Response to the Interactive comment by « Anonymous Referee #1 » on "Carbonate system distribution, anthropogenic carbon and acidification in the Western Tropical South Pacific (OUTPACE 2015 transect)".

Please note that the referee comments are typesetted with normal characters **and our responses to referee's comments are in bold characters. Text from the manuscript is indicated with** *italic characters* **and changes are highlighted** *in red*. **For minor changes mentioned by the referee, we sometimes just mention that we agree with the referee and we will of course make the corrections in the revised manuscript.**

General comments:
The main goal of the manuscript is to report a new dataset of measurements of the carbon system for the western tropical South Pacific (OUTPACE cruise). The authors describe the distribution of the different variables along the OUTPACE transect highlighting the differences between the western (Melanesian Archipelago, MA) and the eastern (western South Pacific gyre, WGY) part of the transect.
The authors also present results for derived properties (pH, Ω cal and Ω ara) of the carbon system and for anthropogenic carbon (CANT) that has been estimated by the TrOCA method. Making use of ancillary data, the authors present temporal changes in the properties (measured and estimated) observing: 1) a decrease in total alkalinity restricted to the MA area that disappears when using normalized alkalinity; 2) an "over accumulation" of total inorganic carbon and an increase in CANT (close to the thermodynamic value) in the upper thermocline waters; 3) a decrease in pH and shoaling of the aragonite saturation depth.
The dataset reported in the study is of high quality and without any doubt complements the decadal P21 hydrographic line. The manuscript is well written and ordered and the results are well presented.
Nevertheless, I have some specific comments that need to be addressed before being considered for publication.
First, we would like to thank Referee #1 for his/her careful evaluation of our manuscript. We believe that his/her comments will help to improve the manuscript. Please, find hereafter our responses to the concerns raised by Referee #1

Specific comments:

Abstract:
Page 1, line 22. Eliminate "of" after C ANT increases.
We agree with this correction.
Page 1, line 23. "in C ANT " instead of "of C ANT ".
We agree with this correction.
Page 1, line 24. "pH T " instead of "pH".
We agree with this correction.

1 Introduction:
Page 2, line 42. Delete "hereafter named".
We agree with this correction.

2.4 Derived parameters:
Page 5, line 138. "calcite (Ω cal)". There is no need to mention this variable because it is not displayed in the distributions (fig. 3) and its temporal change is not estimated. See comment on section 4.
The reference to (Ω cal) estimates was deleted but a sentence has been added to justify why this parameter was not considered.

Seawater pH on the total scale (pH_T) and the $CaCO_3$ saturation state with respect to aragonite (Ω_{ara}) were derived from A_T and C_T with the "Seacarb" R package (Gattuso and Lavigne, 2009). $CaCO_3$ saturation state with respect to calcite was not considered because seawater up to 2000 dbar was supersaturated with respect to calcite (Ω_{cal}>1).

4 Carbonate chemistry along the OUTPACE transect:
Page 7, line 200. Why is C T slightly lower in bottom waters?
A possible explanation is that in the South Pacific, the deep waters are among the oldest waters in the world ocean with high carbon content whereas the northward moving bottom waters have not had the time to accumulate as much carbon (see for example Murata et al. 2007). The sentence has been modified as follow:
The C_T gradient in the upper water column has been described in Moutin et al. (2008). Below 2000 dbar, C_T is relatively invariant with slightly lower values in the bottom waters (below 4000 dbar) due to the presence of very old deep waters originating from the north Pacific relative to the northward moving bottom waters that have not accumulated as much carbon (Murata et al. 2007).

Page 7, line 211. "pH T " instead of "pH".
We agree with this correction.
Page 8, lines 217-220. Not need to add these sentences or maybe use them in section 2.4 to explain why you're not considering this variable for the temporal changes.
These sentences have been deleted and a sentence has been added earlier in section 2.4 to justify that Ω_{cal} will not be considered.

