 among $CO_2/HCO^3-/CO^3-$ due to the temperature dependence of the equilibrium constants.
17 DIC does not change as a result of changes in temperature, except indirectly through gas
18 exchange.

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

26 $0.0423 K^{-1}$ is intended to be an average value for a broad range of ocean conditions, but it is
27 stated to be valid for salinities from 34-36 and temperatures from 2-28°C (Takahashi et al
28 1993). In the Southern Ocean one will encounter conditions outside, or on the far edges of,
29 these ranges. What are the implications of this for the analysis shown here? This seems like
30 something that could be evaluated. Similarly, the calculations assume a constant Revelle
31 factor, but it should be quite straightforward to calculate Revelle factors from the model

32 outputs, giving a range for the range of environmental conditions characteristic of the study
33 area. The conclusions are probably robust to these assumption, I see no reason why not
34 tested.

35

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

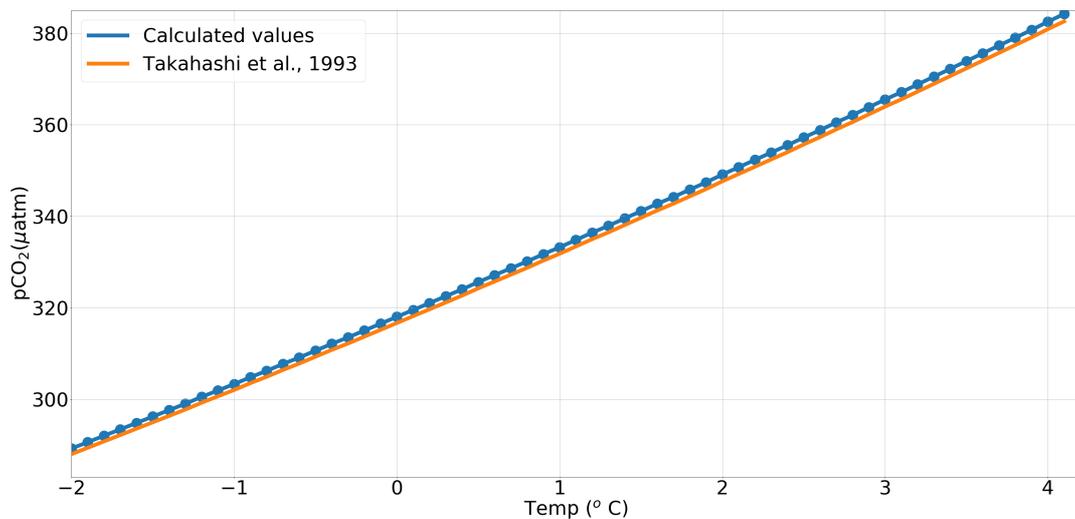
44 This reviewer's comment on the methodology is partly connected to the next comment
45 asking how we ascribed the influence of temperature to DIC and the partitioning of the
46 other DIC terms. We have deferred this part of the response to the next comment, in which
47 this question is raised in a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

52 We examined the applicability of the Takahashi et al., (1993) linear approximation
53 $\left(\frac{1}{pCO_2} \frac{\partial pCO_2}{\partial SST} \approx 0.0423^\circ C^{-1}\right)$ in our region of study. In the Sub-Antarctic zone, surface water
54 has a temperature range of ~4°C – 12°C, which is within the limits (2 – 28°C) provided by
55 Takahashi et al (1993). Since surface temperatures go below 2°C in the Antarctic zone, we
56 have tested whether this relationship can be extrapolated down to -2°C. We do so by
57 comparing the dependence of pCO₂ on temperature for a range of temperature values (4°C
58 to -2° : 0.1°C intervals) using a carbonate equilibrium model and the Takahashi et al., (1993)
59 linearization. Ancillary variables DIC, TAlk, phosphate, silicate and salinity are fixed and the
60 carbonate equilibrium model CO2SYS (Pierrot et al., 2006) was used with K1, K2 from
61 Mehrbach et al., (1973) refitted by Dickson and Millero, (1987), which are the same
62 parameterizations used in the majority of CMIP5 models. We used the mean climatological
63 pCO₂ (Landschützer et al., 2014) and the seasonal mean for nutrients (silicate and

64 phosphate), salinity, TALK and DIC from the same Antarctic region in GLODAP2 as was used in
65 the models.

