We thank referee#2 for reviewing the paper. We address point by point (answers in red) the concerns of the referee.

General comments:

Since the study is based on the normalization of DIC I'm wondering about the used normalization. It was shown that an easy division by salinity is problematic especially in a global perspective. The authors should validate their approach or at least discuss its problems.

We understand that the referee is concerned that the traditional salinity normalization would create artificial variance as argued by Friis et al. (2003), because the traditional calculation ignores the influences of riverine input and upwelling from below the lysocline.

This issue was addressed in our reply to Referee#1 (https://www.biogeosciences-discuss.net/bg-2018-376/bg-2018-376-AC1-supplement.pdf). We added one paragraph to Section 2.2 to explain why this issue is not quantitatively important in our study.

The authors use the GLODAP v2 dataset for the surface ocean. During their calculations they convert data several times into pCO2. I'm wondering if the use of SOCAT as a pure surface ocean data set might be useful in order to check the calculations.

We did not use SOCAT to check the calculation because it cannot provide a point by point comparison (SOCAT sampling points were at different times and places than GLODAPv2 sampling points).

Takahashi et al. (2014) found good agreement between calculated variables of the carbonate system (e.g., TA, DIC, pCO_2) compared to measured values. Values agreed within the measurement uncertainties assumed for the LDEO database. They found "the computed pCO2 values from the TCO_2 and TA data are in agreement with the measured values within ±6.8 µatm". We added one sentence in Section 2.4.1 to accommodate this issue.

What is with seasonality? Are the authors using seasonal average?

We did not discuss seasonality in the text because this is not the aim of this manuscript, and seasonal changes are of a much lower magnitude than the latitudinal gradient. For instance, the seasonal difference at the KERFIX site in the Southern Ocean was measured to be 25 µmol kg-1 (Louanchi et al., 1999), which is much less than the overall latitudinal gradient we are looking to explain, of order 200 µmol kg^{-1}.

In addition, in general GLODAPv2 does not contain enough data to constrain seasonal cycles, particularly in the Southern Ocean where winter data is scarce.

I want to suggest merging the discussion and results sections. There are a lot of parameters discussed and described. Streamlining the sections and shorten it might help the readability. The whole manuscript is not easy to read. This

is partly just to the fact that a lot of conversions are done and the reader had to keep track of it. There is not much that can be done to this.

But the authors should carefully proof read their manuscript as some phenomena are discussed in several different positions of the manuscript. Again I want to advocate shortening it where possible in order to improve the readability. Some formulations sound odd and sometimes it's going back and forth especially in the introduction section.

We thank the referee for providing insights into how to improve the readability. In addition to the specific places where repetitions were found by the referee, we also shortened and modified the text (e.g. changes made in Section 1, Section 2.4, and Section 4.1) to avoid repetition and be clearer. However, we decided not to merge the results and discussion sections because it would lead to some very long sub-sections, which in our view would be off-putting to some readers and make the manuscript less rather than more readable.

Specific comments (pp/ll):

01/24: cite the most actual GCB from 2017 or even 2018.

Changed.

01/29: I'm not sure, but is it worth to explain what CO2* is?

We added an explanation.

02/31 – 03/03: the authors discuss Takahashi (2014) work on page 2. On page 3 they say "since these studies the database was extended: : :". This doesn't make sense.

We thank the referee for pointing out this issue. We have revised the sentence.

03/12: "other processes" sounds very broad. Can you specify?

We have modified the text to read "the above processes (1-4) which affect TA simultaneously".

03/14-16: repetition from before 03/18-20: Repetition from before.

03/14-16 actually summarizes the novelty, with which we expected to give the readers a straightforward view of what is new about this study. 03/18-20 was removed, along with 03/20-22.

04/13: I prefer "water depth" over "seafloor depth".

Changed.

04/25ff: Do the authors take spatial variability of atmospheric CO2 into account?

We used the globally averaged atmospheric CO_2. We now state this.

04/29: what is xCO2air? Please explain.

We explained in 04/27 when this term first appears. $xCO_{2,air}$ refers to the atmospheric mole fraction of CO_2. We

added an explanation to the superscript appeared in Equation 2.

05/1-11: Somehow I got confused. It's a lot of steps for a quite easy process. But right now I also don't have a better solution. Just wanted to mention my first thought.

