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Discussions

Interactive comment on "Two decades of inorganic carbon dynamics along the Western Antarctic Peninsula" *by* C. Hauri et al.

C. Hauri et al.

claudinehauri@gmail.com

MS No.: bg-2015-166: Two decades of inorganic carbon dynamics along the Western Antarctic Peninsula"

Line numbers refer to the track-change document (supplement)

This manuscript represents a very important evaluation of one of the highlights of longtem monitoring of the carbonate system in the Southern Ocean – the PAL-LTER program. The wider utilization of the summertime data to enable extrapolation to annual scales in conjunction with the more prolific surface pCO2 data illuminates the changing nature of the carbonate system and thus ocean acidification. The structure of the work done is very logical and well laid out. The carbonate system reporting and

data analysis is generally performed and well described in accordance with common practice. However, data normalization to deep water values between cruises is not performed. Certain broad assumptions are made regarding the development of carbonate system proxies, nutrient utilization and the physical setting that weaken the scientific merit of the paper and subsequent interpretation of the results.The language of this manuscript would benefit from a general sharpening of the text. The sentences are often long and statements and descriptions of scenarios are repeated. Overall, this manuscript is a valuable contribution to the scientific field and after I suggest that this manuscript be accepted for publication after successfully addressing or challenging the comments laid out below.

General comment: Regular mispelling of ueq – replace with _meq. Use pHT throughout to clearly denote the scale.

Response: We changed pH to pHT. We measured/reported TA in ueq/kg and it is therefore not a misspelling. Since ueq/kg TA is equivalent to umol/kg and because this is becoming the more commonly used unit, we changed it throughout the document.

Abstract P6930 L5 "this" dynamic system Done

L6 change "The discrete" to "Discrete" Done

L8 remove "Analysis shows". Propose "Large spatial gradients were seen in: : :" Done

L8 total alkalinity Done

L9 remove "from values" and bracket (<1 to 3.9) Done

L17 These were not "predictions". They were calculated values but even this is not necessary here. Just use aragonite saturation.

The seasonal values of aragonite saturation state were salinity-based predictions. To clarify, we added "Seasonal salinity-based predictions of Ωarag".

L19 again remove prediction. Replace with measurements? See above.

L23 replace "pointing towards" with "indicating"? substituted "with which could be"

L24 replace "provoke" by "induce"? Done

L25 remove "what" Done

Introduction P6931 L5 use general "change" Done

L9 higher trophic organisms. Krill and fish are not species. Done

L11 oceanographic Done

P6932 L1 not sure what you mean by "timing of sampling". Time of year?

Added Lines 132-133: "..., but possibly also the timing of sampling in relation to the timing of sea ice retreat and phytoplankton blooms"

L1-2 remove "dark" and "months". Done

L8 remove "a" and change timescale to timescales Done

L22. Replace "has" with "have". Done

P6933 L11 remove "of each transit" Done

L26 "variables" not "parameters" Done

P6934 L20 replace "calculations" with "procedure" or "program" Done

L24 remove "well" Done

P6935 L18 remove "of" Done

L24. They are "offsets" in CT and AT and not "errors" Done

L25 "differences" not "errors" Done

P6936 L6. There is no direct AT v T plot and no correlation information in Figure A2.

Response: The correlation information is in the text. We now point to the figure in

relation to the salinity-TA algorithm:

Lines 540-541: "Due to the weak correlation between PAL-LTER temperature and TA (r = 0.50), we based the TA algorithm on salinity only (Figure A2, r = 0.88)."

L20. An evaluation of the error in calculated pH would be useful here too. Done

Lines 552-554: "In combination with the pCO2 measurement precision of 3 uatm, the RMSE of TA prediction resulted in a mean error in calculated Ωarag of 0.0219 units pHT of 0.0043 [Glover et al., 2011]."

P6937 L10 variables Done

P6938 L3 Remove "above-presented" Done

L10 replace with "can decrease (increase) Done

L16 How robust is this assumption considering the high ammonium stock in the WAP region (e.g. Nutrients in the Southern Ocean GLOBEC region: variations, water circulation, and cycling Serebrennikova and Fanning, 2004)

Response: We agree with the reviewer that some data show surprisingly high ammonium stocks in this region. But typical levels are around ~1 uM/liter over much of the study area, which are much lower than NO3 (Serebrennikova and Fanning, 2004). Furthermore, net community production, which is therefore likely based on NO3 uptake, is responsible for the DIC drawdown.