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

70



71

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

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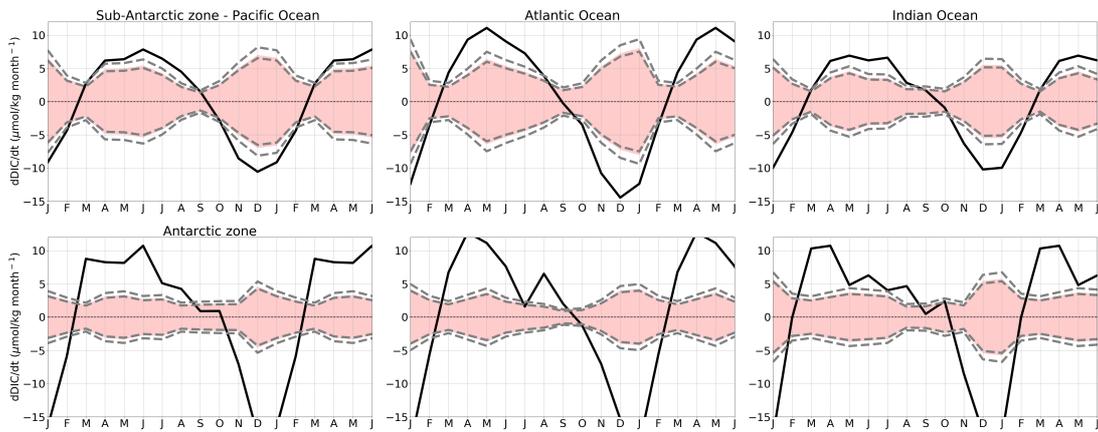
79 In a comparable way, we examined the uncertainties arising from a fixed polar Revelle factor
80 in our calculations. We recomputed the Revelle factor in the Sub-Antarctic and Antarctic
81 zones using annual mean climatologies of TALK, salinity, surface temperature nutrients.

82 Firstly we examined DIC changes for the nominal range of $p\text{CO}_2$ change ($340 - 399 \mu\text{atm}$, 1
83 μatm intervals) and then used this dataset to derive the Revelle factor ($\gamma_{\text{DIC}} = \frac{\text{DIC}}{p\text{CO}_2} \frac{\partial p\text{CO}_2}{\partial \text{DIC}}$).

84 The calculated Revelle factors in the Southern Ocean range between $\gamma_{\text{DIC}} \sim 12 - 15.5$ with an
85 average of $\gamma_{\text{DIC}} = 13.9 \pm 1.3$. This justifies our use of $\gamma_{\text{DIC}} = 14$ for the conversion of the

86 equivalent solubility driven pCO₂ change to DIC throughout the analysis. As an addition, we
 87 now provide the uncertainty in this conversion as it translates into the temperature
 88 constraint, by using the upper and lower limits of the Revelle factor ($\gamma_{DIC} = 12 - 15.5$) in the
 89 model framework. Below we show an example for observations in the Sub-Antarctic and
 90 Antarctic zone, this shows that extremes of the Revelle factor values ($\gamma_{DIC} = 12 - 15.5$) do not
 91 alter the phasing or magnitude of the relative controls of temperature or DIC on the
 92 seasonal cycle of pCO₂ (Fig 2).

93



94

95 **Figure. 2** Seasonal cycle of the rate change of surface total DIC for the Landschützer et al
 96 (2014) data product (black line) for the Landschützer et al (2014) data product and the estimated
 97 temperature driven DIC rate of change ($\frac{\partial DIC}{\partial t}_{SST}$) shaded area, for monthly data given in
 98 $\mu\text{mol kg}^{-1} \text{ month}^{-1}$ at the Sub-Antarctic zone i.e. Pacific Ocean (first column), Atlantic Ocean
 99 (second column) and Indian Ocean (third column). The dotted line shows the uncertainty
 100 boundaries for the Revelle factor extremes accounting to range in the Southern Ocean ($\gamma_{DIC} =$
 101 $12 - 15.5$).

