Reviewer 3 (RC3)

We thank the reviewer for the examination of our manuscript, provided reviews were helpful in improving the manuscript. We hope we have addressed all comments satisfactory below. The manuscript has been improved with respect to the submitted version, below we detail some of the improvements specifically requested.

General comments

Reviewer: These diagnostic tools are based on empirical relations. The author's approach is thus not fully based on model results and leaves me not fully convinced. Are simulated strong DIC variations really due to high biological activities, and is the subsequent respiration really the cause for outgassing? This appears highly speculative.

Response: We understand the reviewer's concern and we are confident that they are mostly due to our too brief explanation of the methodology. We relied excessively on a previous published paper by Mongwe et al. (2016) where the methodology was explained in full detail and we have now improved this part in the Method section. This was also compounded by some missing references, including 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 inexcusable mistake; we have corrected all references in the revised manuscript.

The approach is fully based on model process analysis, though it employed the widely used Takahashi et al, (1993) empirical relationship that

linearized the temperature dependence of pCO_2. We also neglected to provide a description of how we separated the terms contributing to the total DIC surface layer changes, as pointed out by another reviewer. 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).

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

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 new 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.

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

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^{o}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 $\left(\frac{\ln(pCO_2)}{\ln(DIC)} = \gamma_{DIC}\right)$.

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

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).

Using this approach, we were able to link temperature (solubility) issues to the carbon flux discrepancies diagnosed in a group of models that was named Group B, as opposed to the other models in group A. Overestimated warming and cooling rates in the transitional seasons were found to be the source of the CO_2 flux bias in group B models. Our further analysis demonstrated that the estimated physical processes associated to changes in the MLD are not enough to explain the behavior of group A models, and the only process that was evident, was exaggerated primary production (and possibly associated shallow respiration). Therefore, the DIC variations and the related CO_2 fluxes are linked to biological processes only for one class of the analyzed models (group A).

We then proceed to investigate the role of the other terms in eq. 1, namely the biological rate and the vertical DIC entrainment at the base of the mixed layer (we now make clear that the horizontal circulation term is neglected due to the regional averaging). For a more explicit

analysis of the biologically linked DIC changes we have now added Net Primary Production (NPP), carbon export fluxes and oxygen concentrations in addition to the biomass (chlorophyll) that was used in the submitted original manuscript. This approach has improved our analysis of the biological influenced DIC variability; in particular it helped constrain the role of primary production on the seasonal cycle of pCO_2. It's true that the linked role of respiration in the DIC changes remain a speculation as we have not explicitly quantified respiration, nevertheless we now include more information supporting this speculation i.e. NPP and surface oxygen. Because of the lack of appropriate model output variables, the role of respiration is not fully explored here, however we maintain the speculation in the Discussion that the role of near surface respiration on DIC changes remain an important question for future research.

Reviewer: Give additional information on model characteristics: Parameterization of air-sea heat fluxes (e.g., dz(1); which layers are treated? ..) Parametrization of air-sea flux of CO2 (e.g., Wanninkhof, 1992; etc) Is an ice model included?

Response: We have interpreted this question as a request to report the specific parameterizations (or numerical schemes) used in the coupled models to exchange heat and gas between the atmosphere and the ocean. We can confirm that all the models used the Wanninkhof (1992) parameterization for the air-sea carbon fluxes. As regards to the heat fluxes, this information is unfortunately not simple to retrieve as not explicitly written in the publications or in the technical reports. It is specific to the physical and numerical coupling between ocean and atmosphere models used in each ESM. We could only find it for a few models we have direct experience of. For instance, in the NEMO-based models, the heat flux is parameterized as a boundary condition for the vertical turbulent diffusion equation. We believe this is the standard method for all the other models but could not verify it.

Given the fact that all models tend to show the same kind of bias in the simulation of the rate of change of sea surface temperature except for

64 NorESM1, we would not ascribe it to a different choice of the heat flux parameterization.

65 All the climate models participating to CMIP5 have an active sea ice model. We have included more information in the table so that the reader

66 can trace the different components used in the ESMs i.e. ocean, sea ice and atmospheric model, nutrient state variables, vertical coordinates

67 and the various spatial resolutions.