This is clarified here: Lines 421 – 425: "Since nitrate is more abundant than ammonium in WAP surface waters [Serebrennikova and Fanning, 2004], nitrate was assumed as the nitrogen source. With a Redfield stoichiometry of 6.6 mol C/mol N then TA should increase by 1/6.6 = +0.15 mol TA per umol DIC consumed. Precipitation of biological CaCO3 material reduces both DIC and TA with the effect on TA twice as large as that on DIC (2 umol /umol)."

P6939 L1 How can you interpret this from Figure 5? The reader should not have to

evaluate this from interpreting depth from the salinity.

Response: As indicated in the figure, all dots that don't have a black or grey frame are upper-ocean data.

L2 Where is this "excess" AT coming from relative to the end members? Highlight this leading to the discussion on P6945.

Response: This and all other findings are discussed in the discussion (lines 801-815). We don't think that it is necessary to put more emphasis on this finding than on others in the results.

L25. Why have you used a constant PCO2 of 390? Why not use the relevant annual (or even better, seasonal) values over the measurement period?

Response: We think that constant atmospheric pCO2 is good enough for this back-of-the-envelop calculation, especially given the fact that we only have seawater pCO2 measurements from January or February, which are extrapolated to rest of the summer months.

L26 Is this globally averaged transfer rate representative of the Southern Ocean?

Response: The gas transfer rate used for this calculation is the estimated gas transfer rate for the Southern Ocean and is not as previously stated a global mean. We thank the reviewer for paying close attention and are glad that we caught this mistake.

Lines 480-484: "To account for DIC concentration changes due to gas exchange with the atmosphere, we assumed a constant atmospheric concentration of 390 μatm between 1993 and 2012, and a gas transfer rate (k) of 5 ($\pm$1) milli-mol CO2 m-2 μatm-1 month-1, which is the estimated mean rate for the Southern Ocean area south of 62 $\circ$S [Takahashi et al. 2009]."

P6940 L3 As the MLD can be easily calculated from the CTD profiles, why choose a "d" of 50m. The episodic nature of wind-stress and a rapidly evolving MLD require that

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a much more locally informed, at a minimum a monthly climatological value should be used.

Response: There are no CTD profiles for the months November and December. We therefore chose to use the published value of summer average mixed layer depth.

P6943 L7 sDIC We don't understand this comment.

P6944 L14 replace "overlapped" with "coincided" Done

P6946 L18 replace "what was" with "that" Done

L25 Calculate not predicted pH replaced with "estimated"

P6947 L2 Replace "Additional decades" with e.g. " A longer measurement period" Done

L4 replace "predicted" with "calculated" replaced with "estimated"

L16 "to" be able to Done

Table 1. Legend: Remove "statistics for" Done

Why were only selected years chosen for Figure A1? Figure now shows all years.

Table 2. According to your criterion, none of the trends are statistically significant. This needs to be stated more clearly. Why are the regional trends not shown? These are much more important than the dataset mean.

Response: "3.5 Temporal Trends" states this clearly: Lines 630-637: "Trend analysis of the PAL-LTER data showed no statistically significant annual trends (at the 95% confidence level) in the measured carbon parameters, temperature or salinity in surface waters in summer between 1993 and 2012 (Table 2)."

Trends from the north are not shown because PAL-LTER data is only available from the central sub-region.

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Figure 2. These plots clearly show the offset between cruises in the deep water. Why were the data not corrected according to the practice adopted for CARINA, for example? Or can you show that the offset are due to spatial differences?

Response: Since 1980s, DIC measurements were calibrated using the CRM (produced by A. Dickson, SIO), which was, in turn, based upon C. D. Keeling's manometric CO2 determinations. The CRM used are reported to be accurate to ± 1 umol/kg. The WOCE/CLIVAR section and Palmer time-series ocean DIC data presented in Fig. 2 are all based upon the CRM, and the precision of the shipboard DIC measurements has been estimated to be about ± 2 umol/kg. Although measurements are also subjected to expedition-to-expedition variability, differences in DIC values exceeding ± 3 umol/kg may be attributed to time-space variability of the ocean. We will add a brief mention of underlying commonality of the CRM calibration to the text (Line 241).