102

103 **Reviewer** Finally, isn't the total DIC variability by definition the sum of the various
 104 components? So I'm not clear why the temperature driven component should ever be larger
 105 than the total. I find equation 3 and the discussion on 304-311 to be the most confusing
 106 part. We have an observed rate of change of DIC (which is never actually defined), which
 107 one would think would be the sum of the contributions from gas exchange, biological uptake
 108 /rem mineralization and entrainment. But in this case, the index that is considered is that the
 109 total is either greater or less than one of these three components (whose physical meaning

110 is nebulous). To confuse matters worse, we have a reference to "the total DIC seasonal cycle
 111 (dDIC/dt)" (306-307). Doesn't dX/dt imply an instantaneous rate-of-change that will itself
 112 vary over the annual cycle? I really do not understand what is being asserted here. (Also, the
 113 text should say something about exactly what sort of discretization was used in calculation
 114 of trends, e.g., does delta-X/delta-t for November represent a value for Nov. 1 based on a
 115 difference of October and November means, or is it something else? If this is the case, figure
 116 axes should indicate that calculated values are for the first of the month and not the mid-
 117 month.)

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

$$124 \quad \left(\frac{\partial DIC}{\partial t}\right)_{Tot} = \left(\frac{\partial DIC}{\partial t}\right)_{air-sea} + \left(\frac{\partial DIC}{\partial t}\right)_{Bio} + \left(\frac{\partial DIC}{\partial t}\right)_{Vert} + \left(\frac{\partial DIC}{\partial t}\right)_{Hor} \quad (\text{eq.1})$$

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

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

$$128 \quad \left(\frac{\partial DIC}{\partial t}\right)_{Tot} = \left(\frac{\Delta DIC}{\Delta t}\right)_{n,l} = \frac{DIC_{n+1,l} - DIC_{n,l}}{1 \text{ month}} \quad (\text{eq. 2})$$

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

132 In order to compare the role of temperature in scalable terms with DIC in pCO₂ and FCO₂, we
 133 convert the instantaneous pCO₂ changes driven by solubility (using $\left(\frac{\partial pCO_2}{\partial DIC}\right) \frac{1}{pCO_2} =$
 134 $0.0423^\circ C^{-1}$ from Takahashi et al (1993)) to an equivalent DIC change using the Revelle
 135 factor $\left(\frac{\ln(pCO_2)}{\ln(DIC)} \approx \gamma_{DIC}\right)$.

$$136 \quad \left(\frac{\partial DIC}{\partial t}\right)_{SST} = \frac{DIC}{\gamma_{DIC} \times pCO_2} \left(\frac{\partial pCO_2}{\partial t}\right)_{SST} \quad (\text{eq. 3})$$

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

$$141 \quad M_{T-DIC} = \left| \left(\frac{\partial DIC}{\partial t} \right)_{SST} \right| - \left| \left(\frac{\partial DIC}{\partial} \right)_{Tot} \right| \quad (\text{eq. 4})$$

142 which is used to compare the estimated change of (equivalent) DIC as driven by
 143 temperature-controlled solubility with the actual DIC change simulated by the models and
 144 obtained from the observational data product. When $M_{T-DIC} > 0$ indicates that the $p\text{CO}_2$
 145 variability is dominated by the rate of change of temperature and when $M_{T-DIC} < 0$ indicates
 146 that the $p\text{CO}_2$ variability is controlled mainly by DIC changes.

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

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

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

157

$$158 \quad RE = U_e \left(\frac{\partial DIC}{\partial z} \right)_{MLD} \quad (\text{eq. 5})$$

$$159 \quad RE_n = \left(\frac{\Delta MLD_n}{\Delta t} \right) \left(\frac{\Delta DIC}{\Delta z} \right)_{n,MLD} \quad (\text{eq. 6})$$

$$160 \quad \Delta MLD = \frac{MLD_{n+1} - MLD_n}{1 \text{ Month}} \quad (\text{eq. 7})$$

$$161 \quad \left(\frac{\Delta DIC}{\Delta z} \right)_{n,MLD} = \frac{DIC_{n,MLD_{n+1}} - DIC_{n,MLD_n}}{\Delta z} \quad (\text{eq. 8})$$