The principle here is we assumed that sea surface CO_2 changes ($\Delta xCO_{2,sw}$) track atmospheric CO_2 changes ($\Delta xCO_{2,air}$). Then $\Delta xCO_{2,sw}$ was converted into ΔDIC using CO2SYS. We have made a couple of small changes that hopefully reduce confusion.

05/22ff: The formulation is odd. The authors state that they discuss the results in order of their hypotheses with exemptions. There are only three hypotheses so that sentence doesn't make sense to me.

We wanted to discuss the processes step by step. The second hypothesis can be assessed by salinity normalization so we did not have to repeat the analysis. We modified the text to be more accurate.

06/10ff: The authors mention that the increased pCO2 has the potential to elevate values above atmospheric level. But it also can just lower the gradient if seawater is undersaturated.

We agreed with the referee. This paragraph was just to give an explanation of how temperature changes (we used sea water warming as an example) alter the air-sea CO_2 gas exchange and therefore sea surface pCO_2 and DIC. We used the word "potentially" to indicate that this is just one aspect, which sounds most understandable to readers.

07/03: ": : :Antarctic Circumpolar Current (ACC): : :"

Changed.

07/05: The term "L3" is not introduced.

We defined L3 in the caption of Figure 4 and now cite that figure.

07/25: One example of not thoroughly structured the document. The authors talk about phosphorus and Redfield. They don't give a number nor a reference. This comes with part of the discussion here later. Please merge.

The number and reference were given after Equation 12 when it came into the calculation of $nDIC_{surf}$. We moved the number and reference for $R_{C:P}$ from 08/08 to 07/25.

07/33: Do you mean equation 10?

We thanked the referee for pointing out the mistake. Changed.

08/07: Together with Eq. 9 it reduces to nDICsurf = nDICsupply – NCP – 0.5xALK*CaCO3.

Changed.

08/09: RC should be RC:P.

Changed.

08/11: reference to Figure 5b is 5c.

Changed.

08/12ff: Presenting all the values in a table might be easier to read.

We have already got 4 tables and 10 figures, therefore we decided to keep these values in the text.

08/28: the effect has the potential to lower seawater pCO2 below atmospheric values.

Changed.

09/32: Why are you not using the nitrate values from GLODAP?

Because GLODAPv2 has sparse observations in the Southern Ocean. For this reason we used the global gridded product based on the WOA dataset. This much larger dataset is available for phosphate but not for DIC or TA. In response to a comment from referee#1, we decided in any case to use the phosphate rather than the nitrate data from WOA in order to be consistent with the calculation for NCP in Equation 9.

11/01: Is evaporation only happening in the Atlantic?

No, the main reason is the very large water vapour transport across Central America, due to the prevailing wind direction towards the west. To avoid giving a misleading impression, we removed the phrase “due to the intense evaporation in the subtropical Atlantic Ocean”.

11/15: Why is nDICtemp the gas exchange effect? Can you explain?

$\Delta nDIC_{temp}$ is analogous to ΔDIC_{temp}, which is defined in equation 8 (Section 2.4.1), It is a measure of the temperature-driven gas exchange effect because it is the difference between nDIC at in-situ temperature and nDIC at 27C. We have added a citation of equation 8 to clarify. As explained in section 2.4.1: “air-sea CO_2 gas exchange was assumed to proceed until pCO_2 was back to the same level as before resetting the temperature.”

17/17ff: CDIAC is no longer maintained.

Changed.

References

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Louanchi, F., Ruiz-Pino, D. P., and Poisson, A.: Temporal variations of mixed-layer oceanic CO_2 at JGOFS-KERFIX time-series station: Physical versus biogeochemical processes, Journal of Marine Research, 57, 165-187, 10.1357/002224099765038607, 1999.

Takahashi, T., Sutherland, S. C., Chipman, D. W., Goddard, J. G., Ho, C., Newberger, T., Sweeney, C., and Munro, D. R.: Climatological distributions of pH, pCO2, total CO2, alkalinity, and CaCO3 saturation in the global surface ocean, and temporal changes at selected locations, Marine Chemistry, 164, 95-125, http://doi.org/10.1016/j.marchem.2014.06.004, 2014.