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Full name and Source	**Model Name**	**Ocean Resolution**	**Atmospheric Resolution**	**Nutrients**	**Sea ice model**	**Veridical Coordinate & Levels**	**Ocean Biology**	**Reference**
Canadian Centre for Climate Modelling and Analysis, Cananda	CanESM2	CanOM4 0.9^{o} x 1.4^{o}	2.8125^{o} x 2.8125^{o}	N (accounts for Fe limitation)	CanSIM1	z 40 levels	NPZD	Zahariev et al., 2008
Centro Euro-Mediterraneo Sui Cambiamenti Climatici, Italy	CMCC-CESM	OPA8.2 $0.5\text{-}2^{o}$x2^{o}	3.8^{o} x 3.7^{o}	P, N, Fe, Si	CICE4	z 21 levels	PELAGOS	Vichi et al., 2007

Centre National de Recherches Météorologiques-Centre Européen de Recherche et de Formation Avancée en Calcul Scientifique, France	CNRM-CM5	NEMOv3.3 1^{o}	1.4^{o}	P, N, Fe, Si	GELATO5	z 42 levels	PISCES	Séférian et al., 2013
Institut Pierre-Simon Laplace, France	IPSL-CM5A-MR	NEMO2.3 0.5-2^{o} x 2^{o}	2.58^{o} x 1.25^{o}	P, N, Fe, Si	LIM2	z 31 levels	PISCES	Séférian et al., 2013
Max Plank Institute for Meteorology, Germany	MPI-ESM-MR	MPIOM 1.41^{o}×0.89^{o}	1.875^{o} x 1.875^{o}	P, N, Fe, Si	MPIOM	z 40 levels	HAMOCC 5.2	Ilyina et al., 2013
Community Earth System Model, USA	CESM1-BGC	0.3^{o} x1^{o}	0.9^{o} x 1.25^{o}	(P), N, Fe, Si		z 60 levels	BEC	Moore et al., 2004
Norwegian Earth System Model,	NorESM1-ME	MICOM 0.5^{o} x 0.9^{o}	2.5^{o} x 1.9^{o},	P, N, Fe, Si	CICE4.1	ρ 53 levels	HAMOCC	Tjiputra et al., 2013

Norway								69

Specific Comments;

Reviewer: There are large differences between FCO_2 used in this manuscript and values in Mongwe et al. (2016). Explain why you changed the data source. Why is it better than the former one?

Response: Our switch to Landschützer et al (2014) data product was based on the fact that it is based on more observations (SOCAT2, ~ 15 million surface measuremets) and provided as a 1^{o}x1^{o} global gridded product , compared to 5^{o}x 4^{o} in Takahashi et al., 2009, ~ 3 million surface measuremets. This increased the number of grid points per region (Sub-Antarctic and Antarctic) thus most likely making it a more appropriate basis to compare against CMIP5 models. Nevetherless, Landschützer et al (2014) is also subject to uncertainties arising from the empirical Nearest-Neighbour interpolations and we are mindful to refer to this dataset as observational data product. We also provide an uncertainty evaluation in the supplementary material of the revised manuscript, as requested by another reviewer. Since our analysis is based on the Landschützer et al., 2014 data product, we used the mean monthly FCO_2 (1998 – 2011) to compute the standard deviation of the seasonal cycle of FCO_2. to compare against CMIP5 models variability in Fig. 2 & 3 of the original manuscript.

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87 **Reviewer:** Line 115: Which equilibrium constants are used?

88 **Response:** K1, K2 from Mehrbach et al., 1973 refitted by Dickson and Millero, 1987, added to the revised manuscript.

89 **Reviewer:** Line 237: Is the standard deviation also applied over the ensemble members?

90 **Response:** Yes, we applied the standard deviation over the 10 years.

91 **Reviewer**: Line 244 "weakening of uptake or an increase of outgassing"

92 **Response:** Here we mean weakening of uptake since the net CO_2 flux remains in the negative phase of the seasonal cycle (net ingassing flux).

93 **Reviewer:** Line 249: There are more zonal differences in the model results than in the observations

94 **Response:** Though the CMIP5 models show some differences in the seasonal cycle between the basins, the FCO_2 seasonal cycle phasing mostly
95 remain the same between basins in CMIP5 models, except for CESM1-BGC. The observational data product, in contrast, show almost different
96 seasonal cycle phasing in each basin. We clarify this in the revised manuscript.

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98 **Reviewer:** Line 268: I see an overestimation of group A, but no underestimation of group B

Response: The reviewer is correct to point this out, this sentence was not well explained. Here we meant underestimation with respect to observations CO_2 uptake. During the summer season (DJF) group A models (FCO_2 ~ -0.12 gC m^{-2} day^{-1}) show an overestimated uptake, while group B (FCO_2 ~ 0 gC m^{-2} day^{-1}) underestimates the CO_2 uptake relative to observations (FCO_2 ~ -0.03 gC m^{-2} day^{-1}). We rephrased this passage to try to make this point clearer.