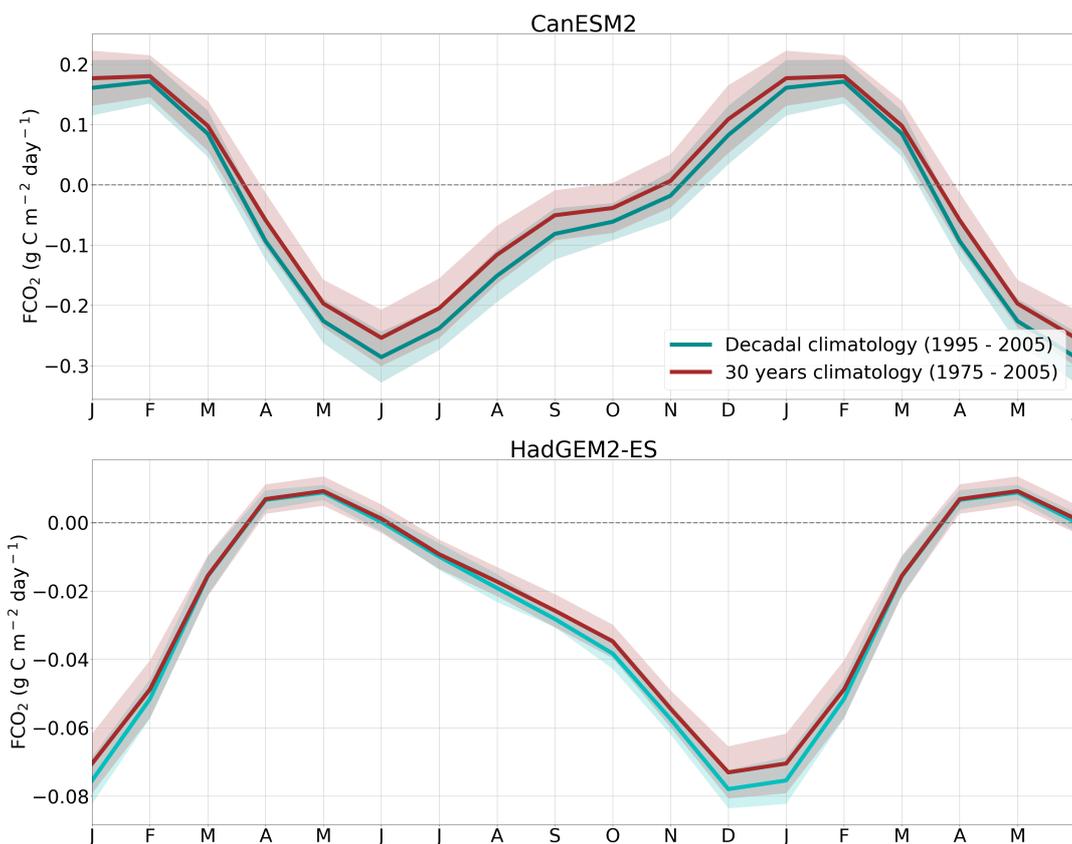
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163 In which U_e is an equivalent velocity based on the rate of change of the mixed layer depth.
164 This approximation of vertical entrainment is necessary as it is not possible to compute this
165 term from the CMIP5 data because the vertical DIC distribution is only available as annual
166 means. We clarified that we use the estimated rate of change of DIC at the base of the
167 mixed to examine surface DIC changes driven by subsurface/bottom DIC changes. We also
168 updated F_{DIC} estimate with GLODAP version 2 dataset.

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

178 **Response:** The choice of the period was to match a period closest to the available
179 observational data product (Landschützer et al (2014), 1998 – 2011). However, the reviewer
180 is correct to highlight that we assumed that the seasonal cycle of CO_2 does not vary
181 significantly on decadal timescales. We have now investigated this assumption for a few
182 models (here we present HadGEM2-ES and CanESM2), comparing the mean seasonal cycle
183 climatology of FCO_2 at the Sub-Antarctic zone for 30 years (1975 – 2005) and 10 years (1995
184 – 2005) of the historical scenario (Fig. 3). It shows that the seasonal cycle of FCO_2 remain
185 the small ($R = 0.99$) in both HadGEM2-ES and CanESM2 over 30 year.