As noted by the reviewer some inorganic carbon synthesis projects such as GLODAP and Carina have used deep-water cross-over analysis and related techniques to generate suggested corrections for DIC. In the regional deep-water DIC data shown in Figure 2, we did not feel that there were sufficient offsets between the Palmer DIC data and the WOCE/CLIVAR DIC to warrant any offset.

Figure 3. This is not a very clear figure. The data density is too great and the colour coding is too similar for many of the years. Please simplify or remove.

Response: We agree with the reviewer. We changed the color-coding to all black (now Figure 3) and just pointed out a few special data points that were mentioned in the text.

Figure 5. This figure does not, contrary to its legend, depict the physical and biological controls on inorganic carbon chemistry We removed the title.

Figure 6. Similarly, the legend is misleading. Not all the processes leading to the movement in TA/DIC space are of biological nature. The grey dots and lines should have a slightly darker shading.

We removed the title and adjusted the figure.

Figure 7. Please explain better the plot in the legend. "Nutrient consumption" is incomplete and incorrect regarding the lower plot. "Nutrient consumption" is removed

Figure 8. There are no "dynamics" shown in this plot. Replaced with "system"

Figure 9. Here is stated that after "clear outliers were removed". In both plots there are differences between the two approaches of 150ppm. What criterion was used to define that these were also not clear outliers?

Response: As stated in the text, we removed all outlier that showed a bigger difference than 150 ppm. To make this more clear, we added the following sentence to the figure caption:

Lines 1159-1160: PAL-LTER pCO2 outliers that underestimate/overestimate pCO2 relative to the underway observations by more than 150 uatm were removed

Please also see the attached figure, which shows that the difference between the two datasets is < 150 uatm after removing the outliers.

Figure A2. Remove "prediction". Plot a. is the specific alkalinity relationship. Plot b. x-axis label "observed" Done

Figure A3. Correct "temperature" Done

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C5250/2015/bgd-12-C5250-2015-supplement.pdf

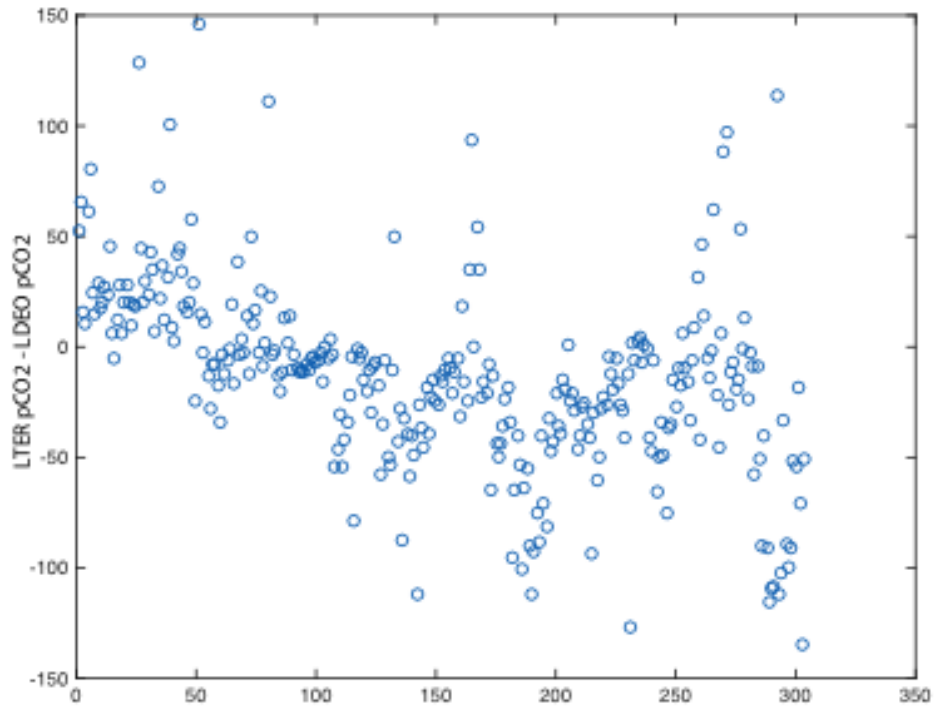Interactive comment on Biogeosciences Discuss., 12, 6929, 2015.

Fig. 1.

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