Reviewer: Line 333 what's about the entrainment of alkalinity? This would damp the DIC effect on pCO2

Response: In the supplementary material of the revised manuscript (referenced to in the method description) we show that the DIC changes due to salinity and total alkalinity are much smaller relative to the total surface DIC changes and temperature driven DIC changes (solubility); As shown in Fig. 2 & Fig. 3, the surface total DIC change $\left(\left(\frac{\partial DIC}{\partial t}\right)_{Tot}\right)_{average} \approx 10\,\frac{\mu mol}{kg\ month}$, while $\left(\left(\frac{\partial DIC}{\partial t}\right)_{Sal}\right)_{maximum} \approx 0.6\,\frac{\mu mol}{kg\ month}$ and $\left(\left(\frac{\partial DIC}{\partial t}\right)_{Talk}\right)_{maximum} \approx 0.4\,\frac{\mu mol}{kg\ month}$. Thus $\left(\frac{dDIC}{dt}\right)_{Sal} \ll \left(\frac{\Delta DIC}{\Delta t}\right)$ & $\left(\frac{dDIC}{dt}\right)_{Talk} \ll \left(\frac{\Delta DIC}{\Delta t}\right)$, these (salinity and Talk equivalent DIC changes) are about 10 orders of magnitude smaller than the equivalent solubility and total DIC change i.e. 0.5 µmol kg^{-1} vs 5 µmol kg^{-1} respectively. Consequently, though salinity and Talk do play a role, they have been neglected from the analysis.

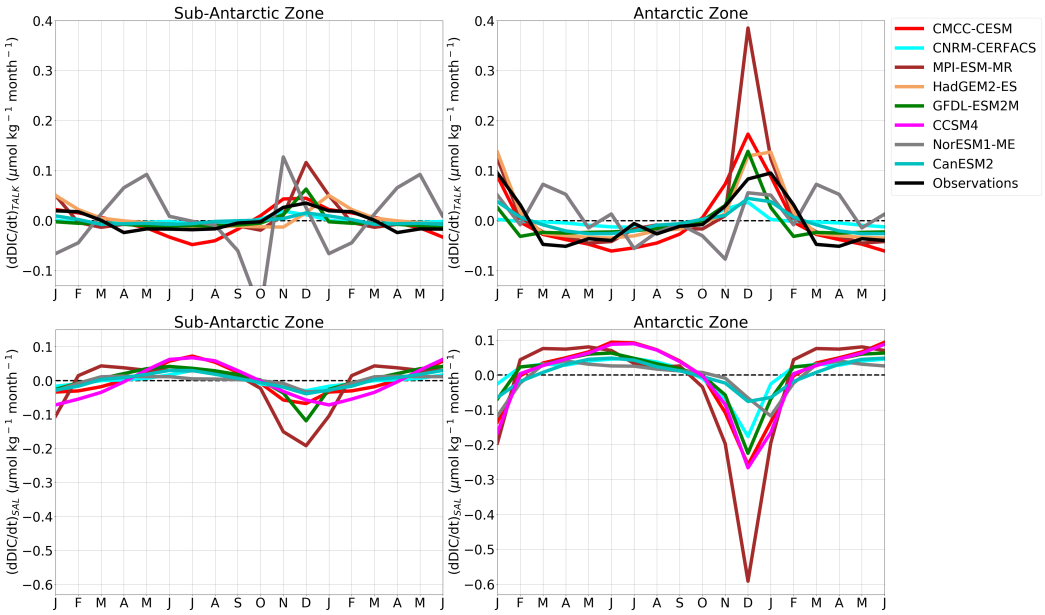

Figure. 2 Shows the seasonal cycle of the rate equivalent DIC changes driven changes in Salinity and total alkalinity computed using Takahashi et al., (1993) formulations.

Reviewer: Line 406: The wording “anticipated” shows the high degree of speculation. By the way, I see only two type A models doing so.

Response: We agree with the reviewer that the phrasing of the sentences sounded speculative. This analysis was strengthened by the addition of NPP and carbon export in the revised manuscript (see also the answer to the main comments above). It is indeed true that the role of

119 respiration remains speculative here since we don't explicitly quantify the role respiration and we clarify this point in the manuscript. All three
120 models in group A do show this behavior, though HadGEM2-ME only shows a modest effect, and this is now highlighted in the revised
121 manuscript.

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188