186 We have now added this sentence in the method section: “The choice of the 10 year period
187 was done to match the simulated pCO_2 values to the period of the observations. We tested
188 the interannual variability of the seasonal cycle over a period longer than the reference 10
189 years for a few models and found no significant variation in the monthly standard
190 deviation.”



191

192 **Figure 3.** Compares the seasonal cycle of Sea-Air CO₂ fluxes over 30 year (1975 – 2005 and
 193 10 years (1995 – 2015). The shaded area shows the standard deviation.

194

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

200 **Response:** Our initial choice for chlorophyll was because of the availability of an
 201 observational data product for comparison and that it was available for most models (9 out of
 202 10) in the CMIP5 portal. In the revised manuscript, we added net primary production
 203 (NPP), carbon export and oxygen for the models with available data. This addition of NPP,
 204 carbon export and oxygen was indeed useful in examining impact of biological driven DIC
 205 changes in particular it helped isolate/constrain the role of primary production on the
 206 seasonal cycle of pCO₂.

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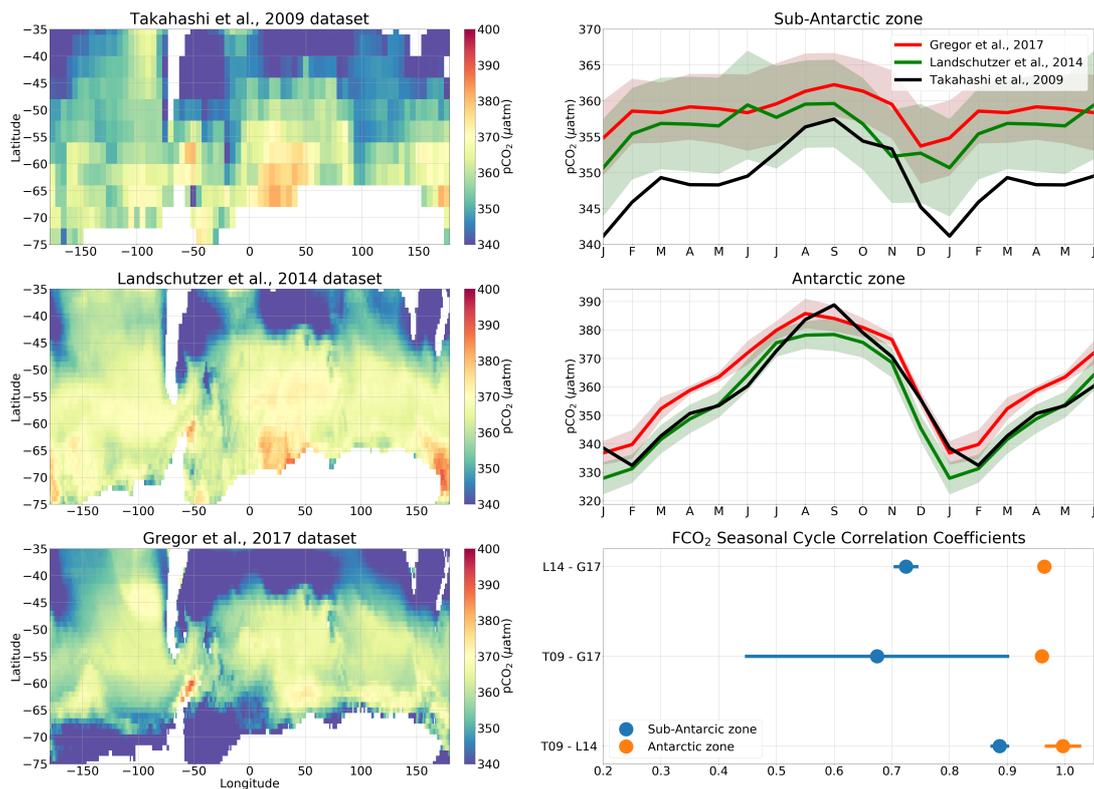
208 **Reviewer:** I think the authors should acknowledge that the FCO₂ data product is not really
209 'observed' in the sense that pCO₂ is. I think they should compare modelled and observed
210 pCO₂, and then discuss what this means for modelled estimates of CO₂ flux, without
211 referring to the Landschuter FCO₂ estimates as observations. CO₂ fluxes in models and data
212 products like this are actually quite different conceptually. When you estimate CO₂ flux from
213 observed pCO₂, the errors in the flux are a linear function of errors in wind speed (or u^2 ,
214 assuming a quadratic parameterization) and the piston velocity. In numerical models, pCO₂
215 and DIC self-regulate to dampen these errors when monthly averaged fluxes are considered,
216 e.g., if both the wind speed and the DIC are too large, the enhanced outgassing flux will
217 reduce the pCO₂ and DIC error. Higher wind speed or piston velocity will tend to drive pCO₂
218 towards atmospheric, and not necessarily towards the 'correct' value if over- or
219 undersaturation exists, so there is no straightforward way to correct for this difference. But I
220 think that the authors should acknowledge that it exists, and that in comparing modelled
221 and 'observed' fluxes they are to some degree comparing apples and oranges. (With regard
222 to point (4) above, if there is no observed primary production, there is no observed CO₂ flux
223 either: both of these are extrapolated from the primary observed field using models of
224 unknown accuracy.)