5 Anthropogenic carbon estimation along the OUTPACE transect:
Page 9, lines 272-279. The authors make the reader notice that denitrification could be affecting their estimates but nothing is concluded. The authors don't explain how they deal with this issue. In section 6 the authors give a reference for the low effect of denitrification over C ANT estimates that could be added in this section as a conclusion of why they don't consider N*.
We agree with this comment of the referee. Based on the suggestions of the referee, we have rephrased this section in order to be clearer.
Finally, it should also be mentioned that, due to the presence of one of the main OMZ area, denitrification occurs in the eastern South Pacific and can be traced by the N parameter (Gruber and Sarminento, 2007). Denitrification, by transforming organic carbon to inorganic carbon without consumption of oxygen, could induce an overestimation of C_{ANT} by the TrOCA method (and other back calculation methods) due to a biological release of C_T that is not taken into account in the formulation of the quasi conservative TrOCA tracer. Horizontal advection by the south equatorial current of the strong negative N* signal originating from the Eastern Pacific towards the western Pacific was previously described (Yoshikawa et al., 2015). Fumenia et al. (2018) have estimated N* along the OUTPACE transect and show slightly negative N* values in the upper thermocline waters at the eastern side of the OUTPACE transect where the highest CANT values are estimated. However, Murata et al. (2007) showed that, based on a direct relation between C_T and N*, the influence of denitrification should be negligible on C_{ANT} estimations in this area. Therefore, the N* correction has not been introduced in the C_{ANT} estimates and the effect of denitrication was not quantified here.*

Page 10, line 285. The year of publication of the reference is 2017.
We agree with this correction. However, following a suggestion of referee #2, this section will be deleted in the revised manuscript.

6 Temporal changes of inorganic carbon in the OUTPACE area:
Section's title: The authors talk about other variables than just inorganic carbon. I suggest to change "inorganic carbon" to "carbonate chemistry".

We agree with this correction.

Page 10, lines 303-305. Add the errors in the trends for A T . What is/are the oceanic process/es behind the change/not change in alkalinity.

Errors have been added for A_T trends. Concerning the main drivers of A_T changes in the ocean (Wolf-Gladrow et al. 2007): The major change in A_T can be attributed to changes in major conservative cations and anions (i.e. salinity). The other important changes in A_T are due to the biological precipitation of calcium carbonate and/or the dissolution of biogenic calcium carbonate. Finally, minor changes in A_T can be attributed to biological assimilation and remineralization of nitrate. A_T in the ocean is not affected by changes in the CO_2 content of the ocean. In our study, when A_T is normalized to salinity, no significant trends in A_T n35 are observed, suggesting that the observed trends in A_T can be attributed to salinity changes. The manuscript has been changed as follow:

However, when A_T is normalized to salinity, no significant trends are observed in $A_{T\ n35}$ suggesting that the observed trends in A_T can be attributed to changes in salinity rather than in calcification.

Page 10, lines 306-307. Add the errors in the trends for C T and C ANT .

We agree with this correction.

Page 10, line 310. Do the authors have an explanation for this “over accumulation”? What is the error in the increase of C T associated to the increase in atmospheric CO2? (line 308).

We agree with the referee that the discussion on this “over accumulation” was not precise enough. In the revised manuscript we will rephrase this section as mentioned in the next comment.

Page 11, lines 315-318. Considering the information given by the authors (page 10, lines 303-305), can the changes in AT still be due to remineralization processes? Can the authors give a possible scenario/explanation for the difference of C ANT between MA and WGY?