225 **Response:** It is indeed true that FCO₂ observations we use are estimates, data products
226 derived from empirical methods of gap filling. We now refer to them in this answer and in
227 the revised manuscript as "data products". We tried to make the point much clearer in the
228 revised manuscript and also provided the uncertainty as given by Landschützer et al (2014)
229 and used the interannual standard deviation over 14 years when comparing the seasonal
230 cycle. However, it also important to note that the majority of our analysis is based on pCO₂
231 estimates rather than FCO₂ in particular. Thus, we do acknowledge the role of wind in
232 providing the kinematic forcing for sea-air fluxes and although the wind influences the
233 magnitude of the fluxes, the direction is determined by delta pCO₂, which is here considered
234 the primary driver of the seasonal cycle of FCO₂. We are mindful that differences in the
235 parameterization of the Sea-Air interactions and wind products chosen are likely to affect
236 the resulting FCO₂ (Feely et al., 2004; Swart et al., 2014).

237 The uncertainty in the data products used for the assessment is an important point that we
238 neglected to highlight in our first version of the manuscript. We now acknowledge the
239 limitations of the data products and have further addressed the issue of uncertainties as

240 requested by other reviewers as well.

241 We have examined the uncertainty of our reference data set by comparing Landschützer et
242 al., 2014 data product with the more recent Gregor et al (2017a) data product, which uses
243 Support Vector Regression (SVR) and Random Forest Regression (RFR), as well as Takahashi
244 et al (2009) to derive a seasonal climatology $p\text{CO}_2$ in the SAZ and AZ of the Southern Ocean
245 in the supplementary material (Fig. 5). Part of the reason we focus on $p\text{CO}_2$ in these data
246 products instead of $f\text{CO}_2$ is firstly, because Gregor et al., (2017a) only focuses on fugacity
247 and $p\text{CO}_2$. Thus we mindful that the choice of wind product and transfer velocity constant in
248 computing $f\text{CO}_2$ is likely to increase the level of uncertainty for the compared data products
249 (Swart et al., 2014). Secondly, while we evaluate $f\text{CO}_2$ biases as the final aim of the paper,
250 the major part of our diagnostic analysis is based on $p\text{CO}_2$ rather $f\text{CO}_2$. Fig. 4 below shows
251 the climatology of the seasonal cycle of $p\text{CO}_2$ in the Sub-Antarctic zone and Antarctic zone
252 with interannual standard deviation between 1998 – 2011. All three datasets mostly agree in
253 the phasing of the seasonal cycle of $p\text{CO}_2$ in the Sub-Antarctic, but show significant
254 differences in the magnitude. Takahashi et al. (2009) shows an amplified impact of primary
255 production on $p\text{CO}_2$ in summer. We see this as a bias in the Takahashi et al., (2009) dataset
256 arising from a period when the space – time coverage of $p\text{CO}_2$ observations was still limited
257 and strongly biased towards summer. In the Antarctic zone all three observationally-based
258 data products agree in both phasing and amplitude. At this stage it is not clear whether this
259 agreement is due to all the methods being equally exposed to the same limited observations
260 or if it is due to a more marked CO_2 seasonal cycle in the Antarctic zone (relative to Sub-
261 Antarctic) that can be captured with less observations