As mentioned in the above comment, this section has been rewritten as follow:

At $\sigma_{\theta\ 25}$, a significant decrease of A_T of -0.20 ± 0.07 µmol $kg^{-1}.a^{-1}$ is observed over the entire OUTPACE area. A decrease of -0.30 ± 0.09 µmol.$kg^{-1}.a^{-1}$ is also observed in the MA area, whereas no significant trend is observed for the WGY area. However, when A_T is normalized to salinity, no significant trends are observed in $A_{T\ n35}$ suggesting that the observed trend in A_T can be attributed to salinity changes rather than changes in calcification. Significant negative trends are observed for $[O_2]$ over the entire area (- 0.31 ± 0.10 µmol kg^{-1} a^{-1}) with - 0.35 ± 0.16 µmol kg^{-1} a^{-1} in the MA and - 0.38 ± 0.11 µmol kg^{-1} a^{-1} in the WGY. The decrease in $[O_2]$ which corresponds to a positive trend in AOU suggested an increase in the remineralization of organic matter at $\sigma_{\theta\ 25}$. Significant increasing trends were observed for C_T over the entire area (+ 1.32 ± 0.13 µmol kg^{-1} a^{-1}), in the MA (+ 1.38 ± 0.21 µmol kg^{-1} a^{-1}) and in the WGY (+ 1.57 ± 0.13 µmol kg^{-1} a^{-1}). For C_{ANT}, the trends were slightly slower (+ 1.12 ± 0.07 to 1.2± 0.13 ± 0.09 µmol kg^{-1} a^{-1}) and not significantly different between the MA and the WGY. Taking into account the OUTPACE dataset does not change the overall significance of the observed trends and only minor changes (mostly within the error of the estimates) are observed. If we assume a C_T increase of 0.5 to 1 µmol kg^{-1} a^{-1} (depending on the buffer factors considered) associated to the recent rise in atmospheric CO_2 (see for example Murata et al., 2007), the C_T increase in the OUTPACE area is faster than thermodynamics would govern whereas the C_{ANT} is closer to this thermodynamic value. The higher increase of C_T could be related to an increase in remineralization as deduced from $[O_2]$ trends, with an overall consistency between the rate of C_T increase and the rate of $[O_2]$ decrease. However, the important increase of C_{ANT} observed between 2005 and 2015 between 10°S and 30°S on the P16 line (at the eastern side of the OUTPACE transect) by Carter et al. (2017) is not supported by significant differences in the trends of C_{ANT} observed between MA and WGY in this study.

Page 11, line 319. Add the error for the change in C ANT.

We agree with this correction.

7 Towards an enhanced “Ocean Acidification” in the WTSP?:
Pages 11 and 12, lines 329, 331, 336, 337, 344, 345, 346, 347, 352, 357. “pH T ” instead of “pH”.
We agree with this correction.

Page 11, lines 341-342. Add the errors in the trends for fCO2, C T and pH T .
We agree with this correction.

Page 12, line 362. Add errors in the trends. They are given in the text of reference.
We agree with this correction.

Page 12, lines 363-364. Add the values of the change in Ω ara (with the uncertainty) that you obtained with your data. Give some explanation for the difference between your values and the ones obtained by Murata et al. (2015).
This section was probably unclear. The aim of this section was to discuss our estimates of “anthropogenic Ω_{ara} change” since the preindustrial period. Indeed, we do not discuss decadal Ω_{ara} changes which were not estimated here. The reason why we compared with the Murata et al. study was to point out the interesting longitudinal differences in the Ω_{ara} decrease observed in the recent years (1994 to 2009) in the OUTPACE area which are attributed, at least partially, to changes in sea surface temperature, that we do not observe on our long term estimates. However, we believe this section was confusing for the reader and we will removed this comparison with Murata et al. (2015).

Page 12, line 368. Add the migration rate observed by Feely et al. (2004) and the period of study.
In the study by Feely et al (2004), upward migration of Ω_{ara} horizons between the preindustrial period and present (late 90s) are evaluated by a method comparable to ours and values between 30 and 100 m are given for the Pacific Ocean. These values will be added to the manuscript.

8 Conclusion:
Page 12 line 375. “pH T ” instead of “pH”.
We agree with this correction