262

263 **Fig. 4** pCO₂ (µatm) spatial (climatology) and seasonal cycle differences in Landschützer et al (2014)
 264 (L14), Gregor et al (2017) (G17), Takahashi et al (2009) (T09) datasets in the Southern Ocean. The
 265 seasonal cycle climatology of pCO₂ in the Sub-Antarctic and Antarctic zone is based on the period
 266 1998 – 2011. The shaded areas show the standard deviation of the interannual variability of the
 267 seasonal cycle for this period. The uncertainty in the correlation coefficient is based on the
 268 correlation coefficient of the mean plus standard deviations seasonal cycle(s).

269 **Reviewer:** I would like to see some discussion of the possibility that the apparently greater
 270 temperature control in the Pacific sector (259-263) is a real effect that arises from iron
 271 limitation. Because terrestrial sources of iron are much greater in Atlantic sector and the
 272 western half of the Indian sector (see e.g., Graham et al 2015 DSR I 104: 9; Tagliabue et al.,
 273 2012 Biogeosciences 9: 2333), it seems logical that the effect of seasonal biological
 274 drawdown on pCO₂ would be greater than in the Pacific and in the eastern half of the Indian
 275 sector. These regions also overlap the regions where the wind speed and the amplitude of
 276 its annual cycle are greatest (e.g., Trenberth et al., 1990, JPO 20: 1742), which will also tend
 277 to reduce the influence of biological uptake

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

281 the revised manuscript;

282 “The observed differences in the seasonal cycle of FCO_2 across the three basins is likely due
283 to differences in the basin properties of the Southern Ocean. Recent studies have
284 highlighted significant basin scale differences in pCO_2 and FCO_2 ascribed to large-scale
285 differences in temperature (Landschutzer et al., 2015), winds (Gregor et al., 2017b and
286 primary production as reflected in surface ocean phytoplankton biomass (Thomalla et al.,
287 2011). The relatively higher chlorophyll biomass (Graham et al., 2015; Thomalla et al., 2011)
288 in the Atlantic Ocean, is likely linked to lower wind speeds (Trenberth et al., 1990) and
289 higher supplies of iron from continental shelves (Thomalla et al., 2011; Boyd and Ellwood,
290 2010; (Tagliabue et al., 2014)2; 2014). This is likely associated with longer periods of
291 shallower MLD (Dec – Mar, Fig. 7), which favor sustained primary production leading to a
292 stronger CO_2 sink in the Atlantic Ocean with respect to the Indian and Pacific Ocean (Figs.
293 3a-c; 6a-c). In contrast, shorter periods of shallow MLD and lower iron concentrations in the
294 Pacific Ocean, as pointed out by Tagliabue et al. (2012), likely account for lower chlorophyll
295 biomass and stronger thermal control of the seasonal cycle of pCO_2 and FCO_2 (Fig. 6b, Fig. 3
296 here). In the Indian Ocean stronger wind speeds are likely responsible for the early
297 deepening of the MLD (Fig. 6c), and thus chlorophyll biomass are lower (Fig. 9). In the Indian
298 Ocean, stronger wind speeds (Trenberth et al., 1990) are likely responsible for the early
299 deepening of the MLD (Fig. 7c), limiting primary production and lower rates of change of
300 temperature (Fig. 5c), ultimately resulting in a relatively constant FCO_2 for about half the
301 year (Dec – Jun). Our plots indicate that CMIP5 models mostly don’t show these basin-
302 specific features highlighted in observational products (Landschutzer et al., 2015; Gregor et
303 al., 2017a and Thomalla et al., 2011) with the exception of three group B models (i.e.
304 CESM1-BGC, CanESM2 and CMCC-CESM) in the Indian Ocean (Fig. 2, 3 a-c). This poses a
305 challenge to the new generation of Earth Systems Models in CMIP6”
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314 **References**

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